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PROCEEDINGS OF THE NINTH INTERNATIONAL CONGRESS
OF LINGUISTS
CAMBRIDGE, MASS., AUGUST 27-31, 1962

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CORNELIS H. VAN SCHOONEVELD

STANFORD UNIVERSITY

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PROCEEDINGS
OF THE
NINTH INTERNATIONAL
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OF LINGUISTS

CAMBRIDGE, MASS., AUGUST 27-31, 1962

Edited by

HORACE G. LUNT



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PREFACE

These *Proceedings* were to have contained the full versions of all papers which were actually presented at the meetings in Cambridge on August 27 to 31, 1962. Unfortunately, three of the participants did not submit their papers in any form, so that only the titles appear here, in the Table of Contents. Other authors did not hand in their full texts: the abstracts they sent in before the Congress have been reproduced from the *Preprints*.

Contributions have been printed in essentially the form they were received. The Editor undertook only to correct mechanical slips and to remove the most glaring foreignisms. The responsibility for proof-reading lies with the individual authors; the Editor has done his best to see that all emendations and corrections have been made according to the authors' indications.

The Table of Contents constitutes at the same time the revised Program of the Congress, recording the events that actually took place and listing all who took part in reading and discussing papers. It is unfortunate that the listing of discussants is probably faulty, since secretaries in some meetings put down only the names of participants who handed in written comments. A few secretaries gave no information at all about the sessions, and the Editor has had to impose an arbitrary order on the "interventions" received.

The discussions themselves are represented very unevenly in this volume, although it was intended that they should be given in considerable detail. Perhaps the majority of the participants did not hand in their remarks at all: here belong most of the replies of the original speakers whose papers were being discussed. Many interventions were mere corrections and small comments which were taken into account when the speaker made the definitive version of his paper for printing. The few of this sort which were received by the Editor have been eliminated. It was also necessary to omit the detailed queries to which no answer was recorded and the precise answers to unrecorded questions concerning details of specialized interest. Some of the discussions ranged away from the main topic of the paper concerned; the fragments of such exchanges submitted by a few of the individual participants made little sense without the context and have therefore not been printed. Many of the remarks in discussions after papers which we have available only in abstracts also had to be left out. And finally the Editor has assumed the responsibility not only of shortening many inter-

ventions by cutting out the laudatory and congratulatory remarks, but of eliminating some of the repetition, the purely discursive material, and the proliferation of examples of phenomena which already seemed to have been well illustrated. Perhaps in a couple of instances this procedure has made it impossible for the reader to sense the urgency and controversial character of some of the points of contention which the participants and spectators felt keenly, yet the Editor feels strongly that, given the failure of participants to furnish the full material, he has preserved the spirit and all significant content of the discussions which are represented in this volume. The editing and proof-reading of the discussions has been his responsibility.

Materials concerning the demonstrations and other meetings that took place during the Congress, as well as such matters of interest to linguists as the announcement of the formation of the Asociación de Lingüística y Filología de América Latina, have not been included.

The Editor wishes to express his thanks to colleagues at Harvard and MIT for their help in organizing and proof-reading the volume.

*Boylston 301
Harvard University
Cambridge, Massachusetts*

HORACE G. LUNT

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WELCOMING REMARKS

JULIUS A. STRATTON

President, Massachusetts Institute of Technology

Professor Locke, President Haugen, ladies and gentlemen:

I have the privilege of being among the first to greet you this morning, and the welcome that I bring to you comes from the whole of this university community of Cambridge and Boston.

The Massachusetts Institute of Technology joins with Harvard University in the hope that you will remember this Ninth International Congress not only for the importance of the papers and discussions, but also for the warmth with which you were received by your friends and colleagues.

Perhaps the most remarkable – indeed the most surprising – circumstance about this gathering is the very fact that one of your hosts should be an institution such as M.I.T. That we, with our roots traditionally in science and engineering, should have become so deeply involved in the field of linguistics is not only revealing evidence of an evolution in the character and expanding interests of M.I.T. but is also, I believe, indicative of the development of linguistics itself as a science.

Now I must tell you that our entry into the field, our present deep involvement, was not part of any initial long-range plan or farsighted calculation. It came about simply through a logical sequence of events – the inevitable consequence of our work in the communication sciences. This goes back a long time, and as in all such developments, it derives from people.

Both before and after the Second World War, Norbert Wiener had an enormous influence in developing an understanding of and exciting an interest in the basic problems of communications.

Vannevar Bush, in the years when he was a Professor of Electrical Engineering, was one of the first to grasp the potentialities of computing machines, to foresee their uses, and most importantly, to stimulate an interest in the underlying theory.

Claude Shannon, first as a doctoral student of Professor Wiener, then at the Bell Laboratories, and recently, I am happy to say, as a member of our faculty, has, as you know, made fundamental contributions to the formulation of information theory.

Through these pioneers and many others on our faculty that you know and that I might name, we have advanced step by step into almost every domain of communica-

tion theory until this now constitutes a central theme of research at M.I.T. – one of our major fields of interest.

From the beginning we have viewed the problems of communications in their broadest context. We have, of course, being the kind of institution that we are, been concerned with the mathematical theory, with communications engineering in the conventional sense, with computer logic, and with the analytical as well as the practical problems of machine translation. But always there has been an interest in the implications of information theory for physiological systems – many of you are familiar, I am sure, with Professor Rosenblith's work in sensory perception and its relation to intelligence, for psychology, for the basic processes of learning.

Now there could hardly be a Center for Communication Sciences without a major involvement in the problems of human speech communication and language – any more than there could be a great development of linguistics at M.I.T. without a major interest in the communication sciences. Over the past fifteen years our Research Laboratory of Electronics has generated much of the mathematical theory and provided the experimental facilities.

It is an interesting reflection upon how projects develop in a university that our particular interest in linguistics grew out of research carried on in a department of electrical engineering, but again it is an accident of people and of intellectual leadership. Professor Jerome B. Wiesner, who was the Director of the Laboratory and who is currently Science Advisor to the President of the United States, had the vision and the interest to encourage these developments. To Professor Locke, who from the first maintained an association with the Research Laboratory of Electronics and as Head of the Department of Modern Languages has been concerned not only with the teaching of language but also with the development of linguistics, we owe a debt for his constant and effective co-operation.

We are fortunate to share with Harvard the tremendous abilities and knowledge of Professor Jakobson. The interest of research has drawn such men as Noam Chomsky, Morris Halle, and Victor Yngve.

We are fortunate also, I believe, in an intellectual climate at M.I.T. that is favorable to free interchange among disciplines. There is a risk, of which I am conscious, for college presidents on occasions such as this of indulging in eulogies of their own institutions. Yet if there is one quality of the contemporary M.I.T. that seems to me most characteristic and appealing, it is just this freedom from the barriers of departmental and disciplinary organization. And for no subject is this perhaps more important than for linguistics, which must draw so widely for its sources – from mathematics, from electrical engineering, from psychology, from the arts.

These, then, are briefly the origins of linguistics studies at M.I.T. Teaching began with informal seminars, and these have extended to an array of regular subjects. There was an increasing demand on the part of students for a formal program. One year ago, upon the recommendation of an *ad hoc* committee that included your President Haugen, we established a graduate program leading to the Ph.D. in linguistics.

All this that has happened at M.I.T., it seems to me, is in keeping with the progress of linguistics itself. Here is a field of scholarship moving from the descriptive phase toward the quantitative and the experimental – in short, toward the status of a mature science.

But from this account, too, we can perhaps see something more. For another bridge is forming between the arts and the sciences. Language opens to us the ideas and the cultures of all ages, of all countries, of all peoples. And now through the study of language – through linguistics, we are making free a continuous path from science to art, from art to science, from the most abstract and mathematical, on the one hand, to the ideas and the literature of humanity on the other.

And so with these thoughts may I, in closing, express again the hope that you will find this a most rewarding experience and a most enjoyable sojourn in Cambridge. May you come again.

INTRODUCTORY REMARKS

EINAR HAUGEN

President of the Ninth International Congress of Linguists

Ladies and gentlemen:

It is a great pleasure indeed for me to be able to welcome this large assemblage of scholars to the Ninth International Congress of Linguists. I am deeply conscious of the honor, but also of the responsibility that has been thrust upon me of directing this first international meeting of linguists in the United States. Fortunately I have been relieved of most of the hard decisions that have had to be made by the invaluable assistance of the other members of the Executive Committee, who are seated on the rostrum here beside me. I should like to introduce these to you, beginning with our Secretary General, Professor William N. Locke, chairman of the Department of Modern Languages at the institution where we are now meeting. The others are professors Albert H. Marckwardt, until recently of the University of Michigan, now at Princeton University; William G. Moulton, of Princeton University; and Uriel Weinreich of Columbia University. All of us have reason to be grateful to the Secretary, Professor Morris Halle, of this institution, whose life has been almost wholly devoted to the Congress over the past year.

Our work in preparing for this Congress has only been made possible, however, by the good will of certain institutions, of which the Permanent International Committee of Linguists is one of the most important. Among those who are familiar with it, it is generally known by its acronym, CIPL [ˈsɪpl], based on the initials of its French name. CIPL is represented here by its president, Professor Alf Sommerfelt of the University of Oslo, and its permanent secretary, Professor Christine Mohrmann, of the University of Nijmegen. CIPL is ultimately responsible for the organization of this and all future international congresses of linguists. It will offer a report on its activities at a business meeting on Thursday afternoon. CIPL is affiliated with another alphabetic organization, the CIPSH, or the Conseil International de la Philosophie et des Sciences Humaines, which in turn, is an arm of UNESCO. Through this hierarchy of organizations, each one of us can take pride in being affiliated with the United Nations in its great work for peace and understanding throughout the world.

In this country The Linguistic Society of America stands as the sponsor of our congress. It is represented here by its current president, Professor Marckwardt, and its secretary, Professor A. A. Hill of the University of Texas. This society, which has been

in existence since 1925, is affiliated with the American Council of Learned Societies, which has made the Congress possible by its generous support. The National Science Foundation in Washington has also been most helpful in supporting the Congress, as have the International Business Machines and Pan-American World Airways. Holding such a Congress in the United States is rather more of a problem than in Europe, and we are therefore deeply grateful to these sponsors because they have made possible a truly international meeting. Finally, we owe a real debt of gratitude to the two universities which joined forces and invited the Congress to Cambridge, Massachusetts, namely Harvard University and the Massachusetts Institute of Technology. Both of these have star-studded faculties in language and linguistics; Harvard's tradition is older than M. I. T.'s but M. I. T. has compensated for it by a remarkable vigor of research in linguistic theory. Both universities have made available their physical plant and their spiritual resources to help make this a successful congress.

Although this Congress is the ninth of its kind, it is, as I already said, the first in the United States. All preceding congresses have been held in Europe, the first at the Hague in 1928. Each of the rest was held in a different European country, in cities with long traditions of eminent linguistic research: Geneva, Rome, Copenhagen, Brussels, Paris, London, and Oslo. I believe it is not saying too much to suggest that its removal to the United States is a recognition of the work of a generation of linguists which can be dated back some thirty to forty years. Among the landmarks of this development we may mention the appearance of Edward Sapir's *Language* in 1921, the organization of the Linguistic Society in 1925, and the appearance of Leonard Bloomfield's *Language* in 1933. But in the vigorous expansion which linguistics has enjoyed in recent years, we should not lose sight of the fact that one of the giants of linguistics was an American of a much earlier generation, William Dwight Whitney of Yale, whose *Sanskrit Grammar* of 1879 is still a classic and whose *Life and Growth of Language* of 1874 was influential even in Europe and is still highly readable.

It is a particular gratification to me to be able to welcome linguists from so many countries to this meeting. While some aspects of our science may be forbidding, I have rarely found a linguist who was. The chance to meet one another, to listen and to discuss, seems to me to be the chief value of such a gathering as this.

Our common subject, the science of language, brings us together. As linguists, we not only study sentences, we also generate them. No other science is in this peculiar position, which has been described by someone as "carrying coals in a wooden bucket". No one should be more aware of the pitfalls offered by language as an accurate expression of thought than the linguist. In the words of Lichtenberg, "unsere ganze philosophie ist berichtigung des sprachgebrauches", or as we might put it, science advances by escaping the trammels of language. It does this by mathematical formulations, which are more and more coming into use in linguistics also, as the papers at this Congress will demonstrate.

Yet in most of our thinking and utterance we are bound to the use of a particular language. That language, whatever it may be, reaches as far as there are people who under-

stand it. But every language now in use stops short of reaching all mankind. In this way we can say that language unites, but languages separate. Our study comprises both of these aspects, that which unites languages and that which separates them. We should therefore, of all scientists, be those who best could learn to understand one another, to transcend the barriers of language, and to reach the truth about our theme.

Alas, this has not always been the case. The metalanguage of linguistics has fallen apart into dialects, thanks to the usual reason for dialects, viz. reduced communication. Some of this has been due to the same barriers which have so grievously separated mankind into mutually incomprehensible speech communities. Some of it has been due to cussedness or creativeness (the distinction is hard to see sometimes) on the part of individual linguists. But some of it has been due to a cliquish insistence on one's own school of thought and an unwillingness to yield a point to the thinking of one's colleagues in other schools. European linguistics fell apart into neogrammarians and neoidealists, a school of Geneva and a school of Prague, one of London and one of Copenhagen, each creating to some extent a whole set of terminology *ab ovo*. If American linguistics is a concept at all, it is merely one more step in this fractionization into scientific metadialects.

One of the most remarkably uniform linguistic areas in the world is the country of Iceland. While there are dialectal differences, there are no dialects in the usual sense, although its population has been stable for a thousand years. One reason for this state of affairs is certainly the annual meeting of its citizens known as the *Althing*, the "meeting of all", at which disputes were settled and business transacted. I see in this international congress a counterpart on a larger scale of the function of the *Althing*. We are met here to settle our disputes and to transact the business we have in common. It may be hoped that even though our meetings are only quinquennial, they will also have the incidental effect of reducing our terminological confusion and increasing our mutual understanding.

If you have studied the program, you will already have appreciated that the papers are organized into a hierarchy almost as complex as that of language itself. Each day at 10:15 we will meet here together to hear the five invitational papers introduced by their authors and discussed by as many participants as time permits. All other papers have been volunteered by their authors and accepted by the Executive Committee. At 2:15 those which fitted into the topics proposed by the Committee have been scheduled for section meetings. Three or four of these are scheduled simultaneously, so that you will have to make a choice between them. At 9:00 each morning the rest of the accepted papers have been scheduled for group meetings, where the actual papers will be read by their authors. Since time is short, the chairmen of the various meetings will have the obligation to maintain the schedules established. We appeal to each member to make their task lighter by imposing on him or herself the time limits stated. Linguists are notoriously bad listeners, being more interested in producing sentences than in receiving them. But they should remember that brevity is not only the soul of wit; it is also the heart of courtesy.

Since we are acutely aware that there are limits to human endurance, even of linguists, and that there are associate members whose patience has been tried by a steady diet of linguistics at home, the Local Committee under Professor Locke has provided for a variety of entertainment and relaxation. These will be of special interest for those of you who come from abroad, or from partibus infidelium in this country. I recommend that you study your programs carefully and take part in these activities. We have set aside one whole afternoon for an excursion to the old Sturbridge Village, and nearly every evening has some form of participation at which you can meet under pleasant circumstances.

With these words I have the honor and the pleasure to welcome you all and declare the Ninth International Congress of Linguists opened.

QUELQUES MOTS D'ACCUEIL

WILLIAM N. LOCKE

Secretary General of the Ninth International Congress of Linguists

Le President Haugen a eu l'amabilité de me permettre de vous dire deux mots d'accueil en français.

Au nom du comité exécutif et du comité d'organisation je prends encore une fois la parole devant vous pour exprimer notre joie aujourd'hui à l'occasion de l'ouverture du Neuvième Congrès de Linguistes, tenu pour la première fois aux Etats-Unis.

La linguistique est une science relativement jeune, et plus jeune encore en Amérique, qu'en Europe et aux Indes. Le fait que CIPL a bien voulu demander à l'American Linguistic Society d'organiser le Neuvième Congrès est signe pour nous, que les linguistes du monde estiment que l'Amérique commence enfin à faire une contribution non-négligeable à cette science. Par le nombre et par la qualité des papiers soumis par des Américains à ce Congrès nous espérons justifier votre choix de notre pays. Les linguistes des universités Harvard et M.I.T. espèrent justifier au même titre le choix de nos universités comme hôtes du Congrès.

J'ai donc le grand honneur, dont je tiens à remercier M. Haugen et mes collègues du comité exécutif de vous souhaiter la bienvenue aux Etats-Unis et à Cambridge et de formuler un vœu pour le succès du Congrès, le souhait aussi que chacun d'entre vous emporte à la fin du Congrès un souvenir agréable de l'Amérique.

ON THE METHODS OF INTERNAL RECONSTRUCTION

JERZY KURYŁOWICZ

1. The goal of linguistic reconstruction is to establish the *relative chronology* of prehistoric stages and the changes immediately preceding the most archaic data. To speculate on the *origin* of grammatical categories such as gender, aspect, mood, and so on, is a glottogonic enterprise which, since it is of a highly problematic character, should be kept out of historical and comparative grammar.

The expression "internal reconstruction" has been used as a technical term by Pisani¹, Hoenigswald², Bonfante³ and others (cf. also the "innere Gründe" of Porzig,⁴ the "indizii intrinseci" of Pagliaro⁵) to denote the diachronic conclusions that may be drawn from a synchronic analysis of linguistic data without or before having recourse to comparison, linguistic geography and "areal linguistics", and glottochronology.

Methods of internal reconstruction have been applied in an increasing degree, more or less consciously and explicitly, by neogrammarians. Thus, e.g., they have rejected the possibility of spontaneous phonetic split and acquired, by means of the concept of "phonetic law", a pretty good knowledge of the usual phonetic changes. Similarly, on account of the ever widening field of linguistic research, more and more attention has been paid to general trends in the domain of semantics.

The opposition between diachrony and synchrony, implying differences of linguistic aims and methods, is not immediately given by the material at our disposal. The transitional and at the same time fluctuating character of the linguistic phenomena, the hesitation between residuary and unproductive procedures, on the one hand, and innovations and living rules, on the other, has been frequently stressed. Everywhere a complete "synchronic" description of a language must have recourse to the notions of archaism and innovation. The ousting of an old form by a new one is not a momentary event but a process extending over time and space. Looked at from the historical point of view, linguistic material, however restricted in time and space, is composed of chronological layers. To realize this point it is sufficient to run over some pages of a careful description of a modern language.

¹ *Paleontologia linguistica*, 1938, 32f.

² *Studies in Linguistics*, II (1944), no. 4, 78.

³ *Word*, 1 (1945), 133.

⁴ *Die Gliederung des idg. Sprachgebiets* (1958), 58.

⁵ *Sommario di linguistica arioeuropea* (1930), 174.

Faced with the task of reconstruction of older linguistic stages, scholars have been aware of the difficulty of a proper selection of material. Forms which could have originated independently in each of the related languages as the outcome of productive derivational rules, e.g. γόνοϛ = Vedic *jána-*, do not testify to the existence of an I.E. prototype (**ǵóno-*). Meillet, among others, has taught us that reconstruction of pre-historical stages is to be based on exceptions and anomalies rather than on the grammatical rules of a language. Another important principle is that if a form is restricted to residuary contexts (idioms) only, whereas the use of its synonym(s) is free, this form must represent an older stage.

Such methodological principles belong to what is called here "internal reconstruction" in the *broad sense*. They cannot be applied in every particular case, but once they are applicable, the results obtained are of a higher cognitive value than the conclusions reached by statistics, areal linguistics, or linguistic paleontology, which are of a stochastic character. In case of contradiction the inference drawn from internal reconstruction will be decisive.

2. The last decades have witnessed essential progress in linguistic theory. While the validity of the above-mentioned methodological devices still remains intact, certain structural notions which have recently gained ground profoundly differ from the conceptual implements of the neogrammarians, cf. *system, predictability, opposition* (phonemic, semantic), the difference between *phonemic* and *morphophonemic* law (e.g. between *vocalic alternation* and *vowel-gradation*), and so on. Let us quote here an example of a concept completely foreign to the old school: the concept of intermediate classes. In many languages the prepositions and the conjunctions occupy an intermediate place between non-autonomous ("synsemantic") morphemes like suffixes and endings, and full words like the noun and the verb. Similarly in phonemics: the labiovelars of certain languages may function on the one hand as elementary phonemes, parallel to labial, dental, or velar stops, and on the other as groups of phonemes (*k+ɥ, g+ɥ*) parallel to *tɥ, dɥ*. – In some languages the phoneme *h* and the aspiration of stops is to be regarded as one element functioning both as a consonant and as a phonemic feature. – In other languages *ɨ* may similarly appear both as an autonomous phoneme and as the feature of palatality. – The so-called "concrete" cases form a class intermediate between adverbs and the so-called "grammatical" cases.

New formulations and new solutions of problems of reconstruction are to be expected. The notion of system in itself presupposes or excludes certain stages or changes admitted by the comparative grammar of I.E. 30-40 years ago, when the analysis of facts taken in isolation easily led to unwarranted conclusions.

To illustrate the application of the modern structural approaches and attitudes to linguistic reconstruction, we shall analyse a series of well-known problems: phonemic, morphonemic, and morphological, of I.E. comparative grammar, and try to point out the practical utility of certain notions which may prove helpful in solving them. The chief aim in view is *relative chronology*.

3. Much linguistic experience has been accumulated in the domain of phonology owing to the limited range of possible phonetic changes and their mechanisms. Since types of phonetic evolution such as diphthongization and monophthongization, nasalization, palatalization, or consonant shift recur over and over again, the linguist looking for analogies and parallels may easily find them even in languages which in other respects are scarcely familiar to him. If he specializes in phonemics he is capable of mastering materials comprising hundreds of different languages.

But the interpretation of phonemic changes still leaves much to be desired. The pioneer work in this domain has been done chiefly by Hoenigswald and Martinet.

On the basis of a comparison of the Germanic consonantism with that of the other I.E. languages certain phonetic laws of the Germanic consonant shift have been established ($p, t, k >$ aspirated fortes; $b, d, g >$ voiceless lenes...). Now the goal of such comparisons ought to be not so much the correspondences between languages (Lat. $t-$ = Germanic $\bar{p}-$; Lat. $d-$ = G. $t-$, etc.) as the identifications and coalescences within Germanic itself versus differences in other languages – and vice versa. The point is that the phonemes involved in the change were correlated ($t: d$ etc.) in I.E.: in certain surroundings, e.g. in word-initial position, they contrasted as *voiceless: voiced*, in certain other positions (maybe in word-final position) the contrast was neutralized in favour of the voiceless member ($d > t$). The Germanic consonant shift represents essentially a reversal of the correlation t (unmarked): d (marked); the I.E. marked phoneme (d), henceforth charged with the *neutral* function, becomes unmarked. The identification of the inherited I.E. d (as in **dekm*) with the neutral dental stemming from I.E. **(e)st(i) = *(ni)sd(os)*, cf. German *ist, Nest*, entails the marked character of the inherited I.E. t , i.e. a reversal of the old correlation; hence the subsequent development of t as aspirated fortis. Later on the coalescence of the inherited I.E. bh, dh, gh with f, \bar{p}, h (from p^h, t^h, k^h), under the well-known conditions of accentuation, leads to the rise of Germanic b, \bar{d}, g , henceforward phonemically correlated to f, \bar{p}, h .

Thus the complete Germanic consonant shift consists of two identifications and not of four separate laws ($t > \bar{p}; d > t; dh > d; \bar{p} > \bar{d}$ [Verner's law]). Physiological speculations, such as the increased intensity of articulation, "prononciation à glotte ouverte"..., do not grasp the *linguistic* essence of these changes, the shift of the *internal* relations of the elements in question being the only pertinent fact. The external stimuli of the changes are extrinsic to the phonemic system. Once we leave language *sensu stricto* and appeal to extralinguistic factors, a clear delimitation of the field of linguistic research is lost. Thus, e.g., the physiological (articulatory) aspect may be a consequence of social factors, the latter being themselves due to certain political or economic facts (conquest, migrations involving bilingualism), caused themselves e.g. by climatic changes (drought, floods...), etc. The question is where to stop when explaining the Germanic consonant shift. It seems that the field of linguistic explanation in the literal sense must be circumscribed by the *linguistic* aspect of the change in question, i.e. by the actual state of the system before and after the

change ("l'état momentané des termes du système" – de Saussure). The change ought to be analysed and accounted for as a shift of the system, therefore attention should be paid above all to points of neutralization, to identifications, and to the rise of new phonemic oppositions.

The accumulating deviations from the traditional pronunciations are not linguistic changes *sensu stricto*. It is only owing to certain *identifications* within the phonemic system that they become pertinent features. The point of time of such a coalescence is to be considered as the moment of the linguistic change in the literal sense, when purely articulatory features are being *phonemicized*. From such a point of view the Germanic consonant shift is synchronous with the coalescence of I.E. *st* and *sd(zd)* and, similarly, "Verner's law" and the change of I.E. *bh, dh, gh* to *b, d, g* are phonemically one and the same phenomenon.

Another example: the Romance vowel system. The key phenomenon is the coalescence of the close and the open vowels in unaccented syllables, thus Lat. *ī, ū* > *e, o*; Lat. *ē, ē* > *e*; Lat. *ō, o* > *o*. The close vowels *e, o*, appearing in position of neutralisation (= in unstressed syllables) become the unmarked members of the phonemic contrasts (*e*: *e*; *o*: *o*). Confronted with classical Latin, where *ē, ō* are marked, and *e, o* unmarked, the Romance system is the result of a reversal of the old correlation: *e* (unmarked) > *e* (marked) etc.

The internal character of reconstruction consists in avoiding any recourse to external, extralinguistic facts, including articulatory facts. The latter may only be inferred from the phonological data. The fact that *t* became in Germanic a fortis, and *d*, a lenis, i.e. that the first became the marked phoneme (aspirated etc.), can be deduced from the coalescence of I.E. *-(s)t-* and *-(s)d-* in favour of Germanic *t*. The correlation *t:d* never ceased to exist. But the zone of neutralization was shifted in favour of Germanic *t* (corresponding to I.E. *d* and *t*).

4. By limiting itself to the phonemic aspect, internal reconstruction avoids slips like those which are continually committed in respect to the problem of I.E. *ḱ, ḡ, ḡh*. According to a widespread opinion these phonemes originated in the satem-group of I.E. through the palatalization of the velars *k, g, gh* before front vowels *ē, ĭ* (or *ī*), whereas in all other positions they are due to morphological factors ("analogy"). In such a way, having recourse to allophones and morphological factors only, one eludes the main question: how did the allophones *ḱ(e), ḱ(i)*, as against *k(o), k(u), k(t)*, etc., get phonemicized, i.e. how did they become autonomous phonemes? In actuality, the problem is more complicated because of the second question: what is the *phonemic* aspect of assibilation, of the rise of phonemic relation between *ḱ, ḡ, ḡh* and the only inherited fricative *s*, in the several satem-languages? The fundamental facts here are coalescences like Skt. *piṣṭá-* < **pis-tó-* and **piḱ-tó-*, Lith. *-rš-* < **-rs-* and **-rḱ-*, etc.

5. Traditional reconstruction may be rendered possible on the basis of a careful

examination of a single language. An analysis of Indo-Iranian data seems to prove the fact that before the action of Bartholomae's law the voiceless aspirates *ph*, *th*, *kh* did not exist in this linguistic group. Since in the case of *bh* + *t* becoming (**bhdh*) > *bdh* a *progressive* assimilation takes place, whereas the assimilation of sonority (voice) is in I.E. *regressive* (*b* + *t* > *pt*), the obvious inference is that *bh*, *dh*, *gh* were not *phonemically* voiced elements at the time of the action of the law. They may have been *phonetically* voiced like *r*, *l*, *n*, *m*, which do not influence a preceding *t* inside the word. Now if *bh*, *dh*, *gh* were not phonemically voiced, it means that they had no voiceless counterpart *ph*, *th*, *kh*. If the contrast *ph*: *bh*, *th*: *dh*, *kh*: *gh* had existed before the period of Bartholomae's law, the result of the assimilation *bh* + *t* would have been (**phth*) > *pth*.

This means that the voiceless aspirates *ph*, *th*, *kh* could not have been a part of the original consonantal system of I.E. On the other hand, Bartholomae's law reminds us of certain restrictions imposed on the structure of I.E. roots: voiceless and voiced aspirated stops are incompatible with each other as root-initial and root-final consonants (impossibility of roots like **peudh*, **bheut*, and so on). It therefore looks as if Bartholomae's law was a special case of assimilation, viz. of assimilation in *contact* (*bh* + *t* > **bhdh* > *bdh*; also *p* + *dh* > **bhdh* > *bdh*), whereas the above incompatibility of *bh* and *t* in I.E. roots seems to presuppose a *distant* assimilation. Therefore, if suffixes like *-to-*, *-ter/tor-*, *-tro-*, and so on, adopt the form *-dho-*, *-dher/dhor-*, *-dhro-* conditioned by the final voiced aspirate of the preceding root, we may assume that the so-called "determinatives" (= enlargements of elementary roots) underwent similar modifications depending on the root-*initial* consonant. If such an assumption is valid, then Bartholomae's law must go back to I.E. It is worth mentioning that if we replace the voiced aspirates by emphatic stops we get similar laws of root-structure for Akkadian (von Soden, *Grundriss der akk. Gramm.*, 1952, p. 53, sec. 51 e).

6. There are cases where the comparison of a language with those related to it, points to the prehistoric rise of a new phoneme in the given language, followed by its subsequent prehistoric disappearance or its identification with an inherited phoneme. In this way the traces of its prehistoric existence are completely obliterated. The problem of Skt. *kṣ* = Greek *κτ*, *χθ*, *φθ*, may be reduced to the following equations:

$$\begin{aligned} \text{Skt. } kṣiti\text{- but } śyená\text{-} &= \text{Greek } κτίσις \text{ like } ἰκτιῖνος \\ \text{Skt. } kṣam\text{- but } hyáh &= \text{Greek } χθών \text{ like } χθές \end{aligned}$$

The Greek prehistoric dental corresponding to Skt. *ṣ* is thus the result of a *coalescence* of inherited *s* and *ḷ* in a determined position, the result of such an identification being a new autonomous phoneme */*ś/*. Later on, this phoneme coincides with *t* (probably in connection with the palatalization of the inherited *t* in certain surroundings).

7. Let us pass to the more important, because *central*, problems of I.E. morphonology and morphology.

Vowel-gradation, being a morphological phenomenon, stands in a close relation to both phonemic *alternations* and morphological (derivational and inflexional) oppositions.

Alternation, a purely phonological fact, consists in an exchange of phonemes (not allophones!) under determined conditions, its basis being formed by the neutralization of a phonemic contrast. Cf. the replacement of *b, d, g* by *p, t, k* in word-final position in Polish and in Russian. Whereas in other surroundings *p, t, k* are negative, and *b, d, g* positive (marked), in word-final position *p, t, k* are neuter. Therefore the phonemes *p, t, k* have two functions: the primary neutral function Φ and the secondary negative function Φ_1 contrasting with the positive function Φ_2 of the marked phoneme.

But alternations may be charged with a morphological function. This happens when the related forms containing the phoneme Φ_2 (positive) and Φ_1 (negative-neuter) are in a pertinent morphological opposition. Let Φ_1 denote a short vowel, Φ_2 a long one, and Φ a short vowel representing the neutralization of quantity under determined phonological conditions (e.g. before a tautosyllabic sonorant or semi-vowel). The transition of the alternation $\Phi_1:\Phi_2$ into *vowel-gradation* (morphological lengthening) may be illustrated by the following diagrams:

	basic form: vocalism Φ_1
	derivative: vocalism Φ_1/Φ (alternation between Φ_1 and the neuter Φ) ⁶
Hence	basic form: vocalism Φ_1
	derivative: Φ_2/Φ

This result is to be accounted for by *polarization*, i.e. the tendency to create the strongest contrast possible between the derivative and the basic form. The neuter phoneme Φ alternating with Φ_1 is interpreted, by opposition to the Φ_1 of the basic form, as the neutralization of Φ_2 – hence the replacement of Φ_1 by Φ_2 in the derivative. The rule of derivation undergoes a change: besides affixation a supplementary morph is introduced into the derivative forms, viz. the lengthening of the short vocalism Φ_1 ($>\Phi_2$). In this way there are two groups among the derived forms, both playing an equally important part in the rise of the vowel-gradation Φ_1/Φ_2 : forms which owing to the presence of Φ allow a reinterpretation of the morphological procedure (“*voces mediae*” as it were), and others where the interpretation of Φ as Φ_2 is explicitly applied ($\Phi_1 > \Phi_2$).

In order to explain the origin of the various kinds of vowel-gradation it is of primary importance to establish the conditions of neutralization characteristic of the corresponding *alternations*. Thus the I.E. vowel-gradation *e:o* must be genetically explained by a corresponding alternation *e:ɔ*, and therefore the essential task is to determine the original point of the neutralization of the contrast *e:ɔ*. There are several possibilities in this respect, chiefly the coalescence of *e* and *ɔ* in the neighbour-

⁶ E.g. an alternation conditioned by open and closed syllables within the paradigm.

hood of certain laryngeals and the identification of the reduced vowels *e/o* in the neighbourhood of sonorants.

Unlike alternation, which is a purely phonemic fact, a phenomenon like vowel-gradation is of a complex character, the phonological trait being subordinate to the morphological function: within a wider range of morphological conditioning there is a narrower range of phonemic conditioning. The umlaut in the German plur. in *-er* (*Wälder, Götter, Güter, Häuser*) is conditioned 1) by the plur. ending *-er*; 2) by the back vowel of the root. The latter condition is subordinate to the former as back vowels occur only in *some* nouns with plurals in *-er*.

Vowel-gradation accompanying affixation is usually a redundant feature because, in a morphological series, forms implying vowel-gradation appear besides others characterized by affixation only, e.g. ἀπο-κοπ-ή < κοπ like ἀπο-τομ-ή < τεμ(ε); ὀπ-ωπ-ή "sight" like ἐδ-ωδ-ή "nourishment"; Skt. aor. *arātsīt* < *rādḥ* "to succeed" like *avapsīt* < *vap* "to scatter"; etc. The redundant character of apophonic changes must be kept in mind when investigating their I.E. origin. They originate and spread within a phonemically determined range, e.g. *o*-grade within roots with *e*-vocalism, the lengthened grade within roots with short vocalism. Since the spread of vowel-gradation is due to its redundancy, to its serving as a reinforcement of the difference between the basic and the motivated forms, we must assume that its original range, circumscribed by pertinent phonemic and morphological oppositions, has been relatively narrow.

8. An example like I.E. **liktʰos* (> **leiqʰ*) shows the following stratification of the three morphs characterizing this derivative: the fundamental morph = the suffix *-to-* which is indispensable in all occurrences of this formation; the suprasegmental morph (suffixal accentuation), neutralized under certain conditions e.g. if the form functions as the second member of a compound; finally the nil-grade of the root which is clearly a complementary morph as it does not appear in cases like **settʰos*, **pektʰos*.

The above example illustrates the hierarchical order of morphs which at the same time function as a whole when referred to the basic word (Skt. *riṅákti*, λείπω, Lat. *linquo*, etc.).

Inserted vowels ("Bindevokale") or consonants are subordinate to the fundamental morph, e.g. Skt. aor. *jambhiṣ-* < *jambh* + *s* + insertion of *i*.

The hierarchical order of morphs need not necessarily correspond to their linear succession. It may be symbolized by a rigorously fixed sequence of transformations (Chomsky-Halle): I.E. (**linékti* >) **leiqʰ* > **leik-to-* > **leik-tó-* > **lik-tó-*.

It is important to keep in mind the transformational aspect of this hierarchy as it permits us to account for important particularities of I.E. vowel-gradation. The *o*-grade of the I.E. diphthongal roots (such as **leiqʰ*, **leuk*) was liable to be decomposed by the speakers into **leiqʰ* > **liqʰ* > **loiqʰ*; **leuk* > **luk* > **louk* – i.e. to be interpreted as a deduction of the fundamental vowel *e*, resulting in the nil-grade of the root, plus a subsequent insertion of the vowel *o*. It is such an interpretation of

the *o*-grade that in the last resort accounts for the so-called Brugmann's law in Indo-Iranian.

Now the chain $T_1ei T_2 > T_1iT_2 > T_1oiT_2$ etc., makes it possible to build morphological *o*-grades *directly* upon the vocalism *i, u* without recurring to *ei, eu* which often are not attested at all. Cf. O. H. German *lûchan*: preterite (perfect) *louh*; *sûfan*: *souf*; *sûgan*: *souc*; O. English *brúcan*: *bréac...*; O. Irish the preterites (perfects) *lelag-*, *nenag-*, *rerag-* (root-vocalism *oi*) $<$ *ligid* "he licks", *nigid* "he washes", *con.rig* "he binds" (*i* everywhere).

9. One of the most important tasks in reconstruction is to establish the position of a form within the system, the pertinent morphological opposition in which it participates. The characteristic feature of a morpheme, of a word, etc., is the lack of functional homogeneity. Its function may be independent or dependent on the semantic (or syntactical) context. In the former case we may speak of the *primary*, in the latter case of a *secondary* function of the given form. Just as in the case of phonetic variants (allophones), the conditioning of secondary functions is to be defined in *positive* terms (= as an exception to the general rule). The primary function is based upon pertinent oppositions, the secondary upon the primary function plus context.

With reference to the secondary function the primary one may be of a higher or a lower grammatical rank. Thus, e.g., the plural morph (primary function: plural) may in certain contexts (e.g. when attached to certain nominal stems) reveal a collective sense (secondary function), or vice versa. There is only one category, the category of the plural in the first case, the category of the collective in the second. But the category of the plural is inflexional, while that of the collective, if it exists as a separate category, is derivational. The difference lies in the fact that the plural is proper to a whole part of speech (the noun) and represents an integral feature of its paradigm, whereas the collective is derived from certain nouns only. The noun (sing.) and its plural are two (inflexional) *forms* of the same word. The noun and the corresponding collective are two different *words* (the basic word and the derivative). Consequently, the morpheme of the plur. has a general and abstract meaning in comparison with the morpheme of the collective, more easily influenced by the semantic contents of the stem. Cf. collective formations differentiated according to the sense of the basic noun: inanimate, animate, personal, etc.

The transition of an original collective into a plural may be called *grammaticalization*, the opposite process, *lexicalization*. In Persian the collective morpheme *-hā* has been grammaticalized owing to its continuous spread, whereas the old plur. morpheme *-ān* has been restricted chiefly to personal nouns.

A relation similar to plur. (inflexional form): collective (derivative) is that between a case-form (e.g. Skt. *múkhāt*) and a *derived adverb* (e.g. Skt. *mukhataḥ*), between aspect and mode of action, between infinitive and noun of action, between participle and deverbative adjective, etc.

The two poles are constituted by inflexional morphemes, on the one hand, and completely lexicalized ("converted") morphemes on the other. In Balto-Slavic the morpheme *-ti* (*-tei*) is inflexional in the infinitive (*něšti*, *nesti*); in Latin it is a suffix of deverbative nouns (*-tiō*); in French it has become a part of the stem in *raison*, *poison*, etc. An intermediate position is held by derivatives of different hierarchical order depending on the range of the given formation. Sometimes it may be difficult to decide whether a form is inflexional or derivational. The Slavic action nouns in *-(t)ǫje*, *-(n)ǫje*, like Polish *krycie*, *pisanie*, may be formed from any verb, and function, in a certain way, as an inflected infinitive. But at the same time they govern the genitive (not the acc. of the direct complement) and have certain secondary semantic functions qualifying them to be treated as separate items in dictionaries.

10. As a rule it is legitimate to consider *inflexional* forms as former derivatives. As regards the rules of *derivation* they may represent an extension of older, narrower rules (in which case we are concerned with grammaticalization) or, on the contrary, they may be the result of the lexicalization of broader rules (even of inflexional categories).

It is a well-known fact that the I.E. plur. of the neuter nouns is more recent than the plur. in *-(e)s* of the common gender. From the point of view of its structure it is related to derived feminines and to abstract nouns (both denominal and deverbative). This relationship enables us to reconstruct nominal derivatives in *-ā*, *-ū*, *-ō*, *-ōs*, etc., functioning as abstract nouns (primary function) and as concrete nouns (secondary function) in semantic subgroups depending on the basic noun: *feminines* of animate or personal nouns, *collectives* of impersonal or inanimate nouns, maybe also "*singulatives*" of mass-nouns.

Not only the plur. of the neuter but also the fem. of the adjective is the result of grammaticalization. Although the fem. of a *noun* is only a *derivative*, the fem. of the adjective is in I.E. (at any rate in its later stage) an *inflexional* form. Analysis enables us to make a grammatical and partly chronological distinction between the three following forms in *-ā*: **néuā*, the fem. of **néuos*; **néuā*, the plur. neuter of **néuom*; *τομή*, the abstract of *τομός*. The oldest layer is represented by the type *τομή*, originally a derivative from *τομός*. A more recent stage are the fem. and the collective derivatives formed from substantives. The collective is grammaticalized in becoming an inflexional form (plur. of the neuter). Finally, the plur. neuter and the fem. in *-ā* are introduced into the declension of the adjective. A *derivational* form with the noun, the fem. becomes an *inflexional* one with the adjective.

A similar evolution and stratification is met with in Semitic: *-(a)t* is a derivational suffix forming feminine, collective, and singulative nouns, and at the same time an inflexional ending of the adjective serving not only to denote feminine gender but also (as in classical Arabic) the plural of the neuter by referring to inanimate or impersonal plurals.

11. Regardless of their specific function, whether semantic or syntactical, all I.E.

nominal cases are grammaticalized in the sense that all nouns of the same gender, common or neuter, distinguish the same number of case-forms.

The structure of the so-called weak and strong cases – as regards both theme and accentuation – reminds us of the relation between *derived* and *basic* forms. Now there are rather strong indications favouring the hypothesis that the oxytone and the barytone stems were originally distinguished only in the strong cases (*māter-*: *bhrāter-*), the weak ones having always been oxytone. Such a state of things is suggested by the mobile accentuation of root-nouns (*rúcam*, nom. plur. *rúcaḥ* as against instr. *rucá*, dat. *rucé*, gen. *rucáḥ*, *rucám*...); by the inflexion of Skt. *pánthāḥ*, gen. *patháḥ*; by adverbs like *dakṣiṇá* < *dákṣiṇa-*, etc. This fact seems to presuppose the adverbial and derivational character of the weak cases: originally they are adverbs derived from nouns (as represented by the strong cases). This principle is still valid in the historical period, cp. the adverbial suffix *-tós* (Skt. *-táḥ*), which in Prakrit became a regular inflexional ending. Whatever the accentuation of the underlying noun, adverbs in *-taḥ* are always stressed on the suffix, e.g. *agratáḥ*, *dakṣiṇatáḥ*, *madhyatáḥ*, *maryatáḥ*, *mukhatáḥ* (from barytones), just like *abhīpatáḥ*, *savyatáḥ*, *ṛbhutáḥ*, *śīrṣatáḥ* (from oxytones).

These two chronological strata of case-forms bring to mind the relation of inflexional case-forms to the grammaticalized prepositional expressions of the modern languages (Romance *de*, *a*; English *of*, *to*).

The primary function of the I.E. acc. is to denote the direct object (complement), an important secondary function being the expression of goal with verbs of movement. This is evident from constructions like *ápo dívam úd vahanti* (AV) “they lead the waters unto heaven”, the syntactical connection between *úd vahanti* and *ápo* being closer than the relation between the verb and *dívam*. The unity of expression for both functions, the acc. of goal as well as the acc. of direct object, may throw a light upon the origin of this case-form. Cp. the Spanish parallel *quiero á la madre* like *salgo á la calle*.

Generally speaking, conclusions may be sometimes drawn from facts of syncretism. The syncretism of the abl. and the gen. sing. everywhere except in *-o*-stems raises first of all the question of the hierarchy of these two functions in forms like Skt. *sénāyāḥ*, *agnéḥ*, *bāhóḥ*, etc. The hierarchy of the chief functions of the abl.-gen. may be represented as follows:

1) adnominal genitive (subjective, objective, possessive, partitive) → 2) *adverbial*⁷ genitive (partitive); and 1) genitive → 2) ablative.

The hierarchy 1) → 2) is to be inferred from the circumstance that the adnominal gen. (subjective, objective) is the result of the syntactical transformation of a sentence (Lat. *amicus peregrinatur* > *peregrinatio amici*; *hostem occidit* > *occisio hostis*) whereas the ablative functions as an adverbial⁷ determination only. The old gen. sing. Skt. *sénāyāḥ*, *agnéḥ*, *bāhóḥ* has therefore a primary function as genitive, just like the

⁷ Not *adverbial*.

English expression with *of* or the French one with *de*. *Genetically* we are of course obliged to admit everywhere an original local (ablative) function. Therefore the penetration of these case-forms into the semantic zone of the gen. (connected with the loss of expressiveness) entailed first of all its renewal in the ablative function⁸; thus in I.E. in the *-o*-stems, probably under the influence of the pronominal forms *tāt*, *yāt*, hence Skt. *vṛkāt*: *vṛkasya*. Prakrit extends this differentiation to other stems, *seṇāo* (abl.): *seṇâe* (gen.), *aggîo*: *aggissa*, *bâhûo*: *bâhussa*. Avesta *-ayāī*: *-ayā*; *-oiī*: *-oiš*; *-aoī*: *-aoš*; Lat. *-ī* (*-e*): *-īs*; *-ū*: *-ūs*; etc.

The conclusion to be drawn from this development is that the original (as against the primary) function of the Skt. case-forms *sénāyāḥ*, *agnéh*, *bāhólī*, etc., is the ablative function and that the loss of expressiveness due to the extension of the functional charge entailed the renewal of the case-form in its secondary (ablative) function. In Balto-Slavic the process is repeated. Penetrating into the domain of the genitive, the ablative in *-t* completely ousts the old *s*-form (thus becoming itself a genitive), and the difference between the abl. and the gen. of *-o*-stems is again abolished. To renew the ablative function the language has recourse to prepositional expressions.

12. The general rule for the inflexion of the adjective is that it is at any moment liable to undergo the influence of the noun. To put it briefly: if the fem. of the masc./neuter *-o*-stems is a stem in *-ā*- (Lat. *bonus*, *bona*, etc.), the *-ā*- must have originally served to form fem. nouns from masculines in *-o*-. Consequently, the type **ekūā*, the fem. of **ekūos* (Skt. *ásvā*, Lat. *equa*, Lith. *ašvā*) might be a derivational archaism (though of course younger than Greek *ὁ ἵππος*: *ἡ ἵππος*). Ousted from nominal derivation by the suffix *-i(y)-* (*vṛkī-*), the formant *-ā*- of the type **ekūā* must be regarded as residuary. On the other hand, it became grammaticalized owing to its penetration into the declension of the adjective. This evolution is confirmed by the subsequent development in Skt.: the use of the new derivational suffix *-ī-* spreads to the adjective (in *-ā-*), entailing a stylistic difference between *-ā*- and *-ī-* in the feminine form.

We thus get a glimpse of the relative chronology of the formants *-i(y)-*, *-ā*-, *-ī/yā-* serving as fem. suffixes or endings. The elementary suffixes used were *-i(y)-* and *-ā*-, whereas *-ī/yā-* must be regarded as a cumulation (conflation) of these elements and therefore as the most recent layer. As the cumulation of suffixes is an expedient to revive the expression of a secondary function, we must admit that in the fem. forms of athematic adjectives the *-ā*- of *-ī/yā-* represents an enlargement of the older suffix *-i(y)-*. Whereas the stems in *-o*- distinguished between the derivational *-i(y)-* (fem. of nouns) and the inflexional *-ā*- (fem. of adjectives), the athematic stems had in both cases the formant *-i(y)-* which, although originally of derivational character, was already charged with an inflexional function in the pre-historical period. The suffix *-ā*-, being unambiguous and therefore expressive, proved a fitting enlargement of the

⁸ Which in the meantime had become secondary.

inflexional suffix *-i(y)-* of athematic adjectives. The compound formant *-i(y) + ā-* must have been originally proper to adjectives. Its redundancy (*-iy + ā-*) in relation to *-i(y)-* accounts for its secondary spread as a derivational suffix of athematic nouns. But residuary instances like Vedic *naptīh*, gen. *naptīyah* from *nāpāt-* clearly point to *-i(y)-* as its predecessor with athematic substantives.

13. Passing now to the fundamental data of I.E. conjugation, let us first of all mention the curious fact that, although the verbal forms referring to the moment of speaking have always been regarded as the basic forms of the conjugational system, certain obvious conclusions concerning the hierarchy of functions and the relative chronology of verbal forms have been missed. We are concerned here, just as in all other sections of morphology, with

- 1) the purely morphic renewal of forms,
- 2) the spread of forms taking over secondary functions of other forms,
- 3) the phenomena of differentiation, grammaticalization, and lexicalization which accompany the changes 1) and 2).

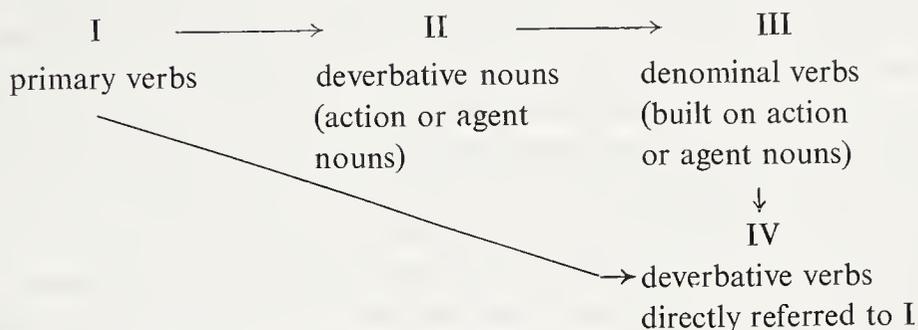
The most important phenomenon which has repeated itself over and over again and has left numerous traces in the old I.E. languages, is the renewal of the *durative* character of the verbal forms denoting the moment of speaking (present-imperfect system). The durative form may easily invade other semantic spheres: general (“timeless”) present, futurity, modality (“capability”, “eventuality”), etc. This expansion, involving the loss of expressiveness (i.e. of concentration on durativity), is the cause of drawing upon derived forms designed to renew the durative function. A formal split is likely to ensue: durative present (new form) and general or indetermined present (old form), present (new form) and future (old form), indicative (new form) and subjunctive (old form).

But the sphere of the non-durative functions, henceforth expressed by special forms, may be in its turn taken over by the new form, the old one being finally reduced to lexical residues.

Both cases are amply exemplified in the old I.E. languages. The semantic split entailed by the renewal of durativity is quite clear in Slavic, where the replacement of the old present by a derived form (the old iterative) has limited it to non-durative functions: future, “timeless” present, expression of capability. Similar processes take place in I.E.: the radical present, pushed back and limited to non-durative functions by characterized formations (reduplicated, with nasal infix, in *-skē/o-* etc.), specializes as a future or as a subjunctive, Vedic *kárat(i)* versus *kṛṇóti*. I.E. present and imperfect partly change their inherited primary function by becoming futures and aorists. Thus the I.E. present-formation in *-s-* left traces both in the future-subjunctive in *-s-* and in the sigmatic aorist. An analogical development, i.e. the renewal of the durative present (-imperfect) and the restriction of the old form to non-durative functions, is met with also in other periods, e.g. in modern Keltic, and in other linguistic families: whereas in the majority of the Semitic languages the old radical formation *ʔa-qtul-u*

is maintained in its imperfective function, in Akkadian and in Ethiopian it has been assigned the role of a subjunctive owing to the appearance of a new imperfective, viz. a deverbative formation characterized by the gemination of the second radical.

The new formations which exert a pressure upon the old present or imperfective are, as a rule, derivatives. Once they penetrate into the conjugational system of the basic verb, they are grammaticalized. The original semantic function of these new presents (imperfectives) is generally iterative. So far as the process of the renewal of the present (imperfective) can be traced back, it develops, as a rule, along these lines:



Therefore the working hypothesis is to interpret presents like (Skt.) *jī-gā-ti*, *pu-n-ā-ti*, *gá-ccha-ti*, etc., which in the historical languages are deverbative, as denominal verbs derived from action or agent nouns (stage III).

This is not, however, the immediate task of reconstruction. We must, first of all, establish for each individual language to what degree the characterized presents, old iteratives (> duratives) by origin, depend upon primary verbs of action or upon primary verbs of state. The next step is to determine the non-durative functions taken over by the ousted present formations: they may remain within the conjugational system of the new verb or constitute small derivational groups or, finally, undergo a complete lexicalization. The individual I.E. languages will behave differently, e.g. the suffix *-skē/o-* played an important role in Iranian where it developed an intransitive-passive meaning (still attested in Middle Iranian), or in Latin where it finally became a mechanical enlargement of the present-stem (cf. Ibero-Romance and Rumanian *-escere*). Both in Baltic and in Germanic (Gothic) the element *n* expresses intransitive and inchoative meanings, appearing as infix in Baltic, as suffix in Germanic. These instances illustrate at the same time the rather frequent change of morphemes denoting a mode of action to morphemes expressing voice (diathesis). Such a development occurs already in the oldest stages of I.E., cf. the iteratives and causatives in *-eje/o-*.

A major difficulty of reconstruction is to ascertain whether a set of verbs characterized by a common suffix represents a derivational series which has not yet undergone grammaticalization, i.e. has not yet been used to express durativity, or one that had already played its parts in the renewal of the present (imperfective) but, ousted by a new formation, has been limited to secondary semantic functions or even totally lexicalized.

14. Conclusions concerning relative chronology may be also drawn from the *perfective* aspect and the *perfect*. As regards the so-called *perfect* the normal evolution seems to be: *derived form* (or verbal noun + auxiliary verb) > *perfect* > *indetermined past* (“passé indéfini”) > *narrative* tense. The derivative is adopted as a regular member of the conjugation in order to replace the old form of the perfect, which, having been additionally charged with the narrative function, has lost its expressiveness. The grammaticalization of the Romance periphrasis *amatum habeo* (a counterpart of the already existing classical *amatus est; mihi amatus est*, with the *dativus auctoris*, > *amatum habeo*) is due to the cumulation of functions inherent in Lat. *amavi*: perfect and narrative (historical) tense. Another example: O. Persian *manā krtam* (perfect): *akunavam* (narrative tense) but N. Persian *karda am* (perfect): *kardam* (narrative tense) > *manā krtam*. At first glance one is inclined to assume that, simultaneously with the adoption of the narrative function, the old perfect retires from its old semantic sphere, which is subsequently taken over by the new perfect form. In actuality the mechanism of this shift is different. By extending its semantic usage, serving both as perfect and as narrative, the perfect form loses its original value; hence the formal renewal of the perfect function. Therefore the semantic sphere of the perfect does not for a moment lose its formal exponent, although the latter may have different degrees of expressiveness.

The usage of the modern continuation of *amatum habeo* as well as of the corresponding Germanic periphrasis has already overstepped the range of the perfect *sensu stricto*. Although in the majority of these languages it has not yet penetrated into the zone of the narrative, it tends, more and more, to denote an indetermined past action (passé indéfini) while the original value (present result of a past action) is receding more and more into the background.

The English series *I have my feet stuck out* > *I have stuck out my feet* > *I stuck out my feet* may serve as a frame of reference for determining the internal chronology of certain conjugational categories.

15. The chief problem of the I.E. comparative grammar is, however, different. When comparing the present-aorist systems of Skt. (Vedic) and Greek we ascertain the following discrepancies:

Form	Function	
	Greek	Skt. (Vedic)
Present	imperfectivity	simultaneity (with the moment of speaking)
Imperfect	imperfectivity	simultaneity (with a past moment; narrative)
Aorist	perfectivity	anteriority (referred to the moment of speaking; present perfect)

Greek has *aspects*. The contrast *imperfective: perfective* is expressed by the forms *imperfect: aorist*, whereas the zone of neutralization in favour of the unmarked member

(imperfectivity) is covered by the present. Skt. (Vedic) has a system of *temporal* relation. The opposition simultaneity (or rather *non-anteriority*): *anteriority* is the pivot of the system. Represented by the present and the aorist, this opposition is neutralized in the imperfect.

The relative chronology of these two systems is an important problem; which of them is to be regarded as the I.E. heritage? To decide this question the most promising clue seems to be the morphological structure of the present-imperfect-aorist forms.

Greek λείπω: $\left\{ \begin{array}{l} \text{ἔλειπον} \\ \text{ἔλιπον} \end{array} \right.$ offers a direct formal opposition between λειπ (normal grade) and λιπ (nil-grade) representing a *minimal* difference. The same is true for the type ἔθελον (imperfect): ἔθελα (sigmatic aorist implying an athematic inflexion) or ἐτίγων (imperfect with affix): ἔγων (radical aorist); etc.

On the other hand, in Skt. examples like $\left. \begin{array}{l} \text{rinákti} \\ \text{áricat} \end{array} : \text{árinak} \text{ or } \begin{array}{l} \text{sárpatti} \\ \text{ásrpat} \end{array} \right\} : \text{ásarpatt}$ the opposition of stems is complicated by the accompanying difference of desinences (primary: secondary) and is therefore deprived of its autonomous distinctive value. Hence the indications are that the Greek contrast ἔλειπον: ἔλιπον should be regarded as older than the Skt. opposition *sárpatti*: *ásrpat* which must be the result of a semantic shift. To explain the latter we shall be inclined to assume as the I.E. starting-point:

Form	Function	
	Primary	Secondary
Present	imperfectivity	simultaneity (with the moment of speaking)
Imperfect	imperfectivity	simultaneity (with a past moment)
Aorist	perfectivity	anteriority (referred to the moment of speaking)

The semantic shift in Skt. resulted from the identification of the imperfective and the perfective past tense: in narration the aorist was replaced by the imperfect, henceforward the only representant of the narrative tense. Ousted from its primary function (perfective past tense), the aorist was limited to its secondary function (anteriority referred to the moment of speaking). A new opposition turned up: anteriority versus simultaneity (= non-anteriority), which reminds us of West European verbal systems (French *j'écris*: *j'ai écrit*, etc.). Between the Vedic aorist and imperfect the difference is much the same as between English *I have written (a letter)* and *I wrote (a l.)*. The durativity of the I.E. imperfect has been relegated to the background (secondary function). In Greek the old opposition *imperfect* (imperfective): *aorist* (perfective) continues to be pertinent. The value of the English present perfect is only a secondary function of the Greek aorist.

In the above instance internal reconstruction is based upon two I.E. languages which distinguish the "past" tenses: imperfect, aorist, and perfect. The range of the I.E. aorist was in Skt. narrowed down in favour of the imperfect. The chronological priority of Greek can be proved by an analysis of the system of inherited verbal forms.

The opposition $\left[\begin{array}{c} \text{present: imperfect} \\ \text{aorist} \end{array} \right]$ is an opposition of aspects. If in I.E. the aorist had the same primary function as in Vedic we should expect its stem to admit primary as well as secondary endings. Cf. French *j'ai écrit: j'avais écrit* parallel to *j'écris: j'écrivais*.

16. The future is rendered in the Old I.E. languages either by the above-mentioned form in *-e/o-* (identical with the "subjunctive") or by a sigmatic derivative. In Indo-Iranian as well as in Greek the oldest texts make use of both formations; as a rule the sigmatic one is ousting the form in *-e/o-*. The varieties of structure we observe in the sigmatic future (*-s-ṛe/o-* in Indo-Iranian, *-s-e/o-* in Greek, *-s-* in Lithuanian) seem to point to an original athematic inflexion. But here again we meet with an identity of the future and the subjunctive, cf. the athematic *s*-subjunctive of O. Irish. We reach the following plausible conclusion:

Just as the replacement of Slavic *pri-pečetō* (old present), *pri-peče* (old imperfect) by the new stem *pripěk-ajetō* (old iterative) has confined the old forms to the (secondary) function of future (*pripečetō*) and of aorist (*pri-peče*), so the future subjunctive in *-s-t(i)* and the aorist in *-s-t* represent the result of the dissociation of a sigmatic present-imperfect system ousted in I.E. by other formations.

The relative chronology of the subjunctives in *-e/o-* and in *-s-* is suggested by the following consideration: the future subjunctive in *-s-* still betrays its derivational character; it became an integrant part of conjugation only in the early historical stages. Moreover, the radical structure of the subjunctive in *-e/o-*, as against the sigmatic enlargement in the future, testifies to the relatively recent origin of the latter. The subjunctive in *-e/o-* has been adapted to the various stems of the conjugational system, the future-subjunctive in *-s-* still preserves its form of a derivative built on the verbal root. As regards the functions, it is a well-known fact that the future is perpetually regenerated from forms expressing a psychic state preceding action. The modal character of such forms has a close affinity to the subjunctive (*modus eventualis*) ("I wish", "I want", "I am to", "I have to"). Cf. the analogous relation between the perfect and the verbs of state, the source of its renewal.

The futurity of the *s*-form is more recent than the futurity of the *-e/o-* forms. In Latin the old future in *-e/o-* is still maintained, an innovation taking place only in the productive classes (*-ā-bo*, *-ē-bo*, partly *-ī-bo*); as mode of subordination the subjunctive has been replaced by the optative in *-ā-*. The relation of the *-s-* future to the *-s-* subjunctive is clear in O. Irish where the old form has been retained for the subjunctive, whereas the corresponding reduplicated form (representing the I.E. desiderative) has been used to renew the future. Hence the two stages:

	Function	
	Futurity	Eventuality
Form 1st stage	+s-	+s-
Form 2nd stage	reduplic. +s-	+s-

17. A similar reasoning may be applied to the problem of the optative. There is a certain analogy between the subjunctive ("eventuality"), functionally related to the future, and the optative ("wish"), related to the preterite. Cp., e.g., Modern Slavic: Polish *oby pisal* "may he write" < *o* + *by* (old aorist) + participle, or Russian *pošël, pošli* "be gone!". In view of such parallels the I.E. optative in $-(i)\check{i}\bar{e}-$ may be regarded as a form which has been deprived of its old preterital function. Such a conception is borne out 1) by the desinences (secondary only), 2) by the characteristic element $-\bar{e}$ originally proper to aorists and as such still attested in Greek and in Balto-Slavic, and 3) by a similar evolution of another aoristic element, $-\bar{a}-$, appearing as such in Balto-Slavic but firmly established as an optative (> "subjunctive") suffix in Italic and in Keltic. In the latter case the chronological stratification is transparent. O. Lat. $-\check{i}\bar{e}/\bar{i}-$ in *siēs, sītis* and in the "subjunctive" of the perfect ($-\bar{i}s, -\bar{i}tis$) is residuary in respect to the $-\bar{a}-$ of *legās, deleās, audiās*: the difference of morphemes between the *infectum* ($-\bar{a}-$) and the *perfectum* ($-\bar{i}-$) proves that $-\bar{a}-$, which appears in the basic part of the verbal system (*infectum*), must be an innovation.

18. A wide-spread phenomenon is the common origin of the passive and of the form denoting the state resulting from a past action (the so-called perfect). Cp. (Lat. and) Romance *amātus est* and *mortuus est*; the former is a passive (perfect passive in Lat., present passive in Romance), the latter a perfect both in Lat. and in Romance. Although the external structure (verbal adjective in $-to-$) is common to both categories, it is clear that the passive develops with transitive verbs only, and that the semantic relation *amo*: *amātus* (state > perfective passivity) is different from *morior*: *mortuus* (state > perfectivity). The genetical relationship between the (medio)-passive and the perfect in I.E. is revealed by the inflexional endings, the elements $-\check{a}_2, -t, -zero, -r$ as against $-m, -s, -t, -nt$ of the active voice of the other tenses. In the historical languages the endings of the mediopassive and of the perfect are already considerably differentiated, imposing problems of relative chronology. The mediopassive, standing in semantic opposition to the active voice, undergoes certain proportional transformations easy to understand (e.g. Skt. $-ti$: $-te = -si$: $-se$) whereas the perfect remains outside this opposition. Later on the perfect, being intransitive, may be reinterpreted as a mediopassive and undergo a corresponding transformation of its inflexional endings. Or again, owing to a more recent layer of *transitive* perfects, the endings $-\check{a}_2, -t, -zero, -r$ are interpreted as active-transitive and a corresponding mediopassive series is created. In this way a *cumulation* of morphs of the mediopassive and the perfect becomes possible: a form like Skt. *ca-kr-é* contains the I.E. ending $-*e$ (Skt. $-a$) plus i , but the accentuation and the nil-grade of the root have been introduced from the mediopassive of the present-aorist system. An inference concerning relative chronology may be drawn from the fact that although intransitive verbs do not form a mediopassive, transitive verbs may form a "resultative" perfect, cf. Lat. *litterae scriptae sunt* > *mihi litterae scriptae sunt* > *litteras scriptas habeo* > *j'ai écrit une lettre*. The Lat. parallel proves that genetically the transitive perfect is secondary and

emerges from the common base *scriptus est* and *mortuus est*. The argument of Homeric Greek, which confirms the primitive semantic range of the I.E. perfect as suggested here, is of course welcome but it must be stressed that, even without it, the clue of the inflexional endings would have been decisive.

Similar developments have also taken place outside I.E., cf. e.g. the Akkadian formant *-ta-* which, originally designed to derive intransitive verbs, became the morph both of the mediopassive and of the perfect.

19. It may be seen from the morphological instances quoted above that one of the most important preliminaries to internal reconstruction is a correct assessment of the distribution of the primary and the secondary functions. The hierarchy of functions within a semantic zone has a panchronic character, in principle independent of the individual languages. This circumstance renders possible a reconstruction of relative chronology, though only for grammatical categories and not for isolated lexical facts with their unpredictability of secondary semantic functions. The more grammatical (i.e. general) a given category,⁹ the less differentiated the secondary functions, the easier to determine the semantic split and the relative chronology. Thus, e.g., the secondary function of the plural, of a case, of certain verbal categories, etc., will be either independent of the semantic contents of the individual roots or dependent on features generating large semantic subgroups (animate: inanimate, personal: impersonal, verbs of action: verbs of state, verbs of movement versus all others, etc.).

In general it may be said that if a grammatical form F has a primary function Φ_1 and a secondary function Φ_2 , the renewal of the form $F / > F'$ may produce a split: $F' = \Phi_1, F = \Phi_2$ with a tendency of the new form F' to penetrate also into the functional sphere Φ_2 . The reason for this must be looked for in the fact that the old form F coexists during a certain space of time with F' in the functional sphere Φ_1 , and such a coexistence may well render possible the penetration of F' into the zone Φ_2 also. In the latter case the grammatical differentiation ceases to exist and the old form F , if it survives, is completely lexicalized or maintained as an allomorph or as a stylistical variant (archaism) only.

The replacement of F by F' may be purely *morphic* and represent a rearrangement caused in the last resort by phonemic changes.

But, on the other hand, a differentiation is also achieved by a *semantic* substitution: F is replaced in its *secondary* function by a new form F' charged with an identical *primary* function. Here again, the tendency of F' to penetrate into the rest of the functional sphere of F may swamp the original differentiation.

In this way phases of differentiation and integration alternate with each other.

To determine the hierarchy of the functions Φ_1 and Φ_2 will often be a complicated, though essential, task. Only after having succeeded in establishing that Φ_1 is a primary function (determined by the opposition within the system and not by the context)

⁹ Inflexional categories represent a higher degree of grammaticalization than the derivational. Within the latter, hierarchy depends on the semantic range.

and Φ_2 , a secondary function (dependent on the context), do we get the essential information about the status of F (Φ_1 , Φ_2) within the given language.

20. It is necessary to draw attention to the notion of isomorphism which, although criticized, seems to be of considerable methodical importance and utility. The argument that the so-called "pleremes" are by far more numerous than the "cenemes" and that the structures and combinations of the former are more complicated and practically indefinite in comparison with those of the latter – in addition to the argument that there cannot be a real parallelism between "expression" and "contenu" because contents is the goal whereas expression is only an implement – does not in our opinion prove the lack of parallelism between the different strata of the language, some of them richer and others poorer in possibilities. Thus the difference between syntactical and paradigmatic refers both to the relations between phonemes and between morphemes. The same is true for the difference between primary and secondary functions, i.e. between what is given by the system and what is determined by the context. Phonemics, where things are simpler and clearer, teach us to look for parallel distinctions in morphology. Looking at the phonemic model we may distinguish between the alternation of a phoneme with its variants and its alternation with other phonemes. Examples of the former possibility: alternation of German *ich-Laut* and *ach-Laut*. Example of the latter possibility: Polish or Russian $d > t$ in word-final position. Both eventualities have their counterparts in the structure of morphemes. The first case is represented by allomorphs, the second by the syncretism, in certain contexts, of a formal difference existing elsewhere. This second possibility may be symbolized by $\left[\begin{array}{c} F(\Phi) \ F'(\Phi') \\ F(\Phi') \end{array} \right]$ where F and F' are morphs expressing the primary function (Φ and Φ') of different categories. F(Φ') stands for syncretism, the morph F appearing with the secondary function Φ' . E.g., Skt.

$$\left[\begin{array}{c} b\bar{a}h\acute{o}h \text{ (gen.) Skt. } v\bar{f}k\bar{a}t \text{ (abl.)} \\ b\bar{a}h\acute{o}h \text{ (abl.)} \end{array} \right]$$

In this example we witness on the one hand the polysemy (ambivalence) of the inflexional suffix *-oḥ* (primary function: genitive; secondary function: ablative), on the other hand the allomorphic relation between *-āt* and *-oḥ*: *-āt* is the *primary form* warranting the independent morphological status of the abl., *-oḥ*, one of the *secondary forms* (of the abl.) relying upon *-āt*. The introduction of the ablative ending *-tas* in Prakrit implying a split between the abl. *aggīo*, *bāhūo*, and the gen. *aggissa*, *bāhussa*, is not equivalent to the rise of a new morphological category, since it is only an extension of the difference gen./abl. already existing in the themes in *-o-*.

21. Such facts essentially differ from changes involving the rise of a *new morphological category which has not previously had a special morphological exponent*. But the latter possibility can scarcely be ascertained. Thus when investigating the origin of the I.E. gender (see above), we cannot be sure whether this category is not older than its historical exponents. Its old exponents might have been totally ousted by the new

ones (fem. *-ā-*, *-iy-*, etc.); or they might never have existed. Internal reconstruction moves on somewhat surer grounds only when investigating the *relative chronology* of attested morphological devices (morphs). Allegations concerning the rise of a morphological category, like the plur. or the fem., will remain conjectural because the contention that the morphs (morphological exponents) that are within the reach of linguistic analysis were the oldest, i.e. the first to express the given category, is hardly capable of demonstration.

Mutatis mutandis the case is comparable to a well-known phonological fallacy. Several scholars have been of the opinion that I.E. had not originally a vowel system at its disposal but only a single vowel (denoted by the symbol *e* or *a*). This theory is based on the following arguments: 1) the vowel *o* always stems from *e*, examples of original *o* being too scarce and uncertain; 2) the vowels *ī*, *ū* always represent the nil-grade of phonemic complexes containing *e* plus a consonantal element (*i*, *u*); 3) the vowel *a* is distributionally limited: it appears in absolute word-initial position and in certain laryngeal neighbourhoods; 4) long vowels are to be explained either by secondary lengthenings or by the contraction of short vowels with following laryngeals. The conclusion drawn from 1) – 4) purporting that I.E. had one vowel only, is just an instance of glottogonic speculation to be avoided. In the first place each of the processes 1) – 4) was of different date. Their relative chronology may be up to a certain degree established. In the second place we may admit *a priori* that the productive morphological processes must have contributed to the spread of the morphologically conditioned *o* eclipsing in a rising degree the original *o* (λόπτω, ὄζω). But from the fact that the majority of *o*'s may be traced back to an original *e* we are not allowed to draw the conclusion that all *o*'s are secondary. There are, moreover, instances of *e* of secondary origin, e.g. **dhues*: nil-grade **dhus*: secondary full grade **dheus* (Germanic *diuza-*), or the so-called *vřddhi*. Summing up, the essential objection is that we do not know whether in the course of the prehistory of the I.E. vocalism (1-4) old vowels have not been absorbed by vowels originating from apophonic processes. The theory of the non-vocalic character of primitive I.E. does not take into account the chronological stratification of the ablaut nor its morphological functions.

22. Certain methodical inferences may be drawn from the above examples. Thus the classical problem of the I.E. palatal series must be restated in *phonemic* terms and not, as hitherto, in terms of *phonetics* and “*analogy*”. An adequate analysis of the German consonant-shift shows that the continuation of a prehistoric by a new historical contrast propounds a special kind of problem, viz. the shift of the neutralization-point. The rise or the change of the latter presupposes an identification of phonemes or at least of phonemic features, to be regarded as the starting-point of linguistic change *sensu stricto*. An identification, in certain positions, of two different phonemes may furthermore result in the rise of a third phoneme, cf. Sanskrit *ṣ* < *s* and *ś* (*piṣṭá-* < **pis* and **pik*). It is scarcely necessary to insist on the totality of the system

which may recommend or exclude certain solutions (cf. the problem of I.E. voiceless aspirates).

23. In morphology important procedures of structural linguistics must be respected in order to arrive at a correct evaluation of the forms and of their changes:

The hierarchy of the morphemes and of the morphs belonging to a given morpheme, and the hierarchy of the semantic and/or syntactical functions of the morpheme. Secondary functions must be defined in *positive* terms. As regards reconstruction we must count with the spread of redundant features entailed by the contrast between basic and motivated forms. On the functional side phenomena of differentiation may be due to rearrangements ultimately started by phonetic change – or they may be the result of semantic substitution, i.e. of the replacement of forms with *secondary* function (conditioned by the context) by forms with an identical *primary* function (conditioned by the system and independent of the context). The ensuing differentiation may be swamped out in a subsequent phase, due to imitation, viz. if the split of F into F' and F is interpreted as the replacement of F by F'.

But the chief keys to reconstruction seem to be certain panchronic laws of functional shift such as:

iterative > durative (present) > (general or indetermined) present
 static verb > perfect > indetermined past > narrative tense
 desiderative > future
 adverb > “concrete” case > “grammatical” case
 sex (in substantive) > gender (in adjective)
 collective > plural

etc.

To put it in a nutshell: there are certain “universals” (universal laws) governing the history of language, independent of its individual features. It is worth mentioning that the expressiveness of new forms is often conditioned by the *hic-nunc-ego* situation of speech: thus, e.g., both the *preterite* and the *future* tense can in the last resort be traced back to special *present* forms.

What could be suggested here only in an eclectic and cursory manner deserves to constitute the subject-matter of a new treatment of the “principles of linguistic history” (cf. the *Prinzipien der Sprachgeschichte* of Hermann Paul) which, in summing up the achievements and experiences of structural linguistics, would lay a new foundation for the methods of comparative grammar.

INTRODUCTION TO THE DISCUSSION

As a rule, a new scientific term represents the crowning of a preceding intellectual effort giving birth to a new concept, but there are cases (“phonetic law”, “analogy”) where the contents of the term, nebulous and confused at the beginning, must be

sorted out and determined *a posteriori* – after the term itself has attained a kind of independent existence. This seems also to be the case of the expression “internal reconstruction”. The stress may be put on the strictly linguistic aspect excluding approaches of a mixed character, like e.g. areal linguistics. The latter has recourse to geographical, i.e. spatial, relations of linguistic forms, not to their internal relations, in order to establish relative chronology. Hence *internal* reconstruction also renounces collaboration with auxiliary sciences like experimental phonetics, psychology, or cultural anthropology.

On the other hand, when speaking of internal reconstruction we may oppose it as something new to reconstruction as practised e.g. by neogrammarians (comparative methods). In this case the difference will lie in the conceptual equipment at our disposal, in our deepened insight into the nature and functioning of language. Reconstruction has also become more “internal” in the sense that in our reasonings we are more apt to discard considerations of experimental phonetics or of psychology – both of them favourite implements of reconstruction in earlier periods.

To sum up, internal reconstruction means purely *linguistic* reconstruction, to be distinguished from other methods aiming at non-linguistic problems or at any rate at problems which are not purely linguistic, like the primitive home of the I.E., the migrations of I.E. tribes, their culture, and so on. The status of linguistics as an autonomous science, independent of experimental phonetics, psychology, cultural anthropology, and so on, postulated by de Saussure, has become a fact not only in descriptive but also in historical linguistics. The epithet “internal” may be excused by the desire to distinguish it from the traditional comparative linguistics.

The report tries, in a fragmentary, not exhaustive way, to illustrate the different methodical devices applied to attain the most important goal of reconstruction, viz. the chronological order of the reconstructed linguistic facts (relative chronology). We are still a long way from the possibility of presenting the methodical devices of linguistic reconstruction in an ordered and systematic manner. Therefore the treatment adopted in the report has been to show their application (or rather the application of some of them) to chosen problems of I.E. phonemics and morphology.

The most reliable implements of internal reconstruction in morphology are certain universals governing the evolution of morphemes, universals whose existence must be recognized because of the ever increasing empirical evidence. Thus the renewal of the plural by means of derivatives conveying a collective meaning; case-forms continuing nominal adverbs or adverbial constructions built themselves on the model of pronominal adverbs; the preterite, ultimately to be traced to a present denoting state, on the other hand the future, stemming from a present designating the inauguration of an action (e.g. “I want to”, “I ought to”, “I am going to”). And so on.

It seems that the progress of reconstruction will be bound up with the formulation of such typical developments, general enough to be independent of specific contextual and extralinguistic conditioning. They are in close connection with the *speech-situation*, which relies heavily upon deictic (demonstrative) elements. The deictic

elements of the verb are obvious, on the one hand the *nunc-tunc* opposition of the tenses, on the other hand the *ego-tu-ille* opposition of the persons. Less obvious are certain nominal categories, like the *hic-hinc-huc* (“here”-“hence”-“hither”) opposition of the local cases or the so-called “elliptic” plural, a term not to be confounded with “collective”, whose prototype is to be looked for in the contrast *ego:nos, tu:vos*. Finally the base of grammatical gender, the opposition *animate:inanimate* (or *personal:impersonal*) is implied by the contrast between *ille* (“neuter”) and *ego, tu* (animate or personal), or between *quis* and *quid*.

The deictic basis of the speech-situation is common to all languages. *Speech*, the eternal fountain of rejuvenation of human language, accounts for the parallelisms and the similarities in morphological evolution which, although continually nourished by the speech situation, consists essentially in an *emancipation from the deictic basis*.

The methods of reconstruction will depend in a large measure upon the progress realized in the analysis of the relation between the deictic and the symbolic mechanisms within language. The results of such an investigation will represent the predictable trends of the evolutionary cycle of *the most general and most important morphological categories*. But the cognitive value of the reconstruction, the factor of probability, will be in direct proportion to the grammatical level of the reconstructed facts.

A remark must be devoted to the danger of confounding reconstruction with glottogonic speculation. The latter exists in its own right. But the aim of reconstruction is to establish the relative chronology of *morphs* implementing a grammatical category and of the functional changes they have undergone. It would not do to consider the oldest attainable morph of the plur., e.g. I.E. *-es*, as being the *first* exponent of this category in Protoindoeuropean. It may have ousted and replaced an older one. Moreover, the plur. has always existed *potentially*, in its embryonic state, in the deictic system of the language under the form *nos, vos*, the elliptic plural of *ego, tu*.

Glottogony and linguistic reconstruction must be clearly distinguished and kept apart.

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DISCUSSION

BIRNBAUM:

In section 2, Prof. Kuryłowicz speaks of “intermediate classes”. He quotes as an example the non-syllabic *u* (*ʉ*) and the *h*, which may function in certain languages both as independent phonemes and as features of particular phonemes. I gather he is referring to Hittite and Sanskrit respectively. He also says: “In other languages *ǰ* may similarly appear both as an autonomous phoneme and as the feature of palatality.” I presume that among the languages Kuryłowicz had in mind here is Common Slavic. Common Slavic *j* (or non-syllabic *i*) may be interpreted in a manner I should like to present here briefly, for the explanation may, I believe, be applicable also to

the labialization (in Hittite) and aspiration (in Sanskrit) as phonemic features vs. *u* and *h* as independent phonemes.

Viewing the phonic chain as alternating consonantal and vocalic segments, where the vocalic segments (in a broad sense) form the crest of the syllables, some prosodic features characteristic of the consonantal segments should, I submit, be added to the prosodic distinctive features posited by Jakobson, Halle and others (i.e. pitch, stress, quantity) for vowels. They would, unlike the inherent distinctive features, characterize the whole consonantal segment in the chain, be it a more or less complex consonantal cluster or – and this is the important thing – by no consonant at all, i.e. a zero phoneme. Palatality is, I believe, this sort of prosodic feature or rather prosodeme in Common Slavic. Every consonant or consonant cluster in late CS had to be either palatal or non-palatal. Only when the consonantal segment was not occupied by any consonantal phoneme (zero), but still was characterized by the prosodeme of palatality, it would be materialized as *j*.

I hope that this extremely brief account may demonstrate that a more adequate phonemic category, namely a category of supra- or adsegmental consonantal prosodemes, can take care of Kuryłowicz's suggested "intermediate classes" in phonemics.

Time does not permit comment on some other controversial points, such as the "transitional and fluctuating character of the linguistic phenomena", the syncretism of the IE ablative and genitive singular, and, above all, the very concept of *internal reconstruction*, which, I believe, Kuryłowicz uses in far too vague a sense.

BONFANTE:

I agree with some of the speaker's comments, the more so since they coincide exactly with what I said in my article on linguistic method in the first volume of *Word*. (I was the first, I believe, to use the expression "internal reconstruction".) I strongly disagree on other points, particularly the assertion that "internal reconstruction" is the only method of linguistic reconstruction, as Professor Kuryłowicz clearly stated. This would simply eliminate from the field of linguistics the whole of comparative linguistics and of linguistic geography, or areal linguistics, I certainly cannot follow Professor Kuryłowicz on this road.

HELLER:

I should like to return to the accepted characterization of the *t/d* contrast as containing unmarked *t* and marked *d*. Is voice really the distinctive mark which is added? Might not perhaps the differentiating mark be the tension of the vocal cords, i.e. voicelessness rather than voicing? If so, then the *t* would be the marked partner. Consequently the *t/d* relationship, marked/unmarked, remains the same when the *t* becomes a fricative (or voiceless aspirate, if one assumes an intermediate stage), and the *d* becomes *t* (that is, *ɸ* or *t^h/t*). The only thing that then would change would be the particular distinctive feature used as the mark, not the relationship itself. The following chart may clarify what I mean:

	<i>Unmarked</i>	<i>Marked</i>	<i>Mark</i>
<i>Stage 1</i>	d	t	tension
<i>Stage 2</i>	t	\bar{p} (or t^h)	friction (or aspiration)

In stage 1, the mark is tension of the vocal cords; therefore the *t* is the marked member of the pair. In stage 2, both phonemes are voiceless (i.e. have tension of the vocal cords); therefore, the feature of tension is no longer relevant to the pair. Instead the mark is now friction (or aspiration). In this correlation the *t* is the unmarked member, but the original relationship remains precisely the same: unmarked to marked. This assumption may help to explain why the cluster /sd/ = [zd] can pass to -*st* when *d* becomes *t*, but why, nevertheless, the original -*st* remains as -*st* and does not pass to - $s\bar{p}$ (or - st^h) when the original *t* becomes \bar{p} (or t^h). If neutralization is the suspension, in a particular position, of the demarcative function of a feature which is contrastive in some other position or positions, theoretically at least, one might expect the archiphoneme to be represented by the unmarked member of a pair. Thus, when *d* becomes *t*, the *d* of the cluster -*sd* can and does pass to *t*; when *t* becomes \bar{p} (or t^h), the *t* of the original cluster -*st* remains the same since in a position of neutralization the new mark, friction (or aspiration), is non-functional. The system has no motivation for the acquisition of a non-functional, hence uselessly redundant, feature.

LEHMAN:

Internal reconstruction is applied when we find a clash between different levels of language. To the present it has been used primarily at the phonological level where a lack of correspondence between phonological and morphological sets is in evidence, as in the stems of strong verbs of Germanic. It may also be used within one level when there are clashes in structure, as in the phonotactic examples cited by Professor Kuryłowicz.

We applaud his proposal to apply the method of internal reconstruction also at the morphological level ... by examining clashes between the morphological and semantic structure of any given language. I would like to caution however against setting up panchronic statements for semantics; we have not found general statements decisive in attempting to account for phonological change. Rather, as Professor Kuryłowicz himself points out, we first determine the phonological and morphological structure of any language at any selected time and then use the method of internal reconstruction. It would be well to set up a similar requirement at the semantic level when we apply the method to the morphology of a language.

ROSÉN:

Coming back to what was traditionally termed "internal reconstruction", namely the rediscovery of lost phonemic or morphemic shapes of linguistic forms, I feel we have to distinguish two types of procedure in internal reconstruction leading to a sequence of linguistic events, say, relative chronology. The one applies whenever we

have a set of earlier and a set of later forms that differ from each other in more than one respect, and we can then establish the chronological sequence of the evolution of these differences that are *intermediate* between the two attested sets of forms. But what if we have only one set of attested forms, all of the same *état de langue*, and make an attempt to reconstruct “back”, to establish prehistoric forms? How can we make sure that the *sequence* or *successivity* we are establishing in such cases is not a fact of *descriptive order* and bears no relation to actual linguistic history? (Comparison with cognate languages hardly helps us in this dilemma.) For example, Lat. *honos* – *honoris* vs. *mitto* – *misi*. We tend to say that the loss of the dental was posterior to the effectivity of rhotacism. However, since we have no attestation of a cluster *ts*, could it not be true that these forms only reflect the descriptive order in which two co-existent and co-effective morphophonemic relations have to be applied in the description of Latin? In such cases, I cannot avoid feeling that the traditional or conventional procedure of reconstruction (not the one in the sense presented by Professor Kuryłowicz) loses a great deal of its value.

STRANG:

I should like to ask a question about one example. Professor Kuryłowicz refers to the English series *I have my feet stuck out* > *I have stuck out my feet* > *I stuck out my feet* as possibly serving “as a frame of reference for determining the internal chronology of certain conjugational categories”. Here the ordinary sign for historical development (>) is used in a connection where it runs counter to the accepted sequence. We may ignore the lexical content of the example as immaterial to the point being made. I shall refer to the conjugational patterns as Patterns 1, 2 and 3 in the order in which Professor Kuryłowicz gives them. Pattern 3 preceded and has subsequently co-existed with the other two. Pattern 1 could only take its present form after the loss of nominal accusative inflection in English (i.e., much later than 2 and 3), but if we interpret it as a modern equivalent of an earlier inflected pattern (as in ‘hie hine ofslægenne hæfdon’) we are dealing with something which is often thought of as preceding Pattern 2. Actually its occurrence in Old English is exceedingly restricted; even in the earliest texts Pattern 2 exists with it and is commoner. I suspect that Pattern 1 (in the inflected form) is thought of as ancient for no better reasons than that (a) it has a look of logical priority and (b) it has not survived and so seems archaic. The historical sequence is therefore first Pattern 3, later Pattern 2, for some period co-existing with (inflected) Pattern 1 (the respective times of origin being irretrievable); then Pattern 1 (inflected) is lost, and 2 and 3 co-exist; then Pattern 1 (as quoted) comes into existence, and all three co-exist. In the light of this I should like to ask what is the force of the > sign in Professor Kuryłowicz’s text, and whether the example chosen is really an illuminating one.

WINTER:

Professor Kuryłowicz seems to accept as a fact the validity of an analysis of the

Indo-European verb system from a focal point in the present tense stem. As a matter of fact, this assumption appears to be necessary for much of his argument concerning displacement and transfer.

Is this assumption of the basic role of the present really warranted? One answer to this question may be suggested by a very simple consideration.

It is easier, as far as questions of method are concerned, to equate primary *vs.* derived nature of a form with simple *vs.* complex morphological structure than it is to correlate primary nature of a form with complex internal structure. If we consider this assumption potentially valid, we come to the observation that the Indo-European verb can be more readily viewed as having not one basic form that recurs as basic form in all verbs, but rather that the focal point represented by the simplest form may be found in various stems. We find present-based verbs such as Greek *katheúdein*, but we also find aorist-based verbs such as *genésthai*: the aorist *katheúsai* is morphologically more complex than the present *katheúdein*, but the aorist *genésthai* is simpler than the present *gígnesthai*. The interesting point now is that the locus of the greatest formal simplicity varies in accordance with the nature of the basic function of a verb. This can be observed throughout the range of individual Indo-European languages: just as the basically punctual Greek verb denoting "to give" has its more complex form in the present *didónai*, so the Russian present *davat'* and the Tocharian A present-based infinitive *essi* are more complex than their respective non-present counterparts, even though the formal means of indicating derivation vary from language to language. It would therefore seem simplest to operate with the assumption of several potential focal points rather than with that of one single focus.

KURYŁOWICZ:

To Birnbaum: There is no essential difference between what has been said on p. 10 of the Report about the *intermediate classes* and Prof. Birnbaum's own explanation. A distinction must still be made between phonemic features capable of becoming autonomous phonemes (like *h* in Sanskrit *dh*) and others which are not (like the voicing of voiced stops in Sanskrit).

To Bonfante: Conclusions based on areal linguistics may serve as subsidiary arguments so far as they do not contradict purely internal evidence. If for example areal evidence points to a chronology "A older than B" whereas the result of internal analysis is "A is derived from B (and therefore younger than B)", only the latter inference can be considered as valid.

Subphonemic evidence can be directly collected from *texts*, oral or written. Subphonemic features or changes of *prehistoric* stages can be inferred from *phonemic* features or changes only. The latter are our fundamental data.

To Georgiev: The term "glottogony" denotes speculation concerning the rise of human language, whereas the example cited by Prof. Georgiev is an illustration of a universal feature proper to full-fledged languages.

In spite of the remarks of the Report devoted to the I.-E. palatal series $\hat{k}, \hat{g}(h)$, the

crucial point has still not been taken. The rise of the palatal series is neither a question of phonetic variants (before $\check{e}, \check{i}, \check{r}$) nor of “analogy” but of *phonemicization* (how did the palatalized variants of k, g, gh acquire phonemic status?). The problem of Sanskrit $k\check{s}$: Greek $\chi\tau, \chi\theta, \varphi\theta$ cannot be solved by the simple assumption of a metathesis (Hittite *tekan* : Greek $\chi\theta\acute{\omega}\nu$, like $\tau\acute{\iota}\kappa\tau\omega < * \tau\acute{\iota}\kappa\omega$). There are examples like *ksināti* : $\varphi\theta\acute{\iota}\nu\omega$ to which such an explanation cannot be applied.

To Hamp: The term “internal reconstruction” as used here refers to devices and procedures applied within *one* language. Internal reconstruction is a *linguistic* procedure since it does not have recourse to extralinguistic considerations like physiology of speech, psychology, and so on. Reconstruction in the traditional sense (*comparative grammar of I.-E.*) has not always been purely linguistic.

It is evident that universals cannot explain linguistic history or prehistory in all its details. What really matters is that, being inherent in language, they form the foundation of particular developments.

To Heller: The criteria permitting us to establish the phonological relation of I.-E. t to I.-E. d come from the distribution in Sanskrit (t replacing d before final juncture), and the non-occurrence of initial $*sd-$, both pointing to the unmarked character of t . On the other hand, there are no indications whatever that might authorize one to posit t as the marked member of the couple $t : d$.

To Klimas: The relative chronology of the subjunctives in $-e/o-$ (Vedic *kṛṇavat* [i] and in $-s-$ (cf. O. Irish $-s-t$, Sanskrit future *karisyati*) is suggested by their formation. The form in $-e/o-$ has been in a large measure incorporated into the conjugation and is built on the different *stems* whereas the subjunctive in $-s-$ behaves like a primary derivative built on the *root*.

To Lehmann: The clashes between morphemic and semantic structures, representing a major source of “analogical” transformations, find a counterpart in relicts (like e.g. the neuters in r/n) which, having escaped such a transformation, constitute according to Meillet the most promising starting-point for comparative reconstruction.

To Strang: The example *I have my feet stuck out* > *I have stuck out my feet* > *I stuck out my feet* is a *frame of reference* symbolizing the normal semantic development *verb of state* > *perfect* > (*passé indéfini*) > *narrative tense*.

To Winter: The durativity or imperfectivity proper to the present tense may be endangered by the meaning of the root, by preverbs, etc. The renewal of the present by a new formation (containing e.g. a reduplication, a nasal infix, a suffix like $-s\acute{k}e/o-$, etc.) is therefore a frequent phenomenon in I.-E. The old non-durative form may be then restricted to the aorist (and to the future) function. We get thus a relation of an enlarged present versus a root-aorist.

However, in spite of this formal difference, the aorist remains the *semantically marked* form and is perceived as being derived from the present stem by means of *subtraction*: $\gamma\acute{\iota}-\gamma\nu-ο-μ\alpha\iota > \acute{\epsilon}-\gamma\epsilon\nu-ό-μ\eta\nu$, etc.

A STATISTICAL STUDY OF ENGLISH SYNTAX

EDWARD R. GAMMON

INTRODUCTION

This paper summarizes a statistical approach to English syntax. We show a segmentation of utterances based on the estimated predictability of grammatical forms. Dealing only with the sequence of forms of an utterance, we require that segment boundaries occur at positions in the sequence where the uncertainty in predicting possible future forms, given one or more immediate forms, is high. By "high" we mean either in a relative sense, or larger than some prespecified value. The segments obtained from sequences of distribution classes coincide with recognizable phrases. Using various systems of phrase labeling, predictability of phrase types yields recognizable clauses and sentences; although these do not necessarily coincide with the intonation patterns indicated by punctuation.

PREDICTABILITY OF DISTRIBUTION CLASSES

The methods used in this paper were suggested by Harris's procedure for discovering word and morph boundaries.¹ In his procedure, an utterance is represented phonemically. From an informant, or a very large corpus, one enumerates the number of different phonemes which may occur after the first n phonemes of the utterance. Segment boundaries are taken at positions corresponding to values of n where the number of possible successors is relatively large. A similar procedure for segmenting sentences represented by sequences of distribution classes was developed by Chatman.²

The number of possible successors to a subsequence of classes may be considered as a measure of difficulty in predicting a successor, but as such, it does not reflect the expected frequencies with which the successors may occur. For this reason, in an analysis based on predictability, we use the entropy or uncertainty, which is the only measure having the following properties: It is zero when one successor is certain, maximum when all are equally likely, and conditionally additive for predicting more than one successor [Khinchin³].

¹ Z. Harris, "From Phoneme to Morpheme", *Language*, 31 (1955), 190-213.

² S. Chatman, "Immediate Constituents and Expansion Analysis", *Word*, 11 (1955), 377-85.

³ A. Khinchin, *Mathematical Foundations of Information Theory*. Trans. by R. Silverman and M. Friedman (New York, 1957).

Throughout, we use the maximum likelihood estimate of uncertainty of Miller and Madow.⁴ Thus, if C_m and C_n are m and n term sequences respectively, the estimated uncertainty in predicting the next n classes given the m immediate classes C_m is:

$$\hat{U}_n(C_m) = - \sum_{C_n} \frac{f(C_m C_n)}{f(C_m)} \log_2 \frac{f(C_m C_n)}{f(C_m)}$$

where f indicates frequency and the sum is taken over all possible, say k , values of C_n . Except in the case of predicting pairs, the estimate is corrected for bias to terms of order $[1/f(C_m)]^2$ by the addition of $\log_2 e (k-1)/2f(C_m)$.

The illustrative examples which follow concern a modified version of Fries's⁵ system of distribution classes. We preserve Fries's notation except for some minor changes. Following Aborn and Rubenstein,⁶ we introduce the group P for *to* in infinite phrases. The Group J is split into the Group R of relatives and the Group S of includers [Francis⁷]. The Group G of *do* as an auxiliary is taken as an auxiliary B, and Group C is labeled as an adverb Class 4. Verbals and nominals used adjectivally are grouped with the adjective Class 3. In assigning classes, reference is made to Roberts.⁸

Figure 1 shows an excerpt from the corpus⁹ from which the estimates were made. Uncertainty in prediction is plotted as follows: A successor given one immediate class \circ ; a successor given two classes \triangle ; a pair of successors given one class \diamond ; a predecessor given two classes \square .

For predicting a successor, the pattern of uncertainty is similar, given one or two classes.¹⁰ If we require that segment terminals occur at relative maxima (that is, at points of most difficult prediction, given two classes), the segments are: *If one believes – that all questions raised – by science – . . .* If we use as a rule that a terminal occurs wherever $\hat{U}_1(C_2)$ exceeds its average value, the segments are: *If one believes – that all questions – raised – by science – . . .* These boundaries are indicated by bars in Fig. 1. For this corpus, the estimates $\hat{U}_2(C_1)$ and $\hat{U}_1(C_1)$ have almost constant differences and the induced segmentation is similar.

We also consider the uncertainty in estimating a predecessor and a resulting

⁴ G. Miller and W. Madow, *On the Maximum Likelihood Estimate of the Shannon-Wiener Measure of Information* (Air Force Cambridge Research Center, 1954).

⁵ C. Fries, *The Structure of English* (New York, 1952).

⁶ M. Aborn and H. Rubenstein, "Word-Class Distribution in Sentences of Fixed Length", *Language*, 32 (1956), 666-74.

⁷ W. Francis, *The Structure of American English* (New York, 1958), Chap. 6.

⁸ P. Roberts, "Fries' Group D", *Language*, 31 (1955), 20-24.— P. Roberts, *Pattern of English* (New York, 1956).

⁹ *The Validation of Scientific Theories*, P. G. Frank, ed., "The Variety of Reasons for the Acceptance of Scientific Theories," by P. G. Frank (Boston, 1956). This as a philosophical essay of approximately 5000 words.

¹⁰ The small difference in uncertainty, given one or two classes, is evidence that a low-order Markov chain would approximate this data. Such a model is being formulated for prediction of average characteristics such as lengths of selected phrases and the distribution of occurrences of maximum uncertainty.

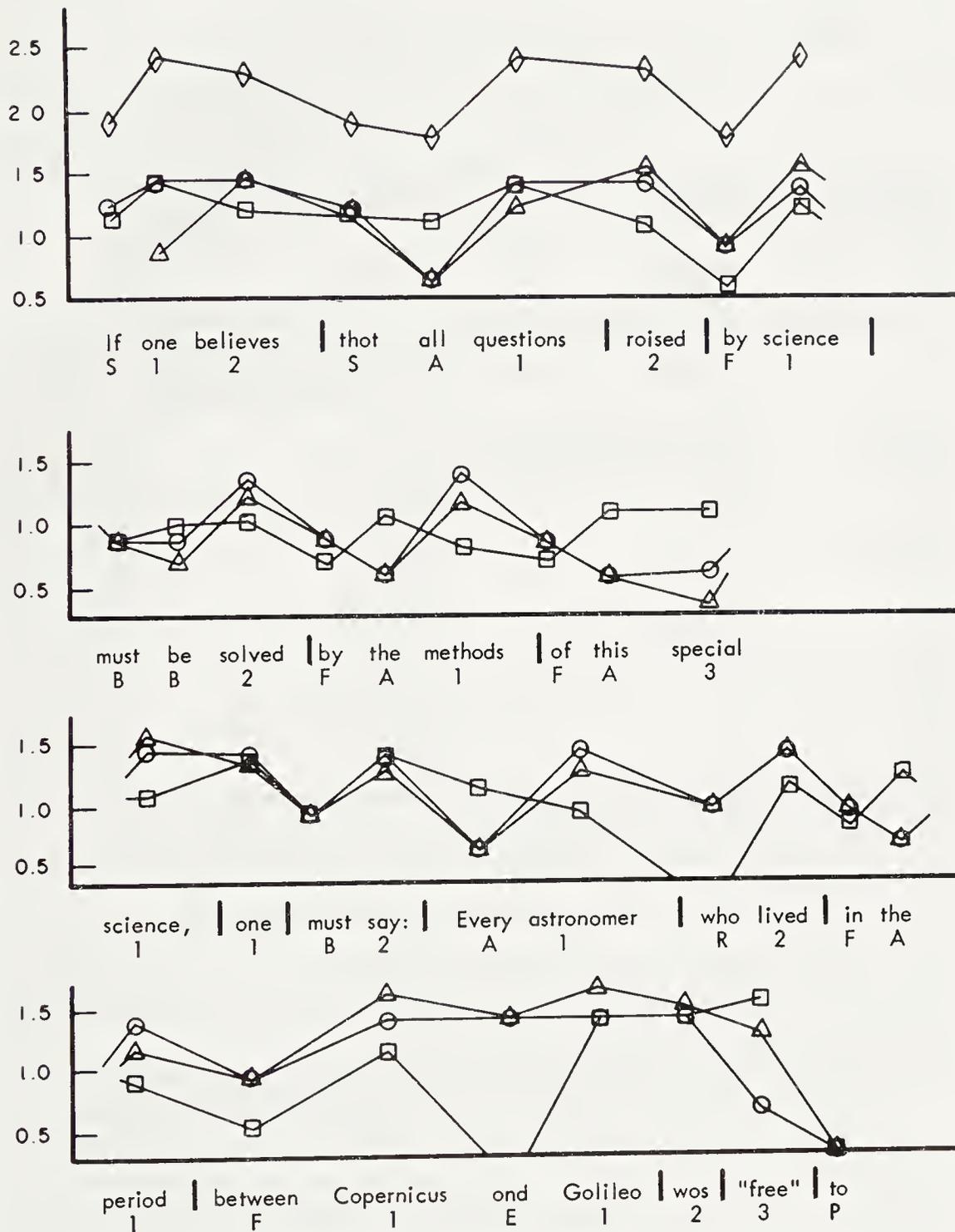


Fig. 1. Uncertainty Predicting Distribution Classes

TABLE I
Classification of Segments

Segment	Phrase Type	Classification		
		I	II	III
(A)(3)1 ^(a)	Nominal	N	N	N
(B)(4)2 } (P)2 } (D)3 } (D)4 } F(A)(3)1(E(A)(3)1)	Verbal	V	V	V
	Adjectival	Adj.	Adj.	Adj.
	Adverbial	Adv.	Adv.	Adv.
	Prepositional: noun object	P	Ps: sentence modifier Pn: nominal modifier Pv: verbal modifier Pa: adjectival modifier	P
F(B)(4)2	Prepositional: verb object	PV	V	V
F(D)3	Prepositional: adjective object	PA	Adv.	Adv.
S(A)(3)1		SN	N	S+N ^(b)
S12		SNV	NV	S+N+V
R2		RV	V	R+V
FR(A)(3)1		FRN	FRN	R+N
H24		HV	NV	N+V
H21		HVN	NV	N+V+N
E	Conjunction	C	C	C
S	Includer	S	S	S
B1		BN	N	B+N
12		T	NV	N+V
PB		B	V	B
R12		RNV	NV	R+N+V
12(A)1		W	NV	N+V+N

(a) The parentheses indicate optional inclusion of one or more of the enclosed classes.

(b) + indicates that the conjoined elements are treated as an ordered sequence of separate phrase types.

(c) The last five segment types account for less than one percent of the segments.

segmentation. Here we might use the rule that terminals occur immediately preceding a maxima in uncertainty. Thus, the initial classes of a segment are distinguished as a set with predecessors relatively difficult to guess.

With this procedure, the segments are: *If – one believes that all – questions raised by – . . .* These segments are quite different from those obtained from forward prediction and do not agree as well with intonation contours. It may be shown that the average uncertainty for predecessor estimation is the same as that for prediction, but here, predecessor uncertainty has smaller dispersion and the segment boundaries are less well defined.

PHRASE PREDICTABILITY

Next, we consider some possible segmentations induced by the predictability of segments which may be longer than a word. For example, suppose that in the spirit

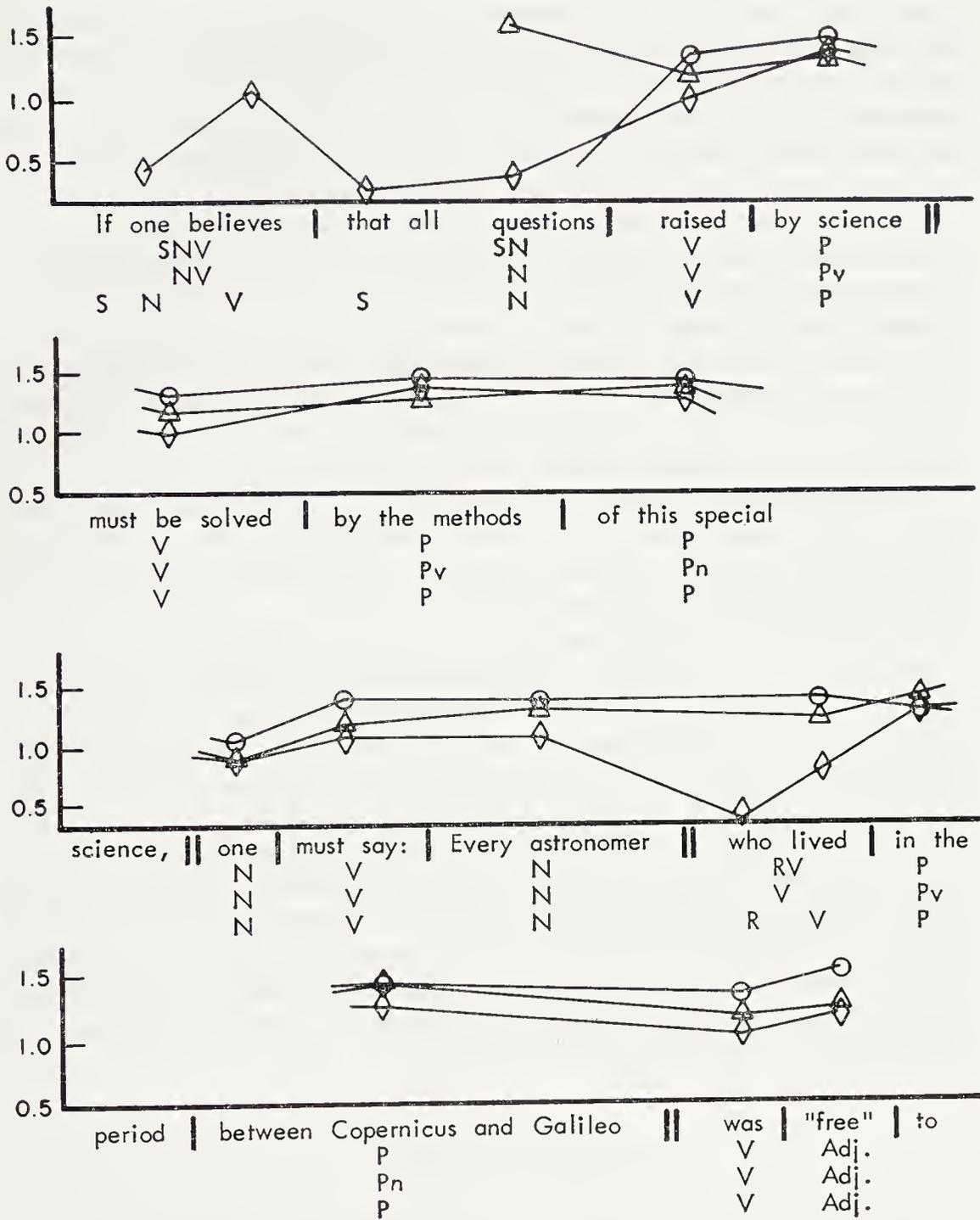


Fig. 2. Uncertainty in Predicting Phrases

of performing a mechanical constituent analysis, we consider the sequences of distribution classes whose terminals have greater than average uncertainty as first-level constituents. We then take as second-level constituents the sequences of first-level constituents with high terminal uncertainty, and so on. We could, for example, require that each distinct sequence of distribution classes belong to its own group, but in this case there would be too many distinct sequences for manageable computation.¹¹ To reduce the number of sequence types we make the mappings shown in Table I. For convenience we refer to the sequence types as phrases. The resulting segmentation is dependent on phrase classification.

Classification I is a mapping based on content of distribution classes.

A second possibility is to consider the grammatical function of the segments. In Classification II, prepositional phrases, the most frequently occurring phrase types, are labeled according to their function as modifiers [Francis]. In addition, further grouping is made to reduce the number of phrase types.

For comparison, we also consider the Classification III, similar to that of traditional grammar. For this classification, phrases are labeled directly without reference to predictability of distribution classes. Elements of discontinuous constituents are labeled according to their component parts.

Figure 2 shows the estimated uncertainty in predicting a following phrase, given two immediate phrases. Estimates using Classifications I, II, and III are denoted by \circ , \triangle , and \diamond , respectively. Since these estimates were made from the same corpus that was used for the preceding section, phrase grouping reduces the sample size and the estimates are not as reliable. The variation in uncertainty is smaller than that found in Class prediction, especially for Classifications I and II.

The general pattern of uncertainty is the same for all three classifications. The segments bounded by maxima for Classification II are indicated by double bars. The segment . . . *one must say: Every astronomer* . . . reflects that this phrasal classification does not directly consider intonation contours. Since the sample size is relatively small, it is questionable whether some apparent maxima are truly so.¹² Examination of larger excerpts from this particular text, shows that sentence beginnings are characterized as sets with relatively low uncertainty.

SOME CONCLUSIONS

The examples here concern a small portion of a single corpus with one particular system of distribution classes. Similar results on class predictability are found for

¹¹ Approximately 60 distinct segments were found having terminals greater than average uncertainty. On the other hand, the number of different sequences of distribution classes in the corpus are 126 pairs and 516 triples. The rank-frequency relationship has approximately a log-log Zipf-like distribution.

¹² This is the subject of a separate statistical study.

other samples of literary English.¹³ The results presented here are representative of the extent of variation in predictability given only the partial information contained in grammatical forms. The average uncertainty in predicting classes and phrases is comparable, but the variation in class predictability is much larger. The segmentation induced by forward prediction agrees quite well with that of intonation contours. Reverse estimation gives different patterns. With the classifications used in this paper, phrase groupings sometimes cross sentence boundaries and reflect that intonation contours are not used directly in the assignment of phrase types.

The approach here may serve as an aid to syntactic analysis, especially with a finer classification of grammatical forms. These classifications need not be distributionally based and may include intonation information. This is the subject of a continuing study.

The author wishes to thank Mr. A. J. Cook for programming.

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¹³ 2000 word samples from H. Fast, *The Edge of Tomorrow* (New York, 1961) and C.G. Darwin, *The Next Million Years* (New York, 1953). The segmentation is similar, but no extensive study has been made to compare individual uncertainty estimates.

MEASURES OF LINGUISTIC DIVERGENCE

JOSEPH E. GRIMES

In this paper I attempt to develop further the notion of quantifying the expression of linguistic distance by means of an index of degrees of phonologic difference (Grimes and Agard 1959).¹ I also apply this index to several languages of Mexico, hoping to clarify a classification regarding which other expressions of linguistic distance disagree.

DEGREES OF DIFFERENCE

Austin has observed (1957, 544) that phonologic change proceeds by minimal steps along one phonetic dimension at a time. We made a phonetic scale by taking the minimal allophonic distinctions necessary to deal adequately with Romance phonology, ranked according to Pike's concept of rank of stricture (1943, 129 ff.; 1955, 23-24) and organized into six independent articulatory dimensions: point of articulation, constriction of the air stream at the median line, effective timing of the central constriction, secondary oral shaping, velic action, and laryngeal action. By finding how two sounds differ on each of these variables, and summing the differences, we arrive at an expression of degrees of difference for the sounds. If the measure is applied to sounds in different languages that correspond genetically, and summed over all sets of sound correspondences, the result is a numerical expression of the linguistic distance between those two languages.²

¹ Works are cited by author and date, sometimes with page reference. William M. Austin, "Criteria for phonetic similarity", *Language (Lg.)* 33. 539-44 (1957); Joseph E. Grimes and Frederick B. Agard, "Linguistic divergence in Romance", *Lg.* 35. 598-604 (1959); Sarah C. Gudschinsky, *Proto-Popotecan*, supplement to *International Journal of American Linguistics (IJAL)*, 25 (1959); Robert B. Lees, "The basis of glottochronology", *Lg.* 29. 113-27 (1953); Robert E. Longacre, *Proto-Mixtecan*, supplement to *IJAL*, 23 (1957); idem, "Swadesh's Macro-Mixtecan hypothesis", *IJAL*, 27. 9-29 (1961); idem, and René Millon, "Proto-Mixtecan and Proto-Amuzgo-Mixtecan vocabularies", *Anthropological Linguistics* 3.4, 1-44 (1961); Kenneth L. Pike, *Phonetics* (Ann Arbor, 1943); idem, *Language*, Part II (Glendale, 1955); Morris Swadesh, "Problems of long-range comparison in Penutian", *Lg.* 32. 17-41 (1956); idem, "The Oto-Manguean hypothesis and Macro Mixtecan", *IJAL* 26. 79-111 (1960).

² The computations on which this paper is based were performed at the University of Oklahoma Computer Laboratory, most of them in connection with the Computer Science Conference sponsored at that institution by the National Science Foundation in the summer of 1962; this aid is gratefully

To take an improbable but illustrative example, let us suppose that we examined two speakers in different segments of a recently split speech community. We might find their behavior identical in all details except that one differentiated between *b* and *p* in a position where the other used only *p*. We would then say that there were 500 or so sets of phonologic correspondences with zero degrees of difference, and one set with one degree. Later in the same divided community we would find more and more one-degree differences, and also some earlier one-degree differences that had become compounded to two-degree differences. Still later some two-degree differences would have gone to three-degree differences (while a few might, owing to the near randomness of the divergence process, go back to one-degree differences), and so on. A graph of the number of sets of phonologic correspondences that differ by 0, 1, 2, *n* degrees of difference, numbering degrees from left to right and the number of sets for each degree of difference from bottom to top, would consist of a straight line sloping down from left to right.³ The longer the two systems had been diverging, the less steep would be the slope of the line. The slope measure would thus be an index of linguistic distance.

When the data originally presented (Grimes and Agard 1959) are organized in this way, the picture is approximately as described above (Table I above the diagonal; higher numbers indicate steeper slopes). Not all points in the data, however, lie exactly on a straight line. There is a scatter of points around the line, a measure of which is given in the part of Table I that lies below the diagonal (higher numbers indicate less scatter).⁴ The scatter measurement and the slope measurement are linearly unrelated; that is, scatter does not necessarily increase or decrease in pro-

acknowledged. The tables that appeared in the preprinted version of this paper were calculated by hand while on an extended field trip, and contained mistakes whose correction required revision of some conclusions presented there. The chief difference is that whereas I formerly held a mediating position between Swadesh and Longacre regarding the Mixtecan grouping, I now agree with Longacre.

³ An exponential function $y = a \exp(-mx)$ gives a better fit to the data by the method of least squares than does the straight line $y = b + mx$ used in this paper. $y = a(1 + cx + dx^2) \exp(-mx)$ would probably give a still better fit. There is, however, no need in the theory to assume that the amount of change that takes place depends on the amount that has already taken place, as is the case with those processes described by an exponential function; therefore it seems worth while to regard the straight line as a meaningful description of the process and consider deviations from it to be significant. Why the skewing tends toward the form of an exponential curve (or perhaps a rectangular hyperbola) is a question for further study. A further problem is the appearance of "kinks" or maxima in the data (Grimes and Agard, 1959, 603, Table 2), which make it likely that a modulated exponential or hyperbolic curve will give the best overall fit. It is my hypothesis that these maxima reflect constraints on the essential randomness of the divergence process, related to the synchronic observation that phonologic distinctions tend to apply to more than one pair of sounds in a language so that, for example, if *t* and *d* fall together, it is likely that *p* and *d* will follow suit; and the maxima of the curves may well reflect these clusterings of changes. It is also possible to reason that the maxima are important enough that while the logarithmic scale of the exponential curve smooths them out unduly, the straight line is more influenced by them and thus reflects them more adequately.

⁴ The scatter measurement is the coefficient *r* of linear correlation. It tells how adequate a regression line is to express the relationships of the points involved. A value near ± 1 indicates a strong linear relationship; a value near 0 indicates that a linear relationship is not worth talking about.

TABLE I

Slope of regression line (above diagonal) and r-correlation scatter measurement (below diagonal) for seven Romance languages. Minus signs omitted for all figures.

	Po	Sp	Ca	Fr	It	Ru	Sa	Mean
Po	—	15.5	14.9	11.9	8.5	7.2	6.8	10.8
Sp	.89	—	15.2	9.6	7.6	7.5	6.6	10.3
Ca	.89	.83	—	14.5	6.0	5.8	5.6	10.3
Fr	.80	.87	.78	—	4.6	6.4	5.4	8.7
It	.87	.88	.87	.57	—	10.9	7.8	7.6
Ru	.86	.88	.84	.62	.80	—	6.3	7.3
Sa	.92	.91	.90	.71	.73	.80	—	6.4
Mean	(.87)	(.88)	(.85)	(.72)	(.79)	(.80)	(.83)	10.2

Po(rtuguese), Sp(anish), Ca(talán), Fr(ench), It(alian), Ru(manian), Sa(rdinian)

portion to the slope.⁵ In linguistic terms, the measure would indicate that languages tend to diverge regularly regardless of distance.

My hypothesis, which this paper should show is not altogether unreasonable, is that the scatter measurement reflects a disturbance of the ideal picture of phonologic divergence brought about by random innovations. The measurement itself gives little clue whether the disturbance should be attributed to borrowing, drift, or anomalous developments within a single language; it only suggests that the phonological development of a language has somehow been skewed from the kind of development that random generation of innovations, and almost random acceptance of them, is expected to show.

It should be kept in mind that these index figures are derived from the results of comparative phonology, not by secondary application of them (as in glottochronology). The data used are the comparativist's statements that in Position *x*, Sound *a* of one language corresponds to Sound *b* of another, to Sound *c* of another, and so on, plus a phonetic description of the articulation of each of the sounds. The index is therefore but a numerically expressed overview of what the comparativist has already said; but it appears to show up certain relationships in his data, such as the scatter relationship, that might not be as easily grasped from the linguistic data as such.

ROMANCE

Groupings of the Romance languages deducible from the line slope figures of Table I may be summarized as follows: The three Iberian languages clump together, with

⁵ For a sample of the Oaxacan language pairs, slope and scatter show zero correlation. It is possible that for certain pairs (such as Popoloc-Amuzgo) high scatter may have influenced the slope measurement slightly. In the aggregate, however, the measures are independent.

Catalán slightly apart from the other two. French appears closer to Catalán than to Portuguese-Spanish, but a good deal closer to the Iberian languages as a group than to Italian. Rumanian and Sardinian are each closer to Italian than to anything else, but not particularly close to each other. This arrangement differs from the one we first presented (in which only the mean degrees of difference per set of correspondences was considered) mainly in the neatness of the cleavage between the western group of languages (those farthest removed from Sardinian) and the rest (which in the earlier study were farthest removed from French). Here the grouping opposite Sardinian holds good, but Rumanian is farther removed from Catalán than from French, though Italian and Sardinian are both farther from French than from any other.

The scatter measurements, as interpreted by the hypothesis, suggest that Portuguese developed with little interaction from the other peninsular languages. French, however, appears to have had more influence on Portuguese than on Spanish, and still more on Catalán. Sardinian shows high independence from the Iberian languages, but not from the rest. Those figures that indicate extreme skewing (French-Italian and French-Rumanian) point up an interesting implication of the hypothesis. Although there was probably continued mutual influence over a period of time between French and Italian, it is doubtful that mutual influence between French and Rumanian was intense enough or protracted enough to account for such thorough skewing as Table I indicates. But French shows high scatter no matter what language it is compared with, and to a lesser degree so does Rumanian. It seems plausible to suggest that French and Rumanian were each skewed individually, giving a high scatter for the pair that is not necessarily due to mutual influence. It is tempting to guess that the scatter in Rumanian could reflect the influence of non-Romance speech communities that have interacted with the Rumanian community (or communities). It is not too likely, however, that other forms of speech influenced French in the same way. The scatter figure may therefore be due to anomalous developments within French itself, or perhaps to developments at differing rates in different parts of the sound system.

OAXACAN LANGUAGES

Table II shows degree of language separation (expressed above the diagonal in terms of slope; higher numbers indicate steeper slope and less separation) and scatter (below the diagonal; higher numbers indicate less scatter) for three Mazatec dialects (Mazatlán de Flores, Huautla de Jiménez, San Miguel Soyaltepec), Ixcatec, Chocho, and Popoloc (Gudschinsky, 1959); Mixtec of San Miguel el Grande, Cuicatec of Concepción Pápalo, Amuzgo of Omtepec, Guerrero, and Trique of San Andrés Chicahuaxtla (Longacre, 1957, and personal communication). All are spoken in or near the state of Oaxaca, Mexico.

TABLE II

Slope of regression line (above diagonal) and r-correlation scatter measurement (below diagonal) for ten languages of Mexico. Minus signs omitted for all figures.

	Mz	Hu	So	Ix	Ch	Po	Mx	Cu	Am	Tr	Mean
Mz	—	18.7	12.4	8.4	7.3	7.6	6.4	5.6	6.3	4.4	8.6
Hu	.79	—	11.3	8.0	7.0	7.3	5.6	4.8	5.6	4.6	8.1
So	.67	.68	—	9.0	7.7	7.6	6.3	6.1	6.4	4.5	7.9
Ix	.80	.77	.76	—	10.4	8.1	6.2	5.4	6.6	4.7	7.4
Ch	.86	.79	.80	.74	—	7.5	6.2	5.2	5.8	4.3	6.8
Po	.81	.84	.82	.81	.80	—	5.6	4.9	6.0	4.7	6.6
Mx	.82	.88	.86	.90	.91	.87	—	7.8	6.2	5.0	6.1
Cu	.87	.91	.92	.95	.97	.95	.77	—	5.6	8.0	5.9
Am	.88	.89	.90	.90	.96	.93	.82	.87	—	2.0	5.6
Tr	.76	.86	.82	.84	.82	.81	.67	.71	.62	—	4.7
Mean	(.81)	(.82)	(.80)	(.83)	(.85)	(.85)	(.83)	(.88)	(.86)	(.77)	7.5

Mz(Mazatlán), Hu(autla), So(yaltepec), Ix(catec,) Ch(ocho), Po(poloc), Mx(Mixtec), Cu(icatec), Am(uzgo), Tr(ique).

Table II shows greater language separation but less scatter than in Romance (compare Longacre and Millon, 1961, 3). The slope measurements support the generally recognized groupings of Mazatec-Ixcatec-Chocho-Popoloc. The more problematical ordering of Mixtec, Cuicatec, Trique, and Amuzgo comes out, by this reckoning, more closely aligned with that of Longacre (1961; Longacre and Millon, 1961) than with that of Swadesh (1960). My quantification, derived from 131 sets of sound correspondences taken from the work of Longacre and Gudschinsky,⁶ shows Mixtec closer to Cuicatec than to any other language, Cuicatec closer to Trique than to any other, and about as far removed from Trique as from Mixtec, and Trique closer to Mixtec than to any language except Cuicatec. In other words, the three form a well-defined group distinct from Amuzgo on the one hand and the remaining languages on the other, and are furthermore related in such a way that Longacre's conclusion that the three reconstruct at a single horizon (1957, 3; 1961, 2) is not unreasonable. Amuzgo, on the other hand, appears to occupy a position equidistant from Gudschinsky's "Popotecan" group and from Longacre's Mixtecan. The extremely low slope figure for Amuzgo-Triple should probably be interpreted together with the figure that indicates extremely high scatter for the pair. Among the three Mixtecan languages, especially between Trique and Mixtec, scatter is high, indicating perhaps a general freedom of communication from early times. Trique, however, like Rumanian shows relatively high scatter in all pairings; this may indicate anomalous development, perhaps of such a nature as would also result in skewing of the slope figures for

⁶ For this run sets of correspondences derived from their summary statements were used. It is not absolutely certain that the summary statements cover all valid sets of phonologic correspondences, but the more detailed statement now in preparation will probably not show noticeably different results from the preliminary statement on which this paper is based.

Amuzgo-Triqué. It is worth noting that while Triqué shows higher scatter with Amuzgo than with Mixtec or Cuicatec, Amuzgo shows much lower scatter with Mixtec and Cuicatec than it does with Triqué, thereby strengthening the notion (which depends on recognition of the unity of the Mixtecan grouping) that there is an anomalous line of development in Triqué itself that is responsible for the difficulties in classification.

COMMENT

The relationship between the index of degrees of difference, comparative phonology, and glottochronology deserves further attention. If, as suggested, scatter indicates the extent to which a randomly based pattern of divergence has been disturbed or constrained, it follows that the historical development of phonologic systems may be influenced by factors not directly associated with the internal dynamism of the languages themselves. The French case, considering the relatively high scatter French shows in all pairings, would point toward rapid and anomalous internal development, perhaps with different portions of the system developing at different rates or in conflicting ways. The Rumanian and Triqué cases might parallel the French, but might equally well point toward long periods of widespread bilingualism involving languages outside the group under study. It is also possible to envision a case in which parallel constraints on a pair of languages would indicate a drift tendency.

Because of its dependence on comparative phonology, the measurement of degrees of difference is a more refined index than glottochronology. For one thing, there is no problem of identification of cognates, which is disturbing in glottochronologic counting even when comparative statements are available, as evidenced by Swadesh's remark (1960, 87): "The important question . . . is which figures are the most reliable in the light of the best available phonological theory. . . . In lexico-statistic scoring, a little flexibility is desirable because it is always possible that genuine cognates may have become obscured." There is, to be sure, a danger in glottochronology of rejecting a cognate that should have been accepted, but in any statistically based procedure, this danger has to be weighed against the danger of accepting as a cognate what should have been rejected. The safeguards against one are not the same as the safeguards against the other, and if the statistical basis of glottochronology is to be maintained, "a little flexibility" is no safeguard at all. At any rate, the glottochronologic index will not lose its usefulness as a quick means of roughing out hypotheses regarding linguistic relationships, though it would be more in keeping with this use of it if the confidence intervals associated with its "dates" were changed. Glottochronological figures are more often interpreted as though they were point estimates of time, rather than confidence interval estimates (i.e., a figure of 53 minimum centuries actually means a date for which chances are asserted to be 9 to 1 that it belongs somewhere in the interval from 44 to 63 minimum centuries; see Lees 1953). I would therefore

prefer to see glottochronologic statements made with say 50 per cent confidence intervals rather than 90 per cent (which would mean roughly that there is a fifty-fifty chance that the relationship lies within the interval given). The narrower interval would reflect more realistically the kind of use made of glottochronologic figures, and the lowness of the interval would be more in keeping with the general lack of refinement of the method, as evidenced by continuing discussion about retention rate (as in this paper), "noncultural" vocabulary, and criteria for counting cognates in the absence of a developed comparative phonology.

On the other hand, the notion that underlies the index of degrees of difference implies that areal influence, which Swadesh considers so important for the application of glottochronology at extreme time depths (1956: 23), may operate as constraints that keep languages from developing according to the ideal pattern of randomly introduced innovations. The knowledge of neighboring languages could have a screening effect on randomly generated innovations, so that those compatible with the sounds of neighboring languages would have a greater likelihood of acceptance.

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MATHEMATICS OF GENEALOGICAL RELATIONSHIP BETWEEN LANGUAGES

GUSTAV HERDAN

I. DETERMINATION OF NEARNESS OF RELATIONSHIP

In his paper on "Philological Probability Problems" (1950),¹ Ross has described certain attempts at mathematical formulation of linguistic relationship. He mentions briefly the method of Kroeber and Chrétien (1937, 1939)² on the relationship of Indo-Germanic languages which he criticises without, however, offering a satisfactory alternative solution.

Kroeber and Chrétien constructed four-cell tables for each language pair and calculated the product-moment correlation coefficient, with values 1 and 0 for each language according to whether a word is found in it or not, and consequently with classes 11, 10, 01, 00 according to whether a word occurs in both languages, in one only, in the other only, in neither. The product-moment correlation coefficient which for a table of this kind is also known as the Bernoullian correlation coefficient, reduces then to

$$r = (ad - bc) / \{(a + b)(a + c)(c + d)(b + d)\}^{1/2} \quad (1)$$

where a stands for the class 11, b for 10, c for 01, and d for 00. The value of r varies between the values of -1 and $+1$, with 0 for lack of correlation. When ad is greater than bc , the correlation is said to be positive (between 0 and $+1$), when bc is greater than ad it is said to be negative (between 0 and -1).

However, it was found that when relationship was measured in this way, the results sometimes contradicted what had been established previously by philological considerations: some such established genealogical relationships appeared in terms of r to be not significant.

This induced Ellegård (1959)³ to use another formula for the correlation coefficient, viz.,

$$r_n = a / \{(a + b)(a + c)\}^{1/2} \quad (2)$$

where, as before, a is the overlap in words between two languages, b the words peculiar to one language, and c the words peculiar to the other. Ellegård was not

¹ *J. Roy. Stat. Soc. (B)* 12, No. 1, 39 (1950).

² *Lg.*, 13 (1937), 83-103 and *Lg.*, 15 (1939), 69-71.

³ *Lg.*, 35 (1959), 131-156.

the first to use that formula. The writer has used it in his book *Language as Choice and Chance*, p. 49ff.⁴ for the determination of vocabulary correlation, and it had been used by others, e.g. Kroeber (1932). This measure of relationship is always positive, and can vary from 0 to + 1. It has the intelligible meaning that "the values of a, b and c stand for all features which are positively recorded in one or both of the languages".

Ellegård regards r as a measure of similarity, and r_n as one of interdependence or interinfluence. A more unambiguous definition and one which will prove helpful in arriving at a more satisfactory treatment of the problem is this. The relationship measured by r is based not only upon the words common to both, and those peculiar to one or the other, languages, but also upon the words belonging to a larger whole and not appearing in either language. The larger whole is what I call the Universe of Discourse which, in the case under consideration, is the total of indogermanic roots established so far. The coefficient r_n , on the other hand, is based only upon what is positively recorded of the two languages, i.e. the words in common and those peculiar to either, the negative class, and with it the universe of discourse, being taken as infinite.

Ellegård is right in regarding r_n as a more suitable measure of linguistic relationship. However, in spite of this, it cannot by itself be regarded as a full substitute of r , because it just leaves out the important feature of the common parentage of all roots, which is the basis of the IE-theory of linguistic relationship. This may account for the great dialectical difficulties in which Ellegård gets involved when trying to supplement the information given by r_n , in order to bring the results of the mathematical method into harmony with the philologically established facts of the genealogical relationship of IE languages. So vast an amount of argument on top of a mathematical method detracts very much from the value of the latter, one of the great advantages of using mathematics being that it makes additional argument largely superfluous. This can be achieved by choosing a more suitable mathematical procedure, and as such a procedure I propose to use the method of *Factor Analysis* for the determination of linguistic relationship. It enables us to work with r_n without abandoning the basic fact of a common ancestor of all the languages under consideration. The interpretation of results is straightforward, and does not require to be supplemented by extensive argument. Its main advantages are:

1. The correlation coefficient r for a pair of languages depends not only upon Proto-Indoeuropean (PIE) as the common parent, but also upon other factors which may be applicable only to that particular pair, e.g. language mixture, geographical position etc. Factor analysis, on the other hand, considers not only particular pairs of languages with regard to the number of PIE roots they have in common, but every language in its relation to all the other languages in the group.

2. It enables us to determine the "saturation" of a given language with PIE as

⁴ Published by P. Noordhoff, Groningen, 1956.

the factor common to all languages, the influence of what may be called secondary factors, such as geographical position, and finally the amount of "specificity" as that which distinguishes a given language from all others in the group.

II. FACTOR ANALYSIS OF GENEALOGICAL RELATIONSHIP OF LANGUAGES

As is well known, the relationship between Indo-Germanic languages is usually based upon the number of roots they have in common. The following table (Table I) of Indo-European roots as given in the paper by Ross was compiled from the standard Walde and Pokorny *Vergleichendes Wörterbuch der indogerm. Sprache* (1926-32). The first column gives selected pairs of Indo-European languages, the second and third columns give the numbers of Indo-European roots in the first language of the pair and in the second language, respectively, and the third gives the overlap, i.e. the number of roots they have in common.

TABLE I

*Number of Roots Common to Certain Indo-European Branches**

	n ₁	n ₂	R		n ₁	n ₂	R*
Ce It-Gr	1,184	1,165	783	Ar-Ir Sk	442	1,016	305
-Ar	1,184	442	333	-Sl Ba	442	1,213	312
-Ir Sk	1,184	1,016	694	-Ge	442	1,256	329
-Sl Ba	1,184	1,213	777	-Al	442	290	130
-Ge	1,184	1,256	865	Ir Sk-Sl Ba	1,016	1,213	657
-Al	1,184	290	236	-Ge	1,016	1,256	693
Gr-Ar	1,165	442	333	-Al	1,016	290	223
-Ir Sk	1,165	1,016	694	Sl Ba-Ge	1,213	1,256	876
-Sl Ba	1,165	1,213	753	-Al	1,213	290	220
-Ge	1,165	1,256	763	Ge-Al	1,256	290	228
-Al	1,165	290	242				

* The abbreviations are as follows:

- Ce It = Italo-Celtic
- Ar = Armenian
- Ir Sk = Indo-Iranian
- Sl Ba = Balto-Slavonic
- Ge = Germanic
- Gr = Greek
- Al = Albanian

The data of the above table enable us to calculate the correlation coefficient by the formula

$$r = R / (n_1 \times n_2)^{1/2} \tag{3}$$

where R is the overlap in Indo-European roots in the given pair of languages and n₁, n₂ are the numbers of Indo-European roots in the two languages concerned. The use of that formula for correlation is discussed in detail in the work referred to in footnote 4, and, as mentioned above, more recently by Ellegård (1959). We then

obtain the following table of correlation coefficients (Table II). The decimal point has been left out in the body of the table. Table III has the same elements, but re-arranged according to magnitude of column total. The figures in brackets represent the values of the correlation coefficient estimated by formula (6).

TABLE II

	Ce It	Gr	Ar	Ir Sk	Sl Ba	Ge	Al
Ce It	—	665	459	633	648	709	403
Gr	665	—	464	636	633	631	416
Ar	459	464	—	455	426	442	363
Ir Sk	633	636	455	—	592	613	411
Sl Ba	648	633	426	592	—	710	371
Ge	709	631	442	613	710	—	378
Al	403	416	363	411	371	378	—
Sum	3.517	3.445	2.609	3.340	3.380	3.483	2.342

TABLE III

	Ce It	Ge	Gr	Sl Ba	Ir Sk	Ar	Al
CeIt	—	709	665	648	633	459	403
Ge	665 (661)	631 (654)	—	633	636	464	416
Gr	709 (669)	—	631	710	613	442	378
Sl Ba	648 (645)	710 (637)	633 (630)	—	592	426	371
Ir Sk	633 (637)	613 (629)	636 (622)	592 (607)	—	455	411
Ar	459 (477)	442 (471)	464 (466)	426 (454)	455 (448)	—	363
Al	403 (421)	378 (416)	416 (412)	371 (401)	411 (396)	363 (297)	—
Sum	3.517	3.483	3.445	3.380	3.340	2.609	2.342

TABLE IV

	A ²	A'	A ² -A'	2A	T-2A	$\frac{A^2-A'}{T-2A}$	Saturation g	Specific s
Ce It	12.370	2.139	10.231	7.034	15.082	.678	.823	.568
Ge	12.131	2.118	10.013	6.966	15.150	.661	.813	.582
Gr	11.869	2.033	9.836	6.890	15.226	.646	.804	.595
Sl Ba	11.424	1.995	9.429	6.760	15.356	.614	.784	.622
Ir Sk	11.156	1.908	9.248	6.680	15.436	.599	.774	.633
Ar	6.807	1.142	5.665	5.218	16.898	.335	.579	.816
Al	5.485	0.918	4.567	4.684	17.432	.262	.512	.859

Using factor analysis in its simplest form as devised by Spearman and assuming as a first approximation, one general factor to be responsible for the correlations, and thus a matrix of rank 1, the saturations of the variables by the general factor are calculated according to the formula

$$\text{Saturation} = \{(A^2 - A') / (T - 2A)\}^{1/2} \quad (4)$$

where the A's are the sums of the rows (or columns) of the correlation matrix without entries in the diagonal cells, T the sum of all A's, and the A's are the sums of rows (or columns) of the matrix whose elements are the squared correlation coefficients of Table II. The calculation is exhibited in Table IV. The coefficients of the specifics s_1 are determined by the condition that the squares of the saturations with g and the squares of the coefficients of s_1 must, for each language, add up to unity.

The last two columns give the saturation with the general factor which, in this case, is, of course, the genealogical nearness of relationship, and the specific factor for each language. For specifying the composition of language with regard to general factor and specific factors, assuming the order to be hierarchal, that is assuming only one general factor and also including specific correlations between the languages, we have the equations:

Italo-Celtic	$z_1 = .823 g + .568 s_1$	
Germanic	$z_2 = .813 g + .582 s_2$	
Greek	$z_3 = .804 g + .595 s_3$	
Balto-Slavonic	$z_4 = .784 g + .622 s_4$	
Indo-Iranian	$z_5 = .774 g + .633 s_5$	
Armenian	$z_6 = .579 g + .816 s_6$	
Albanian	$z_7 = .512 g + .859 s_7$	(5)

We see that by the token of common roots the saturation with PIE as a common factor ranges in our 7 languages from .823 for Italo-Celtic to .512 for Albanian, the "specific" rising in the opposite direction from .568 for Italo-Celtic to .859 for Albanian. Under the assumption that one general factor only is responsible for the relationship between our 7 languages, the observed correlations of Table II should be obtainable from the saturations as the correlations of each language with the general factor. Since the languages are conceived to be correlated among themselves only through a third factor, g, their correlations, r_{ij} should be sensibly equal to the product of two saturations, e.g.

$$r_{ij} = r_{ig} \cdot r_{jg} \tag{6}$$

These correlations were calculated and are entered in brackets next to the corresponding observed correlation in Table III. On the whole, there is very good agreement which shows how powerful the PIE factor is for the relationship between any two of our languages. However, there are certain discrepancies, for instance, the estimated correlation between Ge and Sl Ba (.637) is considerably smaller than the observed correlation (.710); on the other hand, the estimated correlation between Ge and Ar (.471) is greater than the observed correlation (.442) and the same applies to the correlation between Ge and Al (.416 calculated as against .378 observed). This might point to another factor common to some of the languages, but not to all. It may be that the nearness in geographical location between Ge and Sl Ba on the one hand and the differences between Ge and Ar, and Ge and Al, on the other hand,

accounts for the discrepancy between the correlation calculated under the assumption of one common factor only and the observed correlations.

It should be emphasized that in establishing these relations between languages only one language characteristic was used, viz. vocabulary. It follows that to the extent to which vocabulary is independent of grammar and phonemic system, it is by no means certain that the same series of relationships between our languages would hold if other characteristics than vocabulary were taken into consideration.

A perhaps obvious criticism of the results might conceivably be that the numerical estimate of nearness of relationship did not add much to our previous knowledge in the matter arrived at without mathematics.

But even if this were true – it is not altogether true because the merely subjective impression of nearness of relationship is always open to criticism, and can never really claim general acceptance–, so much must be conceded that the *method* used is remarkable for its clarity and for the suitability of its conceptual structure for establishing genealogical relationship between languages. That the method enables one to say to what extent a language is “saturated” with PIE as the common factor of all the languages in the group, and to what extent a language is to be regarded as specific or individual, and insofar different from the other languages – quite apart from the possibility of accounting for special correlations between languages by secondary factors, such as geographical location etc. – and to do all this in terms of strictly defined concepts, shows clearly how superior factor analysis is for our purpose to other methods which have been tried so far, e.g. the method used by Kroeber in his paper on “Statistics, Indo-European and Taxonomy” (*Lg.*, 36, 1960, 1-20) with its mixture of legitimate statistical method, arbitrariness and fanciful geometrical construction.

III. IN DEFENCE OF THE QUANTITATIVE METHOD FOR DETERMINING GENEALOGICAL RELATIONSHIP

Professor Pulgram’s views in the matter of establishing linguistic relationship, and his critical attitude to the methods so far used, are of great interest.⁵ Although he seems to accept the mathematical argument involved, he makes no secret of his distrust of statistical methods in linguistics. Factor analysis as an additional instrument for establishing linguistic relationship enables one to avoid some of the pitfalls in this respect to which he has drawn attention.

Pulgram’s argument rests mostly upon the assumption that the investigator does not know at the outset whether the two blocks of linguistic units he is comparing are different languages, or different stages of development of one language, or languages belonging to different families, and that therefore his conclusion about the parent-offspring relation between the two blocks of linguistic units in terms of the

⁵ *Lg.*, 35 (1959), 421–426 and *Lingua*, X (1961), 18–37.

correlation coefficient r_n may be fallacious. However, the difference in magnitude of the correlation coefficient r_n could show him at once that he had not compared like with like. Whereas the different Indogermanic languages among themselves would have correlation coefficients between, say, .4 and .7, the two stages of German among themselves would most likely have a correlation coefficient close to unity, and French and Arabic a non-significant correlation coefficient. More conclusively, such inhomogeneity of the correlation coefficients would violently disturb the so-called *hierarchal order* of the correlation matrix, *which is indicative of one general factor only as being responsible for all the correlations*. From this the conclusion could be drawn that what was responsible for the association of vocabulary in two different PIE-languages is not identical with what produces the association between the two stages of development of German, nor was it active in such spurious connection as could perhaps be found between French and Arabic.

Thus, it is the mathematical method which could reveal the nature of the relationship: whether it is one between different stages of one language, or one between different genealogically related languages, or one between linguistically unrelated languages.

Factor analysis has developed into a very complete, and complex, method of which I have given here only the very first stage. But I fear that if I were to use all the refinements of the method to help the linguistic research worker, the very first objection to be encountered on the part of the linguist would be that the linguistic research worker does not start *ab ovo*, or double-blind with regard to language history and geography, and the mathematician should not include a lot of heterogeneous word masses in his design. Paraphrasing the well-known couplet in one of Gilbert & Sullivan's operas about the "Policeman's Lot", it would appear that "The Linguistic Statistician's Lot is not a Happy One".

INTRODUCTION TO THE DISCUSSION

This is a good example of what is meant by quantitative linguistics, i.e. linguistics which fundamentally requires quantitative thought on the part of the linguist, and where the mathematics is not something superimposed from outside upon the linguistic thought form. The linguist who wishes to establish genealogical relationship between two languages forms his view by considering what different characteristics they have in common, and thus the overlap in vocabulary, phonemic system, and grammar, excepting that part of overlap which may be due to borrowing from each other or from a third source. All the mathematician does in this case is to put a number to it. To be able to do so, and, in general, to apply mathematics to language, we must look for linguistic variables which are independent of meaning. Such variables are, though by no means all to the same extent: the sounds (phonemes) of language and the corresponding signs of the written language (alphabetic symbols),

for which de Saussure enunciated the axiom of independence from meaning; secondly the word (morpheme) frequency, which in more recent times has been shown beyond reasonable doubt to be a chance variable of language,⁶ the frequency distribution following the same distribution law whatever the meaning of the running text from which the sample which we investigate was drawn; and thirdly, word order within the sentence.

The first independence axiom has made information theory possible, the second stylo-statistics, and the third accounts for both the possibility and the difficulty of machine translation.

There are, of course, degrees of independence from meaning, the axiom applying almost without restriction only to the sounds of language; in a somewhat restricted way it applies to the word frequency where, however, deviations in the use of words due to individual preferences on the part of the writers are not only possible but the very thing one wishes to establish in stylo-statistics; and still more is the axiom restricted in the case of word-order.

However, explaining the possibility of applying mathematics to language as I have done here brings out the fundamental fact that mathematical linguistics appears to be fully in line with the requirement which Leonard Bloomfield set up for linguistics as a science (as delineated so clearly in the admirable paper by Professor Fries on Bloomfield in *Trends of American and European Linguistics 1930-1960*), viz., that individual meaning of words is not to be emphasised if linguistics is to be established as a science in the strict sense of the term. Overstressing the shades of meaning according to the context, as for instance J. R. Firth has done, prevents generalisation, and since generalisation is the soul of science, hampers scientific thought.

Quite independently from Bloomfield's school, mathematical linguistics, by exploring such aspects of the language as are independent from meaning, turns out to be in full accordance with Bloomfield's ideas of linguistics as a science. In this way, it has succeeded in establishing some laws of the general use of language, as different from the individual use, with which the literary scholar is concerned.

University of Bristol

DISCUSSION

KUČERA:

While the mathematical procedure suggested by Dr. Herdan appears suitable for a quantitative evaluation of certain factors of genealogical proximity of languages, the method has some rather serious limitations. These limitations make it doubtful that it would be feasible – as Dr. Herdan suggests – to use the method “for establishing genealogical relationship between languages”.

⁶ See footnote 4.

The problem is, first of all, the selection of the linguistic units used as the basis of the calculation. As long as some lexical units assumed to be "roots" of the proto-language are utilized for this purpose, it is obviously first necessary to set up a list of such lexical items. This can be done, in most cases, only by analyzing the potential cognates in the various daughter-languages of such a linguistic family. It would thus appear that one would have to establish, through a thorough comparative and historical analysis, the existence of genealogical relationship between the languages in question before one could even begin to compile a list of the items to be used in the calculation proposed by Dr. Herdan. Even then, the problems are not over: is one to include in such a list only items which are attested in all the daughter-languages of the family? Or in the majority of such languages, or perhaps only in one branch of the family, or even in two languages only?

If factor analysis is applied without a prior determination of the "roots" of the proto-language and if lexical items are considered to correspond to each other on the basis of some undefined "similarity" (in itself a problematic matter), a host of other difficulties arises, among them the complication of loan words. If one tried Dr. Herdan's formula on such language pairs as Russian-Mordvin or Russian-Mari, one could, on the basis of the results, easily come to a mistaken conclusion that Russian and the two Finno-Ugric languages are genealogically related. Such a conclusion would be due to the extremely high percentage of Russian loan words in the vocabularies of Mordvin and Mari, which in some types of discourse may reach 75 to 80 per cent of the total.

It could be perhaps argued that factor analysis might be applied with advantage to other than morphemic units, for example to phonemes. But this could hardly be very revealing. Dr. Herdan's procedure allows us to consider in the calculation only the information which items are present or absent in one or both of the compared languages. If one simply compared phonemic inventories of two languages from this point of view, one could obviously not expect to obtain any relevant information about the genealogical relationship of the two languages. Since other factors which may be important in this respect, such as relative frequency of phonemes or constraints on the occurrence of phonemic sequences, cannot be accommodated within Dr. Herdan's procedure, the approach – at least in its present form – does not seem to hold much promise in quantitative phonemic typology.

In general, it would seem that Dr. Herdan's procedure might be useful for the confirmation of certain genealogical relationships or for the quantitative evaluation of the proximity of such relationships. It does not appear to me, however, to be a feasible discovery procedure for establishing genealogical relationships between languages where such relationship is not already known.

HERDAN:

It is true that there is a certain unevenness in size of the language sub-groups, but this is not my doing. I took the basic data for my calculations, i.e. the numbers of

Indogermanic roots, from the paper written by a linguist, Professor A. C. S. Ross of Birmingham University, quoted under (1). He collected them from the standard work on PIE vocabulary, Walde-Pokorny, quoted under (5). This seemed good enough for my purpose, which was primarily to devise a suitable method for the problem in question.

The unevenness in the size of language groups in no way affects the calculation, which is based exclusively upon the correlation coefficients for any pair of languages.

There are certain discrepancies between the theoretical (bracketed) and the observed values of Table III, which could be explored with a view to establishing a second and perhaps a third common factor, perhaps geographical position and borrowing. A feature of the proposed method to which I should like to draw special attention is that it gives the answer to the question in which linguists are most interested, viz. to what extent a given language is saturated with PIE (equations on pp. 53-55). No other correlation method tried so far is capable of this. It also provides the proof for a common ancestor of all languages concerned through formula (6).

An interesting point which has been raised is that of possible discrepancy between the mathematical result and the findings by the linguist not using numerical methods. In my opinion such apparent discrepancies are easily explained and in no way detract from the value of the mathematical method. When the linguist arrives at the conclusion that languages A and B are closer related than languages A and C, he does so by considering summarily the number of common words (vocabulary) of grammatical, morphological and phonemic features which the two languages have in common. His view is thus synoptical. The mathematician, on the other hand, who wishes to put a number to these observational findings in order not to be misled by mere subjective impression, must confine his attention and calculations to one characteristic at a time. It is now quite feasible that languages A and B may have a relatively greater vocabulary overlap than A and C, which would make the correlation coefficient in this respect greater for the former pair, but A and B may have less in common as regards phonemic, morphemic and grammatical features, with the result that a synoptic view of their correlations would show A and C to be, on the whole, closer related. This is clearly brought out if the correlation method is applied to each linguistic characteristic separately.

MATHEMATICAL LINGUISTICS – A TREND IN NAME OR IN FACT?

HENNING SPANG-HANSEN

1. In the contact between fields of human knowledge borrowed terms play an important part by bridging some of the gaps of communication between scholars and scientists of unconnected educational backgrounds. Frequently, however, the borrowers are a little careless, and the borrowed things could not on demand be returned in an undamaged condition: By the transferring of a term to a different branch certain aspects may be lost, in particular such aspects that kept this term in position among other terms in the original field. For this reason it may well be asked whether after all such borrowings further or hamper the understanding between different fields of human knowledge.

For instance, what will be the result of the practice now adopted in mechanical data processing of using *word* and *language* as designations of certain non-linguistic phenomena. Or, the result of the increasing use in descriptive linguistics of loan-words from the normative Information Theory (i.a. *redundancy*, *information*, and even *entropy* [of texts!], in its turn borrowed from thermodynamics).

2. Linguistic borrowings of certain mathematical terms form other examples, first of all the designation *mathematical linguistics* itself, which (especially in American literature) is considered the name of a particular trend, cf. the inclusion in the volume *Trends in European and American Linguistics 1930-1960* (Utrecht, 1961) of a particular survey “Mathematical Linguistics”, by Warren Plath (Referred to below as *Trends*).

The subjects dealt with in the survey are in my opinion all very important, and I find Plath’s exposition extremely balanced and lucid. But just due to this lucidity the survey discloses the fact that it is premature, and probably in principle mistaken to introduce a “mathematical linguistics” as a separate and coherent trend or branch. While “schools’ frequently strike the outside observer as distressingly slow in recognizing their own existence” (F. J. Whitfield) it seems that the “trend of mathematical linguistics” has been amazingly quick in this respect. What matters here is not the recency of the designation (coined about 1954) but the fact that one and the same heading is intended to cover (earlier and recent) investigations, efforts, and points of view that are divergent both with regard to their linguistic nature and with regard to their more or less mathematical nature, cf. sections 3-5 below.

In my opinion “mathematical linguistics” does not form a trend in fact, and

accordingly I consider the designation a mere label. As such it might be harmless, but actually its use seems to hamper linguistic discussions, on the one hand by blurring essential distinctions, and on the other hand by giving rise to a – quite superfluous – schism between “non-mathematical” linguistics and the supposed “mathematical linguistics”, often imagined as a new, independent, and more exact approach to linguistic phenomena.¹

3. Such misinterpretations chiefly originate in the fact that when forming part of the designation “mathematical linguistics” the borrowed term, viz. *mathematical*, may be understood in either of two senses, whereas in most other contexts the context itself points out one sense.

“Mathematical” refers to 1) certain *principles* according to which certain subjects have been (and still are) studied, viz. a purely axiomatic (hence non-empirical) and deductive approach, and 2) the *results* (conclusions) of the particular studies thus carried out, i.e. the achieved descriptions of the subjects dealt with; examples of results are the numerical value of π , obtained formulae of solving given (types of) equations, and confidence limits set up in mathematical statistics.

Admittedly, from the point of view of pure mathematics a distinction between axiomatics and results (conclusions) can be considered arbitrary, since within certain limits one is free to alter the axiomatics in question by turning some results into axioms, at the expense of some parts of the first axiomatics. However, when mathematics comes into contact with the study of empirical subjects (e.g., physical or linguistic phenomena) — in other words: when some model of a “mathematical nature” is by hypothesis considered adequate to an empirical description — it makes an essential difference whether, 1) the “mathematical model” confines itself to the very principle of axiomatic-deductive studies, or whether 2) the model consists of a definite set of axioms and conclusions (mathematical results) that need only an interpretation in terms of the particular empirical subject in order to furnish us with a definite description.

4. With regard to linguistic subjects the latter case is found in particular in statistical, or more generally speaking, quantitative linguistics. The relevant mathematical models are partly of an extremely elementary nature, viz. counting together with elementary arithmetic, but often highly refined statistical models are applied. Even though the models sometimes appear as algebraic (symbolic) formulae, and even though in several empirical cases numerical values for insertion cannot actually be ascertained, statistical linguistics is characterized by the relevance of *number* (numerical quantity), viz. numbers of occurrence (repetitions) of linguistic units. On the other hand it is in principle irrelevant to the statistical models *on which basis the counted qualitative units*, e.g. allophones, phonemes, words, sentence patterns, have been set up.

¹ A criticism along similar lines is found in O.S. Akhmanova (red.), *O točnykh metodakh issledovanija jazyka* (Moskva, 1961), p. 3.

Thus statistical or quantitative linguistics forms an additional approach, supplementary to and actually presupposing qualitative linguistic analysis and description. Since number is the basic notion added in this supplementary way of linguistic study, it is no wonder that the supplementary theoretical models are sought among results obtained in particular mathematical disciplines (arithmetic, mathematical statistics). It would be absurd to try to develop independently such models within linguistics itself. This fact does not exclude, however, that the particular needs of linguistics encourage the study of new parts of these mathematical disciplines.

According to its specific methods and its particular theoretical basis, as far as mathematics is concerned, statistical or quantitative linguistics has for some period formed a particular linguistic discipline or branch (which is not the same as a trend) and was recognized as such in 1948 by the establishment of CIPL's Comité de la statistique linguistique. Letting a "mathematical linguistics" now absorb this branch does not get us anywhere. It is worthy of note that in Plath's above-mentioned survey a major division is made between two kinds of models in "mathematical linguistics", viz. statistical models of language, and structural models of language.

5. Turning to the non-quantitative aspects of linguistics, i.e. to the qualitative analysis and description of linguistic subjects, the situation with regard to mathematical models is different. As well known, certain mathematical disciplines (e.g., set theory) offer results that may be applied as models by a qualitative description of empirical subjects, even though such applications have not so far attained the same importance as the applications of quantitative models (logic can hardly be considered an empirical science, in any case not a descriptive one). For examples of set-theoretical models applied to *linguistic* subjects, vide *Trends*, pp. 38-40. Mention shall also be made of B. Sigurd's and L. Gårding's papers in *Studia Linguistica*, 1955. Certainly there can be no objection to speaking of such approaches as "mathematical linguistics", on condition that they are not confounded with statistical (quantitative) linguistics.

The qualitative models dealt with under the heading Mathematical Linguistics — in *Trends* and elsewhere — are, however, not only models developed within some particular mathematical discipline, such as set theory. In fact, every qualitative model on axiomatic principles and applied to linguistic subjects may be found under this heading, regardless of whether the model has been developed within mathematics, within logic, or within linguistics. In other words, most structural approaches — e.g. Bloomfield's "postulational method", Hjelmslev and Uldall's "glossematics", Chomsky's "transformational grammar" — are now rebaptized as "mathematical linguistics".

Plath (in *Trends*) wisely sticks to the heading Structural Models when dealing with models for qualitative linguistics, but it is hard to see in that case, on what objective basis certain — but not all — structural approaches will be regarded as belonging to "mathematical linguistics". Moreover, when "mathematical linguistics" is taken in a sense so as to include all or an essential part of what is usually called structural

linguistics, it is hard to see that the question of the relevance of “mathematical models” to linguistics will not include the general question of The Logical Basis of Linguistic Theory, cf. the theme of Friday’s Plenary Session.

Thus, when used to cover both of the senses 1) and 2) in sect. 3 above “Mathematical Linguistics” takes on the nature of a cuckoo in the nest, striving to grow up at the expense of others, viz. of separate and individual linguistic branches and trends.

6. Instead of a primary division into non-mathematical v. mathematical linguistics (and a secondary division of the latter according to statistical v. structural models, cf. *Trends*), the discussion in sections 4 and 5 suggests a twofold division (shown below) of linguistic research with regard to models.

	Non-structural	Structural
Quantitative (arithmetical and statistical)	(a)	(d)
Non-quantitative	(c)	(b)

Here “structural” means “(research or approach) striving towards an axiomatic (postulational) description of the qualitative part of linguistic phenomena”. If instead of “axiomatic” or “postulational” one prefers “algebraic”(which has gained some ground in linguistic literature) this term should be understood in the sense used in linguistic literature (and once more exemplifying debatable borrowings, cf. sect. 1), i.e. not only of models developed within mathematics or logic, but even of models developed within — and possibly of specific applicability to — linguistics.

The twofold division above is not intended to form a theory or methodology of linguistic research. It merely serves the purpose of forming an adequate frame of reference in the discussion of the part played by axiomatic (including mathematical) models in linguistic studies. In the present paper, however, only a few comments and illustrations can be given.

To (a): In some linguistic studies, here symbolized by (a), the quantitative conditions of certain linguistic phenomena, taken to be units, are investigated, whereas the qualitative basis for setting up linguistic units is considered to be irrelevant to the study in question. Most — but not all, cf. (d) below — statistical investigations of language fall within this heading.

To (b): Here a model of an axiomatic nature is used in describing certain qualitative linguistic phenomena (or at least part of the phenomena dealt with in the study in question). The axiomatic model may be borrowed from another branch of knowledge (from a mathematical discipline, from logic, possibly from psychology or sociology), or may be independently developed for the linguistic investigation. This type of approach covers the majority of what is usually called structural linguistics, but even

much linguistic work done before the introduction of this designation actually falls within this heading.

To (c): (c) represents purely qualitative studies in which no axiomatic model is used — or at least: in which no explicit use of such models is made. I leave aside the general problem of whether *any* description involves a model (a theory), and since I am not aiming at a classification of individual papers the question of where to draw the line between explicit and inexplicit use of (axiomatic) models may also be ignored.

To (d): Here a qualitative description making use of some axiomatic model is linked up with a quantitative description of certain conditions. This is exemplified by Harary and Paper's analysis of phoneme sequences in terms of set theory *and* in terms of ratios, cf. *Trends*, p. 38-9. A particular case of interest to the foundations of linguistic analysis is the possibility of applying quantitative criteria in setting up qualitative units, i.e. of letting *quantitative* considerations give rise not only to a supplementary description but at the same time enter as part of the model used in the *qualitative* description.

The pioneer work of this kind is O'Connor and Trim's establishment of phoneme categories in English on the basis of quantitative differences (*Word*, 9, 1953; not mentioned in *Trends*). Another example is found in the probabilistic (statistical) problems involved in analyses by means of distributional models, cf. my Report for the 8th Int. Congr. of Linguists, Oslo 1957.²

7. The use in linguistics of non-quantitative models borrowed from mathematical disciplines (or from logic or possibly some other branch of knowledge) gives rise to the fundamental question whether in fact such borrowed models are adequate to the qualitative analysis and description of linguistic subjects. In particular the question arises whether borrowed qualitative models permit the solving of specific linguistic problems in the same or even a higher degree than models developed within linguistics.

For several reasons an answer to this question cannot be given once and for all; among other things the result of such comparisons may vary according to empirical conditions of different linguistic phenomena. Thus from the example to be discussed below general conclusions cannot be drawn, but in any case it seems worth while to point to important linguistic problems of analysis and description that have not so far been adequately treated by means of borrowed (mathematical) models and which on the other hand have given rise to the development of models within linguistics.

In the above-mentioned papers of Harary and Paper and of Sigurd and Gårding certain conditions of phoneme combinability are analysed by means of set theory (in both cases one of the authors is a mathematician). The approaches are mutually independent, and differ in essential respects, but certain common features of the

² With special regard to analyses by means of the glossematic relations I have dealt with these problems in *Probability and Structural Classification* (Copenhagen, 1959).

approaches disclose various inadequacies of usual set theory to problems of linguistic combinations:

A. In the set-theoretical models *order* is a basic notion: The pair of elements *ab* is regarded as always different from the pair *ba*. To linguistic analysis, however, order of appearance is relevant only on condition that a change of order entails a functional difference. E.g., word-order forms part of the grammatical structure only in so far as changes of word-order entail changes as to the syntactical function of the words in question (in the remaining cases word-order belongs to the description of usage, and possibly of style). In the domain of phoneme combinations order is irrelevant i.a. to the distinction between vowel and consonant, and the relation (the selection) between these categories can be set up — and be applied to practical analysis — regardless of the position of vowel and consonant(s) within the syllable.

Thus order is no general notion or relation of linguistic phenomena, and a model in which order is an indispensable basic notion cannot be adequate to linguistic analysis in general. In the set-theoretical models the case of irrelevance of order can be expressed by means of the term *symmetry* (of pairs, e.g. *ab* and *ba*), but this forms a roundabout and misleading way of dealing with the situation of basic indifference (neutrality) with regard to order. Moreover, this roundabout way leads to unnecessary problems concerning accidental gaps; e.g., even with regard to a division of phonemes into vowels and consonants one has — on this basis — to discuss such questions as whether in written English the absence of a word *og* (symmetrical to *go*; cf. the symmetrical word expressions *on* and *no*) is due to chance or to systematic conditions, that exert their influence on *ordered* combinations only.

B. In the set-theoretical models a *relation* is a collection of (ordered) pairs, i.e. an inventory of *given* combinations. Thus the binary phoneme sequences (combinations) observed in a certain text form a particular relation, which may — or may not — possess certain properties, e.g., symmetry, transitivity (i.e. the property of including the combination *ac* when both *ab* and *bc* are included). Addition (or removal) of one or more combinations means that a new (different) relation has come into being, and the properties of this new relation may be different from those of the first relation.

This notion of relation is fundamentally synthetic. If one aims at an analytic description of empirical phenomena, e.g. at setting up phoneme categories and phonemes from empirical syllables (empirical words), this kind of model is less adequate than models which emphasize and formalize another aspect of the everyday idea of Relation, viz. the aspect of being a *way* or *rule* of connecting or combining. An elementary model of the latter kind may be set up in terms of excluded v. possible occurrence of combinations (or if one prefers, in terms of restrictions of combinability), cf. my Report for the 8th Int. Congr. of Linguists, Oslo, 1957. E.g., combinations between consonants are excluded as syllables, whereas combinations of vowel and consonants, or between vowels (diphthongs), are possible syllables. The resulting typology of relations is to some extent translatable into set-theoretical terms of “properties of relations” — and vice versa. Thus the possible combinations between

two solidary classes form a completely intransitive collection; whereas complete transitivity corresponds to “free combination”.

C. The set-theoretical models are neither theoretically nor in practice adapted to the problem of accidental gaps v. systematic non-occurrence, because in these models the observed combinations form the very relation. Admittedly, one may for some reason or other add certain “missing” combinations, but thereby a new relation (collection) is set up, the properties of which (e.g. transitivity) may be different from those of the original collection. (In fact the additions may be accomplished just in order to obtain more “agreeable” properties, i.e. a more simple description, cf. by Sigurd and Gårding.) Since, however, postulates on the *possible* occurrence of combinations as part of some future “new word” are empirically trivial, in that they cannot be falsified and are verifiable only by chance, descriptions on this basis are objectionable.

If instead one adopts a model of restricted combinability, i.e. if the *absence* of some combinations is regarded as the constitutive property of relations, cf. B above, the distinction between accidental gaps and systematic non-occurrence (excluded occurrence) can be put into practice in an objective way, and the problem of accidental v. systematic can be solved to that extent in which a reasonable interpretation of the notion “accidental” is at all attainable. In other words: This kind of qualitative model is adapted to a supplementary probabilistic (statistical) model (cf. sect. 6 [d]) that is adequate to the problems of chance conditions involved in empirical research.

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DISCUSSION

STUART:

I shall contain my remarks to one sentence of Spang-Hanssen’s report: “By the transferring of a term to a different branch certain aspects may be lost, in particular such aspects that kept this term in position among other terms in the original field.”

The position occupied by a term, in its relations with other terms, cannot be understood at the lexical level, but is a question of the syntax of the science in question. Confusion over a term like “entropy”, as between physicists and linguists, can only occur in the absence of syntactic formalization, for a theoretical term like “entropy” gains whatever meaning it has indirectly: through the behavior of an expression in the formal representation of theory. It is to the latter we must turn if we want to grasp the concept involved. The point here is that Spang-Hanssen’s argument concerning the distortion which accompanies name borrowing arises just in case we grant formal status to terms in the original field, but deny such status to terms (borrowed or otherwise) in the borrowing field. The *appearance* of a term, e.g., the fact that it consists of such and such signs (letters of the Roman alphabet, etc.) is entirely beside the point. It is also beside the point that a term (i.e., an expression with a given,

invariant appearance) may have different interpretations according as it occurs in one theory or another, though I take it that this is what Spang-Hanssen has in mind. What does matter is the *behavior* of the term within a particular theory. In this respect there is need for formalization of linguistic theories in order, as it were, to lay our cards on the table.

It is only at the level of primitive taxonomy that confusion (over lexical semantics) is final; e.g., if linguists use "phylum", "morphology", etc., in senses radically different from those common among biologists. But the semantics of theoretical terms is ideally vested in the logical-syntax of theory, just, I believe, as the semantics of natural language utterances is embedded in natural language syntax. Science, after all, is not vested in some monolithic corpus of unassailable fact (the only "facts" we have, in the sense of being part of the natural, given order of things, are baffling and irreducible), and to assume that scientific terms are in some way or other tied to particular "facts" (particular empirical interpretations) is to reduce both science and language to ineffable mysticism. In discussing the theoretical terms of any science it is important that we put science firmly where it belongs: in the mind of individual scientists. The need for agreed protocol in making science a public affair (which has nothing to do with so-called public observables) is emphasised by the tendency, reflected in Spang-Hanssen's report, to take issue over the lexical-semantics of scientific discourse.

By way of illustration, I should say that I am properly entitled to use, in any way whatsoever, the letter "E" in any calculus I may construct, and quite indifferently of the fact that the letter "E" occurs crucially in Maxwell's field equations. I am not concerned with the possible domains of interpretation for the term "E" as it occurs in a given calculus, which is a matter of indifference here. What is significant is that there can be different calculi in which "E" occurs as a term, and that in such a case the meaning of the term is indifferent to considerations as to domains of interpretation, but is a matter of the formal restraints upon the occurrence of "E" within any particular one of the calculi. To suppose that theoretical terms having the appearance of natural language words ("entropy" or what you will) are pre-empted by particular calculi or theories is, to my mind, incomprehensible.

The real issue, then, is whether occurrences of terms in linguistics may be formally defined in any interesting way; i.e., whether linguists use their terms under formal restraints defined by the syntax of linguistic theory. Attempts to answer such questions are necessary. That work along these lines is essentially logico-mathematical, that it is part but a distinctive part of linguistics, that there is a trend in this direction, are all, I think, matters of fact.

ŠAUMJAN:

H. Spang-Hanssen has raised an important question which at first sight seems purely terminological, but in fact is important for the strategy of linguistic research. It has to do with mathematical linguistics.

One may ask what mathematical linguistics is.

I fully share the speaker's opinion that there can be no contraposition between mathematical and non-mathematical linguistics.

Linguistics as a science is a single whole, that is why there can be no separate mathematical linguistics.

We must speak not about mathematical linguistics, but about applying mathematical methods to linguistics. Like other sciences, linguistics is interested in exact methods of research. This is why mathematical methods will spread more and more widely in linguistics. As matters stand nowadays, application of mathematical methods in some fields of linguistics has been rather successful, whereas its other branches are only beginning to apply them. As a matter of fact mathematical methods can and must be applied to every branch of linguistic science; therefore we have no grounds to speak about mathematical linguistics as a separate science. Instead of the term "mathematical linguistics", which is meaningless, the term "mathematical methods in linguistics" should be used. This fully corresponds to the actual situation in linguistic science.

While I object to the division of linguistics into "mathematical" and "non-mathematical", I consider it essential to differentiate between various branches of linguistics. Here I mean the difference between structural and non-structural linguistics.

The scheme of subdivisions of linguistics suggested by the speaker is undoubtedly interesting from this point of view. Here it is:

	Non-structural	Structural
Quantitative (arithmetical or statistic)	(a)	(d)
Non-quantitative	(c)	(b)

On the whole I agree with the scheme. However I cannot agree with the criterion by which the speaker opposes structural linguistics to non-structural linguistics. The criterion is whether the axiomatic method can or cannot be applied to building models. By this criterion structural linguistics makes use of this method whereas non-structural linguistics does not.

I believe that use or non-use of the axiomatic method cannot serve as a basis for phonology and phonetics. Between those two closely connected disciplines there is a fundamental difference, owing to which we refer phonology to structural linguistics and phonetics to non-structural linguistics.

Does this difference consist in the fact that phonology resorts to the axiomatic method while phonetics does not? Of course, as things stand nowadays, phonology has already started applying the axiomatic method whereas phonetics has not yet done so. But as a matter of principle, a physical study of sounds may be presented in the form of an axiomatic system. This is as much as to say that the axiomatic method can be applied to phonetics. Let me remind you of the time when phonology made

no use of mathematical methods. But even then phonology was opposed to phonetics as a different branch of science.

Thus the difference between the two branches cannot be reduced to the use or non-use of the axiomatic method but should be sought elsewhere.

I believe that the difference between structural and non-structural linguistics is determined by the difference between the taxonomic and explanatory levels in linguistic research. Similar to physics, chemistry, biology and other disciplines dealing with this or that sphere of reality, linguistic science is concerned above all with an exact description and classification of observable facts. To achieve this, one resorts to experiments and instrumental methods of research. That is the taxonomic level of linguistic science. But linguistics goes beyond a mere description and classification of observable facts; it sets itself the task of revealing the underlying immanent relations among elements inaccessible to direct observation. That is the explanatory level of linguistic science.

In the light of distinguishing between the taxonomic and explanatory levels in linguistics we may give the following interpretation of the above scheme.

(a) The sphere of a statistical description of immediately observable linguistic facts. I should like to emphasize that statistical methods can be used only to describe immediately observable linguistic facts.

(d) The sphere of applying statistical models in connection with non-statistical explanatory models. As I have already said, statistical methods as such do not further in any way the cognition of the essential elements of the language and the relations among them. For this purpose it is necessary to build up non-statistical structural models. However, once a structural model has been built, it may be reinforced by statistical methods. Since statistical models must be based on structural models, the latter may also be called basic models of the language.

(c) The sphere of non-structural classificational research.

(b) The sphere of building purely structural models without any recourse to statistical methods.

On the basis of what has been stated above structural linguistics may be defined as the basic linguistic discipline concerned with building up explanatory models of language.

In defining structural linguistics as the basic linguistic discipline I should like to stress at the same time the unity of modern linguistic science. All branches of modern linguistics are closely linked, and explanatory models of language can be successfully built only on condition that the interconnection and interaction of all branches of modern linguistics be taken into account.

SIGURD:

I agree with Spang-Hansson's view that the term mathematical linguistics is perhaps too freely applied, but I should like to correct some of his misconceptions.

What is used in the papers by Harary and Paper, Sigurd and Gårding is not set

theory – only some simple mathematical terminology concerning relations. And order is not an intrinsic part of set theory.

It is true that order is not relevant (at least not in Danish) to the distinction between vowels and consonants, but order is obviously relevant to consonants. The terminology concerning relations seems to be very useful in descriptions of groups of linguistic units, consonant groups, morpheme groups, word groups.

Spang-Hanssen's suggestion to form an a priori relation containing the observed one does not do away with the difficulty of accidental occurrences – they depend on the a priori relation.

HERDAN:

There is quite an appreciable discrepancy between the title of Dr. Spang-Hanssen's paper and its content. According to the title, the problem which we are to consider is only whether mathematical linguistics is a trend in fact or in name only. This is a clear-cut formulation of a single problem. In the paper itself, however, the author deals with a very great number of problems, partly related to that formulated in the title, partly unrelated.

Taking the paper at its face value, i.e. as dealing only with the question in the title, there can be little doubt about the answer: mathematical linguistics is, of course, a trend in fact. It is simply the whole body of data plus their analysis which refers to those aspects of language which are amenable to mathematical treatment. As I said before, there are linguistic aspects which are independent of meaning, wholly or partially. Since both the data and their analysis by mathematical methods are facts, there can only be one answer to the question put by Dr. Spang-Hanssen, namely, that mathematical linguistics is a trend in fact.

The use of the adjective "mathematical" in this connection is quite in keeping with *how* we speak of the application of mathematics in other fields of knowledge. We have today mathematical physics, mathematical chemistry, mathematical botany, mathematical genetics, physical and mathematical medicine, mathematical economics, etc. Why should the adjective "mathematical" be less suitable to denote that part of linguistics which has become a part of applied mathematics?

ASPECTS ET FONCTIONS LINGUISTIQUES DES VARIATIONS MÉLODIQUES DANS LA CHAÎNE PARLÉE

Contribution à l'étude du Statut Linguistique de l'intonation

GEORGES FAURE

Nous avons essayé de montrer ailleurs ¹ que, loin de constituer un simple élément de complément, l'intonation se situe au contraire, dans la plupart des cas, au cœur même du message parlé.

Ce rôle, généralement décisif, de la mélodie a fait l'objet depuis quelques années, de nombreuses publications sur lesquelles il ne saurait être question de nous étendre ici, mais dont la plupart nous proposent d'intéressantes analyses des structures fonctionnelles de l'intonation et du rythme.

De là l'idée (souvent évoquée, mais peut-être encore trop peu exploitée) que le langage devrait être étudié, plus systématiquement encore qu'on ne l'a fait jusqu'ici, comme phénomène *concret et total*, c'est-à-dire tel qu'il nous apparaît lorsqu'il est engagé et actualisé dans un acte de parole dont les divers éléments sont empruntés, en vue de leur intégration à une nouvelle structure, aux différents *systèmes* d'une langue donnée (système lexical, système syntaxique et système phonique; ce dernier se subdivisant en un système phonématique et en un système prosodique non moins précis, auxquels locuteur et auditeur se réfèrent ensemble, l'un pour élaborer, l'autre pour interpréter, l'acte de communication).

Notre contribution à l'étude de ce dernier système, compte tenu des réflexions que nous ont inspirées nos recherches sur l'intonation anglaise,² se fonde sur un examen aussi précis que possible des deux fonctions majeures de la mélodie de la voix parlée, à savoir :

- 1) une fonction *accentuelle*, essentiellement *quantitative*;
- 2) une fonction *distinctive*, essentiellement *qualitative*, et qui a pour effet de changer le contenu psychologique du message.

Qu'il s'agisse de l'une ou de l'autre de ces fonctions, nous croyons possible de faire apparaître que le continuum mélodique dont l'analyse acoustique a pu paraître relever, pour certains de ses aspects, d'une technique analogue à celle qui fait inter-

¹ "L'intonation et l'identification des mots dans la chaîne parlée (Exemples tirés du français)", *Actes du IVème Congrès International des Sciences Phonétiques, Helsinki, 4-9 septembre 1961* (The Hague, 1962).

² *Recherches sur les caractères et le rôle des éléments musicaux dans la prononciation anglaise* (Paris, 1962).

venir, en matière de courant électrique, les potentiomètres et les rhéostats (pour reprendre la saisissante formule de M. D. L. Bolinger dans une étude récente³ est, en fait, découpé par la conscience linguistique (comme le continuum phonématique qui lui est comparable sur le plan physique) en unités distinctes et opposables, conformément à un code de référence présent dans la mémoire du locuteur comme dans celle de l'auditeur.

Il nous est apparu, au cours des recherches expérimentales que nous poursuivons en collaboration avec M. H. Juričić, Ingénieur au Centre de Recherches Scientifiques, Industrielles et Maritimes de Marseille, grâce au bienveillant appui de son Directeur, M. le Professeur Vogel, que ces unités mélodiques distinctives sont identifiées par l'auditeur, pour un locuteur donné, en fonction de ce que l'on pourrait appeler ses trois registres fondamentaux, c'est-à-dire en fonction d'un registre grave, d'un registre médium et d'un registre aigu, relativement stables et qui seraient ceux en fonction desquels s'établit, pour ce sujet, le schéma mélodique-type de l'énoncé le plus purement objectif.

Ce type de schéma mélodique se rapprocherait assez de ce que M. R. P. Stockwell, dans un article assez récent⁴ considère comme le schéma incolore ("colourless pattern") de ce qu'il désigne sous le nom d'intonation normale. "Normal intonation is in effect", écrit-il notamment, "the consequence of failing to elect an optional transformation of shift. It occurs automatically if the intonation pattern rules are, so to speak, left alone (i. e. not modified by the optional choice of intonation shifting transformations)".

L'appareillage utilisé pour nos expériences est essentiellement constitué par un ensemble de deux magnétophones, d'un générateur de basses fréquences équipé d'un haut-parleur et d'un compteur électronique.

Le mode opératoire est, en peu de mots, le suivant: les phrases à analyser sont enregistrées sur bande primaire, à l'aide du premier magnétophone. Pendant le déroulement de cette bande primaire, le deuxième magnétophone enregistre, sur une boucle magnétique, l'élément à analyser qui se trouve répété toutes les 5 secondes. La fréquence du ton à étudier est repérée auditivement et le générateur B.F. est réglé, par comparaison auditive, pour donner un ton de hauteur identique, dont la fréquence est mesurée au compteur électronique.

Les fréquences moyennes données comme résultat, après plusieurs expériences, sont exactes à environ 1% près pour toutes les syllabes toniques.

Il semble, d'après cette première série d'expériences dont les résultats complets feront l'objet d'une publication annexe, que, pour les divers sujets étudiés, les trois niveaux de référence que nous avons définis soient remarquablement stables. Ils se situent, par exemple, pour le premier de ces sujets respectivement entre 152 et 158 C/S

³ D.L. Bolinger, *Generality, Gradience and the All-or-None* (The Hague, 1961).

⁴ R.P. Stockwell, "The Place of Intonation in a General Grammar of English", *Language*, 1960, pp. 360-367.

pour l'aigu, 110 et 111 C/S pour le médium, et 73 et 76 C/S pour le grave, comme le montrent, entre autres, les phrases suivantes :

“-Don't you -think it's -dangerous ↑ ?”
 152 110 73 117
 “-Don't -trouble to -answer it ↑ ”
 158 111 77 115
 “Ex -cuse me -one moment ↑ .”
 157 76 117

Une deuxième série d'expériences nous a permis d'étudier comment, à partir de ces registres fondamentaux qui marquent, en quelque sorte, les étapes imposées mécaniquement au schéma de l'énoncé strictement objectif (le mouvement de la courbe d'intonation étant largement conditionné au départ, comme le souligne M. A. Martinet,⁵ par “la nécessité de tendre les cordes vocales en début d'émission et par la tendance économique à les détendre dès que s'amorce la fin de l'émission”), un sujet peut choisir de substituer, sur un point précis de la chaîne, un ton déterminé à un autre ton, également déterminé, en vue soit de mettre en valeur tel ou tel mot de l'énoncé, soit de changer le contenu de cet énoncé, la commutation des deux tons ayant alors pour effet, à relief comparable, d'assumer une fonction distinctive.

Nous avons pu constater que la première de ces deux fonctions est généralement liée à une rupture mélodique plus ou moins nette, à partir d'un seuil en-deçà duquel aucune différence accentuelle n'est perçue par l'auditeur. L'écart d'acuité qui détache la syllabe intéressée, soit vers l'aigu, soit, bien que plus rarement, vers le grave (c'est-à-dire de part ou d'autre de la ligne de tension minima qui est celle de l'intonation objective traditionnelle, que nous appellerons *neutre* pour plus de commodité) est, dans une certaine mesure, fonction du degré d'insistance voulu par le locuteur.

On aurait tort, toutefois, de croire que cet écart peut varier à l'infini, chaque variation, si légère soit-elle, étant reconnue comme un trait pertinent par l'oreille de l'auditeur. Nous partageons, sur ce point, l'opinion exprimée par M. K. L. Pike lorsqu'il écrit : “Socially the general amount of stress (quel que soit l'élément qui en assure, par priorité, la perception) may have some significance, but once intensity reaches (at an indefinable boundary) the exclamatory stage, all degrees of it are within the same meaningful super-imposed phoneme.”⁶

Un bon exemple de ces ruptures mélodiques à valeur accentuelle nous paraît être fourni par la phrase :

“\Yes, but -what did ⚭John have to \say about it ?”

où la fréquence affectant le mot “*John*” est passée, au cours de l'une de nos expériences, avec une voix masculine, de 100 à 210 C/S, c'est-à-dire s'est trouvée remontée d'une octave lorsque le sujet a voulu mettre ce mot en relief.

⁵ A. Martinet, *Éléments de Linguistique générale* (Paris, 1960).

⁶ K. L. Pike, *The Intonation of American English* (Ann Arbor, 1945).

Ce qu'il importe surtout de noter, lorsqu'il s'agit de ces ruptures "verticales" affectant un ton statique, c'est que le niveau moyen auquel se situe la syllabe, après son détachement, semble dépendre largement du *registre de référence* en fonction duquel le locuteur émet son message.

C'est ainsi, par exemple, que pour qu'une tonique de tête (c'est-à-dire la première tonique pleine de l'énoncé) soit mise en valeur, et reconnue comme telle par l'auditeur, il faut que cette syllabe se situe nettement au-dessus de l'aigu habituel du locuteur en intonation neutre, l'écart que nous avons pu constater paraissant être, en moyenne, de 60 à 80 C/S pour une voix masculine. C'est ce qui s'est produit, en particulier, lors du passage de:

“-Don't you -think it's -dangerous ↑?” (intonation neutre), à:
 “↑Don't you -think it's -dangerous ↑?” (intonation insistante)

où la fréquence de la syllabe: “*Don't*” est passée de 152 à 236 C/S.

Si, par contre, un effet équivalent est recherché pour une tonique du corps mélodique (c'est-à-dire située entre la tonique de tête et la tonique finale), qui se trouverait dans le registre *médium* en intonation neutre, le résultat est parfaitement obtenu lorsque cette tonique intermédiaire se situe, à peu près, au niveau d'acuité de la tonique de tête, l'écart entre ce niveau et le registre médium étant encore d'environ 80 C/S.

C'est, par exemple, le cas pour le mot: “*any*”, lors du passage de:

“There -wasn't -any in the \box”.
 165 90
 à “There -wasn't ↑any in the \box”.
 165 182

Il importe de souligner ici que les hauteurs que nous cherchons à définir ont toujours une valeur *relative* et que les fréquences que nous venons de relever interviennent essentiellement comme *limites* d'une rupture mélodique qui seule est vraiment significative sur le plan accentuel; cette rupture étant d'autant plus sensible que les syllabes voisines font un contraste plus marqué avec la syllabe mise en relief.

C'est ainsi que dans la phrase:

“Did ↑you re ↓ally↑?”
 222 305 150. 115 270

où la 2^{ème} syllabe de: “*really*” avait été nettement creusée, le mot “*you*” avait été, au contraire, rejeté vers l'aigu, l'écart entre ces deux syllabes extrêmes étant de 190 C/S!

Des substitutions tonales *distinctives*, non plus quantitatives mais qualitatives (généralement liées, sur le plan physique, à la qualité du dessin mélodique, beaucoup plus qu'à l'étendue des ruptures ou des inflexions) interviennent aussi dans de nombreux cas. Nous nous bornerons sur ce point, faute de place, à quelques illustrations, tirées d'un ensemble d'observations plus important que nous envisageons de publier ultérieurement.

Un premier exemple de ces commutations distinctives nous paraît être fourni par les deux phrases :

“They -don't -think he \knows”. et

“They -don't -think he /knows”. où le mot :

“*knows*” s'est trouvé affecté, pour un sujet donné, dans le premier cas, d'un ton descendant de 137 à 66 C/S, et, dans le second, d'un ton ascendant de 76 C/S à 156 C/S, l'intervalle, qui se retrouve, à peu de chose près chez tous les autres sujets, étant, dans les deux cas, d'une octave.

Cette substitution de ton suffit, comme on le voit, dans les limites d'une marge de tolérance assez constante, à faire d'un phrase simplement affirmative, une phrase implicative, comportant un sous-entendu dont le contexte précise le contenu.

Lors du passage de :

“They -don't -think he /knows” à

76.156

“They -don't -think he \knows?”

110.229

le décalage d'une demi-octave vers l'aigu du ton infléchi affectant : “*knows*” (dont l'étendue reste la même pour l'oreille) a pour effet de transformer la phrase en une *question*, colorée de surprise et d'incrédulité.

Nous avons réuni de nombreux exemples de ces commutations qui ont pour résultat de changer le contenu du message, sans altérer sensiblement le relief respectif des différentes syllabes de l'énoncé.

Resterait à examiner le cas (que nous ne pouvons aborder ici faute de place) des commutations de tons qui assument à *la fois* une fonction accentuelle et une fonction distinctive.

On notera enfin que, dans les quelques exemples donnés, la fonction distinctive des substitutions de tons s'applique à la manifestation des réactions du sujet à une certaine situation. Mais il arrive aussi, comme nous essaierons de le montrer dans une autre étude, qu'une simple substitution de tons, parfaitement définissable, ait pour effet de traduire un changement décisif dans cette situation même.

Nul plus que nous n'est conscient des lacunes et des insuffisances de cette trop brève esquisse. Il est bien certain, en effet, que de très nombreux facteurs, dont nous ne pouvons rien dire ici, ont de puissants retentissements sur le jeu mélodique dont nous venons d'évoquer, très sommairement, quelques aspects. Ce sont, en particulier, la distance à laquelle se trouvent locuteur et auditeur, la nervosité du sujet étudié, dont les effets, inconscients, nous ont parfois surpris, la qualité du support sonore offert à l'inflexion mélodique, et qui, selon le cas, en favorise ou en gêne la perception, le facteur temps, qui, lui aussi, compte tenu de l'inertie de l'oreille, peut jouer un rôle décisif, le registre moyen et les qualités individuelles de la voix, etc.

Nous avons seulement voulu montrer pour quelles raisons il nous semble légitime

de considérer le système mélodique d'une langue donnée comme un ensemble solidement structuré de traits mélodiques fondamentaux, opposables les uns aux autres, présents dans un code de référence, également définissable (chaque langue disposant, comme nous essaierons de le montrer ailleurs, d'un certain nombre de schémas mélodiques caractéristiques), code en fonction duquel est élaboré et interprété le message qui l'actualise dans le discours, et sans le secours duquel le simple enchaînement des mots resterait impuissant à manifester ce qui constitue, bien souvent, l'essentiel du message communiqué par l'acte de parole.

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MINIMAL SYSTEMS, POTENTIAL DISTINCTIONS, AND PRIMITIVE STRUCTURES

BERTIL MALMBERG

Since Ferdinand de Saussure it is customary to distinguish between PARADIGMATIC and SYNTAGMATIC relations between linguistic (phonemic) units. Any linguistic element has both and is defined linguistically only by them.¹

In a series of studies on the phonemic system of French and of Italian² I have pointed out that certain phonemic distinctions in these languages ($|e| \sim |\varepsilon|$ and $|o| \sim |ɔ|$ in both, in French also $|a| \sim |ɑ|$, $|\sigma| \sim |æ|$, and $|\tilde{e}| \sim |\tilde{a}|$, in Italian $|s| \sim |z|$), which are definitely functional according to traditional criteria, have a very low degree of STABILITY. Some of them (particularly French $|e| \sim |\tilde{e}|$), but not all, also have a restricted functional load ("rendement fonctionnel").³ Others are distinctive in certain words and certain morphemic types, whereas in others either of the two members may appear almost indifferently. This is particularly the case in French words in *-et* (*billet*, *carnet*, *chevet*), where the use of $|e|$ or $|\varepsilon|$ is more a question of style than of linguistic function in the proper sense of the word. In pairs like *fée* ~ *fait*, *dé* ~ *dais* the opposition is stable. In e.g. the endings of the future or of the "passé simple" (*je ferai* $|e|$ or $|\varepsilon|$ etc.), or in the words (*je*) *sais*, (*je*) *vais*, *quai*, *gai* the choice seems to be free.⁴ It is a matter of fact too that the opposition between $|a|$ and $|ɑ|$ – on the distribution of which in the vocabulary few French people seem to agree though most speakers make use of both⁵ – is not parallel with, say, the one between $|i|$ and $|y|$ or $|u|$ and $|o|$ (variations like $|tas|$ – $|ta:s|$ for *tasse* are extremely common). The state of the intermediate vocalic distinctions in Italian is identical (*lettera* etc. with a regional variation between $|e|$ and $|\varepsilon|$ in spite of the etymology).⁶ I have explained this state of things as a mixture of language systems, in Italian due to mutual influence between the language of Florence and that of Rome on the one hand (where the vowel pho-

¹ This distinction corresponds to Saussure's distinction between "rapports syntagmatiques" and "rapports associatifs" (*Cours*, pp. 172-173).

² See *Acta linguistica*, II (1940-41), pp. 232-246, and III (1942-43), pp. 34-43 and 44-56, and my report to the "Colloque" on the structures of the Romance languages, held in Strasbourg, March 1961; published in *Orbis*, XI (1962), pp. 131-178.

³ See e.g. Gougenheim, *Éléments de phonologie française* (Paris, 1935), p. 34.

⁴ See the examples in my article quoted above, and cf. Fouché, *Traité de prononciation française* (Paris, 1956), pp. 49-50.

⁵ See e.g. Gougenheim, *op. cit.*, p. 18, and Martinet, *La prononciation du français contemporain* (Paris, 1945), pp. 71-82.

⁶ See Giulia Porro, *Travaux du Cercle linguistique de Prague*, VIII (1939), pp. 187-208.

nemes have different distribution within the vocabulary),⁷ and other dialects which ignore the distinction, on the other; in French by a mixture of a more "vulgar" system where the distinctions have been, or tend to be, lost, and a more "distinguished" system which tries to conserve the oppositions. The Italian /s/ ~ /z/-distinction is of the same kind and can be explained in the same way as the vocalic variation (as a regionally different treatment of the originally voiceless Latin -s- with a subsequent dialectal mixture and, as a consequence, a completely irregular distribution of the two sounds within the vocabulary of the standard language).

Similar instances may be given from other languages. The Swedish word accent is typical. For very many words there is a regional variation in the distribution of the two word-accent. This means that, for numerous words (and particularly compound words and names, some of which are extremely common), there is, often even in the idiolect of an individual, free variation between acc. 1 and acc. 2 (so-called word variants). In other word types, however, there is an absolute necessity of using one or the other. A mixture of different regional usages fully explains this situation, which is more or less common in all languages.

A rather different case is the existence in many languages of oppositions which are to be found only in a few words, most of which are of a special character (loan-words, etc.; Germ. initial *ch-* or voiced /ʒ/, in e.g. *Chemie*, and *Gage* respectively, could be mentioned as examples). If such words are left out of consideration, the phoneme system often becomes more harmonious and less complicated. If they are taken into account, the paradigmatic structure may seem extremely complex or abnormal. Comparisons between systems become difficult and delimitation of phonemic areas on the map less instructive. It has always been a problem in phonemic theory to what extent rare or foreign word material, archaisms etc. should be taken into account for the phonemic paradigm. On one hand it may seem unjustified to take into account definitely foreign words which obviously do not fit into a pattern which would otherwise be completely regular. The use of nasal vowels in French loan-words in German or in the Swedish of Finland are extreme instances of this kind. Such phonemes mostly are of a socially restricted use. On the other, words of this type (current loan-words, common technical terms) undoubtedly belong to the system of communicative possibilities which the linguist has to describe, their peculiar pronunciation too. The dilemma created by the existence of different phonemata of the types referred to here, makes it convenient to introduce into the theory of phonemic description the concepts of MAXIMAL and MINIMAL systems. The minimal system is the common denominator for all speakers and is consequently the necessary minimum for a speaker to be understood. The maximal system comprises all existing distinctive possibilities, even the weakest and the least utilized. These distinctions are also phonetically the most complex ones (according to Zipf's law⁸). And they are acoustically and articulatorily

⁷ See Bertoni, *Profilo linguistico d'Italia* (Modena, 1940), pp. 101-103.

⁸ See Zipf, *Relative Frequency as a Determinant of Phonetic Change* (= *Harvard Studies in Classical Philology*, XL) (1929).

the most subtle ones, perceptually those which are most easily confused. There may even be cases where they are manifested phonetically only when the meaning would otherwise be misunderstood. Vowel length in French (phonemically valid only for /ε/) may be looked upon as an instance of such a POTENTIAL distinction (say the one between *mettre* and *maître*, *Rennes* and *reine*).⁹ The opposition of gemination at morpheme boundaries in my own type of Swedish is another (I pronounce e.g. two *t*'s in compound words like *uttala* "pronounce", *bordduk* "table napkin" only in order to make the word-formation clear in cases of doubt, or otherwise in very emphatic speech). A structure is not described exhaustively only in terms of potential distinctions and proved oppositions. The quantitative aspect also belongs to the picture.

A comparison between the maximal and the minimal systems of a language reveals its STRUCTURAL TENDENCIES, i.e. the direction the evolution may be supposed to take as soon as the social pressure is weakened. In full accordance with JAKOBSON'S LAW¹⁰ the distinctions retained are the phonetically most solid ones. The minimal system is normally the one which is found under unfavorable phonetic conditions, i.e. in unstressed positions (unstressed syllables in languages which have considerable differences of syllabic stress), in final position (in the syllable or the word; e.g. the loss of the voiceless-voiced distinction in word-final position in German and Slavic, the loss of numerous consonant distinctions in Spanish in syllable-final position, etc.¹¹) or in positions where, for other reasons, the syntagmatic distinctions are difficult to maintain. There is hardly ever syncretism between extreme distinctions. The Swedish word-accent, which is a typically subtle distinction — identified only with extreme difficulty even by trained foreign listeners —, appears only in stressed words. The reduced vocalic system used in unstressed syllables in many languages, particularly in German and Swedish, but also in English, gives us another good example of this kind. Spanish has a tendency to reduce its vowel distinctions in unstressed syllables (in the direction of the minimal system *i — a — u*).¹² Italian drops its intermediate vocalic distinctions, generalizing the more primitive five vowel-system, in unstressed syllables.

The tendency to reduce the number of phonemic distinctions, which is so often unmistakable in phonemic evolution, is of course a PRIMITIVE tendency. So is the corresponding tendency on the syntagmatic level to simplify the structure of the units (syllables, words, etc.). A language with only open syllables is for instance more primitive than one which also admits closed syllables, one with consonant clusters less primitive than one which can only use a sequence of consonant + vowel, etc. The concept of "primitive" in this content has of course no relation to history, nor

⁹ See my article in *Acta linguistica*, III, pp. 44-56.

¹⁰ See R. Jakobson, *Kindersprache, Aphasie und allgemeine Lautgesetze* (Uppsala, 1942 [and in *Selected Writings I*, The Hague, 1962]), and *Fundamentals of Language* ('s-Gravenhage, 1956).

¹¹ See my article "La structure syllabique de l'espagnol", *Boletim de filologia*, IX (1949), pp. 99-120.

¹² See my *Études sur la phonétique de l'espagnol parlé en Argentine* (Lund, 1950), pp. 39-40 and 48.

to time order. A primitive system may be, and very often is, later than the more complex one from which it originated. The vowel system of Spanish is more primitive than that of Latin. But on the other hand, there may hardly be any doubt that human language, in the course of its long history from undifferentiated sound imitating and/or purely expressive acoustic complexes up to the fully-fledged literary languages of today, must have passed through stages of increasing phonemic complexity on both levels, though linguistic evolution, in the same way as social and cultural development, must have taken numerous steps backwards.

Another question of simplicity versus complexity concerns the possibility of combining different distinctive features within the same linguistic unit (a syllable, a word). Even if the number of phonemic units is large, the distinctive possibilities of a language may be strongly reduced for instance by a demand for vowel or consonant harmony. A language which, within a morpheme or a group, has only either $A - A$ or $\ddot{A} - \ddot{A}$ is poorer in this respect than one which also admits $A - \ddot{A}$ and $\ddot{A} - A$. The former system is e.g. that of Finnish.¹³ Compulsory assimilation has the same effect, in the sense that some feature of one phoneme becomes predictable from the knowledge of another, following or preceding. Any Spanish nasal, at the end of a syllable, is automatically determined, as to its place of articulation, by a following phoneme (*-mp*, *-mf*, *-nt*, *ntf*, *-ŋk*, etc., but never **-np*, **-mk*, etc.),¹⁴ whereas Swedish admits for instance *-mt* (*tomte*), *-nt* (*vante*), *-ŋt* (*tungt*). Modern Greek offers other interesting examples of automatic assimilation,¹⁵ considerably restricting the possibility of building up phonemic chains. A comparative description of, say, Spanish and Swedish syllabic structure must take into account the enormous difference in FREQUENCY of the different types in the two languages compared and the very different degree of stability of the closed syllable in these languages. The same is true concerning the phoneme pattern of Modern Greek.

It is well known that analogous features of primitivity are frequent in children's speech. Even a Swedish child who can make a distinction between dentals and velars or between nasals and liquids often prefers *gocka* (for *docka*) and *lalle* (for *nalle*), thus reducing the number of contrasts within the syntagm.¹⁶ We meet the phenomenon in evolutionary phonetics too (modern French *chercher* for Old French *cerchier*). And we know it is very common in aphasia.¹⁷ As a whole, assimilation is a common characteristic of defective and retarded speech.

I do not need to quote even the main principles of Jakobson's great theory of the hierarchic structure of language. It could be formulated in general terms by saying that the combination of distinctive features into oppositional units and into structures

¹³ See e.g. Sauvageot, *Esquisse de la langue finnoise* (Paris, 1949), pp. 26-29.

¹⁴ See my article "La structure syllabique", quoted in footnote 11.

¹⁵ See Pring, *A Grammar of Modern Greek* (London, 1950).

¹⁶ Further examples in my article "Om vår förmåga — och oförmåga — att behärska ett språk-system", *Nordisk Tidsskrift for Tale og Stemme*, 1961, pp. 41-61.

¹⁷ See A. Ombredane, *L'aphasie et l'élaboration de la pensée explicite* (Paris, 1951), p. 324 (French examples like *titan* for *quittant*, *papo* for *chapeau*, *papinapapeur* for *machine à vapeur*).

made up of such units is not free but subject to hierarchic rules, according to which certain oppositions suppose certain other oppositions but not inversely. The inversed parallelism between the child's language learning and the loss of linguistic capacity in aphasia follows from this, the differences of generality between structures of varying complexity likewise. This "law" is in fact equally valid on the paradigmatic and on the syntagmatic levels. An opposition $|i| \sim |y|$ supposes an opposition $|i| \sim |u|$, but a syllable $|PAP|$ also supposes a syllable $|PA|$, and a syllable $|PLA|$ the syllables $|PA|$ and $|LA|$, and so on. In a language which, like Swedish, has two degrees of phonemic flattening $|i| \sim |y| \sim |u|$ ($\sim |u|$), the distinction between $|i|$ and a labialised vowel comes first in the child's language learning (with $[y] \sim [u]$ as free or positional variants), and only considerably later is the phonemic opposition between $|y|$ and $|u|$ acquired. For a rather long period one of my boys used the two vowels as free variants, with a definite predilection for $[u]$ as the most extreme type, confusing e.g. words like *ny* "new" and *nu* "now"). In the velar $|u|$, where labialisation is redundant, the lip rounding is of the same extreme type as for $|u|$, according to the general principle of maximal differentiation when no intermediate phonemic distinctions are utilized.

Children's speech and aphasic speech are, however, not the only material from which we can get some knowledge about the simplified structures which may be supposed to have preceded the more complex ones and of which there are evident traces in living languages, as reduced and minimal structures and as tendencies towards more simplified types. There are in any language layers of word material which belong, partly or fully, to other levels or functions of language than the one which is commonly regarded as the basic one in modern linguistic communication. It is a simplified idea of linguistic communication that this implies just a transfer of information about something from a sender to a receiver. We owe to Karl Bühler the distinction between the three basic functions of language: 1) the SYMBOLIC function (the relation of the message to the "thing meant"), 2) the SYMPTOM function (its relation to the speaker), and 3) the SIGNAL function (its relation to the receiver).¹⁸ Ordinary phonemic description takes into account only the means of expression used for the first of these three functions. Commutation supposes difference or identity of "meaning" only with regard to the symbol function.¹⁹ The conception of the linguistic message as a mere symbol is not only a simplification, it may be misleading. It is far from certain that this simplified communication process expresses the primary, dominating or most general function of human language at all the different stages of its development, and, under such conditions, the scientific analysis of linguistic communication may in some respects be incorrectly biased. Whatever the first attempts at linguistic communication made by our ancestors may have been like, there hardly seems to be any doubt that the signal and symptom functions originally played a more important part than they do today. There is no doubt either that the child's first attempts in

¹⁸ See Bühler, *Sprachtheorie* (Jena, 1934).

¹⁹ Cf. my *Système et méthode* (Lund, 1945), chap. I.

the same direction are to be looked upon more as signals or as symptoms (of satisfaction or dissatisfaction) than as information about something outside the parts concerned.

The kind of word material I have in mind belongs more to these spheres of communication than to the strictly intellectual level on which our linguistic communication mostly belongs (or should belong!). Let us take sound imitation first. Words like *splash*, *whistle*, *whisper*, *hush*, *hiss*, *rattle*, *cuckoo* differ from normal words not only by being less arbitrary but also by their more primitive phonemic structure, both paradigmatically and syntagmatically. Nursery words belong structurally to the same group, forms like *papa*, *mama*, *dad*, *atta* (words for "father" and "mother" in different languages), or *nanna*, *sussa* (Swedish nursery words for "sleep"); pet forms equally: Eng. *Bob*, *Bet*, *Chris*, *Tony*; *Bobo* or *Bosse* (Swedish for *Bo*), *Kalle*, *Lasse*, *Nisse* (Swedish for *Karl*, *Lars*, *Nils*) etc., not to mention expressions for biological functions common to many (even unrelated) languages (a good instance is a widely spread verb beginning with *p-*, common to e.g. Swedish, Danish, French, Italian, Finnish, and having nursery variants with the initials *k-* and *t-*, thus representing a primitive phonemic stage without distinctions of localisation of stops). As a rule, denominations of this type never prove any genetical relationship. They are either too simple structurally (*mama*), or phonetically not enough arbitrary (*whistle*), to give any evidence for a common origin of the languages under discussion. The same structurally primitive patterns may reappear under unfavourable conditions in any system. We know for instance that they may result from linguistic interference, bilingualism being one of the factors which favour weakening of social norms and of linguistic standards. Geographically they are known to be peripheric (in certain dialects of Swedish, along the linguistic frontier between Swedish and Finnish the distinction between voiceless and voiced stops is given up, just as in the French spoken in Alsace where no distinction is made between *porto* and *bordeaux*; Tokharian had reduced the Indo-European stops to one single voiceless series; and so on; languages like *lingua franca* and *petit nègre* are extreme instances of such reduced structures).

More systematic studies on this kind of vocabulary would probably contribute to a better insight into the structure of the primitive expression systems which were our ancestors' first attempts at organized language. Such investigations would imply a search for fundamental invariance of language within the endless variation of languages.

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DISCUSSION

HAUGEN:

The terms "maximal" and "minimal" appear to cover two rather different contrasts, which it might be more useful to distinguish than to confound. The first is the one between central and marginal systems, as described in the speaker's fourth paragraph. The second is the one between major and minor (or full and reduced) systems within the central system, as described in his fifth paragraph. The first is part of the general indeterminacy of the borders of any language, the second is a feature of the hierarchical organization of language, with its systems and subsystems.

STRESS, JUNCTURE AND SYLLABIFICATION IN PHONEMIC DESCRIPTION

JØRGEN RISCHÉL

1. PROSODIC UNITS IN LANGUAGE DESIGN

In spite of numerous theoretical divergencies, phonemic descriptions are, on the whole, considerably uniform in the basic framework. This is especially true of the large-scale activity in the U.S.A. in the last decades, which has had considerable influence elsewhere. The bulk of phonemic work reflects a tradition which has largely favoured the study of *segmental* units: vowels and consonants. The highly refined techniques of contrasting sounds by means of paradigmatic substitution and of classifying sounds on the basis of distribution and phonetic similarity have been evolved primarily for the purpose of setting up segmental phoneme inventories and transcribing texts in terms of these. Toneme systems have proved a fruitful field for the application of similar techniques, but certain phenomena — stress, pause, and the like — do not fit well into the framework.

Bloomfield observed that stress in English is not a phoneme like /p/, etc., although the *placement* of stress is phonemically distinctive,¹ and more recently a number of specific properties of “*prosodic*” or “*suprasegmental*” units have been noted by various scholars (partly in accordance with earlier “prephonemic” concepts). Bloomfield also pointed out that stress differences in English function on several levels.² The natural conclusion to be drawn from this is that stress cannot be functionally accounted for in terms of substitution classes on one level. In spite of such difficulties there has been a rather dominating tendency to apply the phonemic principle of *commutation* or contrastive substitution and partly also the principle of *unilinearity* throughout the analysis.

The principle of commutation has been widely accepted as valid throughout the domain of phonology. The analyst is thereby forced into a desperate search for a definite number of stress or juncture units, a search which is impeded by the failure of stresses or junctures to commute properly. (A typical example is the current

¹ For an elaboration of the idea, see Einar Haugen, “Phoneme or Prosodeme?”, *Lg.*, 25 (1949), 278-82.

² In *An Introduction to the Study of Language* (1914), pp. 43 ff. he distinguishes syllable-stress, group-stress, and sentence-stress. Similar levels of stress have been set up by several scholars.

disagreement about the number of stress phonemes in English.) In spite of the obvious difficulties in applying the method it has not been generally realized that the principle in question may not be universally applicable at all. It is the merit of Firth to have emphasized this.³

As to the principle of unilinearity, it is often an obvious advantage to interpret ambiguous arrangements as sequential rather than simultaneous. However, it is structurally disputable to base the phonological description on a rigid "vertical" segmentation of the sound sequences, whereby the prosodic or suprasegmental units are reduced to distinctive features of segmental phonemes. One example is the suggestion of Roman Jakobson and his collaborators that stress accents may be considered features of vowel phonemes; another example is their interpretation of the distinction between syllable peaks and margins in terms of inherent features of vocalicity and consonantality.⁴

It is not sufficient to state that the prosodic and segmental (inherent) features behave differently with respect to phonotactic rules. The difference is not a difference of behaviour but a difference of kind. Prosodic and segmental features have no direct relationship to each other; they constitute different parts of the structural model of language. And even though the structural model may be looked upon in different ways, it is obvious that there is a fundamental difference with respect to information conveyed by different kinds of units in minimal and nonminimal contexts: distinctions like voiceless ~ voiced are normally of maximal importance in minimal utterances, whereas stress, juncture, and syllabicity phenomena are only distinctive in non-minimal utterances. The syllable-initial *j* has a high functional load in the utterance *Joe!* (vs. e.g. *Go!*), but a lower functional load in *My name is Joe*. Contrary to this we convey essential information by stating that *aim* has strong stress and is preceded by open juncture in the utterance *an aim*, whereas this statement is at best redundant if applied to the utterance *aim*.

It is hardly to be doubted that linguistic descriptions gain in lucidity and consistency by emphasizing the heterogeneous character of the expression categories and utilizing the advantages of a multidimensional model of the expression form of language, as it has been done by Hjelmslev, who defines e.g. the syllable by the interplay between prosodic and nonprosodic categories (prosodemes and constituents).⁵ However, the diversity of the criteria chosen by different scholars⁶ to distinguish prosodic and nonprosodic units is suggestive of a situation more complex than a mere dichotomy.

³ J. R. Firth, "Sounds and Prosodies", *Transact. Philol. Soc.* (London, 1948), 107-52.

⁴ See e.g. Roman Jakobson & Morris Halle, *Fundamentals of Language* ('s-Gravenhage, 1956).

⁵ Louis Hjelmslev, "The Syllable as a Structural Unit", *Proceed. of the Third Int. Congr. of Phonet. Sc.* (1939), 266-72. As to "exponents" vs. "constituents" see also "Essai d'une théorie des morphèmes" (1938), reprinted in his *Essais linguistiques* (TCLC, XII, 1959), p. 155.

⁶ Surveys in R. S. Wells, "The Pitch Phonemes of English", *Lg.*, 21 (1945), 27-39, see pp. 27-30, and in K. L. Pike, *Language in Relation to a Unified Theory of the Structure of Human Behavior* (Ann Arbor, 1955-), vol. III, p. 53.

2. DEFINING CATEGORIES

The definition of the various prosodic and nonprosodic categories, and even the motivation of the choice of categories, raises serious problems.

It is sometimes stated that the reason for separating prosodic or suprasegmental units from other units in the description is that they belong to separate morphemes, i.e., the distinction is *grammatically* motivated (a recent American exponent of this view is Archibald Hill⁷). This, no doubt, is true to a high extent, but it seems that some features which we should like to call prosodic do belong grammatically with the segmental phonemes; this is true of lexical stress in English and other languages and of the quantity oppositions within stress-syllables in Scandinavian languages (cf. Haugen's analysis of stress and length in Icelandic⁸).

Now, on the other hand it has been suggested that suprasegmentals exhibit specific *phonetic* parameters, namely such that are connected with laryngeal or sublaryngeal activity.⁹ However, it is not very satisfactory to define a formal category of invariants by means of a phonetic criterion of this kind. As to the distinction of mutually contrastive units within one category, it is of course true that every phonological opposition must somehow or other be reflected in the phonetic substance, and it is by virtue of this phonetic evidence that the opposition can be registered. But this criterion fails to apply to categories that are not in contrast. If we distinguish prosodic and nonprosodic or suprasegmental and segmental categories in the structural model, these categories differ only by their different placement in the model; their existence cannot, in the final analysis, be either motivated or rejected on the basis of phonetic discreteness or lack of phonetic discreteness. There is not *a priori* any reason to assume that it is fruitful to make a universal division of the phonetic parameters into two groups of which one is reserved for segmental systems and the other for suprasegmental systems. For example, it may well be that vowel harmony in some languages is most conveniently handled in terms of a suprasegmental system with distinctions which would generally be considered typically "segmental", like front ~ back or unrounded ~ rounded. Thus, gain in structural simplicity is the ultimate motivation of the separation of categories like segmentals and suprasegmentals, or of suprasegmental categories like stress, tone, and intonation in the phonemic description. Since these categories do not function as terms of oppositions, we may expect any degree of overlap among their phonetic parameters. We may be fully justified in setting up a category of syllable stress without knowing how to distinguish a parameter of stress from other parameters on, e.g., the acoustic level,¹⁰ and it does not in itself affect the structural status of a stress system very much whether it can be characterized phonetically as pitch accent or as dynamic accent, or perhaps

⁷ A. A. Hill, "Suprasegmentals, Prosodies, Prosodemes", *Lg.*, 37 (1961), 457-68, see p. 468.

⁸ Einar Haugen, "The Phonemics of Modern Icelandic", *Lg.*, 34 (1958), 55-88, see pp. 63 ff.

⁹ W. F. Twaddell, "Stetson's Model and the 'Suprasegmental Phonemes'", *Lg.*, 29 (1953), 415-53.

¹⁰ For a good survey of research on the phonetic nature of stress, see W.S.-Y. Wang, "Stress in English", *Language Learning*, 12 (1962), 69-77.

as accent of duration. There may be structural reasons for separating categories of intonations and tonemes in some languages, although we cannot easily distinguish two phonetic parameters.

3. FOUR CATEGORIES OF PHONEMIC UNITS

How many main categories of expression units are we to distinguish on purely structural grounds?

One fundamental distinction is between units or features that are primarily *paradigmatic* in function, and units or features that are primarily *syntagmatic* in function.¹¹ The former group comprises all units that commute regularly (in an “either-or” contrast) with other members of the same category — vowels, consonants, tonemes, intonation phonemes in some analyses of English intonation. The pitch accents postulated for English by Bolinger¹² must belong to this category, and so do some kinds of emphasis (constituting a paradigmatic series of gradual contrast), whereas non-emphatic stress typically belongs to the latter group. The units of the latter group contrast syntagmatically with preceding and following members of the same category (in a “both-and” contrast of prominence: Trubetzkoy’s “kulminative [gipfelbildende] Hervorhebung”¹³) and/or have a distinctive placement relatively to other units (cf. Haugen’s criterion of “timing”¹⁴; the latter type is perhaps exemplified by *length* in those Scandinavian languages which have either V:C or VC: in all closed stress syllables), but these units do not enter into regular commutation series.

In this paper I shall call units of the former kind *phonemes*, and those of the latter kind *prosodemes*.

The difference between prosodemes and phonemes thus defined is not always clear-cut. The lexical stress-contrast in Russian (*ˈmuka* vs. *muˈka*) is, in my opinion, prosodic, since both members of the contrast must be present (only their arrangement may be reversed), but it would be theoretically possible to set up two contrasting word-long components of stress: one falling (in *ˈmuka*), and one rising (in *muˈka*); in this case the category would be phonemic (but suprasegmental, see below). Tonemic systems of the type found e.g. in Chinese are clearly phonemic (with paradigmatically contrasting tones), but register differences nevertheless have the prosodic property of demanding a tonal context to be well perceived.

Another important distinction is the (American) distinction between segmental

¹¹ The distinction between paradigmatic and syntagmatic contrast was emphasized simultaneously by André Martinet, “Accent et tons”, *Miscell. Phon.*, II (1954), 13-24, and Luis J. Prieto, “Traits oppositionnels et traits contrastifs”, *Word*, 10 (1954), 43-59.

¹² D. L. Bolinger, “A Theory of Pitch Accent in English”, *Word*, 14 (1958), 109-49.

¹³ N. S. Trubetzkoy, “Grundzüge der Phonologie”, *TCLP*, 7 (1939), 180.

¹⁴ *Language*, 25, 278-282.

and suprasegmental units. However, to serve the purpose here, this distinction must be formulated in strictly formal terms:

A text is assumed to contain at least one unilinear sequence of phonemic units; it is usually advantageous to describe it in terms of more. We may call the chain of vowels and consonants the segmental or *basic* sequence (probably to be defined as the chain which contains the maximal number of successive units). All members of a basic sequence are *basic*; all members of other (phonemically simultaneous) sequences are *supra*-units. The hierarchic organization of the text into stress-groups, syllables, etc., is established through the interaction among these sequences, which are most fruitfully looked upon as a series of *levels*: the basic sequence constitutes the lowest level (since vowels and consonants participate directly in the establishment of small-size constructions only), while the uppermost level is probably constituted by a sequence of intonation terminals (participating in the establishment of whole utterances).

The two distinctions made here provide us with a fourfold grouping of phonemic units into *basic phonemes* (vowels and consonants), *basic prosodemes* (possibly /:/ in some Scandinavian languages), *supra-phonemes* (tonemes etc.), and *supra-prosodemes* (certain stress systems etc.). A rather different fourfold grouping has been suggested by Hill on morphological grounds (cf. fn. 7 above).

4. CULMINATIVE CONTRASTS

In the case of culminative contrasts it may be advantageous to set up a single opposition operating on several levels rather than distinguishing a number of categories. This is not only a simplification but even a necessity in cases where there is no fixed number of levels recurring from one utterance to the other. We may thus postulate a prosodic feature of *culmination* or peak formation, which is found on various levels in the form of stress and syllabicity. Culmination is an abstract feature of widely different manifestations; but its allo-members all share the property of being of a culminative kind.

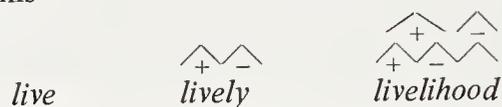
It has been suggested by several scholars that the stress contrast in Germanic Languages is dichotomous. Complex stress patterns are accounted for by Chomsky, Halle, and Lukoff¹⁵ in terms of junctures, whereas Weinreich,¹⁶ among others, distinguishes levels of stress. The latter viewpoint, on which the present paper is based, has been stated very explicitly by Eli Fischer-Jørgensen: "stress in the Germanic languages [is] an opposition between two members in different functional layers . . . and not . . . a category of 3 or 4 members."¹⁷

¹⁵ Noam Chomsky, Morris Halle, Fred Lukoff, "On Accent and Juncture in English", *For Roman Jakobson* (1956), 65-80.

¹⁶ Uriel Weinreich, "Stress and Word Structure in Yiddish", *The Field of Yiddish* (1954), 1-27.

¹⁷ Eli Fischer-Jørgensen, "Some Remarks on the Function of Stress with Special Reference to the Germanic Languages", *Congr. intern. sc. anthrop. Bruxelles 1948* (1961), 86-88, see p. 88.

The stress contrast obviously does not operate on a fixed number of levels. In e.g. German or English, the number of levels may vary within wide limits, depending on the complexity of the sequence. The sequence within which we can describe the hierarchic arrangement of stress, is Hockett's macrosegment: "the stretch of material spoken with a single intonation",¹⁸ i.e., a sequence delimited by a terminal contour. Every macrosegment has its separate number of levels. A monosyllabic macrosegment has no supra-prosodeme of stress (there is no possibility of syntagmatic stress contrast), but with more syllables we get one or several levels of stress contrast, namely maximally $n-1$ levels for a macrosegment with n syllables. Thus, if we render the two terms of the stress opposition as contours with plus and minus, we may present various patterns like this



The relative phonetic stress (in a strictly *impressionistic* sense) of each syllable or syllable group within the macrosegment can be read from such a notation, since (1) a syllable or syllable group "A" has weaker stress than another syllable or syllable group "B" if "A" has a minus-contour, and "B" a plus-contour on the same level (*-li-* has weaker stress than *live-* in *livelihood*), and (2) "A" has weaker stress than "B" if "A" exhibits a minus-contour on a lower level than "B" does (*-li-* has weaker stress than *-hood* in *livelihood*). The latter difference does not reflect a contrast (*-li-* and *-hood* do not contrast except through the higher-level contrast between *liveli-* and *-hood*), but it is a cue to the differentiation of levels of stress contrast.

The dichotomous model leaves no room for considerations of "absolute degree of stress". The morpheme *live* has three different stress patterns superposed in the three words above, although it is current phonemic practice to consider the accentuation of *live* identical in all three cases (on the assumption that utterances having only one vowel are said with a loudness equal to the greatest loudness found in larger utterances¹⁹).

One reason for choosing a representation of this kind is that it corresponds in many ways to the grammatical structure. In the English example above the distribution of plus- and minus-contours clearly reflects the word structure. In German this correlation between the grammatical and phonological hierarchies is very tight, but in English rhythmic factors may change the picture rather much. The hierarchic model of stress patterns is thus not to be considered a part of grammar, although there is a very close *affinity* between phonology and grammar on these levels.

5. JUNCTURE

The phonemic status of junctural phenomena is a much debated problem. In a

¹⁸ Ch. F. Hockett, *A Course in Modern Linguistics* (1958, 1960), p. 38.

¹⁹ G. L. Trager & H. L. Smith jr., *An Outline of English Structure* (= *SIL, Occasional Papers*, 3) (1951), 35.

recent acoustic-phonetic study Ilse Lehiste²⁰ rejects the interpretation of internal open juncture in English as a phoneme; she reduces it to a marker of higher-level boundaries. On this basis Fowler²¹ has reintroduced Bloomfield's old interpretation of open juncture as the placement of an onset of stress.

If stress is accounted for by contours, the difference between close and open juncture must be a difference in the way one contour ends and the next contour begins. We may assume that such contours may or may not *overlap* relatively to the basic sequence, and that syllables with separate contours are phonetically (at least potentially) more separate than syllables with overlapping contours²²; in the Germanic languages overlapping contours are typically found when a stress syllable is followed by an unstressed syllable belonging to the same stress group (i.e., not separated from it by a higher level contour):



The border between two contours is phonemically predictable in some cases but distinctive in other cases, as in



We may define *minus-juncture* as overlap between adjacent stress contours; a basic phoneme on which two contours overlap, is by definition ambisyllabic, cp. *m* in *coming*.

Nonoverlap between adjacent stress contours defines a *syllable border*, as in *an aim*. Phonetic open juncture is just one variety of non-overlap: a pattern resulting in open juncture before a vowel may give no audible break before a consonant, cf.



A syllable border is the potential place of an open juncture. The supra-prosodic configurations must be presented in such a way as to take care of the varying degrees of open juncture at various places in the sequence. If there are several syllable borders in a macrosegment, rules of this kind will apply to juncture in e.g. English: (1) syllables that have separate contours on higher levels are phonetically more separate than syllables which share contours on higher levels; (2) there is generally a wider spacing between adjacent syllables of which neither has a minus-contour than between syllables of which either or both have minus-contours; (3) some combinations of syllable-final and syllable-initial phonemes (to be specified) favour the occurrence of a break, as compared to other combinations.

²⁰ Ilse Lehiste, "An Acoustic-Phonetic Study of Internal Open Juncture", *Phonetica*: Suppl. ad Vol. 5 (1960) — with an extensive survey and bibliography on juncture in English.

²¹ Murray Fowler, "Stress-determined Allophones in English", *Word*, 16 (1960), 344-47.

²² Cf. K. L. Pike, *op. cit.*, vol. II, p. 51 for a similar presentation of minus-juncture.

In some cases we find contours which have a constructional function without exhibiting culminative contrasts. Such neutral contours (level stress contours) account for the syllable borders and the closeness or separation of syllable centers (disjuncture²³), and may probably in some cases account for apparently paradigmatic contrasts between “secondary” and “weak” stress. Stress-neutral contours may also be postulated to account for the delimitation of stress groups of equal prominence (in cases where there is a distinct segmentation before or after e.g. a weakly stressed syllable). In other cases such contours are redundant (cp. *-hood* of *livelihood* above). – In the original version of this paper, such contours were tentatively rendered as plus-contours.

6. SYLLABICITY

It may sometimes be advantageous to recognize a level of culminative contrast below the stress levels in order to account for the distinction between syllable center and margin(s). It is perfectly possible to abstract a feature of syllabicity and to treat it as a supra-feature rather than a segmental feature, because the feature belongs to units of larger extension than the single basic phonemes. While the contours on the lowest level of stress contrast establish syllables, the contours on this syllabicity level establish parts of syllables.

In English and probably many other languages the main division within the syllable is between initial margin and remainder.²⁴ We may, therefore, set up a culminative contrast between a minus-contour, which establishes the initial margin, and a peak-forming plus-contour, which establishes the remainder:



The plus-contour indicates that the peak is situated in the part of the syllable which it covers, but it does not indicate which phoneme is syllabic. The exact specification of this appears to be structurally irrelevant in English (one may set up a purely phonetic rule saying that the peak is formed by the first phoneme of the plus-contour part of the syllable). It is essential for the simplicity of the structural statements that the prosodic contours are set up in such a way that they do not convey redundant information.

The syllabicity feature is a typical example of a syntagmatic feature; it does not work well to set up segmental features “of vocalic” and “consonantal” within a

²³ D. L. Bolinger & L. J. Gerstman, “Disjuncture as a Cue to Constructs”, *Word*, 13 (1957), 246-55.

²⁴ A division of the syllable into initial margin and remainder was suggested by Jerzy Kuryłowicz, “Contribution à la théorie de la syllabe”, *Bulletin de la Société Polonaise de Linguistique*, 8 (1948), 80-114, and even earlier by Ferdinand de Saussure in his *Course in General Linguistics* (Eng. trans. 1960, pp. 49 ff. and 57-58). Hockett chooses this solution for English in *A Manual of Phonology* (1955), 150.

hierarchic model, because vowels and consonants do not enter into commutation with each other under typical conditions.²⁵

In the case of English, the prosodic interpretation of syllabicity enables the analyst to identify /u/-/w/, /i/-/j/, and perhaps /ə/-/h/, so that e.g. a transcription /wuwɫ/ *wool* (following the Trager-Smith notation) might be rewritten as

$$\begin{array}{c} \wedge \quad \wedge \\ - \quad + \\ u \quad u \quad u \quad l \end{array}$$

7. FINAL REMARKS

It is not the aim of the present paper to suggest any change of the principles followed in phonemic transcriptions. For notational purposes linear interpretations of sound sequences are highly to be preferred to any attempt at a hierarchic display, but at the same time such transcriptions may not be structurally adequate. The failure of linear transcriptions to place the various features on their proper levels introduces a considerable amount of redundancy in the transcription — a redundancy which does not reflect the structure of the language but the shortcomings of the transcription system.

There is no reason to attempt to identify the structural model and the transcription system, but the two kinds of representation should supplement each other. The present paper offers an attempt at an interpretation of the stress and juncture marks and the distinction between vowels and consonants in phonemic transcriptions.

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DISCUSSION

HOUSEHOLDER:

The theoretically infinite number of layers postulated in this system cannot, of course, be realized in speech. Such well-known test phrases as “Air-raid warden-post stairway” or “Shoe-string-tip machine operator” would require four or five “contour” levels in Rischel’s sense, but this structure cannot be brought out by the speaker. He can only (a) speed up the rate of utterance, and (b) impose a steadily descending intonation on the phrase.

²⁵ Cf. C. Hj. Borgström’s criticism of *Preliminaries* on this point; *Norsk tidsskrift for sprogvidenskap*, 17 (1954), 551.

PATTERN FRINGE AND THE EVALUATION OF PHONOLOGICAL ANALYSES

LAURENCE C. THOMPSON

In accomplishing phonemic descriptions linguists have been concerned with the conflict between the desire to represent the reality of a language and the drive to find pattern in the material.¹ Beyond the accepted primary principles of contrastive vs. noncontrastive distribution and phonetic similarity, the notions of economy and pattern congruity have often been invoked, but until recently they have been rather vague, generalized criteria, and in effect the analyses so supported have often been simply rival solutions which seem esthetically more pleasing to some, less so to others.

Economy seems for the most part to have been accepted as the notion of stating a system with as few phonemes as possible, and (as Hockett comments) "in actual practice it is of remarkably little help".² However, a more significant definition has now been proposed, with a telling case in point, by Keith Percival: "Economy is not a matter of the number of phonemes set up, but a function of the number of rules required to state the relations between the phonemic and allophonic levels."³ This statement formulates as a specific principle what has of course been the practice of many analysts.

Pattern congruity has come in for more discussion. Hockett's characterization is perhaps most efficient in abstract terms: "... of several alternative analyses which equally well meet other requirements, one should choose that which yields the greatest symmetry, both of phonemes and of allophonic variation with phonemes."⁴ Analyses achieving pattern by this principle have been attacked as violating phonetic realism.⁵ They have also been criticized for circularity.⁶

In connection with a description of southern Vietnamese phonology in 1959 I

¹ I am grateful to Bernard Bloch for reading the draft of this paper and making helpful suggestions. I also owe to him considerable general stimulation about the importance of phonetic realism, and specific influence in my definition of Vietnamese /h/ and the significance of the point of stress onset in English.

² *A Manual of Phonology* (Baltimore, 1955), 159; see also discussion following on next page.

³ "A Problem in Competing Phonemic Solutions", *Language*, 36 (1960), 383-386 (quotation from final page).

⁴ *A Manual of Phonology*, 158.

⁵ E.g. Hans Kurath, "The Binary Interpretation of English Vowels: a Critique", *Lg.*, 33. (1957), 111-22. Cf. esp. 114: "... it is the aim of structural analysis to discover the system of the language, not to impose a system on it, however neat and symmetrical", and 120: "We cannot sacrifice reliable observations and proven principles of linguistic behavior to the ideals of 'simplicity' and 'symmetry'".

proposed a procedure for dealing with the asymmetrical fringe of phonemic analyses, making use of it to evaluate them comparatively. Specifically, the principle suggested that the fewer asymmetrical features an analysis involves which do not directly represent phonetic reality the better it is.⁷

Since that time I have used this technique in phonemicizing other Vietnamese dialects and have considered several of the problems of American English phonemics in this light. It obviously amounts to a check or safety-valve which may be built into the principle of pattern congruity. It now seems pertinent to restate this principle with the following thesis: that the most meaningful description of a phonological system includes a maximum of symmetrical patterning – balance in position and manner relationships (e.g. /p t k, b d g/) and distributional factors (e.g. onset clusters /pr tr kr br dr gr/) – with the minimum of indirect representation of phonetic reality in the inevitable asymmetrical fringe (e.g. /p t k, f s/, no */x/; onsets /pl kl bl gl/, no */tl dl/).

Some specification of what is meant by “indirect representation of phonetic reality” is in order. At least the following kinds of representation appear less than direct:

- (a) Interpretation of simultaneous elements as sequences (e.g. a palatalized consonant represented as /Cy/);
- (b) Interpretation of sequential elements as constituting individual units (e.g. a diphthong represented as a single vowel);
- (c) Interpretation of a single segment as consisting of a sequence of two different phonemes, neither of which have the allophone posited here in any other position – the conditioning factor for each is the other (e.g. [ü] represented as /ui/, where /u/ never includes [ü] except before /i/, and /i/ never includes [ü] except after /u/);
- (d) Interpretation of a segment as zero – i.e. nonphonemic (e.g. glottal stop in analyses of many languages);
- (e) Positing of a phonemic entity which includes special aspects of surrounding phonemes (or even morphemes) without any constant feature of its own (e.g. a phoneme /h/ in standard French in positions where regular liaison and elision do not occur).

The method may be applied to rival analyses of the same material. It may also be used as a step in accomplishing an analysis. Once material has been organized according to the principles of contrastive vs. noncontrastive distribution and phonetic similarity its asymmetrical aspects may be examined to see how well they represent phonetic facts. At the same time close attention should be paid to features which might suggest a reanalysis to achieve greater pattern. Specifically the concerns center around the following kinds of anomalies:

- (1) Gaps or “holes” in the inventory or distributional systems;

however captivating they may be, without introducing a fatal split between synchronic and diachronic linguistics and endangering linguistics as a science.”

⁶ E.g. Einar Haugen and W. Freeman Twaddell, “Facts and Phonemics”, *Lg.*, 18 (1942), 228-37. But see also James Sledd, review of Trager and Smith, *An Outline of English Structure*, *Lg.*, 31 (1955), 312-35, esp. 317.

⁷ “Saigon Phonemics”, *Lg.*, 35, 454-76. Cf. esp. 473-6.

(2) Unique features or "leftovers" outside the main patterns (do they lack features which might place them somewhere within the system?);

(3) Apparent parallelisms of anomalous items;

(4) Apparent contrasts of anomalous items.

Some examples from the analysis of Hanoi Vietnamese follow. With the common sort of division of consonantal phonemes into various kinds of stops, spirants, etc., an inventory like that shown in Table I would be likely to result. Occurring syllable onsets and codas are shown in Tables II and III.

TABLE I

Hanoi consonants/ first analysis

	Labial	Apical	Frontal	Dorsal	Other
Stops, voiceless aspirated		t'			
Stops, voiceless unaspirated		t	c	k	
	b				
Stops, voiced		d			
				g	
Spirants, voiced	v		z		
Spirants, voiceless	f		s	x	h
Nasals	m	n	ñ	ŋ	
Lateral		l			
Semivowels	w		j	ĩ	ə

TABLE II

Hanoi onsets/ first analysis

t'(w)				
t(w)	c(w)	k(w)		
b	d(w)			
		g(w)		
v	z(w)			
f	s(w)	x(w)	h(w)	
m	n(w)	ñ(w)	ŋ(w)	
	l(w)			
w				

TABLE III

Hanoi codas/ first analysis

	b	t	c	(w)k
				(ə)k
				(w)ŋ
	m	n	ñ	(ə)ŋ
	w	j	ĩ	ə

A few comments are needed. /b/ includes a voiced stop as onset and a voiceless stop as coda; phonetic similarity and distribution place them in the same phoneme.

/g/ includes a voiced stop which is limited to onsets following a coda ending in /ŋ/, complemented by the voiced spirant in all other onsets. Glottal stop has been treated as determined – before all onsets in /b d w/ and before vowels; following vowels or cutting off abruptly codas with /m n ñ ŋ w j ɿ ɶ/ in syllables accompanied by certain tones. The semivowel /j/ consists of high back unrounded offglides. The semivowel /ɶ/ consists of lower mid back-central offglides. It is placed with /h/ in the column headed “other” since neither phoneme has a position similar to any other set of consonants.

The gaps or “holes” in the inventory (Table I) for the most part represent phonetic reality directly: there are no frontal voiced stop, no apical spirants, no semivowels with apical articulation. However, there is the glottal stop in phonetic terms, which might be placed in the “other” column in the voiceless unaspirated stop row. If we add this as a phoneme, several interesting realignments result: [d] is now in complementary distribution with [t] (which does not occur preglottalized), and similarly [w] with [v].

Consideration of the “leftovers” of the original system suggests some further re-analysis along the same lines: /t/ is noticeably more lenis than all the other stops (except the stop allophone of /g/); in this respect it is similar to the voiceless spirants and is a candidate for the vacant apical position in that row, which might be redefined as lenis voiceless oral consonants. At the same time, /l/ is lenis and voiced, thus resembling the voiced spirants, and might fill that vacant spot in a row redefined as lenis voiced oral consonants. If /w/ is combined with /v/, as suggested above, then the semivowel row has been partially combined with this new lenis voiced oral consonant row, and the remaining phonemes need to be examined. Clearly /j ɿ ɶ/ are in complementary distribution with /z g h/. There is no difficulty in combining [j] and [z] in a single phoneme /j/, or with adding [ɿ] to the /g/ phoneme. And it is possible to define a phoneme /h/ containing less prominent anticipations and prolongations of syllabics – voiceless vowels preceding, centering offglides following. This phoneme fills both voiced and voiceless positions of the lenis oral consonant categories.

These changes leave a quite symmetrical pattern, as shown in Tables IV, V, and VI. Apparent parallelisms of certain sequences in the distributional table of the new

TABLE IV
Hanoi consonants/ reanalysis

	Labial	Apical	Frontal	Dorsal	Other
Stops, fortis	p	t	c	k	ʔ
Oral consonants, lenis voiceless	f	t'	s	x	h
Oral consonants, lenis voiced	w	l	j	g	
Nasals	m	n	ñ	ŋ	

TABLE V
Hanoi onsets/ reanalysis

TABLE VI
Hanoi codas/ reanalysis

ʔp	(ʔ)t(w)	c(w)	k(w)	ʔ(w)	p	t	c	(w)k	
f	t'(w)	s(w)	x(w)					(h)k	
				h(w)	w(ʔ)		j(ʔ)	g(ʔ)	h(ʔ)
(ʔ)w	l(w)	j(w)	g(w)						
m	n(w)	ñ(w)	ŋ(w)					(w)ŋ(ʔ)	
					m(ʔ)	n(ʔ)	ñ(ʔ)	(h)ŋ(ʔ)	

analysis (Tables V and VI) are now to be inspected. One obvious anomaly is that onset clusters with glottal stop occur only with /p t w/; this checks precisely with the phonetic data, while the earlier analysis left this fact quite obscured (cf. Table II). A more extensive patterning with /ʔ/ in codas is revealed in Table VI which is missing from Table III.

Apparent contrasts in Tables V and VI also correspond directly to phonetic reality, while they are absent from Tables II and III (the contrast between onsets and codas involving /ʔ/ and those without it). The apparent contrasts between the coda clusters with /k ŋ/ and lack of other such codas in both analyses represent phonetic facts directly. (The parallelism suggested between /k/ and /ŋ/, and the contrast with /g/ are also direct representations.)

With these examples to demonstrate the method we may turn to a consideration of some implications for some of the frequently discussed points in American English phonemics. This method obviously does not address itself to a phonemic solution which intends to cover more than one dialect⁸ since phonetic reality is a crucial criterion and the multi-dialect analysis inevitably involves the representation of rather different phonetic phenomena by the same phonemicizations. It would appear preferable, from both the descriptive and historical points of view, to consider such a covering analysis a kind of reconstruction, which (if based on careful analyses of individual dialects) may have some important diachronic implications.

In the controversy over the interpretation of English syllabics some issues would be automatically solved by the consideration of apparent parallels and contrasts. For dialects which clearly have offglides in words like *beet*, *bait*, *boot*, *boat*, as well as in *bite*, *boy*, *buoy*, *bout*, a solution which posits single vowel phonemes in the first group and vowel clusters in the second suggests a contrast which is not phonetically accurate. On the other hand, for a dialect with "pure" vowels in the first group of words a diphthong interpretation would suggest a parallel to the diphthongs of the second group which is spurious. If these items went together to form a balanced symmetrical

⁸ E.g. Trager and Smith, *An Outline of English Structure* (Norman, Okla., 1951), 9. For a discussion of this problem see Sledd (1955) and Kurath (1957) (cited above, footnotes 4, 5) and references therein.

pattern they would not come into question in the application of this technique,⁹ but the fact is that for any one American English dialect they create at best a pattern with many gaps. For example, although there are dialects in which a rather symmetrical set for the Trager-Smith /y/ and /h/ diphthongs would result, the set of /w/ diphthongs is quite defective, and this invites a careful inspection of the whole system. In each case this consideration either justifies the system as containing maximum pattern with minimum distortion in anomalous items or suggests a reorganization with more core pattern or less fringe distortion or both. This makes clear that while the technique does not restrain directly the drive toward symmetry – the kind which is most open to the criticism of circularity – it demands that any resulting anomalies be thoroughly examined in the hard light of phonetic realism.

In the very familiar controversy about the interpretation of the consonantal margin elements in *church*, *judge*, various questions raised by this technique are instructive. Interpreted as unit phonemes, /c j/ are unique in distribution: they do not participate in onset clusters, and in coda clusters they are most similar to /š ž/. They appear in this analysis to be structurally different from onset clusters like /tr dr/, and coda clusters /ts dz/. Consideration of the phonetic reality involved begs the question whether they are not rather clusters /tš dž/. Reanalysis along these lines leaves us with the anomalous onset clusters /tš dž/, which, however, correspond more directly to phonetic facts. (This reanalysis is of course possible only if there are no contrasting sequences /tš dž/, apparent contrasts of this sort having been resolved by the position of onset of stress.) Some economy is gained (in an extension of Percival's terms, as well as in the fact that there are two fewer phonemes) in that the rules covering what follows /š ž/ in codas cover the new coda clusters /tš dž/ as well. For my own speech, the reanalysis also suggests a reconsideration of the basic symmetric sets of English

TABLE VII

English consonants/ reanalysis with leftover /h/

	Labial	Labial	Dental	Apical	Pre- Alveolar	Post- Alveolar	Velar
	Ø	2	2	Ø	2	2	Ø
Obstruents, fortis	p	f	θ	t	s	š	k
Obstruents, lenis	b	v	ð	d	z	ž	g
Sonorants	m	w	l ¹⁰	n	y ¹⁰	r	ŋ

⁹ There are also, of course, other theoretical problems.

¹⁰ In my speech /θ ð l/ share apicodental articulation; in /l/ the point of contact is somewhat farther back and the aperture is of course lateral. /s z y/ all have the narrowest passage along the front slope of the alveolar ridge, although the articulator varies: /y/ has the blade closest after onsets /f v/, elsewhere the front; /s z/ have the apex closest directly before or after /t d n θ ð l/, elsewhere the blade or even front (with the apex against the backs of the lower teeth).

consonants. The spirants clearly belong to a different kind of position series from the stops, and distributionally they form two different sets within their own ranks. We are left with a main 3×3 system of stops and nasals /p t k b d g m n ŋ/, secondary systems of close spirants /f θ v ð/ and sibilants /s š z ž/, and remainders /l r w y h/ with certain properties in common. Further consideration suggests a symmetric set with seven position distinctions, three manners, and leftover /h/ (Table VII). Position columns also distinguish full oral closure (symbolized by Ø) from narrow aperture (symbolized by 2).

This systematization has been achieved without creating any asymmetrical fringe that does not represent quite directly phonetic reality.

It should be clear from the discussion that the method provides a systematic device for assisting in the collation of field data. Carefully applied, it furnishes a thorough index of the analysis to date and points up areas in which phonetic observation may well have failed to get at some crucial details. Hopefully this technique may be of value in connection with typologic comparisons, where there is an obvious need for the development of a systematic method of arriving at phonological analyses which will be more directly comparable. It would seem that the sobering check of phonetic realism exercised on the asymmetrical aspects of a phonemic solution may act as an effective restraint against the creation of excessive distortion.

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DISCUSSION

HULTZÉN:

It would be highly advantageous for certain purposes to have some such arrangement of English consonants as that in Mr. Thompson's Table VII, i.e. a grouping together of the fortis before the lenes, without necessarily separating stops from spirants, and both before the sonorants and semivowels. But there is some question as to the propriety of the column rubrics in this particular table. It is readily apparent that *apical* does not fit in with the other terms for lingual articulation here, the latter all specifying position distinctions with respect to point of articulation whereas the former specifies the articulating portion of the tongue. This is, however, only an infelicity of naming or a slip of the pen, for it is made clear in note 10 that the same label, there shown as *apicodental*, should appear at the head of both the third and the fourth columns, i.e. those marked respectively *Dental* and *Apical*.

The real question, then, is on the propriety of having two columns with the same heading, as also in the case of the first and second columns headed *Labial*, differentiated by symbols for degree of aperture, here Ø and 2. Indeed Mr. Thompson himself says in the paragraph introducing the table that "the spirants clearly belong to a different kind of position series from the stops". To disguise this difference as 2 and Ø is hardly enough to give the reanalysis a convincing appearance of validity. Restore *spirant* and *stop* as rubrics and put them where they pretty much have to go, and we're back where we started from.

PLENIPHONETIC TRANSCRIPTION IN PHONETIC ANALYSIS

H. M. TRUBY

Phonemic distinctions depend upon relative rather than absolute acoustic values. These relative values are relevant to message transfer from speaker to listener. However, valid descriptions of speech-sound continua are unattainable unless *absolute* features are taken into consideration. The values currently considered adequate for a physical description of the sounds of speech are, in the order of their importance, of a frequency spectral, durational, and intensity nature. The *frequency spectral* values vary from speaker to speaker as a function of the fundamental frequency of the quasi-periodic source, the individual morphological conformities specifying resonance cavity complex and aperiodic source characteristics, and the individual habituated patterns of articulatory performance – patterns which reflect idiosyncracies of neurophysiological response to acoustic stimuli. The *durational* values reflect rate-of-performance and, importantly, contextual positioning. The *intensity* values are importantly keyed to relative, overall, time-governed peaks and in a lesser way to componential features; (in fact, within obvious limits, individual formant intensities appear to play an insignificant role – an observation carried over from my critique at the previous congress (1)).¹

It is the purpose of this paper to indicate analysis considerations which *must* be entertained by the describer of speech sound continua if physical validity is the aim. This desired, and desirable, physical validity is *not* dependent upon auditory perception, though auditory perception may be demonstrated to be significantly keyed to the physical reality.

Phonetics may be generally identified as the science and/or study of speech sounds. If this definition is to be valid, the term “speech sounds” must embrace the entire, gross acoustic material – both that which is clearly significant to and that which is apparently redundant for the verbal transmission of information. It is fundamental to speech and hearing aspects of language that certain acoustic features and combinations of features serve as carriers of intelligibility. It is fundamental to communication-theory aspects of language that these features and combinations of features serve as transmitters of information – information which is defined in a special sense as it relates to intelligibility. It is equally fundamental to phoneme-theory aspects of lin-

¹ Numerals in parentheses refer to the works listed at the end of this paper.

guistics that there are to be found in the speech continuum certain dynamic combinations abstractable from the gross sound material which, through practice, are interpreted as distinctive organizations significant to the production and identification of meaningful utterance. Phonetics concerns itself with the gross sound material out of which these special combinations are manifest and thus, though not necessarily, with the distinctive organization – or phonemic – aspect itself.

A phonetic analysis is usually considered adequate if it provides identification of that sound material which *is* significant to the transmission of intelligibility. A strictly physical analysis, however, reveals phonetic details whose contribution is traditionally held to be redundant, on both the physiological and the acoustic planes.² Correlated (that is, synchronized) cineradiographic and sound spectrographic analyses (2,3) of the same activity mutually point up physical nuance which goes otherwise unnoticed in analysis by either single technique, and when supplemented with micro-chronomatic (4, 5, 3) tape-segmentation, a unique vehicle is provided for the identification of otherwise subliminal – and in a sense *sub-phonemic* – elements expressed along the time axis. The physical details which are revealed under such analysis cannot be expressed with ordinary phonetic transcription, however “narrow”. Due *to* their subliminal nature, these details can be recorded adequately only in terms of an elaborate transcription I call *pleniphonetic*. It is tempting, in speech sound analysis, to be content with accepting the evaluation of a speech sound continuum in terms of merely its referent phonemes. This study is an attempted demonstration of the inadequacies – for physical analysis – of traditional phonetic and/or phonemic transcription.

Three concepts must be reviewed as to their pertinence to this discussion. The concepts are coarticulation, commutation, and the phoneme.

A given speech sound in isolation is a physical combination of sound source and articulation. By *articulation* is intended: the combine of motions and positions of the so-called speech organs during the production of speech. A specific articulation or articulatory gesture is referable to an arbitrarily defined speech sound. *Coarticulation* intends at the least a partial coincidence of articulations which results in the simultaneous production of segments of two or more speech sounds (3). For example, the shape and size of the pharyngeal cavity are considerably different during the production of an [a]-vowel and an [u]-vowel (6), as are the traditionally appreciated respective shapes and positions of tongue, oral cavity, and lip-opening (see Fig. 1, a and b). Therefore it is no surprise to find that in two comparable monosyllables, for instance, [la] and [lu], the physical descriptions of the [l]-sounds and [l]-articulations are expressly influenced by the approaching “following vowel” (see Fig. 2, a and b), with, for example, lips wide, pharynx constricted for the [l-] of [la] and lips close-

² In order to exemplify something of the physical – especially, motor – complexity which motivated this study, a 2-minute sample of a 17-minute sound- and motion-picture X-ray film was here offered. The entire film, the first of a series of films in present production and available for distribution, was later projected in full before the Congress. Figures appearing herewith were extracted from the film proper and are representative of the numerous physical considerations which seem to contradict – or at best, to drastically supplement – traditional perceptual evaluations of the speech act.

rounded, pharynx extended for the [l-] of [lu]. The physical identification of such coarticulated material is becoming more and more of a precision operation. The detailed physical analysis of acoustic continua which can be accomplished from sound spectrograms can often be unequivocally supported with correlated (again, in other words, synchronized) cineradiography of the referent articulatory performance or state.

The linguistic concept *commutation* involves the "substituting of sounds" in order to identify or verify phonemes. For instance, the autonomies /bit/, /bɪt/, /bet/, /bɛt/, /bæt/, etc. serve to identify and verify the vowel phonemes /i/, /ɪ/, /e/, /ɛ/, /æ/, etc. This operation I distinguish as *linguistic commutation*. When speech is displayed visually on sound spectrograms, for instance, certain portions of the display are traditionally identified as representative of certain phonemes. If the speech in question is recorded on magnetic tape, physical segments of the tape will correspond to the aforementioned visual portions on the spectrograms. *Mechanical commutation* (3) refers to a physical interchanging of "these tangible sound-segments". It is the physical counterpart of – but because of its nature never equatable with – linguistic commutation, for when *mechanical* commutation is attempted, there is no associated automatic adjustment of the adjacent phonetic environment as there is in the case of *linguistic* commutation. Further consideration makes it clear that no form of microchronomatic segmentation along the time axis can hope to provide "phonemes" or "phones" or even clear-cut "dyads" (9), but it will serve to call attention to certain physical constituents of the speech continuum. *All* commutation and segmentation is artifactitious.

And now, the phoneme. A speaker does not "produce a phoneme" and a listener does not "hear a phoneme". Rather, a speaker performs physiologically (and usually intentionally) in a specific pattern-conditioned manner; this performance brings about a complex of acoustic disturbance (of which silence is sometimes a part); and the resulting complex, when transmitted to an auditor, satisfies specific psychic requirements pattern-conditioned *in* that auditor. The *phoneme*, then, is neither the articulatory complex nor the resultant sound but the psychic prototype. Thus, it is the unit information transmitted from sender brain to receiver brain that is phonemic – any *physical* constituents which appear to figure in the process of phoneme transfer go toward constituting a mere conveyor of this significant information (3). In short, "speech sound" is the *conveyor*, and it can be analyzed as such, but always with reference to the implicit phonemic pattern-conditioning.

The broadest phonetic transcription is essentially a phonemic one. The conventional narrow transcription is often indicative of distinctive feature information, most of which is irrelevant where a physical description of the *acoustic* manifestation is the issue. Now, even the *narrowest* of phonetic transcriptions is delimited by certain predilections regarding the symbols employed. For example, the symbol **p** connotes occlusion, bilabiality, voicelessness, and (in English, in most positions) "aspiration". This connotation represents a heterologous mixture impractical for unit correlation along the time axis. An obvious way to effect a detailed physical transcription of an articulatory complex or continuum is to employ a basic symbol system whose symbols

are not necessarily limited by linguistic or even acoustic implication, and then modify these basic symbols, where necessary, *with* symbols of linguistic and/or acoustic inference. Such a system is offered here.

Fig. 3 is a sound spectrogram of the words "pay", "play", "lay". These words would be considered *exact rimes* by at least every native speaker of English. Yet, "the vowels" of "play" and "lay" clearly may be opposed in visual form to that of "pay" in this analysis, and the obvious differences are contingent upon the phonetic environments of the "vowel(s)". Stated in a simple, if naive, way, the influence that the voiceless bilabial plosive has on the vowel of "pay" contrasts sharply with the influence of the lateral on the vowel of "play". The so-called "vowel transitions" (which might more aptly be termed "consonant transitions") are highly distinctive. Also, the two "turbulent sectors" referable grossly here to the voiceless bilabial plosives of "pay" and "play" respectively are significantly different in appearance. These differences in visual-acoustic manifestation have been conclusively demonstrated in my film (6), book (3), and articles (1, 2) on the subject to be attributable to the physiological prepositioning of the tongue to the lateral resonance articulation *before* the lips burst open for the audible phase of the initial plosive. Synchronized cineradiographic and sound spectrographic analyses of a large number of isolated utterances (60 Americans, 20 Swedes) containing initial /pl-/ reveal incontrovertibly that as a part of the inaudible preparation for the initial audible plosion, the tongue assumes its most extreme lateral articulatory position some 50 msec (on the average) before *any* sound is heard. Therefore, what is ultimately heard as a [p-]-sound is in reality a [p-]-sound in partial coincidence with an [-l-]-resonance, since the referent articulatory complex is physiologically a bilabial plosion (in this instance, voiceless) with pertinent vocal tract articulators already positioned appropriately for a lateral resonance whose position is in turn anticipatory re the particular following (in the phonemic sense) vowel. (It should be kept firmly in mind that *vowel* and *consonant* are linguistic concepts not unambiguously definable on the physical plane (3).)

Focusing our attention for the moment on the utterance [ple], we begin the physical designation implicit in the term *pleniphonetic transcription*. Preceding the plosion is the silence (or effective silence) which is referable to physiological occlusion and which is significant for the identification of any plosive in an other-than-initial position in an audible continuum. Its duration, depending upon rate-of-utterance, varies normally between 30 and 60 msec. This silence is, of course, "automatic" in isolated word-initial position, as here, its duration being immaterial to perception even though the actual physiological occlusion is a measurable (though varying from instance to instance) performance. This silence is usually not indicated in phonetic or phonemic transcription, being justifiably taken for granted. A *physical* description, however, must include it. Since the present considerations *are* physical, it cannot be dangerous to borrow the morphophonemic "zero" symbol, #, for indicating this "silence". (If there is simultaneous voicing, the symbolizing is #.) The physical and perceptual importance of this silence is obvious and is anything but news, Henry



a)

b)

Fig. 1. X-ray photographs of utterance segments filmed at 48 frames per second, with sound.

a) frame extracted during the [-a]-resonance of an utterance of [la];

b) frame extracted during the [-u]-resonance of an utterance of [lu].

The points of extraction are as similar in time-selection for the two instances as the correlation of sound spectrograms and cineradiograms permits.



a)

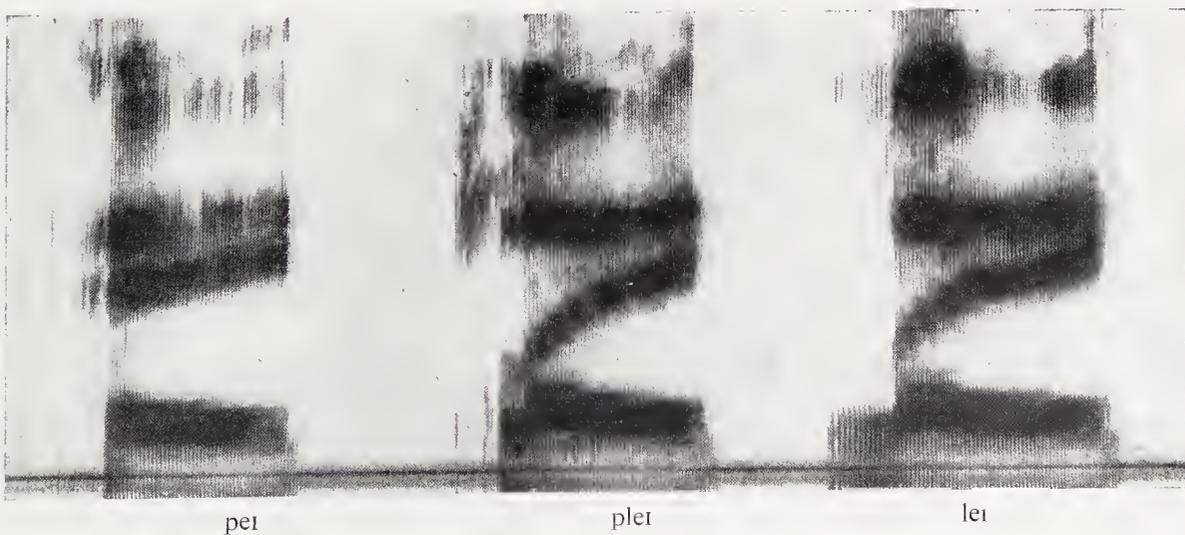
b)

Fig. 2. X-ray photographs of utterance segments filmed at 48 frames per second, with sound:

a) frame extracted during the [-ɪ]-resonance of [la];

b) frame extracted during the [-ʊ]-resonance of [lu].

The points of extraction are as similar in time-selection as the correlation of sound spectrograms and cineradiograms permits.



peɪ

plɛɪ

leɪ

Fig. 3. Sound spectrogram (Sonagram, HS) of an utterance of the words "pay", "play", "lay", GA (General American).

Sweet having written by 1877 (7) that the great peculiarity of voiceless stops is that in themselves they have no sound whatever and are only audible in the moment of transition from or to adjacent sounds, and Edward Wheeler Scripture by 1902 (8) that the occlusions are brief moments of silence that are noticed as such and are just as effective mental elements to the ear as the sounds. This pre-plosion silence, marked [#-] generally, may be specified in this instance [#_p-]. In this particular case, since it is demonstrable that the lateral-resonance articulation positioning is in advance of the actual plosion, a further specification is [#_p^L-] and since there is a calculable interval previous to the assumption of the lateral-resonance position, a sort of "performance diphthong" is transcribed with [#_p#_p^L-], in which the progressive positioning of the tongue is assumed but not marked. Because of the initial position, as here, marking an even previous "silence" with no articulatory implications (in one sense, pause) extends the silence transcription to [#_p#_p#_p^L-]. ((These symbol developments were demonstrated with consecutive SLIDES.)) So far we have not arrived at a point of audibility or even of positive acoustic manifestation. Let us proceed.

Next appears the explosion. From the physical point of view, an explosion has *no duration* – *no time dimension!* (3). It is the movement of the medium in which an explosion occurs that has a measurable duration. Thus the audibility of a plosion begins with the audibility of the resultant molecular friction set up in the medium into which the plosion explodes. This time-dimensionless explosion we shall symbolize [!]. This particular plosion is bilabial, lateralized, and voiceless [!_p^L], features which manifest themselves in various absolute ways. For the purposes of such a special transcription as this, all these features should be indicated. So far we have, then: [#_p#_p#_p^L!_p^L-], the subscript [_p] designating at once, throughout, the bilabiality and voicelessness, the superscript [^L] indicating the lateralization. (If the bilabiality is voiced, a subscript [_b] is used, and the ! may be underscored: !_) We have still not arrived at a point of audibility. . . .

Next appears the above-described, though not yet specified, "release". This phase is characterized by a physiologically modified rush of air, usually audible, measurable, and represented on sound spectrograms by a patch of randomized visual signal. This rush of breath, traditionally labeled "aspiration", is more closely termed *friction*, friction being noise produced when breath is forced through a constricted opening. Friction will be transcribed [*], and when voiced: [*_v]. Here the friction is first bilabial, lateral, and voiceless, thus: [*_p^L], but within 10 msec a separate "lateral friction" portion can be demonstrated by careful magnetic tape segmentation to be devoid of any perceivable bilabiality. Such *voiceless lateral friction* is specified in transcription: [*_L]. And since it is possible to ascertain whether or not any anticipatory articulation gestures have occurred, we have the transcription: [*_L^e]. Up to this point we have: [#_p#_p#_p^L!_p^L*_L^e-].

Next, on the sound spectrogram, and correlatable with the continued lateral articulatory position observable on the roentgen cinefilm, may be seen 7 "steady-state" vertical striations reflecting voicing quasi-periodicity. Generally, as here, this phase is

essentially free of any perceivable frication and might be simply transcribed [l] in its identification as *voiced lateral resonance*. (Any *voiced resonance* is thus [l].) But as I have demonstrated here earlier, the character of a given lateral resonance depends on the "following" vowel; therefore this particular lateral resonance must be transcribed: [l^e]. So far we have: [l^e].

At this time I should like to interject the observation that from utterance to utterance of the so-called "same word", even by the same speaker, there will always be discernible physical differences upon analysis. Spectrograms bear out the commonplace that no speaker can produce the same sound twice. And when, in the course of attempted mechanical coding of speech output, certain postulated segments are not presented to the recognizer due to their omission by the speaker, tolerances must be provided to compensate for their absence. One of my intentions is to stress here the importance of detailed inspections of relevant continua with an idea toward anticipating what may be statistically expected to occur.

Let us now return to the balance of the transcription under way. We are still contending with the lateral. The next phase in the production of any [l]-sound is the *lingual flap* (3), defined as the rapid movement of the tongue away from its resonance phase position of alveolar approximation and contact and corresponding on the sound spectrogram to the drastic movement or sudden shift up of the second formant from the steady-state voiced lateral resonance manifestation and on the cinerontgen film to the above-cited rapid downward movement of the tongue tip and blade especially. This *lingual flap* is transitional and corresponds to any other formant transition phase, though it is of a more drastic sort. Such transitional activity I transcribe [l], and this particular transition is specified [l], and with consideration of the vocalic environment, even more specifically: [l^e].

It is clearly difficult to determine at what point the lateral articulation is over or at what precise point the "pure vowel" has begun, and it is obviously a moot point, since speech sounds are not clear successivities but intimate intergradations. With the synthetic and simulated help of microchronomatic techniques one can attempt evaluations of specific portions of the continuum, but it suffices to follow a logical plan and temper it with such observations. There will come a point in the continuum at hand when one can begin to transcribe "unmolested" vowel. The vowel at hand is, for English, in a nearly constant state of transition, even in open syllable, and the phoneme nucleus direction in which it is moving perceptually can be indicated in the transcription as [el]. Every vowel terminating in open position becomes voiceless long before its positive acoustic features have run their course. Thus we have the subsequent segment ready to be appended as follows: [-el]. And, if one takes the trouble to record these open vowels and listen to them in reverse, one cannot help hearing the strong postvocalic aspiration which we shall transcribe here with [h]. Following this phase are the obvious silence phases analogous to the phases at the beginning of the utterance at hand, and our final pleniphonetic transcription of the uttered word "play" is:

[# #_p #_p^L | L* L*^e e / e_L e_L ih #_h #]

A reliable corroboration of the details here offered has been effected through correlated and synchronized oscillographic, sound spectrographic, cineradiographic and microchronomatic tape-segmentation analyses. The motion picture X-ray films were taken at approximately 48 frames per second – a speed quite ample for disclosing the relevant relationships. The film was 35-mm, and the American English speakers were weeded out very carefully so as to retain, for this study, only those whose speech characteristics were reasonably homogeneous. Tape-recordings were made of the entire operation, synchronizations were mechanical, and supplementary tape-recordings of each speaker were made in a good recording studio.

More directly to the point of this discussion, and in conclusion, may I state that for purposes of descriptive accuracy, for purposes of pedagogical precision, for purposes of machine coding, and for purposes of self-satisfaction, it is not enough to expect that generalizations limited to the tolerance characteristic of phonemes will serve to specify the physical areas to which these phonemes refer.

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DISCUSSION

CARNOCHAN:

I welcome this paper as an important contribution to linguistic analysis, and one that is in sympathy with the prosodic approach to phonology and phonetics developed

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at the School of Oriental and African Studies in London under the influence of the late Professor J. R. Firth. It clearly shows the need for additions to our descriptive vocabulary for phonetic articulatory movements and for acoustic descriptions, and the implications are that there is also need for something rather different at the phonological level than the traditional phonemic segmentation. With our recognition of both prosodic and phonematic elements of a structure at the phonological level, all with their phonetic exponents in the physical plane, we in London are experimenting in this direction.

Truby's pleniphonetic transcription is already a set of abstractions, and as he has shown, not only in his paper, but also in the slides, the symbols of this transcription have their exponents describable in acoustic and physiological terms. I wonder, however, whether he has not gone too far in the number of "places" he recognizes, on the one hand by extending the silence transcription to include initial and final pause, and on the other by requiring two places for $\underset{p}{L}^*$, and three for $\underset{L}{e}$. Would not the recognition of just one place for each of these complex symbols serve his purpose as well, if not better, since in any case you have to describe the physical exponents associated with them in his analysis?¹

¹ Professor Carnochan's strictures refer more to some inadequacies in the reproduction of Truby's complicated transcription in the "Preprints" than to the final version presented here. — *Ed.*

DIALECT BORDERS AND LINGUISTIC SYSTEMS

GIUSEPPE FRANCESCATO

The usual practice in establishing dialect borders is to look for isoglosses and possibly to parallel them with some extra-linguistic factor (geographical, historical, etc.). In this sense, H. Lüdtke's short account of his researches on the dialect border between the Venetian and Friulian dialects¹ is an excellent example of such a method, applied moreover to an area where the suggestions of a linguistic cleavage do not find any support in geographical factors. This is therefore a case of a border which has been very much disputed, and about which many linguists may be inclined to suggest that, rather than a border, it represents only a very slow and gradual zone of transition.² In fact, dialectological research of the traditional kind has repeatedly underlined the impossibility of setting precise dialect borders, and even put in doubt the very existence of dialects.

In spite of similar negative conclusions, the concept of dialect³ is one that linguistic science cannot get rid of, not because of its theoretical preciseness, but because of its practical usefulness. We always speak in terms of "dialect", when opposing different, related or unrelated, linguistic manifestations. The word "dialect" is, to be sure, a sort of common synonym of "language", used in its broader sense, perhaps with the further specification that it does not imply any particular form of literary or social prestige, even though it can be enriched by a local literature, or it can be socially accepted in a wide area. Even the language of the individual – the extreme point of subversion against the very idea of dialect – cannot avoid any reference to dialect itself, when being pointed at as "idiolect". But if we make a large use of the word "dialect" in our terminology, it is by no means true that we have full control of scientific methods for adequately providing a general definition of dialect as such, and a usable technology to deal with distinction of dialects. Nature, or chance, are usually expected to establish for our purposes those definite dialect borders to which the feeling of the speaking people refers so often, implying as a rule that they exist beyond any possible doubt.

The purpose of the present paper, of course, cannot be to describe any absolute

¹ H. Lüdtke, "Inchiesta sul confine dialettale fra il veneto e il friulano", *Orbis*, 1957, pp. 118-121.

² See for instance Th. Gartner, *Rätoromanische Grammatik*, 1883, p. xxxv.

³ Used indeed in a wide range of meanings: one can speak of the "dialects indoeuropéens", and again of the dialect of a family.

methodology for cutting languages into geographical sections which are to be accepted as "dialects" because of perfect coincidence of linguistic, historical, geographical and cultural features. It aims only at pointing out some significant consequences of a closer consideration of some traditional points of view about dialects, when dialect is seen through what we may call the "structural lens".

We have said that one of the main traditional techniques in setting up dialects, or groupings of dialects, is to seek for correspondences of sounds. Some years ago a long-lived and not yet settled controversy was raised among Romance scholars with regard to a group of dialects variously named "Ladin" or "Reto-Romance". This discussion rested mainly upon the possibility of tracing certain phonetic features typical of the entire area, as against other adjacent areas, and upon the means for enhancing the linguistic proof with historical or geographical support.⁴ Whatever the conclusions reached by the supporters of the opposite views, their vision of the problem certainly did not take into account one fact that to modern linguists may seem of primary importance, that is the structural frame of reference of the sounds so much disputed about.

If we apply this manner of viewing things to some of the burning questions of Romance linguistics, we may probably conclude that often in the putting of the problem some essential points have been badly overlooked. This judgement of course does not involve the materials gathered in linguistic atlases as we know them. On the contrary, they keep their full validity, but within the limits of their phonetic character: the patterning of the sounds in the phonological systems of the dialects has to be known from some other sources, or eventually can be gained with appropriate technique from the materials themselves. However, it cannot be left outside consideration.

From the above statements we may infer one more fact, which is of considerable significance, though until today it seems to have deserved only very little concern from the traditional linguists. We are inclined to compare dialects which share a certain border in terms, for instance, of diphthongizations, or of other similar phonetic phenomena. This is the case when we state that, on both sides of the dialectal border, Venetian and Friulian dialects present diphthongization of Vulgar Latin *e*, *o* which results in *iè* and *uò* (*uè*). Here again the occurrence of the same phonetic element, and apparently of the same historical development, does not fit well within the same phonological frame. Widely different conditions for the appearance of diphthongs are provided, to be sure, by the systems of both Venetian and Friulian. If we recognize as indeed we must, the similarity of the phonetic processes, we also have to recognize the dissimilarity of the phonemic systems. The consequences of this view may sound rather strange to the traditional dialectologists: diphthongization of the same sounds has to be recognized as the product of different conditions, and therefore as different, in the two adjacent areas. On the other hand, influence of Venetian *uò* upon the phonetic realization of the Friulian *uè* has also to be accepted to a large extent in the zone toward the Venetian border. Within this zone, Friulian-speaking people accepted the

⁴ See for a summary of the arguments C. Battisti, *Storia della questione ladina*, 1937.

phonetic model of Venetian, but they kept their original phonemic system. This can be further proved, if we observe that in the dialects where $\mu\grave{o}$ replaces $\mu\grave{e}$, it is kept also in the phonological distributions where various variants of $\mu\grave{e}$ (that is $\mu\grave{i}$, $\mu\grave{a}$, etc.) are normal for the standard type of Friulian.

If we consider in this light other much disputed questions of Romance linguistics, another answer, quite different from the traditional one, can be offered. Again in the field of the so-called Reto-Romance dialects, the transition of Latin *a* to *e* has been considered a typical sign of dialects belonging to this group: but other linguists have pointed out that it is to be found elsewhere in Italy, for instance in the Bolognese dialect. Now, all discussion could be avoided if we could only state that Latin *a* becomes *e* in the two areas according to quite different phonological patternings, which actually seems to be the case. Even within Reto-Romance itself, it is by no means proved that the so called isoglosse of $a > e$ responds to the same phonemic conditions everywhere. In a similar way, the appearance of \ddot{u} instead of *u* in Italy, France and part of the Germanic area is certainly to be reduced to quite different phonemic conditions, and therefore cannot be explained with a unique motivation (as the traditional view of the "Celtic" substratum seemed to admit).

Up to this point, I have tried to demonstrate that similar, or identical, phonetic elements can correspond to quite different phenomena when viewed as members of different phonemic systems. But the same holds true even if we reverse the statement and assume that quite different phonetic elements can have a similar or identical role in the common pattern of a group of dialects. This common pattern is what I call a "diasystem",⁵ and I have tried in a paper submitted to a previous congress⁶ to examine the implications of this concept and their bearing on the study of language and its development from the historical point of view. I will try to present here some aspects of the problem on the plane of geographical analysis of language.

If we take again as an example the Romance dialect of Friuli, we will discover that in the different places where Friulian is spoken the vowel system shows a rather regular correspondence of patterning. Everywhere there seems to exist a relevant distinction of strong and weak stressed vowels, and everywhere the weak vowels can be supplemented by two "ascending" diphthongs, such as $\grave{i}\grave{e}$, $\mu\grave{e}$, according to well established conditions. On the contrary, among the strong vowels we find a much wider range of variation, which is reproduced in the following scheme:

Dialect varieties	I.	\grave{i}	\acute{e}	\grave{i}	\acute{a}	\acute{u}	\acute{o}	\acute{u}
	II.	\grave{i}	$\acute{e}\grave{i}$	\grave{i}	\acute{a}	\acute{u}	$\acute{o}\mu$	\acute{u}
	III.	\grave{i}	$\acute{i}\grave{a}$	$\acute{e}\grave{i}$	\acute{a}	$\acute{o}\mu$	$\acute{u}\grave{a}$	\acute{u}
	IV.	\grave{i}	$\acute{e}\grave{i}$	$\acute{e}\grave{i}$	\acute{a}	$\acute{o}\mu$	$\acute{o}\mu$	\acute{u}
	V.	\grave{i}	\acute{e}	$\acute{e}\grave{i}$	\acute{a}	$\acute{o}\mu$	\acute{o}	\acute{u}
	VI.	\grave{i}	$\acute{e}\grave{i}$	$\acute{i}\grave{a}$	\acute{a}	$\acute{e}\mu$	$\acute{e}\mu$	\acute{u}

⁵ See U. Weinreich, "Is a Structural Dialectology possible", *Word*, X (1954), p. 388.

⁶ See my paper for the Xth International Congress of Romance Linguistics, Strasbourg 1962.

One may stop to consider that, phonetically, the main difference of weak and strong vowels is represented by shortness vs. length, while diphthongs accompanying or substituting strong vowels are consistently descending, diphthongs accompanying weak vowels are consistently ascending. The regularity of correspondences, the possibility of retracing such regular differences to the same historical process, can suggest the conclusion that, in the diasystem of Friulian, quite different phonetic types (e.g. *ê*, *éi*, *ía*) can stand precisely in the same phonemic distribution and that eventually they represent, when seen diachronically, some successive steps in the same process of historical development.

If we consider the diasystem of Friulian from this point of view, we can state that, in spite of its geographical differentiation, represented by no less than six (sub)systems, its general phonemic scheme will be everywhere the same, and could be appropriately reproduced by a set of randomly chosen symbols, e.g.

	<i>weak</i>			<i>strong</i>		
a		g	a'		g'	
b		f	b'		f'	
	c	e		c'	e'	
	d			d'		

where the proper phonetic realization could be substituted according to the special "norm" of each subsystem. System regularity could be further underlined by the remark that the extreme positions (a, d, g in the above scheme) will never be substituted for by diphthongs, but their semivocalic variants are the ones occurring as semivowels in diphthongs.

It can be hardly doubted that examples as the one here exhibited are not to be found in large amounts: as a matter of fact, it should be possible to reduce all types of dialectological problems to this model. In other words, all sorts of differences between dialects could possibly be interpreted (at different levels) as aspects of a certain diasystem, and the variations thereof as the manifestations of the different subsystems partaking in the diasystem. Another example of the same nature can probably be quoted from ancient Greece, where – at a literary level – different types of dialects were used with different stylistic implications. At this point it should appear acceptable that the patterning of a diasystem, as we have implied it on the phonological level, could be extended to cover the morphemic and the syntactical level too.⁷

Now, it seems to me that the general idea of "dialect", vague as it can be, receives from the preceding statements a definite support of scientific validity. We can in fact define a dialect as the grouping of various linguistic entities, whose systems all partake in the same diasystem. In other words, a dialect (in the common terminology) represents the concrete manifestation of a diasystem (in structural terminology). Both

⁷ There is no doubt that affinities on the level of lexicology should be easy to detect. But, on the other hand, they are also less significant in so far as the lexical part of the language is the less structured.

represent the result of an abstraction on the part of the researcher who has chosen to define them. The significance of this statement on the historical level has been dealt with in a preceding paper: it is worth while here to try to get similar conclusions as far as spatial differentiation of language is concerned.

One of the first assumptions can be that the area of a certain dialect has to be recognized to extend to the line where the characteristics of its diasystem (independently from their concrete realizations) are still to be perceived. Dialect borders could accordingly be interpreted as the lines where the bundle of isoglosses + separation of partaking in a diasystem are to be found linked together, and possibly more or less coincident within a rather narrow band.

Of course, even these criteria will probably prove insufficient to eliminate all forms of transitions and of merging of dialects, particularly over such borders where no clear cut natural phenomenon supports linguistic differentiation. However, coming back to one of our initial examples, if we consider one of such areas, as it is the flat land we already spoke about at the border between Venetian and Friulian, general conclusions can now more easily be drawn from application of the diasystem criterion in establishing a borderline, even where common admission of a large amount of merging is widespread.

In fact, my personal researches in that area show that between some 46 different features, which can be said to characterize the Friulian type of dialect (that is, the Friulian diasystem), as many as 24 can be still followed down in the system of the border dialects, in spite of many years of extensive reciprocal influence, across the border, of the two involved dialects, of their original similarity and of the prestige-pressure exerted by the Venetian dialect to the expenses of Friulian. A few practical examples can suffice to prove the usefulness of our structural approach in judging the characteristics of such an area.

The typical feature of vowels in the diasystem, that is the weak vs. strong correlation, is maintained in its essential traits and can be recognized in spite of extensive simplification almost to the border suggested by Lüdtke on purely phonetic grounds. Only some points, where dialect merging is most perceptible, show little opportunity for keeping apart the two classes of vowels and their variants. A peculiar feature of the merging zone seems to be the acceptance of phonetic entities extraneous to the Friulian model, but taken from Venetian to be fitted in the distribution proper of Friulian phonemes. A typical trait of Friulian is the opposition of a palatal and a prevelar series of stops, e.g. *k'*, *g'* vs. *č*, *ǰ*. This opposition is later on reduced, in more progressive varieties, to a *č*, *ǰ* vs. *s*, *z* opposition. But along the borderline with Venetian the interdental *θ*, *ð* (or *d*) are taken over to fit exactly the place of *č*, *ǰ*. Therefore *θ*, *ð* extend across the border, but the phonemic conditions of their appearance are quite different in Venetian and in Friulian.

As a consequence, it appears that substantial help to the definition of dialect borders can be drawn from the mapping of the phenomena partaking in diasystems. The research of diasystem characters may enable the linguist – this seems the result of

my experience – to define more easily the line to which a certain dialect is said to extend. Of course, it will be probably impossible to trace this line down to any exact division between adjacent dialects. There will always be some land left for merging, enough to blur all principal features from both sides, so that we still may have to speak of transitions. But again such a transitional area will be more adequately described as the area where, from both sides, the pressure of the diasystem characters is insufficient to give a precise physiognomy to either of the dialects.

On the other hand, if we pick up for study one of the dialects of the transition area, it will be hard to tell which of the adjacent types is predominant in it. It is highly likely that both phonology and morphology show quite different features living together (this amounts to saying that the lexical items of such transition dialects belong either to the one or to the other of the adjacent types), to the point that the speakers themselves will lose sight of the likeness which relates each of these features to one or the other of the side dialects. In this sense Lüdtke's remark can be completely justified, that, according to the speakers themselves, the local manner of speech is considered a sort of "patois",⁸ as opposed both to Venetian and to Friulian dialect.

The present note is only a draft of a theory which has still to be worked out in detail; most examples are taken from the direct and personal experience of the author, and there is still a huge amount of research to be done to control the correspondence between the facts in different conditions and the theoretical situation stated above. From all fields of linguistics, materials could be gathered to support or to disprove the theory. At any rate, it seemed to me worth while to point out the theoretical consequences, as they seem to be possibly deductible, to exploit the concept of diasystem in dialectological research, both from the historical and the geographical point of view. The unity of the linguistic object, the language, that is a unity based both on temporal and spatial relations, seems to grant – at least provisionally – the chance that a concept (the diasystem) which so easily fits into frame of the present ideas about dialect should be a useful means to gain more insight into the nature and aspects of dialect itself.

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⁸ See H. Lüdtke, "Inchiesta...". The word is probably a remnant of the speakers' experience as workers abroad.

STRUCTURE AND TYPOLOGY OF DIALECTAL DIFFERENTIATION

PAVLE IVIĆ

Dialectology can be structural in three respects:

- 1) in regarding language patterns in dialects as structures;
- 2) in examining the social and stylistical structure of the differentiation within particular local dialects;
- 3) in studying the structure of territorial linguistic differentiation.¹

In the first case the structural approach differs from the traditional one by the fact that the investigation is not limited to interdialectal relationships concerning the same linguistic element (E_1 , E_2 , E_3 in dialects 1, 2 and 3 in fig. 1), but includes comparison of relationships between various elements (E . . . and F . . . in fig. 2) in diverse dialects.

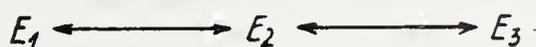


Fig. 1

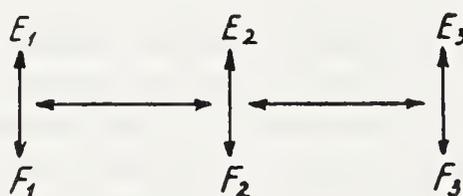


Fig. 2

Thus not simply (interdialectal) relationships between elements, but (interdialectal) relationships between (intradialectal) relationships.

In the second case variants coexisting in a local dialect are envisaged as a system of (linguistic) systems each of which belongs to a social layer or generation, or accomplishes a stylistical function.

The purpose of this paper is to draw the attention of dialectologists to the problems of point 3, which have been so far the most neglected ones.

The structure of dialectal differentiation determines the typological physiognomy of a linguistic landscape. By this term we understand here any part of the territory of a language (or of closely related languages).

¹ This enumeration exhausts the scope of existing possibilities. Dialectology is the discipline dealing with differentiation in linguistic patterns, and the structural approach can be applied either to the study of patterns, or to the study of the differentiation itself. The latter can be vertical (within a local dialect) or horizontal (i.e., territorial).

A necessary prerequisite for the study of differentiation in a linguistic landscape is the knowledge of isoglosses in it, in a number sufficient to be statistically representative. In various language areas this requirement is fulfilled by the existence of linguistic atlases.

By plotting isoglosses from different maps of an atlas on a single map we obtain pictures that in most cases make a chaotic impression, but are as a rule differently patterned in various linguistic landscapes. On closer inspection, the tremendous variegation existing in this respect proves reducible to a limited number of relevant features. All these features are quantitative and measurable. They can be described by numerical indices calculated from the data of linguistic atlases.

1. The *differentiation density* can be determined by counting the isoglosses that cut a straight line of definite length traced on the map. The statistical significance of the index of "isoglosses per mile" or "miles per isogloss" should be secured by calculating the average of results obtained in counting intersection points along a sufficient number of lines traced in the same region. Measurements of this kind will, as a rule, give different results in various parts of a single language territory. It is further possible to divide the linguistic landscape into equal squares in order to count, e.g., "isoglosses per 1000 sq. miles", and also to compute indices of "inhabitants per isogloss".² In all these case figures have a relative character, depending largely on the number of questions in the atlas, which does not necessarily deprive them of statistical significance. If we establish, basing our analysis on an atlas dealing with 500 features, that region A contains 75 isoglosses per 1000 sq. miles, and region B only 25 such isoglosses, it is highly probable that on augmentation of the number of features included would basically confirm this ratio (in an atlas of 1000 features the corresponding figures would not be very far from 150 and 50, and so on). Moreover, it is possible to remodel the indices and to state, e.g., that the differentiation density in the region A is 15% of features per 1000 sq. miles, and in region B 5%.

2. The *linear distribution of isoglosses* oscillates between two extremes: a) even distribution with equal distances between isoglosses, and b) concentration of all isoglosses in a bundle. Although these ideal cases never occur, solutions existing in reality often come close one or the other (figs. 3 and 4). The linear distribution of isoglosses can be determined by tracing a straight line on the map and computing the

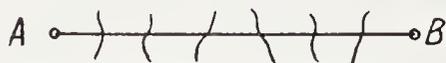


Fig. 3

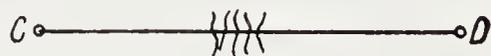


Fig. 4

² A systematic comparison of indices of differentiation in proportion to the area with those in proportion to the population would illuminate the relative importance of these two factors for dialectal differentiation.

average distance between the points where isoglosses cut the line and the points where they would cut it in case of ideally even distribution (fig. 5).

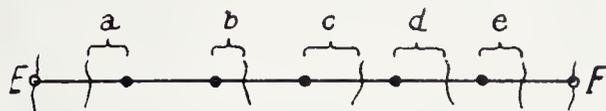


Fig. 5

The index of linear isogloss distribution for the whole landscape can be obtained by calculating the average of indices computed along a statistically representative number of straight lines traced in most different directions. This index will help to answer the question whether territorial dialects do exist in the given landscape (and will help also to realize that the answer to this classic controversial question is usually relative and varies as to landscape). The appropriateness of introducing the notion of territorial dialect in a given landscape is proportional to the closeness of the situation to type *b*. And wherever territorial dialects do exist (in the above sense), it is possible to calculate indices describing the sharpness of their boundaries and their internal uniformity. Numerical indices will also give definitive solutions in controversial cases of dialect classification. The hierarchy of divisions and subdivisions can be expressed in figures.³

3. The *distribution of isoglosses as to direction* (= relative density of differentiation as to direction) can be calculated by comparing the numbers of isoglosses that cut straight lines traced in various directions on the map (fig. 6). Thus it can be discovered that in a landscape A the density of differentiation in the north-south direction is about the same as the density measured along east-west lines, and that in a landscape B the two indices differ greatly. The latter case occurs, e.g., in Gallo-Romance, in German and in Russian, where W-E isoglosses prevail over the N-S ones, or in the South Slavic area, where the NNE-SSW isoglosses (shaded area in fig. 7) definitely outnumber those in the remaining three quarters of the semi-circle.

³ In the author's opinion, methods proposed thus far for dialect classification (traditional historical; synchronically structural, cf. Stankiewicz, *Word*, 13,44-59; diachronically structural, cf. Garde, *Word*, 17,34-62) are of high value, but more for the description of circumstances in dialects than for the actual classification. The genuine criterion of classification is the statistical one. The taxonomy of dialects should be based on the product of the number and the importance of isoglosses by which they are separated. The importance of differences depends on their material extent, their place in structural hierarchy, the number of words affected, and the frequency of these words. With the increasing quantity of material handled the role of the importance of features diminishes, in accord with the growing chance that the coefficient of importance be approximately equal on both sides and with the decreasing index of standard error for the case of disregarding this coefficient. The problem has been discussed in the author's report to the First International Congress of General Dialectology in Louvain 1960 ("Importance des caractéristiques structurales pour la description et la classification des dialectes", summary in the *Programme* of the Congress, 95-96, full text to be published in the *Actes* of the Congress).

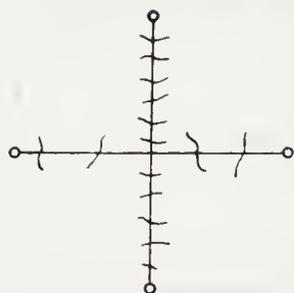


Fig. 6

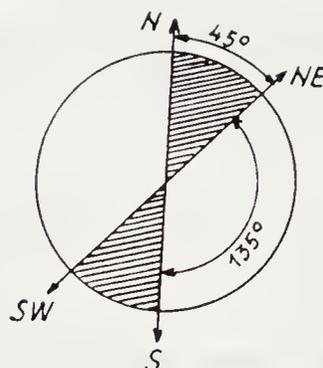


Fig. 7

4. A statistical survey of the *size of areas* of particular features can classify areas in a number of classes depending on their size, and also determine the average value and standard deviation indices. The feature reflects the degree of evolutive homogeneity of a linguistic landscape. If innovation waves in it have a wide range, the number of ample areas will be high, and if the landscape is divided into several portions innovating more or less independently, small areas will prevail by far. Likewise, when innovation areas are in average large, and areas of archaisms small, it is obvious that the dialects involved have undergone a long-lasting common development, whereas the opposite situation shows that this did not take place.

5. The *shape of isoglosses* varies from almost perfectly straight lines to complete disorder on the map (fig. 8, 9 and 10).



Fig. 8



Fig. 9



Fig. 10

The curvature of lines is measurable, and the application of statistics can bring a tabulating of isoglosses as to the degree of curvature, as well as indices of average curvature and of dispersion. The shape of isoglosses being correlated with that of areas, we can alternatively study the shape of areas (e.g., the degree of deviation from the circle as the ideally regular geometrical figure).

6. The crucial point concerning the *relationship between areas* is the question whether feature areas covering partly the same territory include one another (fig. 11) or only intersect (fig. 12).⁴

⁴ Cases when areas have no contact or when they touch each other from outside, are of much less typological interest.



Fig. 11

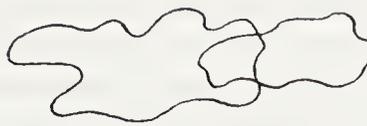


Fig. 12

In every landscape we shall find instances of both kinds, but their ration is variable and belongs to the characteristics of the landscape. In fact, this ratio (if we do not take into account areas of archaisms) indicates the relative share of "Stammbaum" and "wave" pattern elements in the dialectal differentiation of the given landscape. In the Stammbaum pattern, innovation areas never intersect, but either coincide completely, or include one another. This much-debated problem, too, is to be restated in a quantitative way, and the answer varies depending on the landscape.

Features described above in the points 1-3 can be visualized by observing intersections between isoglosses and lines traced on the map, whereas envisaging of features No. 4-6 requires consideration of isoglosses as wholes (which is tantamount to areas as wholes).

In the first triad, feature No. 1 is a simple quantification, and the remaining two concern relationships between isoglosses – No. 2 in a linear, and No. 3 in a two-dimensional perspective.

In the second triad, only No. 6 pertains to relationships between isoglosses. In the plane, there is but one possibility of such relationship: isoglosses either do or do not intersect. On the other hand, two features are possible here which do not affect relationships between isoglosses: areas have their size and their shape (in the first triad a comparable distinction is precluded). The determination of size is again a simple quantification, and as to the shape, the relevant role is played by ratios obtained in comparing results of measurements in two dimensions.

Relevant feature No.:	1	2	3	4	5	6
Constitutive elements:						
Bearing upon areas as wholes	-	-	-	+	+	+
Relationships between isoglosses	-	+	+	-	-	+
Two-dimensional ratios		-	+	-	+	

Numerical indices concerning five out of the six features described are of such a nature that they make possible direct comparisons of results obtained in various language areas. The only exception is made by indices of differentiation density which are influenced by the number of questions in the atlas (and by the number of phenomena left out because they do not present a noteworthy differentiation within the territory covered. Even the problem itself, just what is meant by the term "relative density of differentiation" when applied to areas of unrelated languages, involves

some vagueness. Nevertheless, a careful adaptation of certain methods proposed by glottochronologists and representatives of cognate trends could provide us with reasonably satisfying criteria. Furthermore, it would be a challenging task to compare circumstances in various sectors of the language system, e.g., to take into account only isoglosses of differences affecting the inventory (or the number) of phonemes, or of inflectional morphemes, or of their allomorphs, and so on.

Theoretically, it is imaginable to cover maps of all continents by isoglosses denoting values of indices of the six relevant features listed (with the only limitation concerning No. 1) and furnishing a typological survey of dialectal differentiation throughout the world. This would open the way for two kinds of conclusions. The study of divergences between various *areas* can illuminate the way and the measure in which particular geographic or historical factors influence differentiation within a language area (this would enable us to verify definitively the existing opinions and to give exact foundations to the theory of dialectal differentiation). Likewise, the different behavior of various categories of *features* can suggest conclusions concerning the very nature of these features.

The development of the studies pertaining to the structure of dialectal differentiation will inspire dialectal maps with new life and give new significance to linguistic geography. For illustration let us look at the peculiarities of two dialectal zones in the Serbocroatian language territory. The Torlakian dialect group is characterized by a differentiation density about the SC average, by a very uneven linear distribution of isoglosses and a fairly uneven distribution as to direction, by the size of areas about the SC average, by comparatively straight lines of isoglosses, and by the presence of some Stammbaum elements. The characteristics of the Kajkavian dialect group are: very high differentiation density, even distribution of isoglosses (both linear and as to direction), size of areas below the SC average, comparatively irregular shape of isoglosses, and an almost complete absence of Stammbaum elements. The differences described are at least partly due to the facts that the Torlakian dialects are situated in a region of mountains and valleys, and the Kajkavian ones in a region of hills and plains, and that the former were displaced to a considerable extent by flow-like migratory movements spreading from certain districts, whereas the latter ones had chiefly an organic development without noticeable transplantations.

Besides the six underlying features listed, a number of other phenomena appearing in various linguistic landscapes, and usually evaluated by intuition, can be defined in a more exact way.

A dialectal feature can be labelled *original* if its area is unique, limited and non-diffuse (these characteristics are normally associated with a low degree of probability of a linguistic phenomenon's appearing). Dialectal areas are original when they have a considerable number of original features, and these are, in the sense of the above definition, features occurring nowhere outside of the region (in practice, 5% of such features may be regarded as high). Non-original dialects can be grouped into two major classes. Many of them contain a peculiar combination of features that also exist

elsewhere in various dialects (not always the contiguous ones), whereas others are purely transitional, possessing only features present in one or another neighboring area. Dialects of a whole landscape can be surveyed in respect to the percentage of original features contained; computing averages and dispersion indices would also contribute to a more complete characterization of landscapes. Under certain conditions (wave pattern, even linear distribution of isoglosses, ample areas) we will have the impression that every dialect has a transitional nature.

In cases when dialect boundaries do exist, three basic types of *interrelations* between contiguous dialectal territories are conceivable:

- 1) connection between two *dialects* as wholes (fig. 13; the shaded area represents the diffusion of the common feature/s/);
- 2) connection between one *dialect* and a *part* of the other one (fig. 14);
- 3) connection between *parts* of both (fig.15).

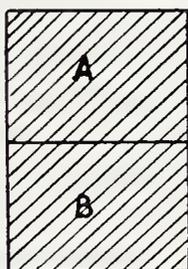


Fig. 13

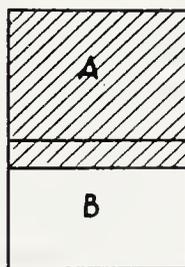


Fig. 14

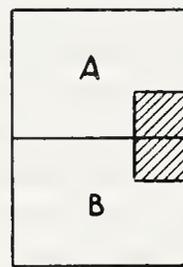


Fig. 15

In the type 2 there is a subdivision: in some cases the relationship is one-sided, but in others it is also possible to adduce some features of dialect B that are present in parts of the area A.

The various types of contacts occur together very often, but not necessarily. All imaginable combinations appear in reality, and one of the future tasks will be to determine the specific factors influencing the typology of interdialectal contacts.

The same distinctions apply to cases of unrelated or distantly related *languages in contact*.

More exact definitions can be given also to other concepts pertaining to the phenomena of linguistic differentiation, such as bundles of isoglosses, degree of concentration of isoglosses in a bundle, kernel and peripheral parts of a territorial dialect, conservatism and progressiveness of dialects, and so on. In all these cases, too, facts are expressible by numerical indices.⁵

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⁵ For a more detailed discussion of some of the questions touched in this paper, see the author's articles "On the Structure of Dialectal Differentiation", *Word*, 18 (1962), 33-53, and "Osnovni aspekti strukture dijalekatske diferencijacije", *Makedonski jazik*, 11-12 (1960-61), 81-103.

DISCUSSION

WEINREICH:

I would like to comment on three points of this very important and stimulating paper, although many other issues could be fruitfully discussed as well.

(1) On the difficult matter of selecting a representative sample of isoglosses, there has been no major progress toward a solution, either in pre-structural or in structural linguistics. However, while the impasse was fatal in geographic studies of the old type (before Gilliéron, in chronology or in spirit), it is much less important for studies of the modern type using extensive questionnaires (hundreds, or even thousands, of items). For it appears that if the number of isoglosses taken is large, their cumulative superimposition approaches a limit of sorts in the continually more detailed subdivision of the area. Five hundred isoglosses superimposed on each other would not necessarily subdivide an area into more parts than, let us say, sixty isoglosses. Consequently, in working with large samples of isoglosses the problem of representativeness does not arise.

(2) The kind of information which Ivić recommends for the characterization of areas may always not be accommodated on a two-dimensional map. There is a need for a great deal of graphic progress in linguistic geography. The use of three-dimensional representations ("mountain diagrams") may be necessary for some of the tasks we have before us.

(3) The availability of electronic data-processing equipment makes it possible for the modern structural dialectologist to calculate the correlations between large masses of facts by automatic means. Whereas for the human clerk the physical superimposition of transparent maps is one of the best ways of making the desired correlation, a machine works best directly from properly coded lists. Hence dialectology is approaching a stage in which the atlas will be only one of the by-products, and merely an intermediate phase, in the total enterprise; machine-produced correlations will be another. Experiments in this field are now in progress in connection with the Language and Culture Atlas of Ashkenazic Jewry.

MCDAVID:

1. Although I should be the last one to decry the cartographic representation of data from linguistic investigations, it seems apparent to one who has been concerned with the social dimensions of speech that this is merely one way of representing the data, and not necessarily the most efficient, that perhaps in insisting on cartographic presentation of data or representation of conclusions we may find ourselves too much influenced by the definition of an Atlas as a collection of maps, and therefore be demonstrating the validity of the Whorfian hypothesis as regards the habitual thought and behavior of one sub-group of linguists.

2. Because the Linguistic Atlas of the United States and Canada, in attempting to assay the social dimensions of language differences in the speech community of North American English, restricted itself to three social levels and chose education as the

critical datum for the identification of these groups, one should not therefore assume that the Atlas investigators have been unaware of the greater complications that might exist within any smaller community, urban or rural, involving the host of traits that social anthropologists have been using in the United States and in other countries for sorting out caste and class groupings within the community. We will concede that the situation in an English city is much more complex than a three-class cutting, with education as the prime datum, can reveal; we can only add that the same observation could be made about any American community as well, with perhaps complications of a few kinds that the investigator in England may not have tried to take into account.

The chief illustration I will offer from the dissertation of Lee Pederson, now in progress at the University of Chicago. He has prefaced his fieldwork by an examination of the complicated interrelationships of the successive waves of settlements that constitute the Chicago metropolitan area. First there were the "Old Americans", predominantly but not exclusively of New England descent. Then followed the Irish and Germans, who built the railways and established breweries and symphonic music as a part of Chicago tradition. Toward the end of the century, with the establishment of the steel mills and other heavy industry, came the Italians, Czechs, Poles, Slovaks, Lithuanians and other groups from Southern and Eastern Europe, with enough Chinese to constitute a compact community in the heart of the city. With World War I came the beginnings of Negro migration from the South; in the last decade have come Puerto Ricans. In each of these groups there have arisen cleavages along lines of educational achievement and economic success, often but not always transcending older associations by race, national origin or religion. Finally, the new flight to the suburbs has resulted in the rise of one-class communities, so that it is possible for a real estate group to discover a rather generally accepted ranking of some ninety suburbs on a six-trait scale.

In the face of this complexity – neither the greatest nor the least that one might find in urban America – social anthropologists and sociologically oriented linguists have concluded that one must approach each community pragmatically, to discover the number of groupings that actually exist. In the meantime, for the overview of regional differences and the dimension of social change, the three-type investigation of the Linguistic Atlas will give a general notion of the direction of change and valuable leads as to the communities where more intensive investigation will be most profitable.

HAMM:

There is no doubt that the structural way of treating linguistical problems during the last four decades has considerably furthered our knowledge of essentials constituting the kernel of lingual units and systems. Since systems were no longer looked upon as fictions in the sense of the young Grammatian School, the adherents of the new way tried to show within the traditional grammatical groups many previously unnoticed correlations in the phonemic and formal systems. They used to this pur-

pose numerical relations and tabulated lists. The impression was overwhelming and eventually led to the delusive perception that such statistical data not merely reflected but represented and *were* the essence of phonemic and inflectional relations themselves. Although investigations made later showed that those contrasting marks were only external and had no direct bearing upon the sources which yielded them, the impressive juxtapositions furthered transformational tendencies in modern linguistics and called into being a new, quite important field of linguistic research work, applied linguistics, with largely transformational attempts, mathematical formulae and machine translations as final scope. The result of this split was a dissension that typifies present-day linguistics, where one part is claiming the heritage of the structuralist schools of the twenties and thirties and concentrating upon statistical and numerical data, while the other keeps to traditional diachronical and functional methods and tries not only to explain the external constitution of all the correlatives concerned but also why they developed in the way we actually find them in language. As always in cases where such dissensions are involved, it would be unjust to call the former one progressive merely because it succeeded the other, or the latter one old-fashioned because it did not desert principles where it still anticipates satisfactory solutions, as it would be simply preposterous to ascribe every unsuccessful transformal attempt to the imposing development of modern technique, especially computing machines with their amazing ability to compute data of the most different sorts and kinds. We must be aware that the development of modern linguistics from Baudouin de Courtenay, Wundt, de Saussure or Fortunatov passed through different stages and that the present one, keeping to one side of the general drift marked by the enormous advance in the field of mechanics, could quite unobservedly swing to conclusions and ways where there is not much left for the hitherto applied autonomous, functional linguistics. Of course, that refers to particularities, but in general both sides are holding to their opposite positions, with antagonistic if not hostile attitudes.

A language, wherever observed, constitutes a system of signs different from any other systems both in the higher units or in the adjoining groups of languages or dialects. The same applies to subdialects. Every dialect – even the smallest subdialect yields its own system. Consequently, the knowledge of dialectology comprehends, or is theoretically supposed to comprehend, all the dialectological systems of the language concerned. Further, since a phoneme gets its structure and meaning in or from a higher (phonemic) unit, other devices, too, get their distinctions in or by the respective systems. They may occasionally be brought into transversal connections with corresponding features in other systems, but the solutions drawn from such connections hardly prove competent to disclose essential elements concerning the structure, origin, growth or shaping of the systems involved. The authors of such solutions approach from the outside, comparing devices of different qualities and evaluating them by the same standards. It is no wonder that they lead sometimes to quite unexpected apprehensions, such as when Stankiewicz states that a common phonemic type “occurs” largely in North-Western and in the Eastern Slavic languages,

including Eastern Bulgarian, but it is also found in some Slovenian dialects"¹ (i. e. jointly in the North-West, East, South-East and South-West Slavic territory), or when Pavle Ivić, on the basis of a merely stationary counting of prosodic possibilities, arrives at the conclusion that "indexes of prosodic possibilities will facilitate a comparison between languages with diverse prosodic patterns" and that, for instance, "a comparison of the relative shares of prosodic distinctions in SC and for example in Lithuanian or Swedish could be highly instructive"² (omitting to say which SC, the literary or the dialectal one, and if the latter, which SC dialect should be compared with the Swedish language, and with which pattern of the Swedish language).³ Languages ought not to be compared like electricity transmitters or fancy dresses. They have something of their own. They create and form their systems by themselves, without intentional help from the outside. Purely external comparisons are bound to be extralinguistical.

Ivić has tried in his paper here to apply numerical presentations to dialects without taking into consideration some of the fundamental differences between dialects and literary languages, the latter being normalized, traditional, tied to conservative, fixed forms, the former free and not tied either by tradition or normalized standard forms. It is quite clear that the latter will provide material for statistical determinations more easily than the former, where practically all devices never occur together and, consequently, can not be counted or statistically evaluated. An example of a similar treatment may be found in the paper by H. Kučera (pp. 713–721) who on 24 positional frequencies tried to set the isotopy of modern Russian and Czech using an average of 100,000 samples of phonemes on each side. The result – the occurrence probability of a phoneme in a given syllabic position (Russian – Czech) of 0.73860365 resp. 0.77221670 – seems both modest and insignificant, because only two of the twelve Slavic languages were compared. But even if all the Slavic languages were comprehended, the numerical differences would show only outside relations, leaving the questions of growth and intrinsic structure unanswered. Therefore Kučera's suggestion (that the isotopy approach may have some interesting possibilities in the investigation of genetic relationship of languages and in measuring the dynamism of phonological change) acquired in the last sentence a restriction that does honor to his scholarship, for he has frankly admitted that "further experiments are necessary before any such usefulness can be claimed".

The difficulty with languages is in the difference of shapes that features can assume. The same feature may be distinctive in one relation (or correlation), redundant in another, or non-relevant in the third. For instance the functional yield of palatali-

¹ "Towards a Phonemic Typology of the Slavic Languages", *Amer. Contrib. to the Fourth International Congress of Slavists* (The Hague, 1958), p. 317.

² "The Functional Yield of Prosodic Features in the Patterns of Serbocroatian Dialects", *Word*, 17,3 (1961), p. 307.

³ And when will the Serbocroatian dialectologists stop quoting Belić's "Zametki" of 1909 (with their notorious inadequacy concerning some prosodical patterns) for the dialect of Novi.

zation features happens to be quite different in the Russian, Czech, Serbocroatian or Polish languages, where the $c z s \bar{z}$ (the last one not occurring in modern Czech, Russian or Serbocroatian) $\sim \check{c} \check{z} \check{s} \check{\bar{z}}$ (with the last one missing in Czech and Russian) only in the Polish language shaped a perfectly complete $\acute{c} \acute{z} \acute{s} \acute{\bar{z}}$ set (with only the first and last elements occurring in SC). Polish possesses e.g. also a special distinctive feature among the vowels which does not occur in any other Slavic language – nasality. Or the i/y as it appears in Russian or Polish, with different coalescent features in Ukrainian. The $g > x > h$ can be taken as distinctive between Russian and Ukrainian, or Polish and Czech, redundant between Slovak or Czech, Ukrainian or White-Russian, non-relevant in Slovenian or Serbocroatian but locally and dialectologically distinctive between adjacent Čakavian and Slovenian dialects (not to mention \hat{e} , capable of constituting distinctive features in one, redundant and neutral ones in other dialects or subdialects of the same language). The interdependent value of such features cannot be fully represented by quantitative numerical indices. If we evaluate the distinctive features by one, the redundant ones by another quantity or number (e.g. 10:8), the products, sums or quotients will only mix together values of different qualities and yield something that does not match any of the participant systems, or disclose what was belonging to the basic or to the compared dialects.

We distinguish three kinds of features where interlingual relations are concerned. To avoid ambiguity with distinctive and redundant features I prefer to call them relevant and non-relevant ones, dividing the first into relevant features of the first and the second degree. Relevant features are pivotal and influence the whole system, the non-relevant ones are closely related to the first but without noticeable influence on it. For instance as relevant in the first degree in the dialects of Lower Podravina and the ekavian literary language could be mentioned the three versus four prosodic stress, the coalescent/non-coalescent \acute{c}/\check{c} , \acute{z}/\check{z} , the $*stj > \acute{s}\acute{c}/\acute{s}\check{t}$ invariants, \hat{e} as e versus e/i (*lêpo – sićam se, dête – divójká*) plus the coalescent/non-coalescent Lpl., whereas the majority of other features could be assigned either to the relevant category of second degree ($*tj > \acute{c}$ not $t-j$, $*dj > \acute{d}$ not $d-j$ in positions where \hat{e} is not concerned, etc.) or to the non-relevant one. The slightest shift, in comparison with other units (e.g. dialects), causes different internal arrangements which must not necessarily lead to different numerical presentations. How devices such as $\supset \cup \parallel \mid > \mid$ in *deľiti, fãľiti, moľiti, soľiti* against *siliti, bãliti* could be satisfactorily presented in a quantitative way, remains beyond one's comprehension, as do allegations that “numerical indices will give definitive solutions in controversial cases of dialect classification” (p.117) or that the “genuine criterion of classification is the statistical one” (fn. 3) If the taxonomy of dialectological devices should be based on quantity, such quantity could not possibly be a “product of the number and the importance of isoglosses”. Both include different qualities and therefore cannot be counted on an equal level. Ivić's paper has other weak spots also. For example, at one point he speaks deprecatingly of evaluations by intuition, asking for definitions in a more exact way, and at another point he talks of isoglosses “in a number sufficient to be statistically

representative" (p. 116). Where the limits of such "sufficiency" lie, nobody knows. But one readily suspects that they will be subject to controversy. On the other side, while speaking of computing the number and importance of isoglosses, the author defines their importance and differences as depending "on their material extent, their place in the structural hierarchy, the number of words affected, and the frequency of these words" (fn. 3). It is quite evident that not one single Slavic dialectological presentation of today is able to furnish all these data (e.g. not one contains, or is even expected to contain a full vocabulary, much less detailed frequency indices⁴), and that every taxonomy based on similarly deficient foundations must in the end prove illusory, if not deceptive.

Somehow I have a slight suspicion that the author arrived at his puzzling conclusions because he followed an irresistible, subconscious wish to overcome the present state of affairs, which is not a very encouraging one, in his native (South Slavic) dialectology and thus was seeking an approach to the vast field of modern applied linguistics. He only went a little bit too far. He asked for things that could not be done at the present stage, where he himself was compelled to cite and use data registered not less than fifty years ago. The one thing a dialectologist could, and perhaps should, ask from people engaged in applied linguistics is an instrument on dictaphone principles which could deliver a true, reliable transcription of all the phonemic and prosodic devices taken on a tape-recorder. To dispense with subjective annotations, individual sign systems and similar shortcomings would render modern dialectology better service than a vague taxonomy based on quantitative relations and frequency indices.

LABOV:

Another means of classifying isoglosses has been worked out recently, in addition to the methods described in Professor Ivić's paper.

This approach describes the relationship of language boundaries to lines of communication; its significance depends upon the fact that many of the non-linguistic features which have been shown to parallel language boundaries – mountains, rivers, political lines – can hardly affect language directly, but only affect the movements of speakers.

We can rate an isogloss according to whether it runs parallel or perpendicular to lines of communication. More exactly, we can count the number of major lines of communication which the isogloss crosses per unit length. Furthermore, in a country such as the United States, we can actually calculate the approximate number of individuals who cross a given language boundary every day. This has been done for all of the major isogloss bundles of the eastern United States, as shown in the *Word Geography of the Eastern United States* and *The Pronunciation of English in the Atlantic States*. The great majority of isoglosses have very low ratings – lower than

⁴ Not even on the level of Victor Garcia Hoz's *Vocabulario Usual, Comun y Fundamental* (Madrid, C.S.I.C., 1953).

any imaginary alternate lines we might trace in the vicinity. We might call these "low energy isoglosses". They mark troughs in the communication network. On the other hand, there are a small number of isoglosses which fall on the opposite end of the scale: a very large concentration of speakers cross these lines every day. We might call these "high energy isoglosses".

The differentiation of isoglosses in this manner has had useful consequences in studying the social meaning of language boundaries, both for the present situation and for historical developments. In particular, the social prestige of a dialect seems to be closely related to this classification of "social energy".

IVIĆ:

Prof. Weinreich's idea that computers could be used for the classification of dialects seems perfectly plausible to me. Moreover, I have the impression that in the future of dialectology their role will be constantly increasing. In many countries popular dialects are in full decline, and before long they will disappear. Dialectology will be oriented on the material collected previously (e.g. that in the atlases), and one of its principal tasks will be processing that material by use of advanced technological devices.

I can also agree with Prof. McDavid, who stresses that atlases should constitute only a part of the results of dialectal investigations. Among others, statistically based diagrams of relative proximity of various dialects should be considered as a relevant part of the final results of dialectological work in any area.

Prof. Kurath is obviously right in stating that not only the number of isoglosses has to be taken into account for classification of dialects, but also their importance in the language. This importance depends on three elements: the material size of the inter-dialectal difference (a difference between /a/ and /i/ means more than a difference between /ɛ/ and /e/, a difference in two phonemes more than a difference concerning only one phoneme, etc.), the frequency of the phenomenon involved (lexical differences concerning very usual words are prior to differences in rarely used lexemes, etc.), and its structural importance (the question whether the imperfect exists in a dialect as a category is more important than differences in its endings). The genuine criterion of classification would be: the product (in the mathematical sense of the term) of the number and the importance of isoglosses.

In contradistinction to Prof. Hamm, I do not believe that isoglosses can be divided in a small number of relevant ones (of first or second order) and an immense number of those without importance. It is true that most of existing classifications are based on a limited number of isoglosses, selected according to the dominating linguistic theory of the epoch or to individual predilections of the investigator. This deprived the dialect classification of objective value and caused numerous and endless arguments. Nevertheless dialectologists are not to blame for this state of affairs: in most cases they possessed neither a sufficiently large corpus of data nor the necessary technical devices to process these data. But the dialectologist of the times to come will

have atlases and computers at his disposal. Of course a huge sample will allow so-called major phonetic changes and structural differences to play the decisive role: they will occur in the sample in a high number of (frequent) words. Thus the picture will not be thoroughly different from that obtained by selecting several fundamental features. On the other hand this picture will be more exact and more complete. It is for instance not irrelevant whether a "major" isogloss is accompanied by hundreds of "smaller" ones forming a bundle with it, or stands alone on the map, perhaps at a random place.

I also believe, with Prof. Labov, that history and geography influence dialectal differentiation first of all by determining the lines of communication. But this is not the only relevant element: we have to add migrations, highly important e.g. for the dialectal picture of Russian, German (especially in the East), American English or Serbocroatian, but almost unimportant in some other areas, for ex. in Gallo-Romance. And even communication exists in different kinds; the deepest influence is exerted by intermarriages.

REMARQUES SUR QUELQUES ISOGLOSSES DIALECTALES

BÉLA KÁLMÁN

A la fin du siècle passé, Ascoli, Paris, Meyer, Gauchat ont mis hors de doute que sauf exception il ne saurait point être question de lignes de démarcation précises entre dialectes, puisque les frontières d'un phénomène dialectal donné ne coïncident que bien rarement avec celles d'un autre phénomène. Même les grands obstacles naturels (rivières, chaînes de montagnes, mers) ne constituent pas toujours de frontières dialectales. Dans les dialectes hongrois, par exemple, l'isoglosse d'un phénomène dialectal ne coïncide qu'exceptionnellement avec le cours d'une rivière ou avec une chaîne de montagne. Même un fleuve aussi important que le Danube constitue une voie de communication et un lien plutôt qu'une ligne de séparation entre les populations vivant sur les deux rives. Par exemple, l'emploi de *á* précédant un *á* (p.e. *kapa* "houe", mais *kápál* "houer"; *apa* "père", mais *ápám* "mon père"; *arat* "récolter", mais *árátás* "récolte") se retrouve dans la région nord-ouest de l'aire linguistique hongroise sur chacune des deux rives du Danube. Les détroits peu larges ne forment pas non plus, entre les dialectes des populations du continent et des îles, des frontières, des lignes de séparation essentielles, p. e. dans les dialectes finnois ou norvégiens. Dans le dialecte des pâtres montagnards roumains les montagnes formaient, dans les siècles passés, un lien plutôt qu'un facteur de séparation. Il n'en est pas de même de ce qu'on appelle "îles dialectales extérieures", soit des dialectes séparés des autres dialectes de la même langue par une population de langue étrangère.

L'atlas linguistique allemand de Wenker et l'atlas linguistique français de Gillieron-Edmond, rédigés sur la base de principes méthodiques entièrement différents, ont ébranlé la confiance même dans les isoglosses dialectales, comme dans la possibilité de séparer sur la carte des phénomènes dialectaux. De chacun de ces deux atlas se dégage la leçon que les phénomènes linguistiques se propagent par mots isolés; que telle ou telle loi phonétique ne se fait pas toujours valoir dans chaque mot sans exception; et que dans la mesure où il est possible de relier les données identiques par des lignes, les isoglosses diffèrent pour chaque mot. Les atlas linguistiques montrent, en plus, que certains changements linguistiques se propagent non seulement sous forme d'ondes, mais aussi en rayons ou par bonds (cf. Bárczi, *Bevezetés a nyelvtudományba* [Introduction à la linguistique], Budapest, 1953, pp. 138-139). Cette bigarrure est rehaussée encore par les défauts méthodologiques de chacun des

deux atlas – défauts de différent caractère il est vrai, mais qui déforment également la réalité. L'atlas linguistique de Wenker a projeté sur la carte des données fournies par des dilettantes et transcrites dans le dialecte en question; tandis que l'atlas linguistique français se sert d'une méthode impressionniste et de la collaboration d'informateurs, souvent uniques et choisis à l'improviste, qui plus d'une fois, ont fourni des données beaucoup plus proches du langage littéraire que de l'état linguistique réel de la commune en question. C'est un fait connu que les atlas régionaux français plus récents, publiés un demi-siècle plus tard que l'atlas Gilliéron-Edmond, contiennent une foule de formes linguistiques plus archaïques que les données notées par Edmond.

Il est hors de doute que, de nos jours, partout où il existe, en dehors du dialecte, une langue littéraire, il faut compter, d'une part, avec la diminution du nombre de ceux qui parlent dialecte; d'autre part avec le fait que les dialectes eux-mêmes subissent l'influence de la langue commune; en se polissant, en s'usant, ils s'en rapprochent peu à peu. Cependant, selon Benkő (*Magyar nyelvjárás történet* [Histoire des dialectes hongrois], Budapest, 1957, p. 9), même aujourd'hui, plus de la moitié des personnes qui parlent hongrois, se servent d'une variante plus ou moins pure de tel ou tel dialecte comme moyen de communication linguistique. Ajoutons le fait mentionné, entre autres, par Benkő (p. 6), que même ceux qui parlent le langage commun (la langue littéraire), s'en servent en le colorant des nuances du dialecte. La distinction des phonèmes *ĕ* (e fermé) et *e* (e ouvert) est un fait qui appartient à la langue commune mais l'emploi de l'*e* unifié s'y trouve également. L'allongement exercée par *-l*, *-r*, *-j* dans une syllabe fermée: *fāl* "mur", *hĕj* (hely) "lieu", *akār* "il veut"; de même que la prononciation sourde de la consonne sonore en finale absolue: *doB* "tambour", *vĕD* "il protège", *vĕG* "fin", sont des nuances qui ne font pas encore l'impression d'une prononciation provinciale. La langue commune permet l'emploi de nombreux doublets phonétiques, et on trouve même des mots à trois variantes: (*fĕl* ~ *fel* ~ *fĕl* "en haut"; *sĕpĕr* ~ *seper* ~ *sĕpĕr* "balayer"); la morphologie autorise, elle aussi, l'emploi de formes doubles (p.e. *aludjĕk* ~ *aludjon* "qu'il dorme"); et la langue commune admet souvent deux mots ou deux variantes de forme différente, p.e. *felhĕ* ~ *felleg* "nuage", *kukorica* ~ *tengeri* (*tengĕri*) "maïs", etc.

C'est donc à bon droit qu'on pose la question si les phénomènes dialectaux ont des frontières, s'il y a des isoglosses ou non. Il va sans dire que ces isoglosses ne sauraient être conçues que dans la synchronie, puisque la propagation et la retraite, voire même l'existence de tel ou tel phénomène sont les résultats d'un processus historique. Ainsi, la ligne des isoglosses, même par rapport au même phénomène, peut différer d'époque en époque (p. e. en hongrois le refoulement graduel du phonème *ly*, qui dure encore). Suivant Benkő (p. 23), les frontières des phénomènes envisagés d'un point de vue diachronique, sont variables du temps, "car dans les dialectes, organismes linguistiques vivants, les phénomènes «se meuvent» d'ordinaire: ils s'étendent ou se rétrécissent quant à leur territoire".

Benkő cite des exemples à l'appui de sa thèse selon laquelle il est extrêmement

difficile de tracer les isoglosses soit par rapport à des phénomènes phonétiques, soit quant au lexique ou à la morphologie. Il résume les résultats de son analyse de la manière suivante: “Nous devons considérer la grande majorité des isoglosses, même quand il ne s’agit que d’un seul phénomène comme une série de lignes ou, plus précisément, de bandes qui représentent l’extension du phénomène telle qu’elle est dans la réalité dans toute sa complexité lourde de problèmes” (p. 24).

En utilisant mes expériences personnelles, j’essayerai de classer systématiquement les phénomènes qui fournissent des isoglosses, c’est-à-dire ceux dont les limites peuvent être représentées par des lignes. Il va sans dire qu’un tel procédé mettra en relief les phénomènes auxquels M. Benkő fait allusion et qu’il appelle la “grande majorité” des cas.

A. PHONÉTIQUE

1. Le surplus ou le défaut d’un phonème donne toujours des isoglosses. Ainsi, p. e., on peut délimiter partout d’une ligne nette la présence du phonème *ĕ* (*ĕ* ~ *e* ou seulement *e*). Cette ligne donne, même sur un territoire aussi bouleversé du point de vue de l’histoire du peuplement que le Hajdúság et la partie plate de Bihar, une bonne isoglosse. Du point de vue des isoglosses, il est sans importance que dans tel ou tel mot – *nem* ~ *nĕm* “non”, *tizenöt* ~ *tizĕnöt* “quinze” – le rendement fonctionnel du phonème est plus ou moins grand. De même, il est possible de tracer la ligne de démarcation dans le cas du phonème *ly* (*l* mouillé) de la région centrale du territoire Palóc même quand ce phénomène est, principalement sur les frontières du phénomène, en voie d’extinction, et qu’il n’apparaît plus que rarement dans le parler des jeunes. L’emploi des deux *é* du système de voyelles longues (Palóc moyen: *ĕ* ~ *é*, nord-est: *ĕĭ* ~ *ĭe* ou *ĕ*) disparaît lui aussi dans tous les domaines de la langue, et non par mots isolés: l’isoglosse a donc là un caractère linéaire.

2. La variété dialectale constante d’un phonème peut être représentée, le plus souvent, par une ligne de démarcation. Tel est l’*à* illabial du palóc (qui figure aussi dans certains dialectes périphériques) qui s’oppose à l’*á* labial de la langue commune et des autres dialectes. Là, la variante de transition est rare. Il est beaucoup plus difficile de tracer la ligne de démarcation entre l’*ǫ*, *ę* (*e* plus ouverts), occidental et oriental, de même que la frontière de l’*ā* et de l’*ǫ* du palóc, et de l’*á* transdanubien, puisqu’on s’y trouve en présence de voyelles de transition à peine perceptibles à l’oreille, et il peut y avoir beaucoup de divergences jusque dans les prononciations individuelles.

Par contre, on peut délimiter avec une certitude suffisante le territoire des diphtongues. Bien qu’on rencontre, même dans les dialectes riches en diphtongues des variantes à monophthongues, on réussit à tracer la frontière de *ó* ~ *ou* (*ó*), de *ő* ~ *öü* (*ő*), de *é* ~ *ĕĭ* (*é*), de même que, dans les régions à diphtongues ascendantes, celle entre *ó* ~ *ou* (*ó*), *ő* ~ *üö* (*ő*), *é* ~ *iĕ* (*é*), puisque ce n’est pas par mots isolés que la diphtongue se propage. Il n’existe pas de dialecte à diphtongues où la diphtongue et

la monophthongue correspondante forment deux phonèmes différents et où on ne prononcerait, par exemple, le mot *óra* "heure" qu'avec une monophthongue et le mot *ouđal* (*oldal* "côté") qu'avec une diphtongue. En de tels dialectes *óra* ~ *ouřa*, *óđal* ~ *ouđal* il y a des variantes pouvant être employées à volonté; le timbre de la diphtongue peut également varier, cependant elle est toujours descendante, c'est-à-dire que la prononciation *uodál* est, dans le même dialecte, impossible. On se trouve donc en présence de trois variétés: diphtongue descendante: *ou* (*aø*); *öü* (*eö*), *ëï* (*eë*); comme variante secondaire on trouve, le plus souvent, *ó*, *õ*, *é*; diphtongue ascendante *uo* (*ua*), *üö* (*üe*), *ië* (*ëe*); comme variante secondaire assez rare: *é*, *õ*, *ó*.

On réussit souvent à délimiter par une ligne de démarcation la présence ou l'absence d'une variante combinatoire. Ainsi p. ex. l'emploi de *á* devant *á*, mentionné plus haut: *kapa* "houe", mais *kápál* "houer"; *apa* "père", mais *ápám* "mon père"; *arat* "récolter", mais *árátás* "récolte"; ce n'est pas à travers des formes transitoires, en certains mots, que cet emploi cesse, mais tout d'un coup, sans transition. Là où on prononce *kapál*, la prononciation *apám* est exclue, et *vice versa*. Il est beaucoup plus difficile, presque impossible de représenter à l'aide d'une ligne, la frontière de l'action dissimilatrice régressive de l'*á*: le type *lábo* "son pied", *hátom* "mon dos" n'existent pas en vertu de la propagation par mots, mais à cause de la transition phonétique graduelle: *lábo*, *lábo*, *lába*, *lába*.

3. Il est beaucoup plus malaisé de représenter l'isoglosse à l'aide d'une ligne dans les cas où il ne s'agit pas d'un excédent de phonèmes ou de son expression phonétique spéciale, mais d'une différence de fréquence. Les lois phonétiques ne sont pas, comme la doctrine néo-grammaticale orthodoxe l'avait enseigné, des lois sans exception. Ce qu'on appelle prononciation avec *í*, *ö*, respectivement *l* (*t-zés*, *ö-zés*, *l-ézés*) se propage, en effet, en des mots isolés; certaines formes en *í*, telles que *nígy* "quatre", *níz* "il regarde", de nombreux mots en *ö*, comme *föcske* "hirondelle", *köll* "il faut", quelques mots en *l*, comme *hel* "lieu", *mágla* "pile", se propagent bien au delà de les frontières des dialectes à *í*, à *ö*, à *l*. De même, la chute de *-l-* dans une syllabe fermée peut différer selon les mots: *vót* "il était", *zöd* "vert" ont une extension beaucoup plus large que p.e. *āma*, *óma* "pomme". Cependant il n'est pas impossible, là non plus, de tracer une frontière, pourvu qu'on considère le changement quantitatif comme qualitatif. Là où, dans deux villages voisins, en face de la majorité des *ë* fermés de l'un des villages (même si ce phénomène se borne à la position atone) nous trouvons *ö* dans l'autre village; ou bien là où aux quelques cas en *í* d'un village correspondent des cas en *í* en masse dans le village voisin, on ne pêche pas contre la réalité en parlant d'une ligne de démarcation.

B. MORPHOLOGIE ET SYNTAXE

Je ne citerai que quelques exemples. L'emploi de morphèmes exprimant un rapport de lieu *-nott*, *-nól* (*-núl*), *-nyi* ("chez, de chez") peut être délimité assez nettement (il va sans dire que chacun d'eux aura une isoglosse spéciale). Les formes *mosuk*

(= mossuk “nous le lavons”), *tuduk* (= tudjuk “nous le savons”) du palóc, et le *mëgmosunk a ruhát* (= megmossuk “nous le lavons”, le linge) de la région de Baranya aboutissent également à des isoglosses linéaires. Il est beaucoup plus difficile de tracer les frontières de telle forme de conjugaison en *-ik*, et cela à cause des nombreuses formes parallèles.

C. LEXIQUE

Dans le lexique cela dépend du mot respectif. Il est absolument impossible de tracer par exemple, les frontières des mots *fukar*, *fösvény*, *zsugori* “avare”, puisque chacun d’eux est employé dans presque tous les villages. L’appellation de *nagyanya* “grand mère” change, elle aussi, dans chaque village ou peu s’en faut, selon les générations et les familles. Le nom de plusieurs espèces de mauvaises herbes diffère presque dans chaque village, et ainsi de suite.

En revanche, il y a des mots tels que *kukorica* ~ *tengeri* ~ *törögbúza* ~ *málé*, etc., et les variantes de ceux-ci dont on peut délimiter l’extension sans bandes de transition. Tels sont encore: *pióca* (*pióka*) ~ *nadály* “sangue”; *réce* ~ *ruca* ~ *kacsa* ~ *kácsa*, etc., “canard”; *iga* ~ *járom* “joug”; *fütyül* ~ *süvőt* “il siffle”; *dörög* ~ *zeng* ~ *görget* “il tonne”; *légy* ~ *bogár* “mouche”; *sonka* ~ *sódar* “jambon”; *pad* ~ *lóca* “banc”; *vánkos* ~ *fejel* ~ *párna* “coussin”, etc.

Il faut ajouter à tout ce que nous venons de dire, que ce sont les variantes dialectales que les isoglosses doivent refléter. De nos jours, on trouve dans chaque commune de Hongrie, si on a la volonté ferme d’en trouver, au milieu d’une population parlant dialecte, des personnes parlant la langue commune. La transposition des données du langage littéraire sur la carte, si elle ne reflète pas le langage moyen du village, est susceptible non seulement d’embrouiller l’isoglosse, mais aussi de déformer l’image de la réalité.

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DISCUSSION

Ivić:

Je voudrais bien souligner qu’il n’y a qu’une grande catégorie de différences dialectales dont les isoglosses sont difficiles à déterminer. Ce sont les phénomènes caractérisés par une transition graduelle et à peine perceptible, comme celle entre les valeurs [æ] et [ɛ] pour le même phonème en hongrois. Cependant il est possible de résoudre les problèmes de ce genre, par ex. en traçant l’isoglosse d’une valeur choisie de la fréquence moyenne du premier formant de la voyelle. – Dans les cas comme celui des trois synonymes désignant “l’avare” dont chacun est employé dans tous les parlers hongrois, il est vrai qu’il n’y a pas d’isoglosses, mais c’est parce qu’en effet il n’y a pas de *différences* parmi les dialectes.

INTERRELATION BETWEEN REGIONAL AND SOCIAL DIALECTS

HANS KURATH

For nearly a century students of the spoken language have focused their attention on regional and local differences in *folk speech*, gathering information by systematic sampling of one kind or another and publishing their findings on maps, item by item. Relying upon these source materials, individual heteroglosses have been established, and dialect boundaries based upon bundles of such dividing lines, whether close-knit or spaced, have been drawn to exhibit the internal dialectal structure of more or less extensive areas.

As a third step, attempts have been made to correlate the boundaries occurring in folk speech with settlement boundaries (migrations), with old and new political boundaries, with stable or shifting economic boundaries, with diocesan boundaries, with confessional barriers that discourage or prevent intermarriage, with physical features of the landscape that channel or hinder trade, and with the rise and fall of dominant cultural centers; in short, with a great variety of factors that are apt to hinder or favor communication.

Many inferences drawn from such correlations are well established; others are probable or at least suggestive. No one can fail to recognize the important new insights that have been achieved in this field of research with regard to the complicated nature of linguistic change in its relation to the various aspects of the life of a community or nation. And yet, considerable refinement in tracing the processes of diffusion and recession in linguistic usage are within our reach.

Two lines of research, already under way, will make for progress in dialectology — the study of living speech. One is the application of the concepts of structural linguistics to the raw data. The other is the systematic recording and treatment of the speech of at least two social levels in addition to that of the *folk* (the peasant or rustic, and the underprivileged city dweller): that of the *cultural elite* and the *middle class*. I shall address myself chiefly to the potential contribution of the investigation of cultivated and middle class speech to our understanding of linguistic change by diffusion.

Until recently the dialectologist has had at his disposal only a more or less adequate record of regional folk usage, which enabled him to trace in realistic fashion the influence of one folk dialect upon another folk dialect through direct contact between the speakers. When confronted with features of vocabulary, grammar, or phonology

that did not seem to fit into either of the two adjoining folk dialects, he resorted to "supra-regional" influence or adoption from the national standard, i.e., to direct or indirect borrowing from a privileged class dialect of another speech area. He arrived at this decision by a process of elimination. For lack of an adequate record of upper class speech current within the area of his immediate concern, or of adjoining or remote areas, he usually could not go beyond a bald assertion. He could not trace the route or routes of such infiltrations in precise terms.

A realistic account of this process is possible only when a record of middle class and upper class usage within the area is available. To supply this information for the various European countries in which folk usage has been systematically recorded is surely one of the major tasks confronting the dialectologist. This need was clearly foreseen forty years ago by Henry C. Wyld, when he said in his *History of Modern Colloquial English*, p. 186: "It is remarkable that while the English of illiterate elderly peasants has often been examined, with the view of recording for posterity the rugged accents of the agricultural community . . . it has not been thought worth while to preserve the passing fashions of speech of the courtly and polite of a former day."

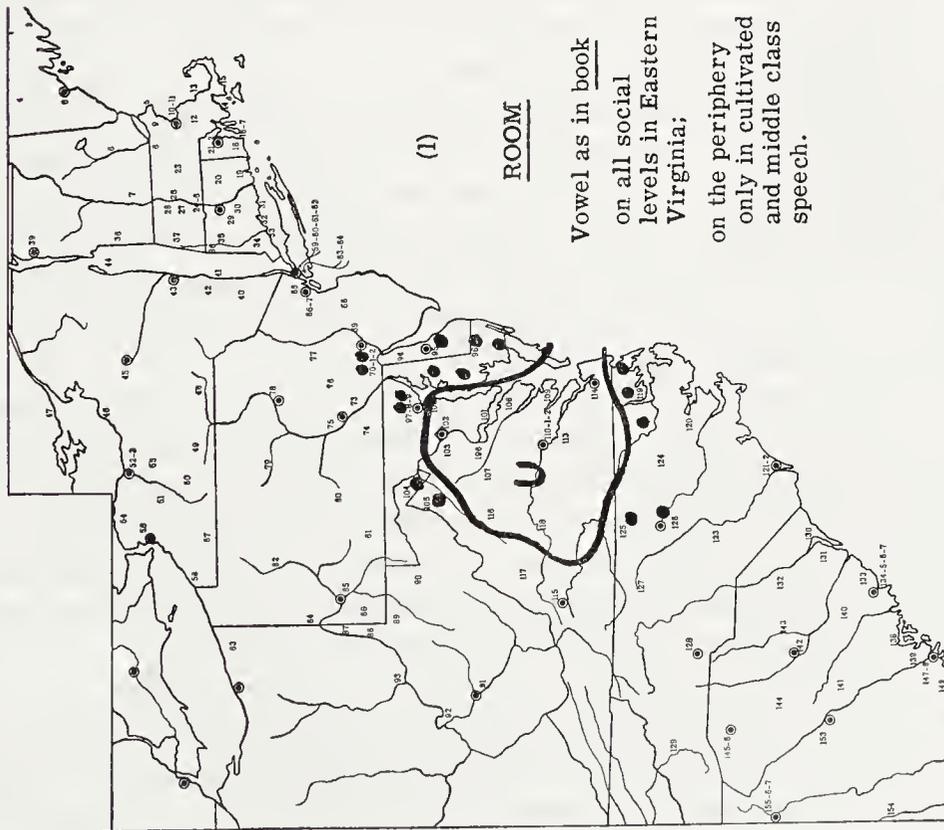
Regional usage of the ever-growing middle group of speakers should in my opinion be the dialectologist's primary object of future research. Without reliable, detailed knowledge of usage on this social level, the influence of cultivated speech upon folk speech, and vice versa, cannot be traced in realistic fashion, since the social extremes do not influence each other directly. It is the middle group that mediates between them.

In the United States a modest beginning has been made in this direction. In the linguistic survey of the Eastern States, the usage of the social extremes — the folk and the cultured — as well as that of the middle group was systematically investigated. Of the 1500 informants, about 700 represent the folk, 650 the middle class, and 150 the cultural elite. The inclusion of speakers from three social levels, I may say, was dictated by the democratic organization of American society, which knows no clear-cut social classes. It is characterized by a continuous gradation from level to level (except, perhaps, in some of the old cities on the Atlantic seaboard) and by social mobility of the individual.

Drawing upon the data recorded for the Atlas of the Eastern United States, I should like to illustrate, in a small way, the processes of expansion and recession of several types of linguistic features from place to place and from class to class in areas where both the areal and the social dissemination of the variants is known.

My sketch maps are somewhat simplified but adhere to well documented facts. I have chosen for my illustrations two markedly expansive focal areas, Virginia and Eastern New England. Six examples will show the density of documentation in the Atlantic States and the location of the focal areas dealt with in my examples.¹

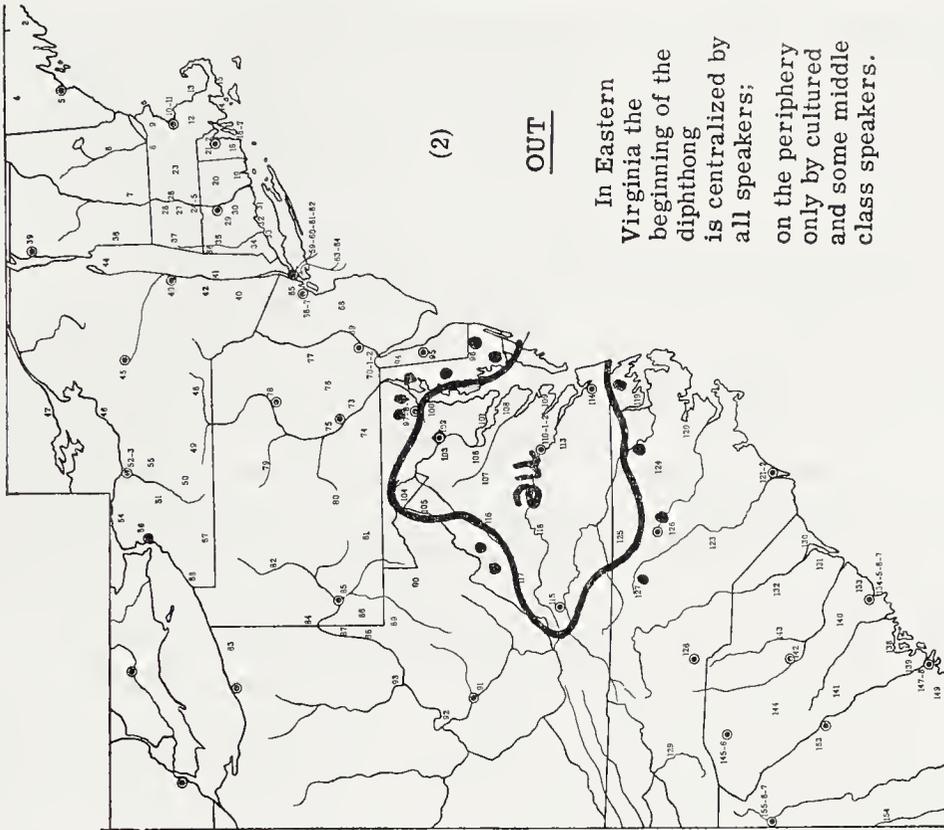
¹ The small numbers on the maps show the location of cultured speakers.



(1)

ROOM

Vowel as in book
on all social
levels in Eastern
Virginia;
on the periphery
only in cultivated
and middle class
speech.



(2)

OUT

In Eastern
Virginia the
beginning of the
diphthong
is centralized by
all speakers;
on the periphery
only by cultured
and some middle
class speakers.

(1) In Eastern Virginia and an adjoining part of Maryland, *room*, *broom* have the vowel of *pull* on all social levels. In adjoining areas the vowel of *pool* prevails in these words, but along the periphery of the Virginia area the vowel of *pull* has been adopted by some cultured and middle class speakers. Since all speakers in the Atlantic States have the vowel contrast illustrated in *pull* vs. *pool*, the adoption of the Virginia pronunciation of *room*, *broom* on the periphery does not introduce a new phoneme; it merely changes the distribution (incidence) of shared phonemes.

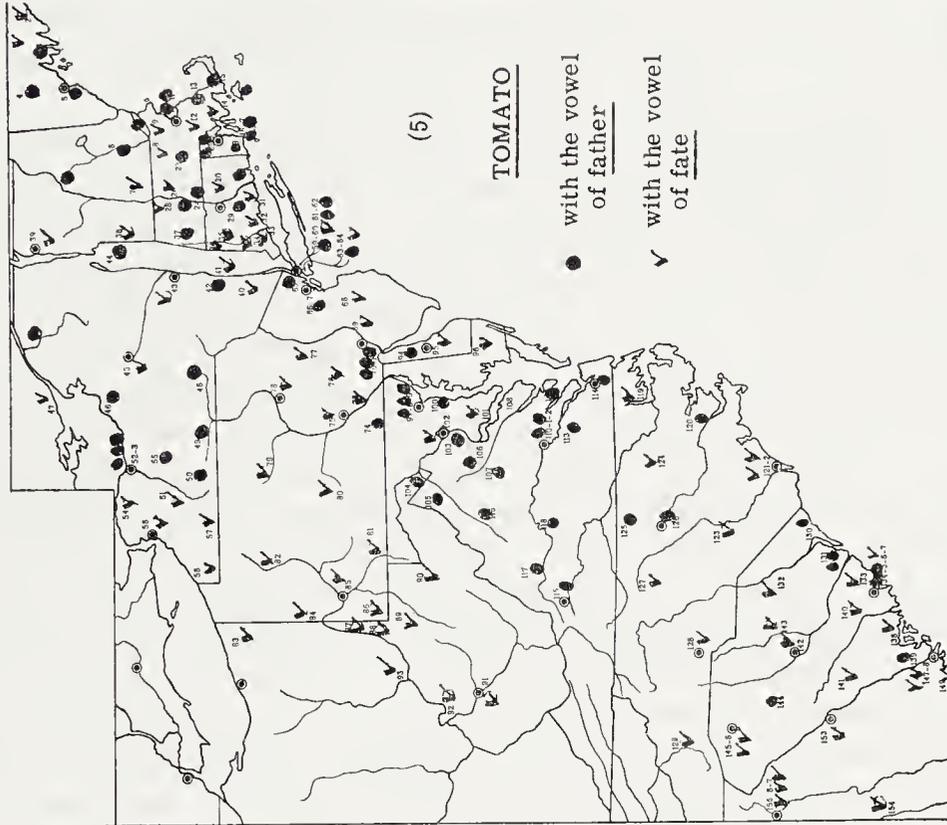
(2) Eastern Virginia has marked allophones of the diphthongal vowel in *house*, *out* on the one hand and in *down*, *cow* on the other. Before voiceless consonants, the diphthong starts approximately in the position for the vowel in *hut* and glides up swiftly; in all other positions it begins rather like the vowel in *hat* and glides up slowly. This feature is shared by all social groups within this area. On the periphery only some cultured speakers in Maryland and North Carolina exhibit these positional allophones, which they have clearly adopted from the Virginia speech area. To say *house* and *out* in the Virginian way carries social prestige. This innovation does not involve a phonemic change; it merely introduces an allophonic variation.

(3) In a large area of the Upper South – Eastern Virginia and adjoining parts of Maryland and north-central North Carolina — historical postvocalic /r/, as in *hear*, *care*, *car*, *door*, *forty*, is no longer pronounced as such on any social level. Along the periphery of this area, the /r/ generally survives in the speech of the folk and the middle class; but cultured speakers fairly regularly “drop” the /r/ in imitation of the prestige dialect of Eastern Virginia. The same process can be observed in the lower Hudson valley, which is dominated by “r-less” Metropolitan New York, and on the periphery of the “r-less” South Carolina-Georgia area.

Since the /r/ is thus replaced by an unsyllabic /ɻ/ in *hear*, *care*, *door*, and the sequence /ar/ in *hard*, *card* by a vowel differing from that in *hod*, *cod*, the adoption of these prestige features involves the addition of two new phonemes to the peripheral dialects. It is significant, however, that some of the innovators do not imitate the distinctive quality of the vowel in *hard*, *card*, but substitute for it a prolonged variant of the vowel they have in *hod*, *cod*. Such approximations or “compromises” are well enough known, when a feature of another dialect is adopted.

(4) We know from earlier observations that the replacement of one phoneme by another occurs step by step, and that the process may extend over generations of speakers before the replacement is completed. This complicated process is in need of thorough investigation. For best results, both the social and the regional dialects of an area must be sampled systematically and a rather large selection of pertinent items must be included in the survey, so that the changes can be traced from section to section, from social class to social class, and from word to word.

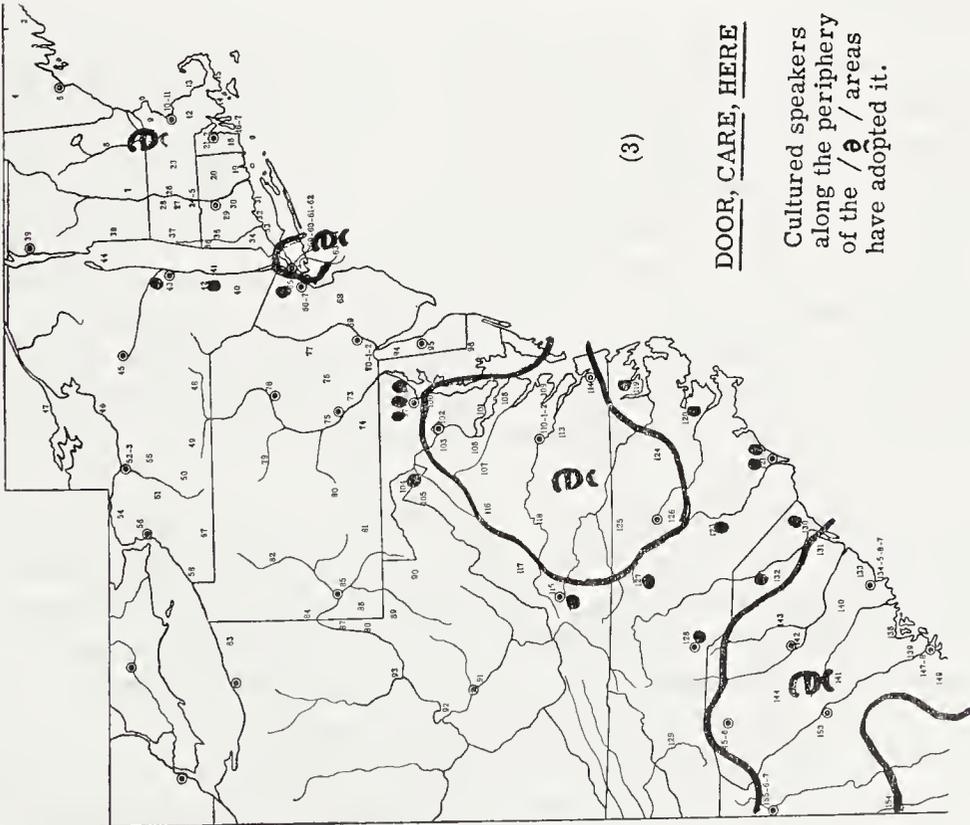
Walter S. Avis has investigated from this point of view the recession of the checked vowel /ə/ in such words as *whole*, *road*, which is in contrast with the free /o/ of *hole*, *rode*, etc., in the New England states. Having at his disposal a record of the incidence of /ə/ and /o/ in 24 words in the speech of 420 informants living in about 200 com-



(5)

TOMATO

- with the vowel of father
- ▼ with the vowel of fate



(3)

DOOR, CARE, HERE

Cultured speakers along the periphery of the /ə/ areas have adopted it.

munities — 150 belonging to the folk level, 220 to the middle group, and 50 to the cultured class — he can trace the replacement of checked /ə/ by free /o/ from area to area, from class to class, and from word to word.

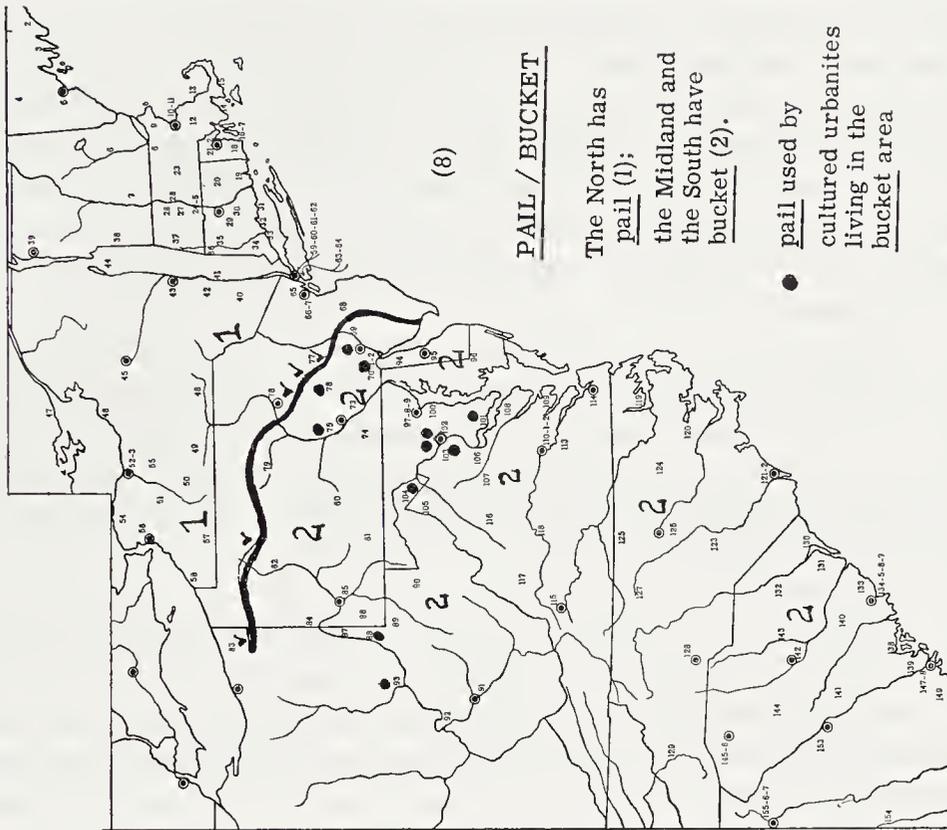
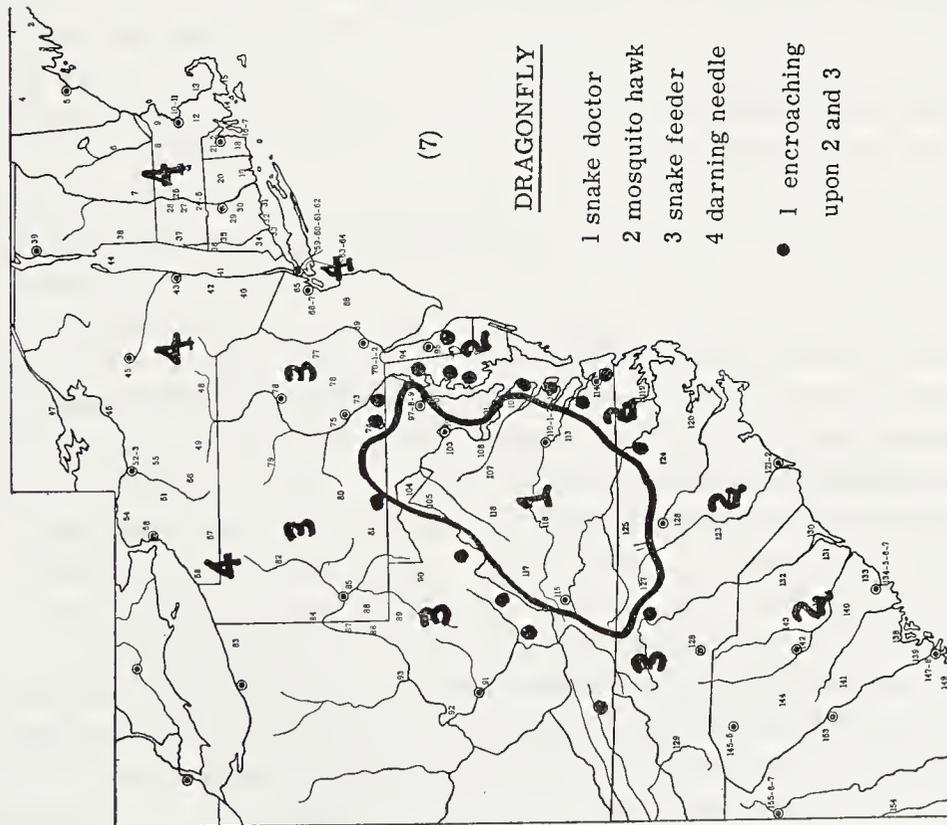
Avis finds (a) that the recession of /ə/ proceeds in a northeasterly direction, i.e., from the industrial and urbanized southern section to the rural north; (b) that in communities where usage is divided, the younger and/or better educated speakers have adopted the free /o/; and (c) that the rate of replacement varies from word to word: that, for instance, the free /o/ has been adopted in *home, stone, road, smoke* by many speakers who retain the checked /ə/ in *toadstool, stone wall, back road, smoke-stack*, words that are in daily use on the farm but not in school. Curiously enough, some speakers hang on to the checked /ə/ in *the whole thing* and *wholly* long after they have discarded it in other words. Such “strays” are well known to the dialectologist, both as relics and as spurious innovations. See *Language*, 37 (1961), 544-558.

(5) *Tomato* pronounced with the vowel of *father* is rather common in the cultivated speech of various sections of the Atlantic states, notably in New England (22/42), New York State (15/24), and Virginia (14/15). In New England and in Virginia north of the James River this pronunciation occurs also to some extent among the middle class, rarely elsewhere. About half of the cultured speakers interviewed rimed *tomato* with *potato*, as do the vast majority of the middle group. This dissemination of variant pronunciations is rather unique in America, indicating as it does a sharp class cleavage. It is apparently a recent fashionable acquisition of urbanites, perhaps in imitation of British English.

(6) The past tense form *et* (riming with *let*) of the verb *eat* is current to some extent in most sections of the Atlantic States, notably in New England and New York State, along Chesapeake Bay, and in South Carolina. In New England, *et* is now quite rare in cultivated speech and is clearly yielding ground to *ate* (riming with *late*) also among the middle class, except in Maine, parts of New Hampshire, and the islands off Cape Cod. In 55 out of 66 communities where both forms were current in 1930, the younger and better educated speaker used *ate*, the folk speaker *et*; and in about half of the 200 communities investigated only *ate*, with the vowel of *late*, was recorded. Here again we can observe the gradual dissemination of a linguistic feature from the top downward and from the urbanized southern section of New England in the direction of the rural northeast. It is of interest to observe that in England the trend has been in favor of *et*.

(7) From Pennsylvania southward, three words for the dragon fly are widely current: *mosquito hawk* along the Atlantic coast, *snake doctor* in the piedmont of Virginia, and *snake feeder* in the Appalachians. From the Virginia piedmont and adjoining parts of Maryland and North Carolina, *snake doctor* is spreading. As usual, it is mostly the better educated speakers that have adopted the prestige word *snake doctor* (in 17 out of 20 communities with divided usage).

(8) Two terms are current for a metal container used on the farm for drinking water or milk: *bucket* in the greater part of Pennsylvania and southward, *pail* in all of



the North. The boundary line between them is rather sharply defined. However, there is evidence from social dissemination that in Pennsylvania *bucket* is spreading northward in middle class speech. On the other hand, Northern *pail* has been adopted by cultured urbanites — and only by members of this group — not only in Philadelphia, but also in the cities along the Potomac and the Ohio River. Whereas the northward expansion of *bucket* is in all probability due to oral communication, the southward dissemination of *pail* in cultivated speech must clearly be attributed to the printed page or to schooling.

These examples illustrate the spreading of various features that do not affect the structure of the receiving dialect, as of checked /ʊ/ in *room*, the positional allophones of /au/ in *house*, *out*, the past tense *ate*, and the words *snake doctor* and *pail*. They also exemplify the spreading of features that involve a change in the phonemic system, as the introduction of unsyllabic /ɹ/ in *hear*, *care*, *four* and of free /ɑ/ in *car*, *garden*, and the loss of checked /θ/ in *road*, *whole*.

The data adduced show that this spreading takes place on social levels, cultured speakers being the first to adopt features of a neighboring prestige dialect. The middle class probably adopts these innovations from the cultured in their own communities, though direct contact with the middle group or the cultured in the adjoining area may also play a part. In the end such changes may find their way into the speech of the folk. It is readily granted that the latter events are in need of much fuller documentation than has been presented here.

Though structural innovations, such as the addition of phonemes to the native system, can be imported from a prestige dialect or from a foreign language, they are relatively rare. English is a striking example of this fact. Despite the wholesale adoption of words from French and Latin from the thirteenth century onward, Modern English has only two phonemes that are not native to it: the diphthong /oi/ of *joint*, *boil*, etc. (almost entirely replaced by the /ai/ of *pint*, *bile* in early Modern English, but then restored), and the medial consonant /ʒ/ of *measure*, *vision* (derived from the foreign sequence /zj/ < zi). The French rounded front vowels were doubtless used by bilingual speakers during the Middle Ages, but they were ultimately replaced by similar native sounds, as in *due* and *beef*.

The investigation of the adaptation of folk dialects to a national standard will shed light upon this important problem. I should like to present two cases to illustrate the behavior of dialects in this situation.

In his detailed study of the speechways of a mining community in northern England (Byer's Green in Durham), Harold Orton found a rather marked influence of the Received Standard upon the local dialect, which differs sharply from the Received Standard in its system of vowels. Thus the native sequence /jɑ/ of *bake*, *gate* is sometimes replaced by /ē/; the /jɑ/ of *bone*, *home* by /ō/; the /aʊ/ of *folk*, *soldier* by /ō/; the /jɛ/ of *moon*, *book* and the /u/ of *hound*, *sound* by /öu/. All of these substitutions are prompted by the Received Standard of England, but none of them are accurate imitations of the phonemes occurring in Received Standard *bake*, *bone*, *moon*, *hound*.

And, above all, none of these replacements introduce a new phoneme. Native /ē/ occurs in such words as *day*, *wait* (Middle English *ai*), native /ō/ in *coat*, *rose*, and native /ö/ is normal in *down*, *doubt* and in *blue*, *new*. Hence the integrity of the native system of vowels is fully preserved. There is no addition to the system, nor any loss. Only the number of instances of /ē, ō, ö/ is increased at the expense of /īa, au, īz, u/.

In conclusion I shall present a German example. A South Bavarian folk dialect (Carinthian) adopts many words from Standard German and from the colloquial speech of a neighboring city (Villach); but, to my knowledge, not a single foreign phoneme has been adopted. There is no trace of the Standard German phoneme /z/ of *sagen*, *lesen*, or of the /p/ of *packen* (both *packen* and *backen* begin with the weak voiceless stop /b/). The rounded front vowels /ū, ü, ö, ö/ and the diphthong /oi/ of Standard German *Mühle*, *Sünde*, *König*, *Köpfe*, *neu* are not adopted, the native /ī, i, ē, e, ai/ being retained in such words. The native "centering" diphthongs /īa, ea, ua/ of *lieb*, *schön*, *rot*, *gut* /līab, šēan, rə̄at, gūat/, unless retained, are replaced by /ī, ē, ō, ū/, which occur as native phonemes in such words as *wissen*, *besser*, *hoffen*, *Butter* /wīsn, bēsr, hōfn, būtr/.

One would like to know whether other regional folk dialects behave in the same way, rejecting foreign phonemes and making adjustments within the native systems of sounds, when the speakers make concessions to a socially "superior" dialect. The dissemination of features from a privileged dialect has, of course, been documented again and again; but the crucial question of whether the receiving dialect stays within its own system of sounds and forms, when it accepts elements from another dialect, or whether it alters its systems, has hardly been asked.

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Ann Arbor*

DISCUSSION

JAKOBSON:

Interlingual and interdialectal borrowing of phonemes, and even a more intense display of phonemic diffusion – borrowing of alien distinctive features – is a widespread phenomenon, which may be achieved through loan-word or even without any lexical influx. In the former case phonemes, originally confined to foreignisms, in the further development of language often prove to be adopted by the native phonemic code as well.

P. Ivić:

To examples already adduced of phonemes introduced with loanwords, we could add some others from South Slavic (e.g. /z/ and /ž/ as voiced counterparts of /c/ and /č/). In all these cases the phoneme adopted fills a gap in the phonemic pattern, thus the fact corresponds to Prof. Jakobson's definition that phonemes can be introduced

by borrowing, but not distinctive features. On the other hand, distinctive features can be abolished by interlingual contact (this is the case of word tone contrasts in many peripheral Serbocroatian dialects). Moreover, the phenomenon of *Sprachbünde*, based on the presence (or absence) of distinctive features, shows that even such features can be, under given circumstances, transferred from one language to another.

STRANG:

Professor Kurath's paper records what is in effect the adding of a dimension to dialect studies. He speaks of making 'a modest beginning' in the analysis of the social dimension by distinguishing the folk from the middle class and the cultural elite, and in introducing the paper he specified this dimension as one to be measured in terms of quantity of education. What is surprising to a speaker of British English is that any single parameter, perhaps especially that one, yields useful results. There are four essentials for a situation of social dialect influence such as concerns him. There must be a prestige dialect, a mediating dialect, a non-prestige dialect, and also an awareness by the speakers of the socially inferior dialect that their usage does lack prestige. Such a situation is familiar in British English, but it is very complex in structure, for there are many different prestige groups with distinct linguistic characteristics. Perhaps to some extent those with a high quantity of education form an elite; but what is much more important is education at one place rather than another – one kind of school or one particular school, a particular university or service training establishment. Outside these considerations, there are many aristocracies, of birth, of wealth, of youth, etc., each of which, in its typical forms of English, may provide a goal for imitation by those aware of lack of privilege. And this less privileged group too is far from homogeneous, not only in its actual usage, but in its choice of a prestige-group whose English shall be a target-language. So we find that the influence of social upon regional dialects cannot be assessed in terms of a single dimension, but in terms of a complicated network of interactions between an indefinite range of prestige groups and an indefinite range of linguistic self-improvers.

ON SYSTEMS OF GRAMMATICAL STRUCTURE

KENNETH L. PIKE

Extensive work through the Summer Institute of Linguistics is going on (often from a tagmemic point of view) in some 260 languages – largely those of preliterate tribes – of Peru, Mexico, New Guinea, the Philippines, and elsewhere.

Empirical data from these researches are being published as rapidly as possible.¹

In order to explain these data, we have been forced to develop a number of theoretical concepts and new field procedures. Tagmemics is a theory growing out of empirical work. No component of the theory is allowed to remain if it does not prove fruitful under field test. This paper is a brief report to show the direction of progress of some of these concepts, and to indicate where the results can be found.

1. WELL-DEFINED UNITS

Tagmemic theory has so far been largely addressed to the problem of determining the nature of a unit of relevant human behavior. Units of all kinds of purposive behavior are within the scope of the theory, though stimulus for it comes from and tests thus far have been largely applied to linguistic behavior.

The basic assumption: Any unit of purposive human behavior is *well-defined* if and only if one describes it in reference to (a) contrast (and resulting identification), (b) range of variation (with its essential physical manifestation), and (c) distribution (in class, in hierarchical sequence, and in systemic matrix.)

Contrast: One does not know what an item is until one knows what it is not. Note the long-standing attention paid to phonological opposition by the Prague school. Once items are thus separated off from others, the contrastive features in further environments sometimes allow for identification of items even under conditions where one of two members of a contrast does not occur.

Variation: Within tagmemic theory no unit is relevant to the behavioral system unless it has a physical component. (A mental entity would have a physical neurological

¹ For a recent listing, see *Twenty-Fifth Anniversary Bibliography* of the Summer Institute of Linguistics (Glendale, Now Box 1960, Santa Ana, Calif., 1960). Special volumes on the languages of Ecuador, of Peru, and of New Guinea will be appearing soon in the Linguistic Series of the Summer Institute of Linguistics of the University of Oklahoma, Benjamin Elson, Editor. Just off the press is *Studies in New Guinea Linguistics* (= *Oceania Linguistic Monographs*, No. 6) (Sydney, Australia, 1962).

component.) The manifestation – or realization – of the unit could vary substantially, leading to etic variants, or allounits.

Distribution: A well-defined unit is a member of a class of units *appropriate* to a particular *slot* in a construction. (A tagmeme is in grammar a unit comprising a slot with its appropriate emic class. An emic class must also be well-defined.)

A well-defined unit must be seen as a member of a hierarchy of units. An utterance therefore is multiply segmentable – segmentable successively into units of various levels of a hierarchy, with smaller units entering larger units of the hierarchy. (As morpheme sequences are segmentable into larger, word units, phrase units, clause units, discourse units, etc.)

The segmentation can be determined by boundaries of units, with nuclei indeterminate, or by nuclei of units with boundaries indeterminate² (as when /a/ and /i/ smear in /ai/). (A sharp-cut unit with both the nucleus and boundaries determinate should be treated as a special case of segmentation.) Such a view removes a number of the problems inherent in previous treatments of segmentation.

A unit must be seen as placed within a system. European scholars have long been interested in such matters, especially as related to phonology. Tagmemics³ labels as a matrix an array of units as an intersection of contrastive features, in some such fashion as an articulatory chart of phonemes.

2. MULTI-HIERARCHICAL STRUCTURE

Implied in the previous section, but now made explicit, is the assumption that language is multi-hierarchical. A mono-hierarchical view, with progression from phoneme to morpheme to word to clause to sentence to discourse – or the reverse – is too thin. A richer theory is needed to accommodate the empirical data.

(a) In a lexical hierarchy, of specific lexemes, tagmemic theory treats morpheme at the lowest level, with specific units from word to phrase – or idiom – to clause to sentence to discourse (or sonnet, etc.) at the top.

(b) In the phonological hierarchy phones are the minimum etic segments – phonemes the minimum emic segments – with various larger etic (and emic) units of ascending⁴ size.

(c) In the grammatical hierarchy there is an interlocking of tagmemes with con-

² For a discussion of indeterminacy of segmentation of stress groups, see my “Practical Phonetics of Rhythm Waves”, *Phonetica*, 8 (1962), 9-30.

³ If one equates contrast-identification with feature mode, variation-manifestation with manifestation mode, and distribution with distribution mode, one can then see that the thrust toward units, well-defined, represents the heart of my *Language in Relation to a Unified Theory of the Structure of Human Behavior* (Glendale, Now Box 1960, Santa Ana, Calif. Part I, 1954, Part II, 1955, Part II, 1960); an edition of the work will be appearing with Mouton and Co.

For well-defined constructions see my “Dimensions of Grammatical Constructions”, *Language*, 38 (1962), 221-244. Here, also, is found the basic treatment of grammatical matrix.

⁴ For my attempt at an exhaustive etic treatment of medium layers see “Practical Phonetics of

TABLE I
Kernel Matrix of Kanite Clauses

	Neutral Transitive	Indirect Transitive	Direct Transitive	Intransitive	Equative
Independent Indicative	1	3	5	7	9
Dependent Indicative	2	4	6	8	10

Numbers in the cells refer to clause types, and will be illustrated below.

structions. Tagmeme sequences make up constructions, which in turn enter slots of larger constructions,⁵ etc.

Boundaries of units of one of the three hierarchies at times reinforce and at other times clash with borders of the other hierarchies. Without this fact of clash only one hierarchy could be maintained.

3. KERNEL OR DERIVED MATRIX (WITH ANTI-REDUNDANT PRESENTATION OF CONSTRUCTION RELATIONSHIPS)

Each of the tagmeme and construction units must be well-defined. It is not sufficient, from the viewpoint of tagmemic theory, to list in miscellaneous or arbitrary fashion various constructions in relation to a general typology – i.e., etic classification. Rather one must attempt to see constructions in contrast with each other – and answer concerning them the question: “What is a minimal pair?” Constructions, like phemes, must be subject to analysis as well-defined units. Constructions contrast, in this view, only when there is a dual⁶ structural difference between them. The etics and emics of grammatical constructions thus come to the fore. (An active versus passive clause shows contrast, for example, both by having a different subject tagmeme – subject as actor versus subject as goal – and by a different predicate tagmeme, as well as by absence versus presence of a special agent tagmeme.)

Once constructions are well defined, their symbols can be placed in the cells of a contrastive matrix. Note, in Table I, a very simple array of some Kanite⁷ (New Guinea, Eastern Highlands) clauses.

Rhythm Waves”. For a brief emic sample, “Abdominal Pulse Types in Some Peruvian Indian Languages”, *Language*, 33 (1957), 30-35. See, also, an article on Marinahua Phonology by Eunice Pike and Eugene Scott, *Phonetica*, 8 (1962), 1-8. For other layers, see my *Language... Behavior*.

⁵ See *Language... Behavior*, § 12.

⁶ See Robert E. Longacre, “String Constituent Analysis”, *Language*, 36 (1960), 63-88, for the initial theory here; for more formal presentation, see *Dimensions*.

⁷ Kanite data here and below are from Joy McCarthy, Summer Institute of Linguistics. A somewhat different presentation – and more complete – will be seen in her *Kanite Clause Chains* (in preparation).

Two advantages come from such a display. (a) The relationship between these clauses is implicitly defined and shown as the relationship between intersecting classes of clauses (which are grouped according to the implied presence and absence of certain contrastive components). (b) The redundancy of presentation is sharply cut, as over against a presentation which treats clauses one at a time. The intersecting classes of clauses are posited by the rows and columns of the matrix, and can be referred to by groups.

A further anti-redundancy advantage comes through matrix presentation when a simple chart like this one is treated as the nucleus – or kernel – of the system. From it a derived matrix can be obtained by “multiplying” it by a further feature which affects each cell. Thus the formulas:

$$\begin{array}{l} M \cdot \text{Inter} = M_{\text{Inter}} \\ M \cdot \text{Imp} = M_{\text{Imp}} \end{array}$$

state that by suitable tagmemic modifications the constructions in the cells of the nuclear matrix can be transformed into interrogative and imperative clauses, etc. (Only the *fact* of their occurrence is shown by this display – the structure of the derived constructions is given in tagmemic formula elsewhere in the description of the system.)

Such matrices have proved to be of substantial methodological importance, for two reasons: (a) They call attention to *gaps* in a system. If a matrix implies a cell, but no illustration for it is present, one may be stimulated to look for the implied data which the language contains but which have escaped the analyst. Note that I happen to have no data to illustrate (8), implied by the system. Presumably a check with the informant would supply it.

In addition (b), the array can be used as a *search matrix*, for analysis. In many of the languages of the New Guinea highlands, a dependent clause has various ties (of subject, identity, or of time) with a following independent clause. If, on the basis of known language characteristics of the region, the possible concord components of the first clause are listed at the left of a matrix, and of the second clause along the top, bilingual elicitation often can locate many forms rapidly – to be checked later against text material. In reverse fashion, if in a particular language numerous morphs have been segmented out of the two clause positions but with meanings unknown, the two morph lists can be placed on left and top of a search matrix, with systematic trial with a bilingual to seek for their semantic interrelations.

4. SYNTAX PARADIGM (UTILIZING CONTROLLED REDUNDANCY)

In the preceding section a matrix approach was used to cut redundancy in presentation. Now we take the opposite approach, utilizing a matrix approach in a manner which allows us to exploit redundancy of presentational material for rapid reading

of unfamiliar material – material where the reader may be uninterested in learning to speak or read the particular language as such, but wishes merely to grasp its structure as quickly and easily as possible.

The data are presented in a *syntax paradigm*⁸ in *citation form*. Just as in the presentation of a morphological paradigm the choice of stem is kept constant,⁹ with only inflectional changes, so in a syntax paradigm the same illustrative words and morphemes are used whenever possible. This leads to *controlled redundancy* as an aid to understanding – since an entire grammar can be read intelligibly on the basis of a few dozen (?) forms. Every new word or morpheme introduced into the citation paradigm warns the reader that the contrastive structure has somehow changed.

Accompanying the citation paradigm is a *tagmemic-notation paradigm*, in which each construction type is given with cut-down tagmemic formulas – not in citation form. These formulas are enclosed in braces {} to symbolize their emic status – as are morphemes as against morphs.

A *tagmatic-notation paradigm* – in square brackets to show its etic status – then gives a breakdown of the constructions (a) to show, further, both slot function and class of filler of each slot (without which no tagmemic analysis can be checked for accuracy) and (b) to show variants of tagmemic order, or optional occurrence of included tagmemes. Variants of emic constructions can be shown by a tilde: {[] ~ []}.

In Table II we now list as a citation paradigm illustrations of the six types of clauses implied by the matrix of Table I. For each dependent clause type two illustrations are given (marked as 2a, 2b, etc.). The first of the pair^f has its subject the same as that of the following independent clause (not cited, but suggested by the three dots following the dependent clause). The second of the pair has a subject differing from that of the implied following clause. Ditto marks are used to allow faster recognition of some of the repeated forms.

TABLE II
Citation Paradigm of Kanite Clauses

(1)	naaki eka vie-mo-ʔa	—————	maya no-te-ka	--ne-kah-i-e
(2a)	” ” ”	—————	” ”	--” -te-no . . .
(2b)	” ” ”	—————	” ”	--” -t-e-ke-no . . .
(3)	” ” ”	neʔ-mo-na	” ”	a-mi-kah-i-e
(4a)	” ” ”	”	” ”	”-” -te-no . . .
(4b)	” ” ”	”	” ”	”-” ^ʔ -t-e-ke-no . . .

⁸ For detailed presentation, see “A Syntax Paradigm”, *Language*, 39 (1963), 216-230; with illustration via Bilaan, of the Philippines.

⁹ A morphological paradigm is therefore artificial. So is a syntax paradigm. Its elicitation is subject to all the dangers of morphological elicitation. Its results must, as in morphology, be checked against uncontrolled text. And, as in morphology, its value as a pedagogical or description device is sometimes substantial. Supplementary illustrations from text give support to and test the correctness and completeness of the analysis.

(5)	”	”	”	”	”	”	„-ke-kah-i-e
(6)	”	”	”	”	”	”	? ? ? . . .
(7)	”	”	”	_____	_____	”	--u-kah-i-e
(8a)	”	”	”	_____	_____	”	--u-te-no . . .
(8b)	”	”	”	_____	_____	”	--u-t-e-ke-no . . .
(9)	”	”	”	_____	kanare? vie	”	--mai-kah-i-e
(10a)	”	”	”	_____	”	”	--mai-te-no . . .
(10b)	”	”	”	_____	”	”	--mai-t-e-ke-no . . .

Lexical Items:

naaki	<i>so</i>			a-	<i>it</i>
eka	<i>tomorrow</i>			-mi-	<i>give</i>
vie-	<i>man</i>	ne-	<i>eat</i>	-ke-	<i>see</i>
-mo-	<i>equational</i>	-kah	<i>will</i>	u-	<i>go</i>
-ʔa	<i>he</i>	-i-	<i>he</i>	kanare?	<i>good</i>
		-e	<i>indicative</i>	mai-	<i>to be</i>
<hr/>					
neʔ-	<i>boy</i>				
-na	<i>oblique</i>	-te- ~ -t-	<i>first</i>		
maya	<i>sweet potato</i>	-no	<i>he₁₋₂</i> (when in structure 2a, etc.)		
no-	<i>house</i>		<i>he₂</i> (when in structure 2b, etc.)		
-te-	<i>at</i>	-e-	<i>he₁</i>		
-ka	<i>locative</i>	-ke-	<i>transitional</i>		
<hr/>					

In Table III we give free translations of the citation components of Table II.

TABLE III
Translation of Citation Paradigm of Clause

- (1) So tomorrow the man will eat sweet potato at the house.
- (2a) So tomorrow the man will first eat sweet potato at the house.
(Implying: and then he – the man – will . . .)
- (2b) So tomorrow the man will first eat sweet potato at the house.
(Implying: and then he – someone else – will . . .)
- (3) So tomorrow the man will give sweet potato to the boy at the house.
- (4a) So tomorrow the man will first give sweet potato to the boy at the house.
(Implying: and then he – the man – will . . .)
- (4b) So tomorrow the man will first give sweet potato to the boy at the house . . .
(Implying: and then he – someone else – will . . .)
- (5) So tomorrow the man will see the sweet potato at the house.
- (6) ?

- (7) So tomorrow the man will go to the house.
 (8a) So tomorrow the man will first go to the house . . .
 (Implying: And then he – the man – will . . .)
 (8b) So tomorrow the man will first go to the house . . .
 (Implying: And then he – someone else – will . . .)
 (9) So tomorrow the man at the house will be a good man.
 (10a) So tomorrow the man at the house will first be a good man . . .
 (Implying: And then he – the man – will . . .)
 (10b) So tomorrow the man at the house will first be a good man . . .
 (Implying: And then he – someone else – will . . .)

We now give, in Table IV, the same paradigm in tagmemic notation. (We might have omitted here those optional tagmemes which serve no contrastive function between the clauses – introducer, time, independent subject, and location. They would then have appeared as expansions in the tagmatic paradigm.)

Each predicate type differs from the others. For sample breakdown formula see Table V.

TABLE IV
Syntax Paradigm in Contrastive Tagmemic Form

(1)	{	±Intro	±Time	±Subj	—	±Obj	±Loc	+Pred _{NeuTrIndep} }	'eat'
(2)	{	"	"	"	—	"	"	+Pred _{NeuTrDep . . .} }	'eat'
(3)	{	"	"	"	±IndirObj	"	"	+Pred _{IndirTrInDep} }	'give'
(4)	{	"	"	"	±IndirObj	"	"	+Pred _{IndirTrDep . . .} }	'give'
(5)	{	"	"	"	—	"	"	+Pred _{DirTrIndep} }	'see'
(6)	{	"	"	"	—	"	"	?	'see'
(7)	{	"	"	"	—	—	"	+Pred _{IntrIndep} }	'go'
(8)	{	"	"	"	—	—	"	+Pred _{IntrDep . . .} }	'go'
(9)	{	"	"	"	—	+Compl	"	+Pred _{EquaIndep} }	'be'
(10)	{	"	"	"	—	+Compl	"	+Pred _{EquaDep . . .} }	'be'

The plus and plus-minus signs indicate obligatory versus optional occurrence. Subscripts indicate predicates with contrastive internal structure.

We could continue now¹⁰ to give formulas – in etic brackets – amplified to symbolize the fillers of the tagmemic slots. Such formulas would thus show that specially marked noun expressions fill the time slot; that specially marked noun, pronoun, and

¹⁰ But shall not; see, rather, reference to Paradigm in fn. 8.

verb expressions fill the location slot; and would show separate contrastive verb structures for each of the different predicate tagmemes.

In addition, the tagmatic notation would show the subject concord seen in the citation paradigm of Table II, above, where (2a) and (2b) would be related as $\{2\} = [2a] \sim [2b]$, etc.

The neutral-transitive clause (1) contains (a) a class of neutral-transitive verb stems ("eat"), (b) optional independent object on a clause level but (c) obligatory absence of dependent object prefix in the verb, and (d) obligatory absence of an indirect object on a clause level, whereas the indirect transitive (3) contains (a) a class of indirect-transitive verb stems ("give"), (b) optional independent object on a clause level but (c) obligatory indirect-object prefixal tagmeme in the verb, (d) and optional indirect object on a clause level. The direct-transitive clause (5) contains (a) a class of direct-transitive verb stems ("see"), (b) optional independent object on a clause level, but (c) obligatory object prefixal tagmeme in the verb, and (d) obligatory absence of indirect object on a clause level. The prefixal indirect- and direct-object tagmemes in the verbs are homophonous; the same set of pronouns fills the functionally contrastive slots of the respective tagmemes. I have chosen here to treat these various structural differences as sufficient to contrast, emically, $\{1\}$ with $\{3\}$ and $\{5\}$ and $\{7\}$. If, however, an analyst were to feel that these differences are minor, it is conceivable that he might choose to treat the first three as subtypes of transitive. If so, his formulas would require that

$$*\{\text{Transitive Clause}\} = [\text{NeuTr}] \sim [\text{DirTr}] \sim [\text{IndirTr}],$$

and separate tagmatic formulas would then be required for the direct versus indirect alloconstructions.

Before one can see in formula the full structure, however, a breakdown formula must be given for the fillers of each of the predicate tagmemic slots. As a sample, note Table V. By this symbolization fuller explanation is provided of the verbal morpheme occurrences illustrated in Table II. In the verbs of independent clauses, the subject suffix tagmeme Sub_1 has a different morpheme class (e.g., *-i* "he") from that of the suffix tagmemes of the dependent verbs. The dependent verb has, obligatorily, a suffixal subject tagmeme in concord (see subscript in Sub_c) with the following independent clause (implied in Tables II, III, and IV by the three dots). When the subject of the dependent clause is the same as that of the independent clause, the concord subject in the dependent clause implies both the person of the dependent and the person of the independent clause (cf. *-no* as "he₁₋₂" in the legend of Table II). When, however, the subject of the dependent clause differs from that of the independent clause following it, the concord pronoun of the dependent verb is retained unchanged, but a further dependent subject tagmeme Sub_d is added (e.g., *-e-* "he₁"). Between the two a morph *-ke-* is placed called transitional (see Transit, in Table V) by McCarthy.

TABLE V
Internal Tagmemic Structure of Verbs

(1)	±Aspect	±Neg	—	+NeuTrVbStem		+Subj _i	+Mood	+	'eat'
(2)	"	"	—	+	±TimeRel	±(Subj _a	+Transit)	+Subj _e	'eat'
(3)	"	"	+IObj	+IndirTrVbStem		+Subj _i	+Mood		'give'
(4)	"	"	+	"	±TimeRel	±(Subj _a	+Transit)	+Subj _e	'give'
(5)	"	"	+Obj	+DirTrVbStem		+Subj	+Mood		'see'
(6)	? ?								
(7)	"	"	—	+InTrVbStem		+Subj _i	+Mood		'go'
(8)	"	"	—	"	±TimeRel	±(Subj _a	+Transit)	+Subj _e	'go'
(9)	"	"	—	+EquaVbStem		+Subj _i	+Mood		'be'
(10)	"	"	—	"	±TimeRel	±(Subj _a	+Transit)	+Subj _e	'be'

As implied earlier, the dependent object and dependent indirect object tagmemes in the formulas (5) and (3) of Table V are manifested homophonously. The tagmemic difference between them – their functional difference – is determined by their relationship to the embedding clauses.

Some further tagmemes which the verbs may optionally contain – aspect and negation – are not illustrated in the data here but are symbolized by the formulas. Time Relation tagmeme – also not illustrated – may show time relations between clauses.

5. FIELD THEORY

The handling of language systems in matrix terms encourages us to believe that a field theory of language is well worth developing, with matrix as one component. Analysis of language behavior yields segmental units. Analysis of the system of units yields field structures – matrix units, matrix types, matrix hierarchies. Implications of this view are beginning to emerge.¹¹

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DISCUSSION

WEINREICH:

It is hard to judge so many theoretical suggestions for one who is unfamiliar with the languages from which the examples are drawn. Insofar as I have understood the speaker, I cannot see the difference between a 'matrix' and an old-fashioned paradigm.

¹¹ See my *Language . . . Behavior*, § 12.1; "Language as Particle, Wave, and Field", in *The Texas Quarterly*, 2, 2 (1959), 37-54; "A Note on System as Field", unpublished; "Matrix Rotation and Matrix as an Emic Unit", a paper to be presented to the December meeting of the Linguistic Society of America, 1962.

I also fail to see the justification for so much redundancy between the description of, say, verb words, verb phrases, and clause types, unless it be some apriori assumption for the need to distinguish these levels absolutely. In the light of recent studies on the importance of recursive rules in grammar, Mr. Pike's insistence on autonomous descriptions of the word "level", the phrase "level", the clause "level", etc., strikes me as a step backward, particularly disappointing when proposed by someone who in the 1940s led the struggle against a scholastic rigidity in the separation of "levels". Finally, I am surprised that all this novel theory has been illustrated by examples from such exotic and little known languages. Are we to understand that these languages of South America and Melanesia deviate so much from more familiar types that a new linguistics is needed to describe them? If not, why not show us how the novel theories are superior to better known ones in the description of a language where the facts are known and beyond dispute?

CONCERNING THE LOGICAL BASIS OF LINGUISTIC THEORY

S. K. ŠAUMJAN

The logical basis for linguistic theory should be, we believe, the two-level principle which is the backbone of the modern logic of science.

According to this principle two levels of abstraction must be strictly distinguished in any theoretical science: the observation level and the level of constructs. The observation level deals with immediately observable objects, qualities and relations, which are usually referred to as elementary. Constructs are such qualities and relations as are inaccessible to immediate observations. Constructs are related to the observation level by means of the "correspondence rules".

Concerning the term "observation level" we must make the following remark. The observation level must be understood in a logical (not psychological) sense, that is as a sum total of initial facts subject to a theoretical treatment. The question of what can be and what cannot be observed by us may be considered a debatable one, if it is approached psychologically. However, from the point of view of logic the concept "observation level" is not ambiguous at all. We mean those facts which are considered observable within a given science and which, by virtue of this definition, form its empirical base. Therefore alongside of the term "observation level" we may use such terms as "protocol base of science" or "empirical base of science".

The two-level principle in the logic of science corresponds to the black box theory in cybernetics. According to this theory the object of cognition may be regarded as a kind of black box, whose contents are inaccessible to direct observation. The investigator can form a hypothetic idea of its contents only by subjecting it to various external influences (input state), and observing its reactions to those influences (output state).

We believe that consistent application of the principle of two-level abstraction in structural linguistics will divest it of a number of fundamental difficulties which confront it at present. However, it must be emphasised that this does not mean imposing a priori the scheme of the logic of science on structural linguistics. On the contrary, the need to overcome the profound antinomies contained in structural linguistics itself inevitably leads to the idea of two abstraction levels. The logic of science merely helps us to realise this necessity.

In our book *Problems of Theoretical Phonology* we have already put forth the two

level system of concepts as applied to phonology. We now deem it possible to apply the two-level system of concepts to the whole of structural linguistics.

The most general features of the two-level system of concepts in structural linguistics are as follows.

In phonology we split the concept of distinctive feature and phoneme into two pairs of correlated concepts: that of differentor and differentoid and that of phoneme and phonemoid.

The starting point for the formation of the two-level system of concepts in phonology is the necessity to overcome the fundamental antinomies that arise when the existing phonological theories are applied to the analysis of the phonological system of the language. Let us consider one of these antinomies, the antinomy of transposition. The essence of this antinomy is in the following.

Some of the basic theses of modern phonology are:

- 1) Phonemes are elements serving to differentiate linguistic units;
- 2) Phonemes are acoustic elements.

If both these theses are accepted, then it follows from the first that phonemes can be transposed from the acoustic substance into other forms of physical substance (graphic, chromatic, tactile, etc.), but the second implies the reverse, i.e. that phonemes cannot be transposed into other forms of substance.

To overcome this antinomy we must split the concept of phoneme into the concept of phoneme proper and the concept of phonemoid; the latter is a bit of physical substance serving as the substratum of the phoneme. By similar argumentation we split the concept "distinctive feature" into the concepts "differentor" and "differentoid". Phonemes and differentors pertain to the level of constructs and phonemoids and differentoids to the level of observation.

Differentors and phonemes are abstract diacritical elements realizable in various physical substances: acoustic, graphic, chromatic, tactile etc. Differentoids and phonemoids are acoustic elements embodying definite differentoids and phonemes. We suggest that the following categories should be strictly distinguished and never confused: 1) acoustic properties and bundles of acoustic properties (sounds) as purely physical notions, 2) differentoids and bundles of differentoids (phonemoids) as relational physical notions, 3) differentors and bundles of differentors (phonemes) as purely relational notions.

In grammar we split its basic concepts – those of morpheme and syntagma into the concepts of morpheme and morphemoid, syntagma and syntagmoid. Morphemoids and syntagmoids are subject to substitution which is an operational test at the observation level. As far as morphemes and syntagmas are concerned they are regarded as pure constructs, postulated to account for the functioning of morphemoids and syntagmoids.

The necessity of splitting the concept "morpheme" into the concepts of "morpheme proper" and "morphemoid" and the concept "syntagma" into the concepts of

“syntagma proper” and “syntagmoid” is dictated by the fundamental difficulties that arise when these concepts are applied in studying the grammatical system of the language. Let us dwell on some of these difficulties.

One of the fundamental facts of all languages is the linear ordering of linguistic units. This was long ago pointed out by F. de Saussure: “The signifiant evolves only in time and is characterized by the features borrowed from time: a) it is an extension; b) this extension lies in one dimension: it is a line. This quite obvious principle is often not mentioned at all, probably because it is considered too simple; nevertheless this principle is basic and its consequences are uncountable. The entire language mechanism depends on it” (F. de Saussure, *Cours de Linguistique Générale*, part 1, ch. 1, § 3). Linearity, being a fundamental quality of the language and conditioned by its physical nature, comes into contradiction with the underlying syntagmatic relations of linguistic units. This contradiction is most clearly manifested in the existence of the so-called discontinuous morphemes.

Let us take for example an English sentence: *He put it up*. The linear features of this sentence contradict the actual connection between *put* and *up* as the elements of a single whole. This is also the case with constructions having auxiliaries. E.g. in the phrase *I have taken* its linear features contradict the actual connection between *have* and *-en*, which though separated by the element *take* are the elements of a single whole.

In what way is it possible to overcome the contradiction between the linear features of grammatical constructions and the actual relations between their elements? We deem it necessary to split the concept morpheme into the concept of morpheme as a construct and the concept of morphemoid as an element which serves as the substratum of a morpheme. Thus, in the above examples the single morpheme /PUTUP/ is embodied in two morphemoids *put* and *up*, the single morpheme /HAVEEN/ is embodied in two morphemoids *have* and *-en*. Splitting the concept of morpheme into the concept of morpheme proper and the concept of morphemoid involves splitting the concept of syntagma into the concept of syntagma proper and the concept of syntagmoid. So, in the first examples the syntagma /PUTUP IT/ is embodied in the syntagmoid *put it up*, and in the second example the syntagm /HAVEEN TAKE/ is embodied in the syntagmoid *have taken*.

In connection with splitting the concepts of morpheme and syntagma into the concepts of morpheme proper and morphemoid, the concepts of syntagma proper and syntagmoid, a necessity arises to split the concept “distribution” into the concepts of metrical distribution and of linear distribution. By linear distribution we mean the distribution of morphemoids and syntagmoids, whereas by metrical distribution we mean the actual ties between morphemes and syntagmas irrespective of any spatial considerations.

The concept “morphemoid” must not be confused with the concept “morph”. In linguistic literature the shortest grammatical elements are usually called morphs. If phonemically different morphs are in relation of complementary distribution they

are joined into classes, called morphemes, and as morpheme variants are called allomorphs. Thus morphemes and morphs are related to each other as invariants to their variants. But in our treatment morphemes and morphemoids are not related as invariants and variants. We regard morpheme and morphemoid as heterogeneous elements belonging to different abstraction levels and therefore the problem of variants and invariants must be set for each level of abstraction separately. Let us consider an example.

We shall take three Russian phrases: *уютная комната* (a cosy room); *комната уютна* (The room is cosy), and *сделать комнату уютной* (make the room cosy). In these phrases we see three concrete adjectival morphemes – an attributive morpheme in the word *уютная* which will be designated by the symbol A_1 , a predicative morpheme *уютна* which will be designated by the symbol A_2 and a post-complement morpheme *уютной* which will be denoted by the symbol A_3 . Morphemes A_1 A_2 A_3 can be joined into a class of adjectival morphemes. It is possible to prove that the principal parameter of adjectival morphemes is the attributive parameter, therefore A_1 must be considered the invariant morpheme, and A_2 and A_3 must be considered variants of morpheme A_1 .

Morpheme A_1 is embodied in the morphemoid *-ая*, morpheme A_2 in the morphemoid *-а*, and morpheme A_3 in the morphemoid *-ой*. The morphemoids *-ая*, *-а*, *-ой* are joined into a class of adjectival morphemoids. Since the morphemoids *-ая* and *-ой* may be treated as successive transformations of the morphemoid *-а*, the latter must be considered the invariant morphemoid, and the first its variants.

Thus we see that the problem of invariants and variants must be solved separately for morphemes and morphemoids. Moreover, between morpheme-invariants and morpheme-variants on the one hand, and morphemoid-invariants and morphemoid-variants on the other there is no one-to-one correspondence: so in our example the morphemoid invariant *-а* is the substratum of the morpheme variant A_2 , while on the contrary, the morpheme-variant *-ая* serves as the substratum of the morpheme-invariant A_1 .

Analogically, the problem of invariants and variants must be considered separately for phonemes and phonemoids. Let us take an example from Ancient Greek. In Greek it was possible to distinguish between two concrete phonemes “ S_1 ” (before consonants – in groups like *spr*, *sp*, *sm*) and “ S_2 ” (before vowels after occlusives in groups *ps*, *ks*). The phoneme “ S_1 ” was embodied in the phonemoid $S^>$, and the phoneme S_2 in the phonemoid $S^<$ (the diacritical symbols $>$ and $<$ serve as symbols of implosive and explosive pronunciations). If on account of functional criteria “ S_1 ” must be considered phoneme-invariant at the phonemic level and “ S_2 ” a variant of phoneme “ S_1 ”, then at the level of phonemoids not the phonemoid $S^>$ must be considered the invariant, but the phonemoid $S^<$, because from the physical point of view more independent is the position of explosive pronunciation. Thus we see that in phonology there is no one-to-one correspondence between phoneme-invariants and phoneme-variants on the one hand, and phonemoid-invariants and phonemoid-

variants on the other. Indeed, the phonemoid-invariant $S^<$ serves as the substratum of the phoneme-variant " S_2 ", whereas the phonemoid-variant $S^>$ serves as the substratum of the phoneme-variant " S_1 ".

In connection with distinguishing between the two levels of abstraction we find it useful to introduce into structural linguistics two general concepts: that of linguistic genotype and that of linguistic phenotype. The first refers to the level of constructs, and the second to the observation level. Genotypes are differentors, phonemes, morphemes, syntagmas. Phenotypes are differentoids, phonemoids, morphemoids, syntagmoids, etc. We recommend that the level of constructs be named the genotype level, and the observation level the phenotype level.

Correlation of the invariants and variants of the genotype and phenotype levels may be presented in table I.

TABLE I

Levels of abstraction	Aspects of language			
	Phonology		Grammar	
Genotype level	phoneme-invariant	phoneme-variants	morpheme-invariant	morpheme-variants
Phenotype level	phonemoid-invariant	phonemoid-variants	morphemoid-invariant	morphemoids-variants

Such is the two-level system of linguistic concepts, as presented most generally.

Differentiation between the genotype and the phenotype levels of abstraction throws new light on the theory of linguistic models. A systematic exposition of the consequences of this differentiation will be postponed until another time. Here we shall merely mention one of the aspects of the problem of modelling which seems particularly essential to us. We mean an appraisal of the IC model from the point of view of distinguishing between the two levels of abstraction.

In his outstanding works N. Chomsky has given a brilliant criticism of the IC model, convincingly showing its merits and demerits. As Chomsky has proved, the IC model must be supplemented by a transformation model. One of the main shortcomings of the IC model, as has been pointed out by Chomsky, is its forbidding the permutation of grammatical elements. Now, because of this prohibition the IC model cannot be applied to strings with discontinuous morphemes (which are central in the systems of many languages, like, for example, English whose system of conjugation is rich in auxiliaries that are very often discontinuous morphemes); therefore as a means to overcome this defect Chomsky suggests the transformational model.

In view of the two-level principle one may ask to which level of abstraction the IC model refers.

It turns out that the IC model is bound to the phenotype level. In fact if the IC model prohibits permutation of elements it means that spatial considerations typical of the phenotype level are essential for it.

By supplementing the IC model with transformations of permutation we do not rise above the phenotype level. Therefore if we want to build a homogeneous model pertaining to the genotype level we must look for a new type of model different from the IC model. As a model for investigating linguistic genotypes we suggest a generative model capable establishing relations of domination between the elements of a sentence by means of a so-called operation of application. A description of the applicational model is given by us in another paper.

Here we shall point out that the essential feature of this model is a complete freedom from any spatial considerations. It takes into account only immanent relations between linguistic elements.

Since the applicational generative model is completely free of spatial considerations there is no necessity to introduce in this model any transformations of permutation or any other transformations dictated by spatial considerations. On the other hand transformations which make it possible to establish relations of invariance between various types of phrases and reveal their actual relations are quite essential for this model.

Such are some consequences from the two-vowel principle for the modelling of language.

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DISCUSSION

PILCH:

Is there any special reason why there should be just two rather than three or more levels of abstraction in linguistic theory? In particular, I am puzzled by Mr. Šaumjan's suggestion that linguistic entities such as phones (phonemoids) and morphs (morphemoids) are immediately observable objects. What we immediately observe is, I believe, speech as a physical or perceptual continuum. This continuum can be articulated into discrete segments such as phones or morphs only through the process of abstraction known as linguistic analysis. Phonemes, morphemes, and ICs are higher-level units set up through yet another process of abstraction. It therefore appears to me that both -emes and -emoids (phonemes, phonemoids, morphemes, morphemoids) are hypothetical constructs (belonging to two different levels of abstraction) and that neither belongs to the observation level.

I also disagree with Šaumjan's antinomy of transposition. The statement: "Phonemes are distinctive", is, in my opinion, not reflexive. It implies that "some distinctive elements are phonemes", but not that "all distinctive elements are phonemes". Therefore, if phonemes have acoustic substance, it does not follow that all distinctive elements must have acoustic substance.

ON THE FUNDAMENTALS OF SENTENCE STRUCTURE

PAAVO SIRO

Generative grammar drives at a general theory of language. This conceals a postulate that every individual empirical language, so-called natural language, is a particular variant of the same system. Accordingly, any individual language more or less reflects properties of this general theory. The main problem is the recognition of the significant features.

In generative analysis there has so far been no treatment of sentences general in form. What is for example the notion underlying Chomsky's "Aux"? Obviously it is a shortening of the English noun "Auxiliary". It is a makeshift taken from the traditional vocabulary of English. Thus the formal rules he presents¹ are of ad hoc-character, and therefore have little bearing on other languages.

We could say the same about the notion "complement". What is "complement"? I have been surprised to notice that, as far as I know, linguists have not agreed on common collective designation for linguistic elements such as prepositions, postposition, some oblique cases, and some adverbs in different languages, whose function in simple sentences is nearly the same. There seems to be a need for a stringent terminology emphasizing general functions rather than its manifestations in different languages. In my search of one common term, I have chosen to speak about "quasi predicate".

Let us first examine some phrase markers (Chomsky, 1962) for some simple sentences. As I must confine myself to the simplest verbal sentences, only to the verb as predicate and to its so-called arguments, we can simplify the notion the terminal strings (Chomsky, 1957, 30). Let N stand for a noun phrase, V for a verb phrase, and P for the quasi predicate, and we obtain the diagrams on the next page.

Under the line we have the simplified descriptions NV, NVN etc. The following list shows a complete set of these predicate types:

N V
N V P(N)
N V P(N) P(N) . . .

¹ Noam Chomsky, *Syntactic Structures*, pp. 39, 111. Hereafter cited as Chomsky 1957; Chomsky refers to his paper "The Logical Basis of Linguistic Theory", in the present volume, pp. 914 ff.

$$\begin{array}{l} N \ V \ N \\ N \ V \ N \ P(N) \\ N \ V \ N \ P(N) \ P(N) \dots \end{array}$$

I shall now examine this system. But I shall not do so with a transformational model (Chomsky, 1962, f.). Instead I will discuss it in a metalanguage. And I think we can do it quite exactly. The idea is this. We have two undefined notions, one axiom, and a set of exact rules, which resemble Prof. Chomsky's rewriting rules (Chomsky, 1962, f.). The object language comprises the sentences to be analyzed and also the simplified notions of these (NV etc.). In metalanguage we talk about object language. And we can do so in exact terms.

This model will be a very abstract one. The so-called essential qualities which are connected with some special conditions are here omitted, for example the morphemes distinguishing the transitive verbs from the intransitive ones. In spite of this the empirical representations are not difficult to recognize. Nor do we need to state here which words satisfy the rules. If there are some words of such kind, this system contains these sentences. Thus this is a system of possibilities.

My aim is to attempt a description of some very common, and probably universal, types of predicates in terms sufficiently general to characterize language, not individual languages.

I will call this system a simple sentence model. In metalanguage, let F mean the verb V and n the noun N . The simplest sentence is then

$$F(n)$$

which covers, for example, *Dry Wood burns*. We will accept this as our only AXIOM. It means: There is in this language a sentence NV.

The corresponding transitive sentence type, which covers, for example, *We burn dry wood*, could be derived from the axiom by the rule

$$R1 \quad F(n_1) \rightarrow F(n_2, n_1).$$

Cf. the rules R3, R5 and R7 below.

The QUASI PREDICATE is a very important construction. We introduce it by the following rules:

$$R2 \quad F_1(n_1), F_2(n_1, n_2) \rightarrow F_1(n_1, fn_2)$$

$$R3 \quad F(n_1, fn_2) \rightarrow F(n_3, n_1, fn_2)$$

The idea underlying rule R2 is that the quasi predicate ($= f$) properly corresponds to a predicate with subject and object. The counterpart of the subject is dropped, and the phrase contains only an object. Instead of subject there is a VALENCE. The valence will be fulfilled by some other element in the sentence.

According to the rule R3 in a intransitive sentence there is the subject which so to say completes the quasi predicate (fulfils its valence), and in a transitive sentence the object stands in the same relation to the quasi predicate. In other words, the subject

in an intransitive sentence and the object in a transitive sentence have common properties.

Besides an indirect object of the form fn , there is also another one of the form f . The rules R4-5 deal with it, corresponding to the rules R2-3 above.

$$\begin{array}{l} \text{R4} \quad F_1(n_1), F_2(n_1) \rightarrow F_1(n_1, f) \\ \text{R5} \quad F(n_1, f) \rightarrow F(n_2, n_1, f) \end{array}$$

On the empirical side, this is equivalent to a certain kind of adverbs as in English *in, out, down, abroad*, etc. The amount of them is always limited. For example: *I came in.*

According to the rules R6 and R7

$$\begin{array}{l} \text{R6} \quad F(n_1, f_1), \dots, F(n_1, f_i), F(n_1, g_1 n_2^1), \dots, F(n_1, g_j n_2^j) \rightarrow F(n_1, f_1, \dots, f_i, g_1 n_2^1, \dots, g_j n_2^j), \text{ for} \\ \quad i = 1, 2, 3, \dots, n, \text{ and } j = 1, 2, 3, \dots, m. \\ \text{R7} \quad F(n_1, f_1, \dots, g_1 n_2^1, \dots) \rightarrow F(n_3, n_1, f_1, \dots, g_1 n_2^1, \dots) \end{array}$$

there cannot generally be two or more objects in the sentence: the other object or objects complicate the sentence structure less, when there is an element which shows their relation to another central constituent of the sentence. And this is achieved by an indirect object with a quasi predicate as its head. Really, many peculiarities in sentence structure can be explained on this basis.

A verbal sentence generally has only one direct object, though it can have several indirect objects. This is the "principle of not having more than one object". Of course, there can, however, be instances where we can observe two objects. But they always have restrictions of their own, and in one language they are different from what they are in another. For example in English: 1) *They told the police their secret.* Cf. *They told their secret to the police.* 2) *The people crowned Richard king. They named their son Henry.*

To illustrate the empirical ramifications of this theory, a vast number of examples from different languages might be cited. For instance, in Finnish, we can say *Juna tuli asemalle* "the train came to the station". The form *asemalle* is a so-called local case (from the noun *asema* "station"), allative, and the quasi predicate appears as a suffix, a case ending (*-lle*). Finnish has a complete system of local cases, 8 in all. The quasi predicate can also be included in the verb, as in the Hungarian sentence *Jancsi átugorta a hálót* "Jancsi jumped over the net". Cf. *Jancsi ugrott a hálón át*, which means the same and where *át* is a postposition. Still other modifications are possible, for example, it is conceivable, that in some language besides $N_1 V N_2 P(N_3)$ we can have $N_1 P-V N_2 N_3$.

In one important respect these formulations are not quite exact. When a quasi predicate forms the head of an indirect object, it has a further property which distinguishes it from the other quasi predicates occurring in a sentence. I will call the indirect object BOUND, as the other phrases with a quasi predicate as head are FREE. We can talk about bound and free quasi predicates, too. For example, in the sentence *In America he came to Boston* there is a bound quasi predicate *to* and a free one *in*.

The free phrase, a modifier, is therefore free, that it can be followed by anything. But the indirect object *to Boston* is bound, because it is closely connected with the verb *came*, and the substitution class occupying its place is always restricted.

We can develop the simple sentence model in many directions. But we should notice that the number of different modifications will increase very rapidly, and that the choice of solutions must depend on empirical analysis of large linguistic materials.

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FUNDAMENTALS OF SYNTAX

ANTON REICHLING *and* E. M. UHLENBECK

Every speech event is characterized by the coexistence and the subtle cooperation of linguistic and non-linguistic data. In view of recent discussions on syntactic problems it seems not superfluous to point out the essential role played by non-linguistic data in the process of speech production and interpretation. This implies that a theory of syntax which assumes that language is a self-contained closed system which is used context- and situation free, is bound to give a distorted and incomplete picture of the linguistic facts.

Careful analysis of the act of speech, the natural starting-point for all language-study, reveals that within the non-linguistic data one has to distinguish between the frames of reference of speaker and hearer on the one hand and situational factors on the other.

If we first look into the act of speech from the side of its production, we notice that the speaker cannot fail to start speaking from certain points of reference relevant to the subject matter in hand and consisting of various past experiences and previously acquired items of knowledge. *Mutatis mutandis*, the same can be said to be true of the hearer. In his act of interpretation of the utterance he will let himself be guided by his own referential frame, that is by the realm of his knowledge of the subject matter.

If the hearer's frame of reference coincides with the frame of reference of the speaker, the possibility is given of complete understanding of what the speaker wants to convey to him. Very often however there will be appreciable differences between them and if the hearer is not aware of these differences and does not get the opportunity to share the relevant referential data no communication or at most incomplete communication will be the result.

In this short lecture we have to refrain from analysing the situational factors. We have to limit ourselves to the obvious remark that the actual situation in which the utterance is made, the text, that is the preceding utterances if any, and lastly the knowledge which the hearer may possess about his speech partner may equally furnish interpretational clues to him.

The importance of the frame of reference and the situational setting of the utterance can readily be proved by attempting to analyse sentences completely detached from them. If one does this, one notices that any utterance which within its actual setting

does not cause the hearer any interpretational trouble, will become impossible to interpret because they are open to two and very often more than two different interpretations between which one is not able to choose. The sentence *Paul has beaten Bobby* may refer to two boys who had a fight, but it might also have been heard a few months ago during the chess contest in Curaçao, Paul being the first name of the famous Russian chess-player Keres, Bobby the Christian name of the well-known American champion Fisher. This observation permits us to stress the important point that speech loses its efficiency as soon as it is severed from its referential and interpretational background.

It is highly illuminating to notice that the phenomena of ambiguity are not always of the same order. There may arise ambiguity as to the relational structure of the utterance as for instance in cases like *Old men and women* and *they are flying planes*. Beside these cases of syntactical homonymy another type of ambiguity also may crop up.

The sentence *the picture was painted by a new technique*, may of course be thought to refer to the fact that a certain artist has made use of a new pictorial technique, but it is also possible that it refers to a new automatic process of picture-production, which is by no means far-fetched in view of the present stage of development of modern art.

Let us give a second example which is slightly different from the one mentioned a little while ago, *The teacher beats his child*. In the absence of extra-linguistic data this sentence is also open to different interpretations; the child involved may be the teacher's child or somebody else's.

It can hardly be denied that whatever the interpretation, the relational structure of the two last sentences remains the same. Moreover both sentences are made up of the same elements. That the content of the sentences is different is caused exclusively by the fact that the interpretation of two words in these sentences, resp. *by* and *his*, may be different. This again permits us to draw an important conclusion: sentences which are different in content and which consist of the same elements may have the same relational structure. Therefore in the study of syntax it is an unpermissible apriori to postulate the existence of syntactical, that is relational differences between two given sentences on the ground that these sentences are intuitively felt to be different in content.

Let us turn now to the sentence itself. The study of child language and the study of the phenomena of aphasia confirm the basic insight gained from normal speech that in every language the sentence is a double-layered structure. In every sentence one has to distinguish two components: the intonational or melodic layer and the phatic layer, the layer which consists of discrete elements in linear succession. The number of these elements may be only one, in most cases however it will prove to be much higher.

The phatic components of the sentence follow each other in time. This is the well-known *principe linéaire* of De Saussure which looks like a truism but which enables us to draw a conclusion fundamental for the study of syntax.

The temporal, linear sequence is what we may call the natural framework of speech.

In all speech everybody has to adhere to the iron law that one cannot use two words or two wordlike elements at the same time. This means that temporal succession in itself does not constitute a syntactic phenomenon belonging to one language in particular. It is nothing else than the canvas on which the syntactic patterns of the different languages have to be embroidered. Or in other words the pure fact of this linear sequence does not automatically furnish the listener with the knowledge how to connect the elements offered to him. His ability to do this rests on a quite different principle which was for the first time formulated by Stenzel and Reichling in the early thirties. This principle holds that elements following one another linearly, may remain unconnected and kept present until an element or elements appearing in the utterance much later can be connected with them.

What we have provisionally called discrete elements in linear succession may be entities of different character. It seems to us that two never verified assumptions have been equally harmful to progress in syntax: the one which aprioristically holds that every sentence could be completely split up into words and the other equally unwarranted assumption that it could be analysed ultimately without any residue, into morphemes. In our opinion one has in the first place to reckon with the possible occurrence in the spoken chain of elements which by their syntactical and semantic properties can neither be classified as words or as parts of words or morphemes.

Secondly no language can dispense with words, that is with shiftable units of an unique semantic character. Nevertheless one cannot stay blind for the fact that this category comprises widely divergent entities. It is common knowledge that languages have their own set of word-class distinctions and their own set of distinctions within each word-class. However, this does not prevent us from recognizing the important fact that there are some distinctions which seem to be universal or at least extremely widespread. It seems to us that in many languages one finds at least four different types of words, to wit (1) naming words, a term which we coin as a translation of the German *Nennwörter*, (2) deictic words or, to use Jakobson's felicitous terminology, shifters, (3) proper names, (4) technical-grammatical words. These technical or accessory words can be opposed to all the three other types by their lack of semantical independence. To use an often wrongly interpreted distinction: they are syncategoremata. The three categories with independent meaning are semantically not on a par. The *Nennwörter* not only indicate but also name, the proper names merely indicate, while the deictic words indicate in relation to the participants of the speech event in which they occur or to preceding or following elements in the context or in the text.

Each of the three types displays its own modes of semantic application. The naming words are characterized by their polysemy and by the possibility of metaphorical use, the proper names also display a special type of metaphorical use, but are not polysemous, the deictic elements lack the modes of meaning-application we find with the naming words, but they can be used anaphorically. All words of all four types possess the property of twofold suppositional use. We are referring here to cases like "*two*" is written three letters, or this "*if*" is to be deleted.

There is no time left to elaborate any further these distinctions only sketchily indicated here, but this short survey perhaps is sufficient to make clear that the speaker has at his disposal a rich variety of semantically different tools. However if one looks more closely into these tools and especially into the way the meaning of the naming words operates in actual speech, one is struck by the fact that their meaning — understandably enough in different degrees and different ways — has a highly mercurial and flexible quality. By means of one and the same word the members of a speech community may refer to a multitude of different things. This principle of flexible symbolization may be positively appreciated in the light of the fact that man although possessing a limited memory, needs an apparatus to refer to a virtually unlimited number of things. Nevertheless this flexible and mobile character of the meaning of large numbers of words would endanger mutual understanding, if it was not supplemented and held in check by a second general principle which we may call the principle of combinative symbolization. We refer here to the fact that in every language the entities of the phatic layer whatever they may be, are connected into groups and these again into larger units according to certain rules different for each language.

After this short account of the fundamental aspects of the utterance, we may try to give a more exact answer to the question what the clues are which the hearer derives from the utterance in order to grasp what the speaker intends to convey to him.

First it is necessary to remember that the purpose of the speaker is a semantic and communicative one. It is his purpose to convey something to the hearer and this something has to be taken in an extremely wide sense. However, one thing is certain: the speaker arrives at his purpose *inter alia* by connecting the meanings of various elements. Now the main task which confronts the hearer is to find out what the connections are. It is fundamental for the study of syntax to observe that the hearer is able to do so only with the help of formal indications. It has been a fateful error of traditional syntactic theory, immediate constituent analysis and phrase-structure grammar alike to assume that the study of syntax consists of an analysis of the overall content of the sentence. The classical practice of parsing with its initial division of the sentence into subject and predicate still flourishes everywhere.

What are then the formal clues to the relational structure of the sentence — we are used to call them syntagmatic indications — which may occur? They can only be of three different types; to wit, intonational indications, the arrangement of the entities of the phatic layer and finally the form of the entities themselves. Given the double-layered structure of the utterance it is impossible to conceive of other types of syntagmatic indications.

It is often assumed, especially in so-called transformational syntax, that there are semantical limitations to the combining of elements. This view seems to us to rest on an incomplete observation of the facts and it is probably caused by logical considerations. To posit for instance a linguistic distinction between transitive and intransitive verbs in English or to frown at the combination of logically incompatible elements like *square* and *circle* is not in accordance with what one observes in actual speech and

therefore linguistically unacceptable. Putnam's *pepper does not sneeze me*, Chomsky's *sincerity admires John*, or Lees' *John astounded the dark green* are not only linguistically impeccable combinations but they are not even deviant combinations in any linguistic sense. They are built fully in accordance with the rules of English syntax. We completely agree with Curry when he observes, in his recent paper read at the symposium on the structure of language and its mathematical aspects, that absurdity is not a grammatical concept but an aletheutical one.

Every English adjective may be connected with any noun, and language as a system places no semantic bar on the combinations of adjectives and nouns chosen by its users. Of course certain combinations occur more frequently than others. It is probably true that *John plays golf* will be less rare than *golf plays John*, but the crucial point is that as soon as a speaker of English feels the need to convert the game *golf* into a player, he is able to do this and what is still more important the resulting sentence will not attract undue attention, precisely because it fits the occasion. The speaker of English does nothing else than make use of a normal opportunity offered to him by his language. The use of metaphoric expressions is nothing special or abnormal; there is nothing deviant about them, because metaphor is a universal mode of application of meaning which belongs to what one may call the normal semantic techniques of every language.

No semantic rules limit the possibilities of connection, but this does not mean that there is no limitation and that there are no rules at all. There is one limitation and there are a set of rules. The limitation consists in the fact that elements become incombinable if they don't fit into the frame of reference on which they depend. The sentence "*he comes as frequently as six feet*" discussed by Lees is only "a forbidden example" if one supposes that this utterance is spoken with a mixture of two quite different frames of reference, to wit the frame of reference of arithmetic and measuring and the frame of reference of ordinary life. However, interpreted for instance from a background of personal experience with somebody who has the peculiarity of measuring everything in terms of six feet, even this sentence is a permissible and acceptable English sentence.

We sympathize with the point of view that our descriptive effort consists of formulating rules which ought to have predictive force, that is they ought to do more than to summarize what is found in an arbitrarily limited corpus. We also agree that the rules finally established have to enable us to produce sentences in the language concerned. We only stress that these rules are formal rules of valence. A simple example may make clear what we mean. The rule for the connection of the English article and the English noun is easy to formulate. In order to be connected the article has to satisfy the condition that it precedes the noun, although there is no need that it immediately precedes the noun, witness groups like *the man* and *the long, lazy and normally very untidy man*. If this condition of order is not met, two words belonging to these two classes cannot be combined. It is such formal rules which the student of English syntax has to describe accurately and exhaustively and the term

deviant may be reserved for those cases in which the existing formal rules of combination are clearly violated.

And now it is time to conclude. Our view of the nature of syntactic study implies that one has to distinguish between the relations which are established between the entities of the phatic layer of the sentence on the one hand and the formal, syntagmatic indications of these connections on the other hand. A mono-lingual syntactic description will consist of a set of rules which indicate the relations and the formal correlates of these relations. In order to be able to do so one needs a method for formalizing the relations. We are inclined to predict that the mathematical theory of digraphs recently developed by Harary and associates¹ will prove to be an extremely useful tool to this end.

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DISCUSSION

GALTON:

Linear succession in the *chaîne parlée* is normally associated with sequence in time, i.e. time develops along its single axis while the events constituting the linear succession take place. In the course of the utterance, however, and for purposes of linguistic analysis, time can very well be suspended, as it were. Some languages go to greater extremes than others in this respect. In the utterance of a German sentence such as *Ich richtete den von vielen Schicksalsschlägen gebrochenen Mann wieder auf* ("I restored the man who had been broken by numerous blows of fortune") we find that time is actually suspended until the completion of the sentence, when the meaning of the verb has been clarified by the ensuing "pre"fix that constitutes one entity with the preceding verb.

LAROCLETTE:

La description d'une langue, telle que la conçoivent les linguistes de l'école qu'on a appelée "anti-mentaliste", repose au fond sur deux postulats dont on retrouve l'écho dans l'article de M. Uhlenbeck. Le premier est que le structural exprime le sémantique. Le second est qu'il y a moyen de décrire la structure formelle du discours sans jamais faire appel au sens des formes.

On peut se rendre compte que l'un et l'autre de ces postulats sont inacceptables en examinant le rapport qu'il y a dans le discours, entre les relations structurales que les signifians ont entre eux, et les relations sémantiques que les signifiés ont entre eux.

1. Une construction n'*exprime* pas une relation sémantique entre ses constituants; elle se borne à *indiquer* qu'il y a une relation, ce qui n'est pas la même chose.

¹ Harary-Norman-Cartwright, *Structural Models, An introduction to the theory of directed graphs* (1963).

La nature de la relation et sa *direction* doivent être déduites, soit du sens des constituants, soit du sens d'un autre élément de la phrase, soit encore du contexte linguistique ou de la situation. Structuralement, "J'ai perdu *ma clef*" et "j'ai raté *mon train*" sont des phrases identiques, mais il est évident que la nature de la relation que je puis avoir avec ma clef est très différente de celle que je puis avoir avec le train que j'aurais pu prendre un jour.

Une même construction "*l'amour des parents*" figure dans les deux phrases suivantes: "L'amour des parents est une obligation pour les enfants", "L'amour des parents est une bénédiction pour les enfants". Elle y indique une relation sémantique qui est de direction différente dans les deux cas et qui est explicitée par les mots "obligation" et "bénédiction".

La situation contraire peut se produire: deux constructions différentes peuvent indiquer la même relation sémantique. On s'en aperçoit en comparant les phrases: "Il se promenait dans les fraîches prairies", "Il se promenait dans la fraîcheur des prairies" (cette dernière construction est souvent préférée dans le style littéraire). La relation structurale entre "fraîches" et "prairies" est celle de complément à principal (ou de complément à complété, ou encore de complément à nucleus, selon la terminologie que l'on voudra adopter). La relation structurale entre "fraîcheur" et "prairies" est celle de principal à complément. Dans les deux cas, la direction de la relation sémantique est la même.

Il en est encore ainsi lorsqu'on compare "Jean a battu Paul" "Paul a été battu par Jean". Le changement de direction structurale n'affecte pas le changement de direction sémantique: c'est toujours Jean qui fait l'action.

Retenons de ces exemples que la relation sémantique n'y est pas déterminée par la relation structurale, qu'elle jouit d'une certaine indépendance: cela est bien compréhensible puisqu'aucune langue ne peut posséder un système d'accords, de cas ou de prépositions qui permette d'*exprimer* toutes les relations possibles entre les idées.

2. Le sens d'un constituant de la construction ou d'un signe extérieur à la construction peut, non seulement préciser la nature et la direction de la relation sémantique entre les constituants, mais aussi permettre d'*identifier la construction*.

Zellig S. Harris cite la phrase suivante "She made him a good husband because she made him a good wife" (*Structural Linguistics*, p. 271) et la commente ainsi: "We know that there is a difference in meaning between the two occurrences of *made*, and since we know this without any outside information beyond hearing the sentence, it follows that indication of the difference in meaning and in construction can be derived from the structure of the utterance". C'est là une manière assez inexacte de présenter les choses. Comment procédons-nous pour comprendre cette phrase? Nous savons à l'avance que le verbe peut se construire de deux façons: 1. to make somebody President 2. to make something for somebody. Pour chacun des membres de la phrase, l'auditeur envisage les deux constructions et identifie celle qui figure réellement dans la proposition, en constatant que l'*autre construction donnerait à la proposition un sens absurde* (contradiction sémantique entre les éléments).

Il faut donc bien admettre que dans un cas semblable, c'est le *sens* d'un élément de la construction qui permet d'identifier celle-ci.

Quand une même séquence d'éléments morphologiques peut représenter deux structures différentes et que la phrase ne fournit aucune indication sémantique permettant de les reconnaître, la séquence reste ambiguë et la construction ne peut être identifiée que par le contexte ou la situation. Soit la phrase "Je lui ai fait faire un costume". Sans information extérieure, il est impossible de savoir quelle est la fonction structurale de "lui" et par conséquent de savoir qu'il s'agit d'un costume qui a été fait *par quelqu'un* ou *pour quelqu'un*.

3. Nous avons vu plus haut que la relation sémantique qui existe entre les constituants d'une construction n'est pas liée à la relation structurale qui existe entre ces constituants. Dans le cas de l'hypallage la relation sémantique unit des mots qui appartiennent à des constructions différentes. Quand un écrivain impressionniste écrit: "Il fumait une triste cigarette", le lecteur comprend fort bien qu'il ne faut pas attribuer la tristesse à la cigarette, mais au sujet de la phrase. Il en est de même dans une phrase beaucoup moins littéraire: "Il fumait tristement une cigarette", car la tristesse ne peut être attribuée à l'action de fumer.

Cette propriété qu'ont les signifiés de pouvoir établir des connexions entre signes appartenant à des constructions différentes, nous allons la retrouver en examinant les rapports qu'il y a entre les classes structurales et les catégories sémantiques.

Une catégorie sémantique n'est pas une sous-classe, comme l'emploi de certains symboles par les partisans d'une linguistique de la forme pourrait le faire croire: on ne peut décrire valablement les catégories en se contentant par exemple d'écrire *Nf*, *Nm*, *Nn* pour distinguer dans le substantif l'opposition féminin/masculin/neutre, bien que souvent un même morphème soit utilisé par une langue pour marquer dans un mot à la fois la classe structurale et la catégorie auxquelles il convient de le rattacher.

Une classe structurale se définit par l'aptitude qu'ont certaines formes à compléter et à être complétées par d'autres formes, c'est-à-dire par leur aptitude à faire partie d'une construction dont elles constitueront la partie nucléaire ou non-nucléaire, (le complété ou le complément); un mot qui appartient en français à la classe de l'adjectif peut compléter un substantif et être complété par un adverbe.

Or les catégories permettent d'indiquer des relations sémantiques, non seulement entre signes appartenant à la même construction, mais entre signes appartenant à des constructions différentes; elles établissent de cette façon des liens syntaxiques entre ces constructions.

Qu'on prenne par exemple la phrase: "Pierre a rencontré sa soeur elle lui a remis le paquet". "Elle" est en connexion sémantique avec "soeur", bien que les deux signes appartiennent à des constructions différentes, et cette connexion sémantique unit les deux propositions, en l'absence de tout lien formel.

Voyons un autre exemple: "Elle entra; son mari lisait le journal" "Il lisait le journal, sa femme entra". Le "temps" des verbes "lisait" et "entra" établit une connexion sémantique entre ces verbes, qui font partie de constructions différentes et constitue

un lien syntaxique entre les constructions. Si l'on disait simplement "Il lisait le journal", à moins qu'il ne s'agisse de la réponse à une question, l'auditeur sentirait que la phrase n'est pas complète.

Les relations que les "temps" et les "modes" permettent d'indiquer entre les verbes appartenant à des constructions différentes sont de même nature que les relations que l'anaphore permet d'indiquer entre les noms: les unes et les autres relèvent de la *syntaxe des signifiés*, qui est différente de la *syntaxe des signifiants* et *qui peut y suppléer*.

En effet, bien des langues font abondamment usage de la possibilité de lier les constructions par des liens de nature sémantique plutôt que par des liens de nature structurale, et ce n'est pas par hasard que beaucoup de langues africaines, qui ont un système très développé de catégories nominales ou verbales se passent presque totalement de relatifs et de conjonctions.

Le Zande par exemple possède beaucoup plus de "temps" (et de "modes") qu'on ne l'a reconnu jusqu'à présent. Parmi ces temps, il y en a un bon nombre qu'on ne peut jamais employer seuls. Certains (un antérieur général, un concomitant général, un consécutif général) ne peuvent figurer que dans une phrase qui contient un autre temps de l'indicatif, quel qu'il soit. D'autres ne peuvent figurer dans une phrase que si elle contient un des temps d'une série donnée. Du point de vue structural, il serait impossible de décider quel est le membre de la phrase qu'il faut considérer comme principal et quel est celui qu'il faut considérer comme subordonné, s'il n'y avait pas une indication supplémentaire: les propositions subordonnées utilisent à la forme négative le morphème "ya", les propositions principales le morphème "te".

Affirmer, comme on l'a fait souvent, que des langues de ce type affectionnent la *parataxe*, parce qu'elles n'utilisent guère de relatifs ou de conjonctions est une erreur qui provient de la méconnaissance du caractère syntaxique des catégories sémantiques.

Il n'est pas possible d'éviter cette erreur si l'on a décidé de s'en tenir systématiquement dans la description des langues, à la description des signifiants, en négligeant les signifiés.

Le caractère syntaxique des catégories nominales apparaît le plus clairement dans les langues où la distinction entre les catégories n'intéresse en aucune façon la structure, où les catégories nominales ne sont pas marquées dans le substantif, où elles ne servent ni à l'accord, ni à la rection, mais uniquement à l'anaphore. Le Ngbaka ne remplace pas par les mêmes pronoms, les noms d'êtres animés et les noms d'êtres inanimés, le Banda les noms d'êtres vivants et les noms de choses. Le Zande distingue dans les pronoms la personne en général, la personne masculine, la personne féminine, l'animal et la chose. Le Mondunga distingue par des pronoms différents d'une part les personnes, d'autre part les animaux et les choses. Dans aucune des langues citées, les oppositions catégoriques ne sont marquées dans le nom, et il n'y a pas d'accord.

Dans la description grammaticale d'une langue, il est nécessaire d'exposer des faits de deux ordres différents: ceux qui relèvent de la structure morphologique et ceux qui relèvent des catégories sémantiques. Quelle que soit la manière dont on répartira

la matière, il faudra expliquer dans une grammaire du français qu'il y a des temps simples et des temps composés et montrer le rapport structural qu'ils ont entre eux; il faudra exposer ensuite que ces temps simples et ces temps composés s'opposent catégoriquement.

Se contenter de classer d'une part les formes simples et d'autre part les formes composées sans décrire les liens structuraux et sémantiques qu'elles ont entre elles, c'est présenter un travail d'analyse préparatoire à l'établissement d'une grammaire, mais ce n'est pas faire une grammaire.

IS THERE AN INTERLINGUAL ELEMENT IN SYNTAX?

A. F. PARKER-RHODES

This is no rhetorical question: the practicability of machine translation depends in part upon the answer. When one surveys the immense diversity of the world's languages, and the very different ways in which different languages render what are presumed to be the same thoughts, one is struck by how little they all seem to have in common. Yet the very fact that we have the term "linguist" implies that all languages are at least open to a common discipline of study. This common discipline has been, in fact, the starting point of our enquiry.

If all languages can be studied in a similar way, it should be possible to formulate the procedure. The simplest part of language to formulate in this way is the description of grammatical rules. We thus arrive at the notion of a model of grammatical description. If we could find such a model of sufficient generality and logical power, we should have found something, at least, common to all languages.

The subject of grammatical models has been well reviewed by Hockett,¹ though one important class of models has been developed since, named below as the KT type. Historically the oldest model for grammatical description was the Word-and-Paradigm or WP model, which originated with Appollonius Dyscolus² and held the field unchallenged till modern times, when in the face of wider knowledge of language types ill-suited to its limited categories it fell out of favour; but it has its strong points still, as Robins has shown.³ This was followed, first, by the Item-and-Process or IP model, definitively formulated by Sapir,⁴ and later by the now more popular Item-and-Arrangement or IA model, often called, in the terminology of Wells,⁵ the "immediate constituent model". This model was the first explicitly to recognise the fact that every sentence in every language can be analysed as a hierarchy of constituents (though to apply the analysis strictly one must allow that a constituent may be discontinuous or represented by a zero form). With slight modifications, mainly in the logical basis of the system, this model forms the basis of the work described here.

The most recent addition to the list of these models originated in the work of

¹ Hockett, C. F., "Two models of grammatical descriptions", *Word*, 10 (1954), 210.

² Dyscolus, A., *De syntaxi seu constructione orationis libri iij.* Ed. A. Schotto (Frankfurt, 1590).

³ Robins, R. H., "In defence of WP", *Trans. Philol. Soc.*, 1959, p. 116.

⁴ Sapir, E., *Language* (New York, 1921).

⁵ Wells, R. P., "Immediate Constituents", *Language*, 23 (1947), 81.

Harris, but has been mainly developed by Chomsky;⁶ this is the Kernel-and-Transformation or KT model. Though very useful for various specialized applications, this model seems to be less suitable for MT purposes than the IA type, and certainly leads to a more cumbersome and intractable description than ours.

None of these models has been presented as either a rigorously defined procedure of analysis, or as a properly integrated methodology. Both these sophistications add to the power and precision of a grammatical description, and it is in this direction that most current research is proceeding. The main impetus towards the formulation of grammatical descriptions as *mathematical deductive systems* has come from work on machine translation and information retrieval. This work cannot be adequately reviewed here: the bulk of it has been done in the United States and in Israel, and is exemplified among others by Mooers, Lambek, and Bar Hillel & Shamir.⁷ Much of this mathematical work suffers from being guided more by the example of pure mathematics than by the necessities of programming, and in consequence the development of the model described here has gained relatively little from their experience, even though it belongs to the same class of work. The methodological sophistication of the models is a more recent development, associated especially with the name of Halliday.⁸ This author takes great pains to describe the processes of description, so as to eliminate redundancies and omissions. This discipline is perhaps more immediately useful to pure linguists than to machine translation workers, but it has imported standards of rigour into the subject which make much early work seem naive, and also shows up how much we mathematicians allow ourselves in the way of simplification. But Halliday's work does not yet seem to tell us whether, in practice, we have *over-simplified*; if we have not, these complexities are not of immediate practical value to us.

BASIC PRINCIPLES OF SYNTAX NOTATION

The syntactic model developed by the Cambridge Language Research Unit may be described as a mathematization of the immediate constituent model. It rests on two basic notions, the relations of equipollence and componency. These relations apply to recognisable segments of text, of various sizes from single morphemes up to (at least) whole sentences. Such word-groups are called substituents. This term has been given a strict logical definition⁹ which need not be repeated here: it is sufficient to

⁶ Harris, Z. S., *Methods in Structural Linguistics* (Chicago, 1941); Chomsky, N., *Syntactic Structures* (The Hague, 1957).

⁷ Mooers, C. N., *The Mathematical Theory of Language Symbols in Retrieval*. International Conference on Scientific Information (U.S. Nat. Acad. Sci. Washington, 1958); Lambek J., "The Mathematics of Sentence Structure", *Amer. Math. Monthly*, 65 (1958), 154; Bar Hillel, Y., Shamir, E. & Perles, M., *On Formal Properties of Simple Phrase-Structure Grammars* (Jerusalem, 1960).

⁸ Halliday, M. A. K., personal communication (1962).

⁹ Parker-Rhodes, A. F., *A New Model of Syntactic Description*. Internat. Conf. on Machine Translation of Languages and Applied Language Analysis (U.K. Nat. Phys. Lab. Teddington, 1961).

say that any sequence of words which could be replaced by a single word without changing the syntactic structure of the sentence in any other way is a substituent. Any sentence is described as a hierarchy of substituents, and if a substituent A consists of two parts, both themselves substituents, B & C, then we say that B & C are *components* of A. This is the relation of componency. Any two substituents which can replace each other, in any sentence where either occurs, without changing the syntactic structure of the sentence, are said to be *equipollent*.

By a syntax notation I mean a system of representing the syntactic structure of a sentence in such a way that the componency and equipollence relations of all the substituents are correctly represented. The hierarchical structure implied by the componency relation can be readily represented by a system of nested brackets. A method of indicating equipollence will be described when I have explained the bracket notation.

If a, b are two components of a compound substituent, the latter will be written (a b). If to these two we add a third component c we could derive several formulae: (a b c), ((a b) c), or (a (b c)). Each such formula makes a definite and unambiguous statement about the componency relations between the substituents indicated. Although in general symbols like a, b & c can be applied to any substituents, the notation becomes fully explicit if we restrict the use of *single* letters to *simple* substituents (A simple substituent is one having no separately recognisable components of its own.)

The relation of equipollence is denoted by two different devices. The first device is to adopt the rule that in writing bracket formulae we must never insert a single bracket between two substituents which are equipollent. Thus, the two phrases (*tall (dark houses)*) and (*dark (tall houses)*) are syntactically equivalent, and it is clear from this that *tall* and *dark* are equipollent. The rule stated therefore says that both these formulae are incorrect: instead, we must write (*tall dark houses*) or equivalently (*dark tall houses*). However, this rule is obviously not sufficient for the purpose, for we are not *obliged* to insert a bracket between *non-equipollent* components (if we were, we could not represent a substituent whose components were not equipollent). We supplement it by the rule, that whenever a substituent is equipollent with one of its *own* components, the latter shall be marked by a following comma. Thus, in the above example, the compound substituent *tall dark houses* is itself equipollent with the simple substituent *houses* (they can be interchanged in any sentence without altering its syntactic structure otherwise); we therefore indicate this fact by writing the formula (*tall dark houses,*). In this formula, the fact that the last substituent has a comma after it while the others do not, indicates that it is not equipollent with the latter; while the fact that there is no bracket between these can now be used to show that *tall* and *dark* are equipollent.

The notation is further helped by the fact that no compound substituent need have more than one component of more than one equipollence-class: that is, that any substituent represented as having four components, two and two equipollent together,

can be better regarded as having a compound structure in which this is avoided. Thus, let us consider the English phrase which might be written (*tall dark houses, trees*). Here, by the rules so far given we have *tall* and *dark* equipollent to each other but not to the whole phrase, and *houses* and *trees* both equipollent to the whole phrase. But such a phrase is obviously capable of being split up further than this. According to how we interpret it (because it is rather ambiguous as it stands) we could write it as ((*tall houses*), (*dark trees*),) or (*tall dark (houses, trees)*),) or in various other ways. It is always possible to break down such compound substituents so as to have only substituents obeying this rule: that at most one component has any other component equipollent with it. In any such compound we can write the odd component last, for in general any consistent bracket notation involves sacrificing the original word-order of the sentence; by this convention we reduce considerably the number of ways in which a given formula can be written.

It is convenient to regard this rule as defining one of the components of a compound substituent as the "governor", even when there are in fact only two components altogether. The governor, then, can be defined as that component which *may not* be equipollent with any other component which could be added to the substituent without changing the syntactic structure of the sentence. This defines *houses* as the governor in (*tall houses*), because we can add *dark* as a third component and it will not be equipollent with *houses*, whereas a word which would be so, such as *trees*, we cannot add as a third component to this particular substituent. Similarly, in a phrase like *not men* we can add a third component equipollent with *men* and get *women not men*, but cannot add a third component equipollent with *not* without changing the structure. Thus, by the rules so far given, this phrase should be written (*women, men, not*), showing *not* as the governor.

This rule cannot be extended to cases where no third component can be found equipollent to either of the given components. For example, we have in English prepositional phrases, such as *in bed*, which have *at most* two components; if we want to extend the idea of governors and dependents to such cases, we can only do so by a formal rule – but such a rule ought to be guided by a sound analogy. Consider the expression *someone with good advice*: this contains the prepositional phrase beginning with *with*, which however is syntactically equivalent to a participial phrase: *someone giving good advice*. But whereas a preposition can only have one noun attached to it, a suitable verbal participle may have two: thus, we could say *someone giving me good advice*. In the last case, by the rule already defined, *giving* is the governor of the compound substituent *giving me good advice*. Therefore, by analogy, the preposition which can replace it when there is only one noun must be the governor of the substituent *with good advice*. Finally, in cases where even such analogies fail us, it is safe to say that the decision is unimportant and may be made arbitrarily. It is sufficient that we should be able to decide, for every compound substituent, which component to write last, in a manner consistent with the rule as given in the cases where it applies.

There remains only one point in connection with the bracket notation which must

be cleared up. We have to be able to apply the notation to texts of any length, and not only to single sentences. It is therefore necessary to be able to indicate what is a sentence; for otherwise in a long text there would be no fixed points and no way to distinguish complete sentences from components of sentences. We must also recognise that up to now our analysis has not gone beyond the sentence level, and that inter-sentence relations may exist of a kind different from those obtaining within sentences. Such relations would require a notation in which the sentence as a unit could be unambiguously distinguished. The notation I propose for this is to write a full-stop . at the end of any completed sentence, or of any smaller component which could, but for its structural subordination to some other substituent, be a complete sentence. Thus, the sentence *I thought he did it* could be bracketed by the rules already given as *I (he did it) thought.* But in this formula the sequence *he did it* would itself be a complete sentence, if it were not for its inclusion in a larger form. Thus, transforming it by the notation into *he (it did).* we get the formula *I (he (it did).) thought.* If we had included the subjunction *that* before the subordinate sentence, it would of course appear as a component of the total sentence too, whose formula would then be *I (he (it did). that) thought.* Note that we write the subjunction last, indicating it as the governor; we do this on the grounds of its analogy with a conjunction such as *and*, which is always the governor of a group containing it by the rule of uniqueness.

We have now obtained a complete set of notational conventions, by the application of which any sentence in any language can be written so as to show its syntactic structure as given by an immediate constituent type of analysis, together with the equipollence-relations between its substituents of all different ranks in the hierarchy. These conventions are reduced to the form of rules in Appendix I. When I refer hereafter to "the notation" I shall mean the notation as defined in this Appendix.

OCCASIONAL PARADIGMS

The derivation from the notation of a systematic description of the range of syntactic functions possible in any language depends on the notion of the "occasional paradigm" of a substituent. This notion can best be apprehended in conjunction with two others, the "chain of determination" and the "total paradigm".

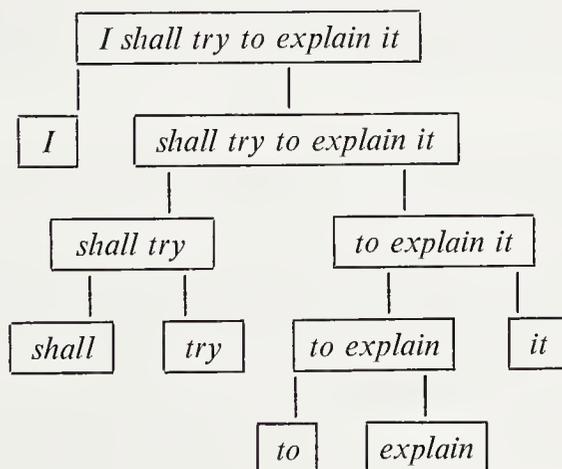
The *chain of determination* of an occurrence S_a of a substituent S in a given *text* is the set of all occurrences of substituents T_a, U_a, \dots of which S_a forms a part in this text. Thus, in *I shall try to explain it* the chain of determination of *shall* contains the substituents *shall, shall try, shall try to explain it*, and finally the whole sentence.

The *occasional paradigm* of an occurrence S_a of a substituent S in a given *language* is the set of all chains of substituents link-by-link equipollent with the chain of determination of S_a . Thus, the occasional paradigm of *shall* in English consists of all chains of the form $O.O < OA.O < O.Z < Z.Z$, where $O.O$ is a substituent equipollent with *shall*, $OA.O$ is one equipollent with *shall try*, and so on. We can regard the

occasional paradigm of an occurrence of a substituent as the set union of all the chains link-by-link equipollent with its chain of determination, or equally as the chain of determination expressed in a generalized form (like the above $O.O < OA.O \dots$) in which the particular exponents of each link in the chain are left unspecified.

The *total paradigm* of a given *substituent* S (not of any one occurrence of S) is the set of all equipollence-classes of substituents in the given language which contain any occurrence of S. The total paradigm of S is evidently the set union of all the occasional paradigms of occurrences of S. In this terminology we apply the term chain of determination to the particular instances of words-in-context of which a given text is composed; the occasional paradigm is the same thing at the syntactic level of generality, that is without paying attention to the particular words involved; while the total paradigm represents a yet further generalization, applying not to one particular occurrence of a given word but to any use of it occurring anywhere in the language.

I shall represent the chain of determination of a word in a given context as having the given word as its bottom element, and the sentence in which it occurs as its top element; I shall not consider any higher grouping in this paper, though the extension could be made. Each point in such a chain is the bottom element of a sub-chain, and to each of these subchains there corresponds an occasional paradigm. In a particular context, each chain of determination occurs as one member of a hierarchy. Thus, the chain of determination of *shall* in the above example is just one of many chains forming the following hierarchy:



In order to find any one chain, starting from the whole sentence at the top, we have to make a choice at each step, whether to take one component (the governor) or the other (a dependent). As we saw when building up the bracket notation, each bracket contains only two equipollence-classes of substituents, for if there are two or more dependents these are all equipollent; thus the choice at each step is always a binary one. Writing G for governor and D for dependent we can thus specify any one chain,

in the hierarchy representing any given sentence, by a formula such as GDGD: this means choose the governor at the first step, dependent next, governor next, dependent next. This will get us eventually to the word *explain*.

One might think that the OPs or occasional paradigms belonging to each step in such a chain would be distinct: but this is not always so. Consider the chain *all men die* > *all men* > *men*. Now the last two substituents are equipollent with each other; that is, they have the same syntactic function which we may denote by S.Z. Let us also denote a complete sentence by Z. Now, it follows from the definitions that the OPs of the three sub-chains contained in this chain are respectively Z, Z > S.Z, Z > S.Z > S.Z. The last two differ from each other only in duplicating the last member; when this happens we call the OP with the repeated link a *protraction* of the shorter form. At each step as we come down a chain we thus have a limited number of possible *kinds* of choice to make, which may be described as follows:

1. Conjunct: the Governor has a new OP, the Dependent a protracted one.
2. Recursive: the Governor has a new OP, the Dependent is equipollent with a complete sentence.
3. Endocentric: the Governor has a protracted OP, the Dependent has a new OP.
4. Exocentric: both branches have new OPs, neither being equipollent with a complete sentence.

Note that at the first step down a chain, where the previous OP is that of a sentence, the first two kinds of branch-point are identical. We can now use this classification of four types of branchpoint to characterize every possible type of OP, subject to the conditions that (a) any part of an OP prior to a point equipollent with a sentence shall be ignored, and (b) OPs differing only by protraction shall not be distinguished.

THE SYSTEM OF OCCASIONAL PARADIGMS

A chain of *one* point simply represents a sentence; it has only one possible OP, and I shall denote this by Z (note: I shall denote occasional paradigms by letter formulae underlined, and total paradigms by similar formulae not underlined).

On coming down to the second link, we have three possible types of choice to make. In the conjunct/recursive type, I denote the new OP introduced by the governor by ZC. In the endocentric type the new OP introduced by the dependents is denoted by S. In the exocentric type, we assign this same S to the dependent and for the governor we provide a new symbol O. We thus have the following three types of formulae: for a recursive sentence, Z ZC. for an endocentric sentence S S Z. For an exocentric sentence S S O. (In each case, duplication of S implies that there *may* be, though there need not be, more than one exponent of the given function.)

When we come to chains of three or more points, we come upon much greater variety. To simplify, let us denote the OP of the point at which we make the choice

of branches by \underline{X} ; we then have to consider four different types of choice, and we shall provide symbols for each of the types of OP which these require incorporating the variable \underline{X} . To find all *possible* OPs resulting, one must substitute for this \underline{X} each of the other OP symbols in turn. The four types of choice are as follows: in the conjunct type, the OP of the governor is \underline{IC} . In the recursive type, since the OP of the compound is not indicated by that of the dependent, which is \underline{Z} by definition, it must be indicated by the new OP of the governor; we use for this the symbol $\underline{ZC.X}$ to show that this OP, unlike \underline{ZC} itself, includes the OP \underline{X} as a subset. In the endocentric type, the new OP of the dependent will be denoted by \underline{XA} . Since \underline{X} may be any OP, it could itself be, say \underline{SA} , in which case the new OP would be \underline{SAA} ; evidently there is no fixed end to this series. But in practice, for reasons connected with the principle of depth-avoidance introduced by Yngve,¹⁰ the series terminates; all sequences of two or more A 's can be identified, and we denote them all by the second letter \underline{B} . Thus, the dependent of an endocentric substituent with the OP \underline{SA} has the OP \underline{SB} . The formula for a compound substituent of this type will be $(XA \ XA \ X,)$; and for one of the recursive type it will be $(Z. \ ZC.X)$.

In the exocentric type of structure both branches introduce new OPs, and the function of the compound represented by the point of branching (which is not determinate, being any X) must be indicated by an appended letter in the same way as was done in the formula $\underline{ZC.X}$. We denote the OP of the governor by $\underline{O.X}$ and that of the dependents by $\underline{S.X}$. This device like the last generates a potentially open set of OPs, since if \underline{X} stands for say $\underline{S.SA}$ then $\underline{O.X}$ will stand for $\underline{O.S.SA}$, and so on. In practice this series also terminates quite quickly. In fact though ternary OPs such as $\underline{O.S.SA}$ do occur in some languages I know of no case in any language where a fourth term is required, and in many languages (such as Chinese) primary and secondary are sufficient. The general formula for a compound substituent having this exocentric type of structure may thus be written $(S.X \ S.X \ O.X)$.

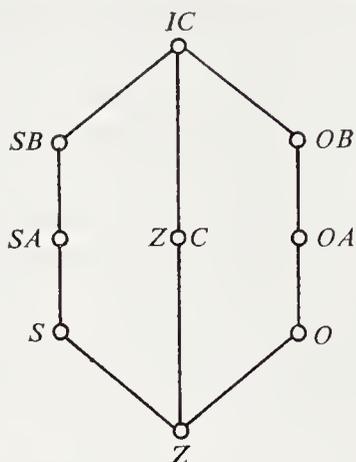
Now, we have already seen that an OP can be represented by a formula in which symbols for the function of each link are separated by the sign $>$. Since any such sequence includes, as a subset, any sequence formed by removing the last term, we can represent the complete system of OPs as a partially ordered system of sets. Note that the *first* term in an OP formula must always be $\underline{Z.Z}$ for a complete sentence.

If we at first confine attention to those OPs which are *not* of the form $\underline{X.Y}$, which we may call *primary* OPs, we observe that

1. \underline{Z} is included (as the first term) in all OPs.
2. \underline{S} is included in \underline{SA} , and \underline{SA} in \underline{SB} .
3. \underline{O} is included in \underline{OA} , and \underline{OA} in \underline{OB} .
4. all other OPs are included in \underline{IC} .

These relationships are summarized in lattice 1.

¹⁰ Yngve, V. H., "A Model and a Hypothesis for Language Structure", *Proc. Amer. Phil. Soc.*, 104 (1960), no. 5.



Lattice 1. The primary OPs

This system is evidently not only a partially-ordered set, but a lattice.

As regards the secondary OPs, it is evident on the same principles that $\underline{O.X}$ and $\underline{S.X}$ both include \underline{X} whatever value this may have, and that likewise $\underline{ZC.X}$ includes \underline{X} . Where a substituent with an OP such as $\underline{O.X}$ is itself compounded endocentrically, we shall have as the OP of its dependents $\underline{OA.X}$ and this of course (as by (3) above) includes $\underline{O.X}$. It follows that to account for the secondary OPs we must erect, on each point of the above lattice 1, a further lattice isomorphic with it. And to account for tertiary OPs we must further erect on every point of this compound figure again a lattice isomorphic with lattice 1. The system, though in principle an open one, soon terminates as we have seen; I shall disregard tertiary OPs in what follows, and assume that primary and secondary OPs alone exist, as this greatly shortens the exposition and loses no essential point. In any case, as we shall see, the system of *total* paradigms depends only on the simple lattice 1.

The methods of assigning OPs to particular positions in all possible bracket-structures, as outlined above, may conveniently be summarized for further reference in the condensed form shown in Appendix II.

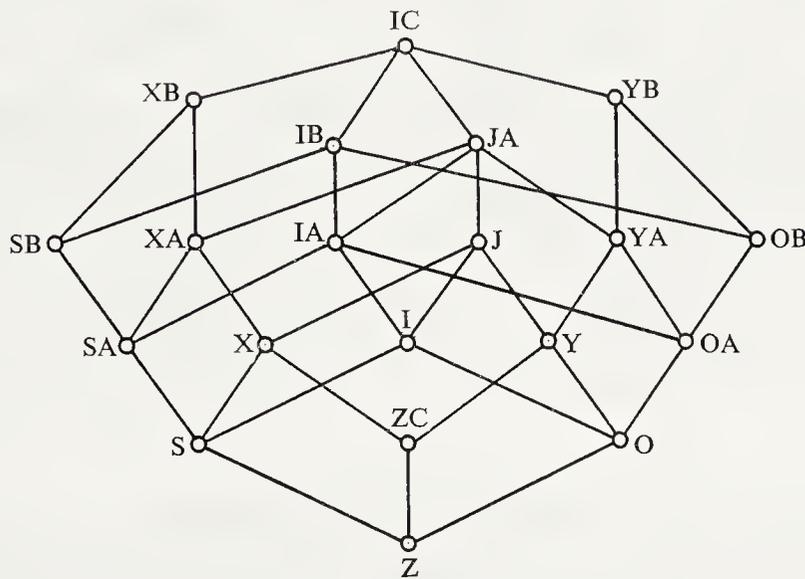
THE SYSTEM OF TOTAL PARADIGMS

We have already seen that the total paradigm of any substituent is simply the set union of the occasional paradigms of its occurrences. This enables us to derive the system of total paradigms straightforwardly from that of OPs. As regards the primary TPs the procedure is particularly simple.

If a given substituent is used invariably in chains of determination belonging to a given OP \underline{X} , the OPs of all its occurrences are identical. In such a case I shall denote the corresponding TP by X , simply omitting the underlining. If a substituent is used sometimes in chains belonging to the OP \underline{X} , and sometimes with OP \underline{Y} , then by

definition the TP will be the set union of X and Y , which I shall denote by $X \cup Y$.

The system of primary TPs can thus be obtained from the lattice of primary OPs by adding to it points corresponding to all the set unions concerned which are not already represented in lattice 1. Thus, the set $S \cup SA = SA$ which is already there; but for example $S \cup ZC$ is not identical, in set theory, with its join in lattice 1, which is IC , and so a new point has to be introduced in the system of TPs to represent this case. The result of adding all the necessary set unions is the following lattice:

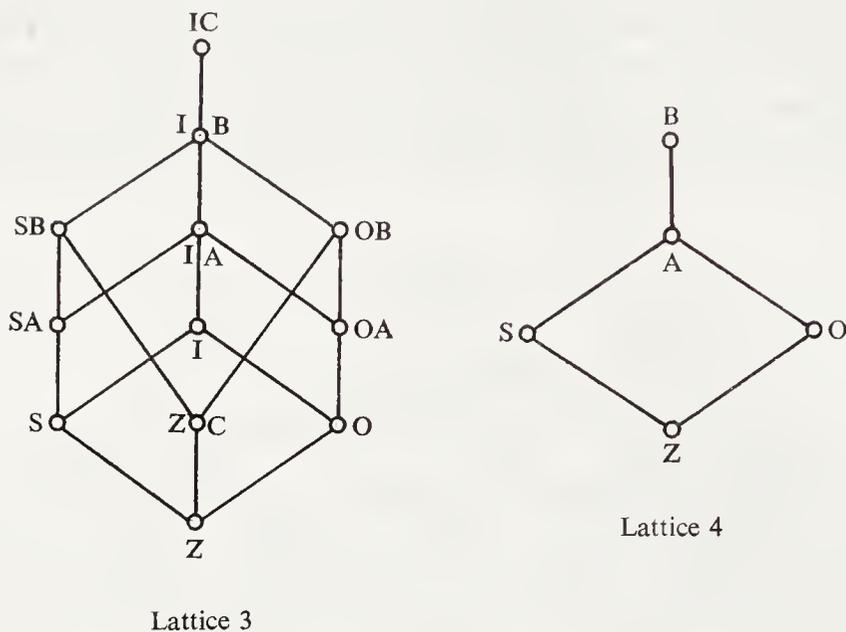


Lattice 2. The primary TPs

However, it is quickly found in practice that many of the distinctions which this lattice allows us to make are redundant. For instance, the point XA represents the total paradigm of a substituent able to the function either as a subjunction (ZC), or as an adjective (SA). While, of course, it is quite likely that some word exists in some language which has this combination of functions, it is safe to say that no language will have an *important class* of substituents characterized in this peculiar way. Actual languages operate with a simpler system of TPs than what is theoretically possible. Still further simplifications are possible if we restrict our attention to *compound* substituents. On account of the practical importance of these simplified systems it is worth while to give them briefly here.

The first stage of simplification consists in confounding those points, representing the combination of ZC with other functions, which are of no value in the description of actual languages. For this purpose we confound X, XA, XB , with SB ; Y, YA, YB with OB ; and J, JA with IC . This gives us lattice 3 shown below. The second stage eliminates the TPs IC and ZC , which stand for functions which need not be attributed to any compound substituents except with conjunct structure; at this stage we also confound SB, OB , and IB , also SA, OA , and IA . This preserves only the minimum

set of distinctions required to preserve the distinction between endocentric and exocentric structures, and yields the lattice 4:



THE SECONDARY TOTAL PARADIGMS

Lattice 2, representing the complete system of primary TPs, still contains no representation of secondary TPs, corresponding to the secondary OPs such as are represented by S.O, O.S.SA, & c. These must now be brought into the system. The TP of a substituent whose occurrences have OPs including, say, SO, S.SA, S.SB, is, as we saw above, the set union or join of the TPs proper to substituents having only one each of these various uses. The TPs of these latter we may denote by just omitting the underline, as *S.O*, *S.SA*, *S.SB*. Now all these are modifications or special cases of the one primary TP *S*. Their union therefore must be included in *S* (i.e. be a successor of *S* in the lattice of TPs), while at the same time including each of the separate TPs named. In the example cited the second elements in the OP symbols, which are SA, SB, O come from both sides of the OP lattice 1, so that their set union is identical with the union of all the sets represented in that lattice; this being so, the union of *S.O*, *S.SA*, *S.SB* is identical with the union of all secondary TPs beginning with *S*, which is simply *S* itself. Thus, we can assert of the system of TPs we are seeking, that

$$S.O \wedge S.S = S \quad (1)$$

We have already seen that the OP of a compound substituent, being precisely that part of the chains of determination of its several components which is common to all of them, is the intersection, in the system of OPs, of the OPs of its components. In particular, the OP of a substituent whose components are O.X and S.X is thus

$\underline{S.X} \cup \underline{O.X} = \underline{X}$. Since the system of OPs must be contained as such as a subset of that of the TPs, it follows that

$$O.X \wedge S.X = X \quad (2)$$

But, as a particular case of this, we have that $O \wedge S = Z$. We may subsume this case under the same formula as (2) by writing, for the TP corresponding to \underline{O} , not O but $O.Z$; and likewise add a Z after every OP of primary form. Thus we have to compare with (2) the equation

$$O.Z \wedge S.Z = Z.Z \quad (3)$$

Using the same notation, equation (1) can be rewritten as

$$S.O \wedge S.S = S.Z \quad (4)$$

On comparing (3) and (4) we see that whereas when S and O appear as primary components of TPs their *meet* is Z , when they figure as secondary components, as in equation (4), it is their *join* which has to be identified with Z . Now in lattice 2 it is the case that $S \wedge O = Z$; it therefore appears that the lattice representing the secondary TPs must be the *dual* of lattice 2. The two lattices clearly express the same set of relationships, but have to be interpreted (in the only cases which occur in applications to language) in the dual sense. However, if we adopt this convention, equation (2) also has to be rewritten; it becomes

$$O.X \wedge S.X = Z.X \quad (5)$$

which shows that the new symbol $Z.X$ must be given the same interpretation as the old X , which we have already decided to rewrite as $X.Z$. The complete system thus contains *two* lattice points representing each primary TP, one of the form $X.Z$ and the other of the form $Z.X$; this occurs for every value of X . From this complication we gain the advantage that *every* formula of the form $X.Y$ corresponds to a point on the lattice. It at once follows, from elementary lattice theory, that the lattice representing the complete system of primary and secondary TPs together is the set product of lattice 2 with its dual.

Given this lattice (which, it will be realized, is much too large for convenient representation by a graph), we can find the TP of a compound substituent from the TPs of its components by a simple extension of the algorithm that holds for OPs. This is, that the required result is the meet of the two component TPs in the self-dual-product lattice, with the proviso that if this is of the form $Z.X$, for any X , it must be replaced forthwith by $X.Z$. This system, as already indicated, can be considerably simplified in practice. We have reason to believe that the lattice product of lattice 3 with the dual of lattice 4 would give a sufficient description of the syntactic variety of most if not all actual languages. But these are matters still under test.

RECORDING OF TOTAL PARADIGMS IN DICTIONARY READINGS

In any actual language-translation procedure, the syntactic structure must be elucidated, by a strictly mechanizable algorithm, from information supplied solely by the

APPENDIX I

1. (a) RULES OF THE NOTATION

1.0. *The graphic elements*

a *syntactic formula* is a sequence of signs forming either a substituent formula or a sentence formula, each sign being either

- 1.0.1. a *term*, that is, any lexeme in the given language represented by its conventional spelling, or by an assigned abbreviation; or any variable symbol taking such as its values. Or,
- 1.0.2. a *punctuation*, which is any one of the signs (. or , the use of which is defined below.

1.1. *The Representation of Componenty*

A *substituent formula* is any term, or any sequence of substituent formulae (called its *components*) enclosed between (and) and possibly separated by , or .

1.2. *The Representation of Equipollence* (for definition, cf. fn. 9)

Any component of a substituent formula

1.2.1. is equipollent with the substituent formula if and only if it is followed by the punctuation ,

1.2.2. if not last in its substituent formula, is equipollent with the first component

1.2.3. is not equipollent with any substituent formula separated from it by brackets, except when this follows by inference from 1.2.1 & 1.2.2.

1.3. *The Representation of a Complete Sentence*

Any sequence of substituent formulae followed by the punctuation . is a *sentence formula*.

(b) EXAMPLES

dogs [as in *dogs bark.*] is a term.
so also is *D* [as in *D B.*]

so also is *x* when we say that *x* is equipollent with *y* in the substituent (*x y z*)

dogs is a substituent formula
so also is (*barking dogs*,) of which *barking* is one of the components

(*barking dogs*,) is equipollent with its component *dogs* but not with *barking*.

in (*noisy barking dogs*,) *noisy* is equipollent with *barking* but not with *dogs*.

in (*tomorrow* (*never comes*,)) *tomorrow* and *never* are not equipollent. The formula (*noisy* (*barking dogs*,)) is wrong because it contravenes this rule.

dogs bark. is a sentence formula
so also is (*noisy dogs*,) (*often bark*,).

APPENDIX II

2. (a) RULES FOR THE ASSIGNMENT OF OPS

- 2.0. An OP is assigned to every term, and to every substituent formula; in the latter case the OP is deemed to be attached to the (and) which enclose it, but the punctuations , and . are not given any OPs.
- 2.1. A substituent formula, when followed by punctuation “.”.
 - 2.1.1. if it is initial in a syntactic formula, has OP Z
 - 2.1.2. but otherwise, has OP Q
- 2.2. A substituent formula, followed by another carrying the OP Y which is itself
 - 2.2.1. followed by “.” has the OP S
 - 2.2.2. followed by a 3rd. subst. formula, has OP Y
 - 2.2.3. followed by) with the OP X, has OP S.X
 - 2.2.4. followed by “,” has the OP Y.A
- 2.3. A substituent formula followed by) with the OP X and
 - 2.3.1. preceded by “.” has the OP ZC.X
 - 2.3.2. preceded by “,” has the OP IC
 - 2.3.3. preceded by any other sign , has the OP O.X
- 2.4. A substituent formula followed by “.” has the same OP as the substituent formula of which it is a component

(b) EXAMPLES

terms & punctuations: *I (come shall,)*.
 OPs attached to them: S O OA O O

in *Yes*. the term *yes* has the OP Z
 in *I came*. the term *came* has OP Q

in: *I (it doing) (like shall,)*.
S S S O S O OA O O
 (*it doing*) is followed by (*like shall*).

I is followed by (*it doing*) & this by a 3rd
it is followed by *doing* where) has OP S
like is followed by *shall*,

in: *He (I came. when) up got,)*.

S O OA S O Z ZC OA OA OA O O
 when before) with OA after . has ZC OA

in (*men, women, and*) the *and* has OP IC
 in 2.2. exple, *doing* before) with S has O.S

in (*men, women, and*) *men* has the OP S
S S S IC S

dictionary readings attached to the individual words (lexemes) encountered in the running text. The form which these readings are given is a determining factor in the efficiency of the procedure. For this purpose, it would be inconvenient to code the information relating to a given word in the form of its *own* TP. What we want to know about it in a given context is what larger groupings (larger substituents) it could be functioning as a component of. In the programs currently under test by CLRU we have accordingly devised for our dictionary readings the form which we call the "participation-class" of each word. This consists in the answers to a set of questions of the form "How can this word participate in compound substituents of type X?" The "how" is interpreted currently in four bits, of which two tell whether it can be governor, dependent, both or neither, and the other two give information about its possible position, first or last, in the expression of X-type substituents in its language. The classification of substituents, from which we get the values of X, is based on their function (represented by the TP) and their constitution (whether they are endocentric, exocentric etc., and whether either of their components have secondary TPs). We are working on a supposedly complete list of such substituent types, of which, after omitting combinations of function and constitution which are logically impossible, and confounding types likely to be unprofitable to distinguish, we recognise 22, together with conjunct groups which are treated separately as presented peculiar problems of recognition.

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SOME SYNTACTIC RULES FOR MANDARIN*

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The systematic study of the Chinese language from a grammatical point of view was ushered in by the work of Mǎ Jiàn Zhōng¹ toward the end of the last century.² Although the *Mǎ Shì Wén Tōng*³ itself was a rather Procrustean effort of fitting the Chinese language into a model based on Greek and Latin, progress made on Chinese grammar since that time has been sizeable. Sparked on by such nationwide activities as the May Fourth Movement, mass literacy campaigns, orthography reforms, and alphabet rivalries, there has accumulated a substantial linguistic literature⁴ ranging from dialectology to sociolinguistics.

Among the "complete" grammars of the contemporary language in Chinese, we may cite the important works of Wáng Lì, Lǚ Shù-Xiāng, Gāo Míng-Kǎi, Lí Jīn-Xī, and many others. These grammars provide a wealth of diverse materials, as well as grammatical insights on how these materials are to be organized. They are invaluable in that they laid the foundation for the field, much as the works of men like Jespersen, Poutsma, etc. have done for Modern English. However, in the light of recent developments in linguistic theory, I believe that there are several basic aspects in which subsequent grammatical research should depart from the approach of these pioneering works. Since these issues have been discussed at length in the recent linguistic literature, though not necessarily with respect to Mandarin grammar, I will only briefly identify them rather than give detailed arguments. The rest of the paper will be devoted to presenting some syntactic rules as a preliminary step toward constructing a generative grammar for Mandarin,⁵ and discussing the implications of the program of research exemplified by these rules.

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¹ Chinese references and examples are given in the pinyin notation, cf. *Hànyǔ Pīnyīn Cíhuì* (Peking, 1958).

² For discussions of the history of grammatical studies on the Chinese language, cf. Chén Qí Xiáng, *Yǔyánxuéshǐ Gàiyào* (Peking, 1958), pp. 325-332; Hú Fù and Wén Liàn, *Xiàndài Hànyǔ Yǔfǎ Tànsuǒ* (Peking, 1955), pp. 159-180; Wáng Lì Dá, translator and editor, *Hànyǔ Yánjiū Xiǎoshǐ* (Peking, 1959).

³ First appeared in 1898. The sentences analyzed therein range from those of the early Qín period (221-206 B.C.) up to those from the writing of Hán Yù (768-824) of the Táng dynasty.

⁴ For one sector of this literature, cf. my *Synchronic Studies in Mandarin Grammar — A Selected Bibliography* (= *POLA Report No. 2, Ohio State University Research Foundation*) (Columbus, 1962).

⁵ For the theory of grammar on which the present work is based, see N. Chomsky, *Syntactic*

The central task in linguistic research, as I understand it, is the design of a grammar which can at least meet the following two conditions. The grammar must be able to generate (i. e., provide a grammatical explanation for) all (and only) the sentences of the language in a simple and precise way. Also, the grammar must be interpretable as having a formal structure that is compatible with grammars of other natural languages; that is, the grammar must be based on a linguistic theory that is not biased toward any particular language.

Most of the earlier grammars of Mandarin have these principles implicit in their design to varying degrees. However, certain aspects of these grammars are not consistent with these principles.

One aspect of these grammars may be called "non-integratedness". For example, a given grammar may contain descriptions of BA-sentences, reduplication, and the existential verb *yǒu*. These descriptions usually occur in different chapters, perhaps in those dealing with sentence types, morphology, and verb classes respectively. In this kind of arrangement, we run into difficulty when we want to exhibit the relations between these three subsystems, as any adequate grammar must (and perhaps use these relations to simplify the grammar). For instance, some verbs can take BA only if they have been reduplicated (or something else, cf. rule CS-5), whereas *yǒu* neither reduplicates nor takes BA. These facts explain why the following starred sequences are ungrammatical:

- | | |
|--|------------------------------------|
| (1) a. <i>Nǐ pīpíng péngyǒu.</i> | (2) a. <i>Nǐ yǒu péngyǒu.</i> |
| b. <i>Nǐ pīpíng pīpíng péngyǒu.</i> | b. * <i>Nǐ yǒu yǒu péngyǒu.</i> |
| c. * <i>Nǐ bǎ péngyǒu pīpíng.</i> | c. * <i>Nǐ bǎ péngyǒu yǒu.</i> |
| d. <i>Nǐ bǎ péngyǒu pīpíng pīpíng.</i> | d. * <i>Nǐ bǎ péngyǒu yǒu yǒu.</i> |

But since each of these facts relates to several portions of a non-integrated grammar, as most grammatical facts would, repeating them at each relevant portion violates simplicity, not stating them violates completeness, and stating them at any one portion seems arbitrary. Furthermore, it is extremely difficult to extract from such a grammar any sort of coherent formal structure.⁶

Another aspect of the earlier grammars that I would like to comment on is the definition of grammatical structures by semantic or "notional" criteria. In this connection, it is important to distinguish between using semantic labels heuristically (or for pedagogical purposes) from using semantic criteria in the design of a grammar.

Structures (The Hague, 1957). This book has been edited and translated into Chinese by Qīng Yūn Qīng and myself, entitled *Biànhuànlǚ Yǔfǎ Lǐlùn*, forthcoming.

⁶ Our criticism here is close in nature to that voiced by Lǚ Jì-Píng in his review. ". . . HÀNYŪ méi nénggòu tíchū íg kēxuéd, shíhéyǔ hànǚ tediǎnd yǔfǎ tǐxì . . . dú wán zhì hòu, qián hòu liánxì qǐlái ikàn, jiù huì gǎndào zài tǐxishàng bú gòu yánmì. Tā hǎoxiàng yùdào izhōng xiànxìàng jiěshì izhōng xiànxìàng, ér bù néng bǎ zhèxiē xiànxìàng liánxì qǐlái, gǎikuò qǐlái, jiànli íg yánmìd tǐxì lái tōngshè íqiè". See *Zhōngguo Yǔwén*, June 1962, pp. 279-284; p. 283. If we take seriously Grammont's characterization of language as "un système où tout se tient", then clearly a grammar must be integrated to reflect this feature of language.

The former, when used carefully, is not only helpful mnemonically, but also suggestive of correspondences that might or might not exist between the semantic and grammatical structures of the language. The latter usage, on the other hand, is harmful on two counts. One, we will have introduced criteria whose vagueness violates our desire for precision in our grammar. Secondly, since the semantic and grammatical structures are not isomorphic, these criteria will frequently lead us to wrong decisions. An example from English is the active-passive relation which has been mistakenly thought to be "meaning-preserving". Had we taken this semantic criterion seriously, we would be disallowed from considering as being in an active-passive relation such sentences as;

- (3) a. Everybody loves somebody.
b. Somebody is loved by everybody.

which are clearly not equivalent semantically.

As an example from Chinese grammar, we have the so-called "execution form" with the prepositions *bǎ* and *jiāng*. The semantic criterion which Professor Wáng Lì offered is quoted here:⁷

Chùzhìshì shì bǎ rén zěnyàng ānpái, zěnyàng zhìshì, zěnyàng duìfù; huò bǎ wù zěnyàng chùlǐ; huò bǎ shìqíng zěnyàng jìnxíng.

This statement exemplifies the intrinsic vagueness that accompanies notional definitions. As an instance of the non-correspondence between the sentences notionally defined in this way and the formal class of BA sentences, let us consider the following cases:

- (4) a. *bǎ tā dǎle.*
b. *bǎ shū mǎile.*
c. *bǎ gānz ihuǎng.*
(5) a. *bǎ nǐ wàngle.*
b. *bǎ yákèle.*
c. *bǎ liǎn ihóng.*

Professor Wang admits that these sentences are formally the same. However, he is led by the above-quoted definition to treat these two groups as distinct types, calling them *chùzhìshì* (execution form) and *jìshìshì* (consecutive form) respectively, adding a definition for the latter type that is at least as vague as that of the former.⁸ Hence his grammar suffers both in precision and simplicity.

⁷ *Zhōngguo Xiàndài Yǔfǎ*, pp. 161 ff. This book was first published in 1943-44. Page references here are to the (Hongkong, 1959) edition.

⁸ *Zhōngguo Yǔfǎ Lǐlùn*, p. 170 f. First published in 1944-45. Page references are to the (Peking, 1955) edition. This terminological division into these types was objected to by Lü Shú-Xiāng in "Bǎ zòngfǎd yánjiù", pp. 126 ff., in *Hànyǔ Yǔfǎ Lùnwénjí* (Peking, 1955) (article was first published in 1948); and by Hú Fù and Wén Liàn, *op. cit.*, pp. 124 ff. For a defense of Wáng Lì's semantic definition, see Wáng Huán, *Bǎjiù hé Bèizhìjiù* (Shanghai, 1957), pp. 11-16. Sentences with *bǎ* are called the "disposal form", in some grammars, e.g., *Modern Chinese Reader* (Peking, 1958), pp. 434 ff.

The third and last aspect of the early grammars that will be discussed may be called “mixedness”. That is, the language described in these grammars is often taken from earlier chronological stages of the language, or from several dialects, or from several styles. For example, sentences are frequently cited from Hóng Lóu Mèng, or even Shǔi Hǔ Zhuàn, literature of some two or three hundred years standing, mixed in with those of modern colloquial speech. The inappropriateness of such dated sources is comparable to using Jonathan Swift for Modern English or Voltaire for Modern French.⁹ While it is true that as linguists we aim at being able to account for such phenomena as historical change, dialectal variation, and grammatical relations between styles, we can best hope to achieve this goal by trying to explain a rather delimited range of linguistic materials, say the set of “non-deviant” sentences of a contemporary dialect,¹⁰ and then project from this grammatical foundation onto phenomena less well understood. A grammar intended to indiscriminately account for several different systems at once would obscure the very distinctions and relations that we would like to uncover.

The syntactic rules presented in Appendix 1 (pp. 199-201 below) constitute but a small fragment of what a generative grammar of Mandarin may look like. These rules are essentially a formalization of much of the grammatical information which are discursively scattered in the literature of Chinese linguistics. Although an adequate generative grammar for Mandarin will be long in forthcoming, as it will be for any language, the sketch presented here is intended to be a modest first step in that direction. Clearly, many of these rules will need to be modified and refined, and numerous grammatical categories will need to be added as increasingly more facts are built into the grammar.

The rules are of two sorts,¹¹ constituent structure (CS) rules and transformational (T) rules. Most of the units manipulated by these syntactic rules at the various levels of representation are theoretical constructs posited for the simplification of the grammar, called formatives¹² (e.g., NOM, DEM, ?, etc.). In the CS rules, one formative is rewritten into one or more formatives or morphemes. Alternative rewritings

⁹ Professor Wáng Lì indicates the inadequacy of his own source materials in the preface to the new edition of *Zhōngguó Yǔfǎ Lìlùn*, p. 6, and again in his lǐyán to *Zhōngguó Xiàndài Yǔfǎ*, p. 1. Unfortunately, the opinion has often been expressed to confuse the issue that the grammar has changed very little in the last 2-300 years, cf. Zhū’s preface to *Zhōngguó Xiàndài Yǔfǎ*, p. 4.

¹⁰ By this we of course do not mean to suggest that there is an algorithm for such delimitation, or that this kind of algorithm is to be sought at the present time. Hilary Putnam has provided an enlightening discussion of the problems of “deviancy”, “corpus”, etc., in his “Some issues in the theory of grammar”, in *Structure of Language and Its Mathematical Aspects*. Ed. R. Jakobson (Providence, 1961), pp. 25-42.

¹¹ For a full discussion of the form of the grammar and its underlying theory, see the works cited in footnote 5; also R. B. Lees, “O pereformulirovanii transformacionnyx grammatik”, *Voprosy jazykoznanija*, 6.6 (Moscow, 1961); and C. J. Fillmore, *The Position of Embedding Transformations in a Grammar* (= *POLA Report No. 3, Ohio State University Research Foundation*) (Columbus, 1963).

¹² This term was first proposed by D. L. Bolinger, “On defining the morpheme”, *Word*, 4.1, (April 1948), pp. 18-23, p. 21.

for the same formative are enclosed in braces, e.g., in CS-8, DET may be rewritten as either DEM ANN or NUM ANN, which may represent, for example, *nèi zhāng (zhǐ)* or *sān zhāng (zhǐ)*. A CS rule may be restricted to apply only in a specified context, e.g., in CS-10, ANN is written ANN; only if it precedes a N of class *i*; for example, it may be rewritten *zhāng* if the following noun is from a class of nouns which contains *zhǐ*, *zhuōz*, *huà*, etc. Parenthesized items are optional in the application of the rule, e.g., in CS-3, VP_{NTR} may be rewritten as Vi_{action}, but not *EMP Vi_{action}. These may be illustrated respectively by *(tā) kū*, *(tā) néng kū*, *(tā) hěn néng kū*, and **(tā) hěn kū*.

The T-rules are either obligatory or optional. Obligatory rules are starred. They must be applied if the sequence produced by the grammar is to be grammatical. For example, if we did not apply *T_{DIR}, the grammar would produce such sequences as **tā zǒu chū lái mén* instead of *tā zǒu chū mén lái*. Also T rules are either simple or double. It is primarily to the double T rules, that a generative grammar owes much of its recursive power with their building of increasingly complex sentences out of simple ones. An example is T_{Qbu} which takes such sentences as *nǐ máng* and *nǐ bù máng* and produces *nǐ máng bù máng?*

With these preliminary remarks as to the form of the grammar, let us briefly sample some of the rules proposed, keeping in mind their highly tentative nature.¹³ The first rule reflects the belief that all (or at least the major portion of) Mandarin sentences should be considered either directly of the form NOM VP, such as *nǐ mǎile zhǐ*, or to be most simply derivable from this underlying form, such as, *zhǐ nǐ mǎile*, *nǐ bǎ zhǐ mǎile*, *zhǐ gěi nǐ mǎile*, *zhǐ nǐ mǎile méiyǒu?*, *nǐ mǎile zhǐ méiyǒu*, and so on. The formal justification for this decision would have to come eventually from demonstrating that alternative decisions would complicate the grammar, such as treating the above sentences as to be derived independently from each other, or using some other underlying form, say VP NOM. Intuitively however, we feel we are on solid ground because it is clear to us that these sentences are grammatically related, and our choice of the underlying form merely follows the insight of the traditional Chinese grammarians who inevitably discuss first and at great length the "normal" sentence, i.e., the *zhǔdòngshì*.¹⁴

CS-2 states that there are five major types of transitive verb phrases, three of which take a nominal, one takes any kind of sentence, and one takes a special kind of complement. We would say, as an example of VT_{action}, that a sentence like *nǐ mǎile* is derived from CS-2 where the NOM in the VP is deleted by a later transformation, even though it bears a superficial resemblance to *nǐ xǐng*, which is derived from CS-3

¹³ In many matters of grammatical decision and terminology used here, we have followed the works of Professor Y. R. Chao. See his *Mandarin Primer* (Cambridge, Harvard University Press, 1948).

¹⁴ For example, Lí Jīn-Xī, in his *Hànyǔ Yǔfǎ Jiàocái* (Shanghai, 1957) says that "a sentence has only two major elements, the subject and the predicate", p. 16. These two elements correspond respectively to our NOM and VP. In fact, this grammatical decision forms the fundamental principle in Lí's method of sentence diagramming, see p. 27 of this *Xīnzhù Guóyǔ Wénfǎ*, rev. ed. (Shanghai, 1957); in this work he uses *shùyǔ* instead of *wèiyǔ* to denote the predicate.

without having had a NOM at all. The formal reasons for this distinction are quite clear — these sentences are related to different classes of constructions. For example, *nǐ mǎile* can take a NOM whose selectional restrictions are imposed by *mǎi*, and it can occur before or after *nǐ* and after *mǎile*. As indicated in CS-5, it can take on certain complements in place of the aspect particle *-le* (*nǐ mǎid zháo shū*), it can reduplicate (*nǐ mǎi mǎi shū*), etc., whereas *nǐ xǐngle* cannot.

The NOM formative in CS-4 is divided into nonhuman nouns which can follow determinatives, pronouns which can precede men, human nouns which can do either. and location nouns and names which do neither. The human nouns, pronouns and names share the property of being the indirect object of double-object transitive verbs, as indicated in CS-5. In CS-4, the determinative and the *men* cannot co-occur. Thus we have *sìg rén* and *rénmen*, but not **sìg rénmen*, and so on.

The EMP formative which occurs in some of these early rules is later divided into pre-posed and post-posed ones, such as in *hěn máng, máng jile*. The complementary distribution between them is reflected here by the mutually exclusive choices in CS-9, which makes it impossible for the grammar to produce forms such as **fēicháng mángd hěn, *shifēn máng tòule*, etc. The rule also shows that only preposed emphatics can be selected before auxiliary verbs (*hěn néng kū* but **néng kūd hěn*), and transitive verbs of quality (*hěn xiàng bàba* but **xiàng bàbad lihai*). There is another type of EMP that duplicates adjectives, DUPA, which has not been included here. It is also in complementary distribution with both EMP_{pre} and EMP_{post} (*gāo gāod gèz* but **hěn gāogāod gèz, *gèz gāogāo jile*).

Now, to illustrate briefly the purposes of some of the transformations. T_{Qbu} forms a question by conjoining two strings of formatives where the corresponding NOM's and ADJ's are identical. The question mark is inserted in the transform to bring into play later phonological rules which might adjust the intonation. T_{tel} produces sentences like *wǒ qǐng tā hē chá*, from *wǒ qǐng COMP_{tel}* and *tā hē chá*. The X may stand for such additional elements as *xiǎng, bù xiǎng, hěn bù xiǎng*, etc. The Y may stand for NUM ANN or NUM ANV or adverbs, and so on, as *wǒ qǐng tā hē sān bēi, wǒ qǐng tā hē sān cì, wǒ qǐng tā hē màn idiǎn*. In this rule, as in the other rules, X and/or Y may be null, T_{NOM} produces sentences like *mángd péngyǒu méi gōngfu* from *péngyǒu mángd* and *péngyǒu méi gōngfu*. T_{d.o.1.} forms sentences like *wǒ gěi tā shū kàn* from *wǒ gěi tā shū* and *tā kàn shū*. The obligatory transformations which follow arrange the formatives into the proper order.

The task of designing a generative grammar cannot be considered complete without a set of end rules which convert the sequences of abstract units into actual acoustical forms of speech. These rules need for their construction a great deal of precise phonetic knowledge of the language. As examples, the following facts have been obtained in our recent work in this area. (1) In non-final position, everything else being equal, tone 2 has the greatest duration, then tones 3, 1 and 4. In citation syllables, the order is 3, 2, 1 and 4. (2) Within a fixed position, syllable finals have comparable durations regardless of the number of their constituent phonemes. (3)

When not reduced, the vowels in *ci*, *chi*, *si*, *shi*, *zi* and *zhi* have distinctly vocalic properties and approximate a lower high-central position both physiologically as seen in radiograms, and acoustically as seen on a two-formant plot. Such facts need of course to be systematized within a theoretical framework before they can be incorporated into the grammar. The accumulation of this type of physical information for Mandarin on a significant scale has barely begun.

In the construction of this kind of grammar, one is forced to be precise and explicit in each rule. As a direct reflection of the "tout-se-tient" or integrated character of natural language, all the rules are inextricably interwoven and the formulation and/or positioning of one individual rule may have severe effects on the overall complexity of the grammar. The implications of such grammars are both deep and many for our understanding of linguistic structure. They are especially relevant for recent attempts to simulate linguistic processes by machine.

We will discuss two of these implications here. They are: the explication of grammatical ambiguity, and the search for linguistic universals.

It is important to bear in mind that the ability to account for ambiguities is *not* a prior condition placed on grammars. Had we imposed such a defining property (i.e., our grammar is required to have more than one way of producing every ambiguous sentence), we would hardly have "explained" anything with the resulting grammar. But if a grammar, justified on the grounds of formal simplicity, can provide a basis for predicting certain types of linguistic intuition, in this case ambiguity, it is an important external validation.

We will now consider the following pair of forms taken from Professor Chao's recent article on ambiguity in Chinese.¹⁵

- (6) a. *zhīmá 'dàd shāobǐng*
 b. *'zhīmá dàd shāobǐng*

Although these forms are easily distinguished by the placement of stress,¹⁶ they are ambiguous on a syntactic level. Not only are they to be both represented by the sequence N Adj de N, they are also bracketed the same way, i.e. ((*zhīmá dà*)*d*)*shāobǐng*.

If the brackets are labeled with formatives, then the elements in (*zhīmá 'dà*) and (*'zhīmá dà*) can be regarded as standing in different constructions, resulting in distinct constitutes.¹⁷ But aside from the rather ad hoc nature of this solution (and the consequent increase in the number of formatives), some basic facts about sentence formation and sentence relations would be obscured. The nominalization transformation is a very general process which forms many types of nominals, such as *lái d háiz*, *chífàn d háiz*,

¹⁵ See Professor Chao's "Ambiguity in Chinese", in *Studia Serica Bernhard Karlgren Dedicata*. Ed. by S. Egerod (Copenhagen, 1959), pp. 1-13. His "covert forms corresponding to different meanings" (p. 10) are mostly those whose structural ambiguity cannot be conveniently explained by IC but by transformations.

¹⁶ They do become homophonous, however, when differential stress is applied to the first word in 6a.

¹⁷ Here we follow C. F. Hockett's use of these terms; see p. 164 of his *A Course in Modern Linguistics* (New York, 1958).

bìnglèd háiz, etc., from *háiz lái*, *háiz chīfàn*, *háiz bìngle*. It seems very likely that in an adequate grammar (6a) would also be produced this way, i.e., *zhīmá 'dàd shāobǐng* is a nominalization of *shāobǐng zhīmá 'dà*. Similarly, many other forms can be given which are transforms respectively of *péngyǒu miànz dà*, *xìyuàn rén duo*, *fángz jiàqián guèi*. On the other hand, we have no forms like **shāobǐng 'zhīmá dà*, *zǔibā 'yīngtáo dà*, *xīn huǒ rē*, or *shì tiān dà*. Although I do not know how sentences like (6b) are to be accommodated within the grammar at present; it is clear from the above considerations that they are not to be derived like (6a). Hence, the distinction between (6a) and (6b) emerges as a result of considerations of simplicity in the design of the grammar, with a consequent explication of a type of syntactic ambiguity.

The other implication of this kind of grammar that I would like to mention has to do with the recent discussions on the concept of "sentence depth".¹⁸ It has been observed that in a number of languages,¹⁹ the constituents in more complicated sentences tend to be multiply related whereas those in simpler sentences are usually binarily related. Everything else being equal, the number of nodes in an average path is less for the former type of CS than for the latter (deeper) type.

Since in our conception of grammar, complicated sentences are generally built from simple ones by means of T-rules, the question is whether T-rules characteristically reduce depth. While the CS of some sentences can be automatically determined from their underlying CS rules, knowledge about how T-rules assign CS to their transforms is still lacking.²⁰ Conclusive answers to the above question will need to be based on such knowledge as well as grammars which are much more complete than the fragment presented here.

As an initial step, however, I have been working on the CS of sentences related by such "meaning-preserving" T-rules as those of transposition, BA and BEI. For instance, many Chinese grammarians have stated that in Mandarin the verb and its object never get separated very far. When they are separated by an elaborate verbal complement or a heavy modificational structure before the object, these rules are obligatory in some cases, and in others their application is preferred.

Preliminary investigations have shown that this observation may be an instance of the depth-reduction phenomenon mentioned above. For example, consider the following set:

¹⁸ See R. E. Longacre, "String constituent analysis", *Language*, 36.1 (1960), pp. 63-88; V. H. Yngve, "A model and a hypothesis for language structure", *Proceedings of the American Philosophical Society* 104.5; Chomsky (pp. 12 ff.) and Lees (pp. 266 ff.) in *Structure of Language and Its Mathematical Aspects*, cf. fn. 10.

¹⁹ These observations are mostly for English, but Longacre also gives examples from several Amerindian languages and Yngve cites several other languages (including Chinese) as conforming to his depth hypothesis in his "Computer programs for translation", *Scientific American*, June 1962, p. 76. It should be pointed out that "depth" is used here in a general sense whose exact nature is not yet well understood. There appear to be striking counter examples to Yngve's method of counting depth; cf. C. J. Fillmore's description of Japanese in *Proceedings of the Princeton Symposium on MT-Oriented Syntactic Analysis*. Ed. Harry Josselson, 1962.

²⁰ For a hypothesis regarding how elementary T-rules assign CS, see Lees, *Vopros jazykoznanija*, 6.6.

- (7) a. $*(T\bar{a}) (((m\grave{a}d) (h\check{e}n\ lihai)) (n\grave{e}ig\ r\acute{e}n))$.
 b. $(T\bar{a}) ((b\check{a}) (n\grave{e}ig\ r\acute{e}n) ((m\grave{a}d) (h\check{e}n\ lihai)))$.
 c. $(T\bar{a}) ((m\grave{a}) (n\grave{e}ig\ r\acute{e}n)) ((m\grave{a}d) (h\check{e}n\ lihai))$.
 d. $(N\grave{e}ig\ r\acute{e}n) (t\bar{a}) ((m\grave{a}d) (h\check{e}n\ lihai))$.
 e. $(N\grave{e}ig\ r\acute{e}n) (b\grave{e}i) (t\bar{a}) ((m\grave{a}d) (h\check{e}n\ lihai))$.

This is an instance where the application of one of these T-rules is obligatory. If our CS analysis is correct, and we have no way of knowing this for sure at present, then 7c-e are all less "deep" than 7a,b.

Out of this kind of investigations, we may hope to arrive at precise statements for such valuable insights as "the verb and its object never get separated very far". Parallel investigations of diverse languages on this kind of formal basis, I believe, are our best hope toward uncovering underlying syntactic universals and a deeper understanding of natural languages.

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APPENDIX 1: ILLUSTRATIVE RULES FOR MANDARIN

Abbreviations:

ANN - auxiliary noun for nouns	EMP - emphatic
ANV - auxiliary noun for verbs	DUPV- reduplicative for verbs
ASP aspect	TRAN- transposition
DEM - demonstrative	V _{ta.o.} - double object transitive verbs
DET - determinative	

$$\text{CS-1} \quad S \rightarrow \text{NOM (bu)} \left\{ \begin{array}{l} \text{VP}_{\text{TR}} \\ \text{VP}_{\text{NTR}} \end{array} \right\} (1e)$$

$$\text{CS-2} \quad \text{VP}_{\text{TR}} \rightarrow \left\{ \begin{array}{l} \left\{ \left\{ \begin{array}{l} (\text{EMP}) \text{V}_{\text{tquality}} \quad (-le) \\ ((\text{EMP}) \text{AUX}) \text{V}_{\text{taction}} \end{array} \right\} \left(\begin{array}{l} \text{NUM ANV} \\ \text{DUPV} \end{array} \right) \right\} \text{NOM} \\ \text{V}_{\text{tclassifactory}} \\ \text{V}_{\text{tquotative}} \quad \text{S}^1 \\ ((\text{EMP}) \text{AUX}) \text{V}_{\text{ttelescopic}} \quad \text{COMP}_t \end{array} \right\}$$

$$\text{CS-3} \quad \text{VP}_{\text{NTR}} \rightarrow \left\{ \begin{array}{l} (\text{EMP}) \left\{ \begin{array}{l} \text{ADJ} \\ \text{V}_{\text{tstatus}} \quad (-le) \end{array} \right\} \quad (\text{NUM ANV}) \\ ((\text{EMP}) \text{AUX}) \text{V}_{\text{taction}} \left(\begin{array}{l} \text{NUM ANV} \\ \text{DUPV} \end{array} \right) \end{array} \right\}$$

- CS-4 NOM \rightarrow $\left\{ \begin{array}{l} (\text{DET}) \left\{ \begin{array}{l} \text{NOUN}_{\text{human}} \\ \text{NOUN}_{\text{nonhuman}} \end{array} \right\} \\ \left\{ \begin{array}{l} \text{NOUN}_{\text{human}} \text{ (men)} \\ \text{PRONOUN} \end{array} \right\} \\ \text{NOUN}_{\text{location}} \\ \text{NAME} \end{array} \right\}$
- CS-5 VT_{action} \rightarrow $\left\{ \begin{array}{l} \text{Vt}_{\text{action}} \left(\begin{array}{l} \text{COMP}_a \\ \text{ASP} \\ \text{DUPV} \end{array} \right) \left(\begin{array}{l} \text{TRAN} \\ \text{BA} \\ \text{BEI} \end{array} \right) \right) \\ \text{Vt}_{\text{d.o.}} \left(\begin{array}{l} \text{-le} \\ \text{guo} \end{array} \right) \left\{ \begin{array}{l} \text{NOUN}_{\text{human}} \\ \text{PRONOUN} \\ \text{NAME} \end{array} \right\} \left(\begin{array}{l} \text{TRAN} \\ \text{BA} \\ \text{BEI} \end{array} \right) \right) \\ \text{Vt}_{\text{perceptual}} \left(\begin{array}{l} \text{COMP}_p \\ \text{ASP} \\ \text{DUPV} \end{array} \right) \left(\begin{array}{l} \text{TRAN} \\ \text{BEI} \end{array} \right) \right) \end{array} \right\}$
- CS-6 NOUN_{nonhum} \rightarrow $\left\{ \begin{array}{l} \text{N}_{\text{inanimate}} \\ \text{N}_{\text{animate}} \\ \text{N}_{\text{abstract}} \end{array} \right\}$
- CS-7 NOUN_{location} \rightarrow $\left\{ \begin{array}{l} \text{N}_{\text{place}} \\ \text{N}_{\text{time}} \end{array} \right\}$
- CS-8 DET \rightarrow $\left\{ \begin{array}{l} \text{DEM} \\ \text{NUM} \end{array} \right\}$ ANN
- CS-9 EMP \rightarrow $\left\{ \begin{array}{l} \text{EMP}_{\text{pre}} \text{ in } \left\{ \begin{array}{l} \text{---AUX} \\ \text{---Vt}_{\text{quality}} \end{array} \right\} \\ \text{EMP}_{\text{pre}} \\ \text{EMP}_{\text{post}} \end{array} \right\}$
- CS-10 ANN \rightarrow $\left\{ \begin{array}{l} \text{ANN}_1 \text{ in --- } \text{N}_1 \\ \text{ANN}_2 \text{ in --- } \text{N}_2 \\ \text{ANN}_3 \text{ in --- } \text{N}_3 \\ \cdot \quad \cdot \quad \cdot \\ \text{ge} \end{array} \right\}$
- T_{Qbu} Structural Description $\left\{ \begin{array}{l} \text{NOM}_1 \text{ ADJ} \\ \text{NOM}_1 \text{ bu ADJ} \end{array} \right\}$
 Structural Change NOM₁ ADJ bu ADJ?

T_{tel}	S.D.	$\left\{ \begin{array}{l} \text{NOM}_1 \quad X \quad \text{Vt}_{tel} \quad \text{COMP}_{tel} \\ \text{NOM}_2 \quad \left\{ \begin{array}{l} \text{Vt} \\ \text{Vi} \end{array} \right\} \quad Y \end{array} \right\}$	Condition: $\text{Vt} \neq \text{Vt}_{class}$		
	S.C.	$\text{NOM}_1 \quad X \quad \text{Vt}_{tel} \quad \text{NOM}_2 \quad \left\{ \begin{array}{l} \text{Vt} \\ \text{Vi} \end{array} \right\} \quad Y$			
T_{NOM}	S.D.	$\left\{ \begin{array}{l} \text{NOM}_1 \quad \text{VP}_1 \\ \text{NOM}_1 \quad \text{VP}_2 \end{array} \right\}$			
	S.C.	$\text{VP}_2 \quad \text{de} \quad \text{NOM}_1 \quad \text{Vt}$			
$T_{d.o.l.}$	S.D.	$\left\{ \begin{array}{l} \text{NOM}_1 \quad \text{Vt}_{d.o.l.} \quad \text{NOM}_2 \quad \text{NOM}_3 \\ \text{NOM}_2 \quad \text{Vt} \quad \text{NOM}_3 \end{array} \right\}$			
	S.C.	$\text{NOM}_1 \quad \text{Vt}_{d.o.l.} \quad \text{NOM}_2 \quad \text{NOM}_3 \quad \text{Vt}$			
* T_{BA}	S.D.	$\text{NOM}_1 \quad \text{Vt} \quad X \quad \text{BA} \quad \text{NOM}_2$			
	S.C.	$\text{NOM}_1 \quad \text{BA} \quad \text{NOM}_2 \quad \text{Vt} \quad X$			
* T_{BEI}	S.D.	$\text{NOM}_1 \quad \text{Vt} \quad X \quad \text{BEI} \quad \text{NOM}_2$			
	S.C.	$\text{NOM}_2 \quad \text{BEI} \quad \text{NOM}_1 \quad \text{Vt} \quad X$			
* T_{TRAN}	S.D.	$\text{NOM}_1 \quad \text{Vt} \quad \text{TRAN} \quad \text{NOM}_2$			
	S.C.	$\text{NOM}_2 \quad \text{NOM}_1 \quad \text{Vt} \quad X$			
* $T_{EMP_{post}}$	S.D.	$\text{EMP}_{post} \quad \left\{ \begin{array}{l} \text{Vt} \\ \text{Vi} \\ \text{ADJ} \end{array} \right\}$			
	S.C.	$\left\{ \begin{array}{l} \text{Vt} \\ \text{Vi} \\ \text{ADJ} \end{array} \right\} \quad \text{EMP}_{post}$			
* T_{DIR}	S.D.	$\text{Vt} \quad \left\{ \begin{array}{l} \text{lái} \\ \text{qù} \end{array} \right\} \quad \text{NOM}$			
	S.C.	$\text{Vt} \quad \text{NOM} \quad \left\{ \begin{array}{l} \text{lái} \\ \text{qù} \end{array} \right\} \quad X$			
* T_{le}	S.D.	$-le \quad le$			
	S.C.	le			

APPENDIX 2: SAMPLE LEXICON

ADJ	→ <i>gāo, máng, kèqì, guèi, ...</i>
ANN	→ <i>gè, zhāng, liǎng, bǎ, suǒ, ...</i>
ANV	→ <i>cì, xià, huéi, tàng, ...</i>
ASP	→ <i>-le, zhe, guo, ...</i>
AUX	→ <i>kéyǐ, néng, huì, gǎn, ...</i>
BA	→ <i>bǎ, jiāng, ná,</i>
BEI	→ <i>bèi, gěi, ràng, ...</i>

DET	→ zhè, nà, měi, gè, ...
EMP _{post}	→ jile, tǒule, de hěn, de lihai, ...
EMP _{pre}	→ hěn, fēicháng, shífēn, ...
N _{abs}	→ lǐyóu, xiànxàng, yìjiàn, píqì, ...
N _{ani}	→ niú, mǎ, mǎyǐ, māotóuyīng, ...
N _{con}	→ lǚguǎn, zhuōz, lúnz, fādiànjī, ...
N _{hum}	→ nánrén, lǎoshī, kèrén, mǔqīn, mùjiàng, ...
N _{place}	→ lǐtou, wàitou, shàngbian, ...
N _{time}	→ zǎoshàng, wǎnshàng, libàitiān, ...
PRON	→ wǒ, nǐ, tā, ...
V _{taction}	→ dǎ,mà, mǎi, mài, qīfù, pīpíng, ...
V _{tclassificatory}	→ shì, xìng, ...
V _{td-o-1}	→ gào sù, gěi, sònggěi, ...
V _{td-o-2}	→ jiào, chēnghū, ...
V _{td-o-3}	→ dàng, ...
V _{tperceptual}	→ kàn, tīng, wén, ...
V _{tquality}	→ pà, xìn, xiàng, ...
V _{tquotative}	→ zhīdào, shuō, ...
V _{ttelescoping}	→ qǐng, ràng, jiào, ...
V _{istatus}	→ téng, bìng, ...
V _{iaction}	→ xǐng, lái, kù, xiào, rǎng, ...

BIPARTITE DIVISION OF SYLLABLES IN CHINESE PHONOLOGY

TUNG-HO TUNG

Abstract

It is still the common practice among students of Chinese phonology today to speak of the segmental components of syllables primarily in terms of "initials" and "finals" though many of them are obviously not the smallest sound units. The merit of doing so is here discussed.

Types of initials and finals in regard to their constituent elements in all dialects may be comprehensively illustrated as /t-, t'-, ts-/ and /-a, -ia, -ai, -iai, -an, -ian, -ain, -at, -iat, -ait/¹ respectively. Structurally, finals with /-n/ are comparable with those with /-i/, thus may also be regarded as diphthongs. Considered together with the distribution of tones, /-t/ is but allophonic to /-n/. Besides, neither /-n/ nor /-t/ behave the same as their counterparts in the initial position. The distinction between the initial and the final, therefore, is made on the basis of the partition of consonantal and non-consonantal sounds, as well as their relative positions in the syllable.

An initial may contain more than one consonantal sound and a final more than one nonconsonantal sound. Thus, a list of the initials and finals naturally brings out with economy the whole picture of the distribution of sounds in the two major sound classes.

The initials and finals of any dialect are counted only in dozens. They never make a formidable amount of units for us to operate with.

It may be added that forms like /t'-/, /ts-/ and /-ai/ are traditionally, yet justifiably, considered as simple phonemes, but not sequences of phonemes.

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¹ /t-/ stands for any simple consonant, /t'-/ any aspirated consonant, /ts-/ any affricate, /a/ any syllabic vowel, /i/ any non-syllabic vowel, /-n/ any final nasal, and /-t/ any final stop.

THE SIGNIFICANCE OF ACCENTUATION IN ENGLISH WORDS

GEORGE S. WALDO

1.1 In general, the location of the primary accent in English words is systematic, not "free". There are exceptions, but about 95% of the words are accentuated predictably. English has an unusually complicated system of accentuation, and this is presumably the reason its systematic nature has remained hidden so long. Before we discuss its significance, let us see as extended a sketch of the accentuation itself as space permits, noting the patterns and a few examples, of both regular forms and irregular ones. The details of the full description may be consulted when it has finished going through the press.¹

1.2 This sketch is presented in a form of Northern American, though Southern British differs from it little. Where variant pronunciations exist in the standard language, those choices have been made that fit the patterns and keep exceptions to a minimum. The term "vowel" includes diphthongs as well as monophthongs, and a /j/ or /w/ is called a consonant when it precedes the crest of the syllable, but a vowel when it is part of a diphthong at the end of a syllable; this corresponds to everyday usage for *y* and *w*.

2.1 The key to accentuation in English lies in the end of the word: words that end alike tend to be accentuated alike. For example, there are hundreds of words ending in *-/ɪk/*, like */mæg`nɛtɪk/*, */ɑpə`rætɪk/*, */`pɪknɪk/*, */ɑjs`ləndɪk/*, and */ɪ`ləktrɪk/*. All may be accentuated on the next-to-last vowel except *Arabic*, *arithmetic* (when a noun), *arsenic* (when a noun), *bishopric*, *Catholic* and *catholic*, *chivalric* (in British usage; some Americans say *chivalric*), *choleric*, *heretic*, *lunatic*, *politic*, *rhetic* (when a noun), *turmeric*, *valeric*, *Benedick*, *Limerick* and *limerick*, *bailewick*, and *Bolshevik*.

2.2 The words ending in *-/vɔwn/*, mostly spelled *-oon*, are accentuated on that final syllable: */bə`lvɔwn/*, */tɑj`fvɔwn/*, */hɑr`pɔwn/*, */mə`rvɔwn/*, etc. I have discovered no exceptions.

2.3 For most endings, however, there are a few exceptions, often words borrowed from languages with different accentual systems but not yet Anglicized to fit English accentuation.

3.0 Many times, the accentuation depends not only on the ending but also on other factors. – There is no doubt *when* other factors intervene; if there were, the accentua-

¹ See *The Accentuation of English Words*, to be published shortly by Longmans Green Co., London.

tion would not be systematic. – But at various times, the following factors are relevant:

3.1 It is important to distinguish morphological boundaries, that is, to distinguish prefix + root from root + suffix or root + root. Most words ending in *-l/* are accentuated two vowels before that *-l/*, as (*saj*'*kalədʒl/*, /*kə*'*tæstrəfl/*, /*fə*'*tagrəfl/*, and /*ləjə*'*bɪlɪtl/*). If the final *-l/* is a suffix or part of one, though, as in *-y* (meaning “like a —”) and *-ly* (“in a — manner”), adding the suffix does not move the accent from its previous location, so the following are not exceptions: *corrùptly* (§ 4.2), *vàriously* (§ 3.63), and *habitually* (§ 3.41).

3.11 It is mistaken morphological analysis that prompts common mistakes like accentuating *misled* as /*ˈmàjzəld/* instead of /*mɪs*'*lɛd/*, and *bedridden* as /*bə*'*dɪrdən/* instead of /*ˈbɛdɪrdən/*.

3.2 For accentual purposes, “compound” also includes doubled words like *hula-hula*, *cancan*, *beriberi*, *Sing Sing*, *boogie-woogie*, *helter-skelter*, *mishmash*, *chitchat*, and our old friend *hocus-pocus*. Unfortunately, our present scope cannot be stretched to cover a sketch of compounds.

3.3 Accentuation usually works backward from the end of the work. What, then, of words without enough syllables? For example, unlike most in *-l/*, the words ending in *-məwnl/* are accentuated three vowels before the *-l/*:

3 2 1
`ælməwnl

3 2 1
`mætrɪməwnl

3 2 1
`pætrɪməwnl

or, in England, without the secondary accent:

3 2 1
`ælmənɪ

3 2 1
`mætrɪmənɪ

3 2 1
`pætrɪmənɪ

2 1 2 1
What of *harmony* and *simony*, which have no “3”? The answer is simply to go back as far as possible and stop, on the first syllable. The implication is that a great many words happen to be accentuated on the first syllable when in fact the system for their groups is to accentuate three or two syllables before the last. This may well be why we have the impression the English words are usually accentuated on the first syllable when this is not the full reality.

3.4 Sometimes the length of the word also affects its accentuation. If a word ends in only one consonant, its accentuation depends on whether it has only two syllables or more. Of those with only two syllables, the nouns are usually accentuated on the first of the two syllables (as the words ending in *-ɪjn/* (/ˈgæŋgrɪjn/, /ˈbenzɪjn/, etc.), but verbs, adjectives, and other parts of speech on the second (/kənˈvɪjn/, /abˈstɪjn/, /səˈrɪjn/, etc.).

3.41 If such a word has three syllables or more, though, its part of speech is not important, and it is accentuated on the last vowel but two (as in /həjˈpætən(j)ʊws/, /ˈkerəsɪjn/, /ˈɛpɪsɪjn/, /əˈsetəlɪjn/, and /həˈbɪtʃʊəl/), unless the last vowel is immediately preceded by two or more consonants, when the accentuation is found on the last vowel but one (/pəˈrɛntəl/, /pəˈtɜrnəl/, /trəjˈəmfəl/, etc.).

3.5 Finally, as we have just seen, sometimes the part of speech is a factor. The relevant parts of speech are (i) nouns, (ii) adjectives, and (iii) verbs-and-all-others. There are not many pronouns, conjunctions, and prepositions, anyway, but these and the adverbs have the same accentuation as the verbs, with only the most occasional of exceptions.

3.6 To give a few more examples: words ending in *-/ʃən/* are accentuated on the preceding vowel, as */səs`pɛnʃən/*, */pə`zɪʃən/*, */rɪ`dɔkʃən/* = *[rɪ`dɔkʃən]*, and */ɪnvɪ`teɪʃən/*. Most are spelled *-sion* or *-tion*, where there are no exceptions.

3.62 Most words ending in *-/əʊwsɪs/* are accentuated on the */əʊw/* diphthong in the penultimate syllable, as */sɑj`kəʊwsɪs/*, */ɑz`məʊwsɪs/*, */tjʊwbɜrkjə`ləʊwsɪs/*, */njʊ`rəʊwsɪs/*, etc. The only exception would be rather deliberate pronunciations of */ə`pɑdəʊwsɪs/* and */mɛtə`mɜrfəʊwsɪs/*.

3.63 Most words ending in *-/ɪs/* are accentuated on the preceding syllable, as */pəʊlɪəʊmɑjə`ləɪtɪs/*, */tɑnsɪ`ləɪtɪs/*, */njʊ`rɑɪtɪs/*, */`præktɪs/*, */e`prɛntɪs/*, and */`dʒɛstɪs/*. An exception seems to be */`ɑrmɪstɪs/*.² If one does not distinguish between */ə/* and */ɪ/* in unstressed syllables, a few more exceptions will be added from among the words spelled *-ous*, which are accentuated on this syllable only if two consonants immediately precede the final *-/əs/* (*/məʊ`mɛntəs/*, */pɜr`tɛntəs/*, etc.), but otherwise one syllable earlier (*/kə`læmətəs/*, */sə`lɪsətəs/*, */`vɛ(j)rɪəs/*, etc.).

4.1 It is a temptation to continue discussing individual endings, but it is time to get to the point, which is that these individual endings group themselves into much larger categories, the sum of which is a whole system of accentuation. These larger categories are illustrated just below, namely by words ending in more than one consonant, which is indeed a large category. In what follows, let us remember that we are considering only uncompounded morphemes without affixes.

4.2 *Words Ending in More than One Consonant* depend on their part of speech, but not their length. Nouns are accentuated two syllables before the last (as in */`ɑrkɪtɛkt/* and */`æmpɜrsænd/*); verbs-and-others, on the last (*/rɪ`trækt/*, */kən`vɪkt/*, */ɪntər`sɛpt/*, etc.); and adjectives depend on what the last consonant is. If it is */b/*, */t/*, */s/*, or */z/*, the adjective is accentuated on the last syllable (*/dɪ`rɛkt/*, */rɪ`vɜrs/*, */kə`rɛpt/*, */sɜv`pɜrb/*, etc.), but if it is any other, the adjective is accentuated two syllables earlier (*/`mæni-fəʊld/*, */`rɛvərənd/*, */`jʊwnɪfɜrm/*, etc.).

4.21 *Words Ending in Only One Consonant* are discussed above, §§ 3.4-3.41. It should be noted, however, that there are several sub-groups accentuated according to exceptional patterns: *-/ɪk/*, *-/əjd/*, *-/ɪʃ/*, *-/græm/*, *-/ɪjən/*, *-/təm/* when preceded by a vowel, *-/ɪjn/*, *-/ʊwn/*, *-/ʃən/* § 3.61 above, *-/ɪjr/*, *-/əʊwsɪs/*, *-/tɪs/* § 3.63 above, *-/ɪt/*, *-/ɛjd/*, *-/grɛjd/*, and *-/ər/*.

4.22 There is a set of words accentuated on *-/ɪjən/* as well as a set accentuated one vowel earlier: */ɛpɪkjʊr`ɪjən/*, */gæɪl`ɪjən/*, */jʊrə`pɪjən/*, etc., as against */ʃɛjk`spɪrɪjən/*,

² It is interesting that in his *English Pronouncing Dictionary* Daniel Jones gives [*ɑ`mɪstɪs*] as a second pronunciation in England. There seems to be an unconscious attempt to fit exceptions into the regular patterns, both in Anglicizing foreign words and in reaccentuating existing exception.

/jʊw'klɪdʒən/, /mɛdɪtə'reɪnjən/, /sɪŋə'pə(w)rɪjən/, etc. This is an unusual case where there are two established and competing patterns. In nearly every other case, there is a clearly dominant pattern, and the exceptions are only about 2% (by actual count). The other serious cases of competition involve endings that are sometimes a suffix and sometimes not, as *-able*, which is a suffix in *returnable*, *insufferable*, and *transferable*, but not in *formidable*, *indefatigable*, *hospitable*, and *polysyllable*.

4.31 *Words Ending in a Single Vowel*. Many words end in *-ə*/, usually spelled *-a*. With exceptions, they are accentuated on the vowel before the final *-ə*/, as *tapiòca*, *propagànda*, *nàphtha*, *umbrèlla*, and *inflùenza*.

4.32 Words ending in *-əw*/ are generally accentuated on the vowel before the final *-əw*/, as *concèrto*, *falsètto*, *Plùto*, *mezzo-sopràno*, and *Tolèdo*, except a sub-group of words ending in *-məw*/, which are accentuated one vowel earlier: *generalìssimo*, *fortìssimo*, *dýnamo*, *Èskimo*, *duodècimo*, etc.

4.33 Words ending in *-ɪ*/ have been described in § 3.1 but this is the place to add that there are five major and two minor sub-groups that behave exceptionally; *-arkɪ*/ and *-məwnɪ*/ are accentuated three syllables before the final *-ɪ*/; *-ənsɪ*/, as if the */n*/ were the final single consonant; and *-lɪ*/ and *-mænsɪ*/, on the first syllable.

4.34 Final *-ɪj*/ is usually a suffix, as in *employee*, *divorcee*, etc.; when it is not, it is accentuated, as in *repartèe*, *chimpanzèe*, *decrèe*, and *agrèe*.

4.35 Most words ending in *-ej*/ are accentuated on that final syllable in America.³ Examples are /fɪjən'sej/ or /fɪj'an'sej/, /nɛglɪ'zɛj/, /dɪ'kɛj/, /mɛj'leɪ/ = *melee*, /mə'leɪ/ = *Malay*, /ə'leɪ/, /brɪ'treɪ/, /dɪs'pleɪ/, /dɪs'mɛj/, /dɪ'freɪ/, /ə'freɪ/, /ə'reɪ/, /hə'reɪ/, and /pər'treɪ/. The exceptions are /'həlɪdeɪ/, /'pɑpɪndzɛj/, /'nɔwzgeɪ/, /'mɑrgeɪ/, /'fɔreɪ/, /'rɑwndələɪ/, /'vɪrələɪ/, /'ɛseɪ/, /'kærəweɪ/, and /'gæləweɪ/.

4.36 The rest of the vowels, */ʊw*/, */ɑ*/, and */ə*/, will not be exemplified here because */ʊw*/ and */ɑ*/ end only a few dozen words, and */ə*/ ends mostly monosyllables like *law* and *draw*.

4.41 *Words Ending in Two or More Vowels* must have then in separate syllables, of course. In the overwhelming majority of such words, the next-to-last vowel is */ɪ*/.⁴ It appears before */ə*/, */ɑj*/, and */əw*/ in such words as /sɪ'næriəw/, /'reɪdɪəw/, /reɪdɪəj/, /'nɪjʊwklɪəj/, /vɪk'tə(w)rɪə/, /jʊ'təwpiə/, /mə'le(j)rɪə/, /'kæmiəw/, and /'stɪjʊwdɪəw/.

5.0 That, then, is a sketch of English accentuation, even though there is not space to list all the exceptions and small subgroups. You will recall, too, that it has con-

³ One of the few differences between English and American location of primary accents is that in loan words from French, Americans tend to retain the French accent on the last syllable, while the English tend to transfer it to the penultimate. The following examples are (phonemicized renditions of) the first pronunciations in Kenyon & Knott's *A Pronouncing Dictionary of American English* and Daniel Jones's *An English Pronouncing Dictionary*, respectively: /gə'rɑ:z/ – /'gæɾɑ:z/, /klɪj'fɛj/, /ɛtə'fɛj/ – /ə'tæ:fɛj/, and /fɪjən'sej/ or /fɪj'an'sej/ – /fɪ'ɑ:sɛj/. (The phonemicizing of loan words is treacherous, of course, but I follow Jones in the opinion that the distinction between */ɑ*/ and */an*/ is one that educated Englishman usually make.)

⁴ In the speech of many people, of course, it becomes a */j*/ in the same syllable as the final vowel: /nɪj'məwnɪə/ or /nɪj'məwnjə/, /rɪ'geɪlə/ or /rɪ'geɪljə/, /dɪf'θɪ(j)rɪə/ or /dɪf'θɪ(j)rjə/, etc. This does not affect the accentuation, of course – merely our caption over it.

cerned only free "root" forms, omitting, for the present, prefixes, suffixes, compounds, and the doubled words. The title of this paper promised some suggestions about the significance of these accentual patterns, and while the following have occurred to the author, the hopes that other discussants will not restrict themselves to commenting on these, but will also present others.

5.1 The first significance of all this complication lies in the function of the accent. In 95% of the lexicon, the location of the accent correlates with the ending of the word, the length, etc.; the placement of the accent, therefore, cannot be distinctive (that is, cannot operate "phonemically"), for it is "tied" to the structure of the word. Within the regular 95%, possibilities of distinctive accentuation scarcely arise. (They do arise, however, in pairs of words composed of two exceptions or of one exceptional and one regular word.) This is the reason that it is so very hard in English to find such minimal pairs⁵ as /'bɪləw/ = billow, and /bɪ'ləw/ "below"; or /vəjə'lɪn/ "violin", the musical instrument, and /'vəjəlɪn/ "violin", a bitter metric found in the common violet; or /brə'vəw/ "hurrah!" and /'brəvəw/ "desperado"; or /'əgəst/ "August" and /ə'gəst/ = [ə'gəst] "august, venerable or majestic"; or /'ɪnsəns/ "perfumed smoke" (n); "to make such smoke (v)" and /ɪn'səns/ "to anger"; or /'ʃɪvərij/ "shivery" and /ʃɪvə'rij/ "charivari", "chivaree"⁶; or /'trəsti/ "trusty" and /trəs'ti/ "trustee"; or in the Southern U.S., possibly /'ɪntə/ "enter" and /ɪn'tə/ "inter".

6.1 Among the regular 95%, the scant possibilities that arise involve different parts of speech, like /'ɪmpækt/ (n) and /ɪm'pækt/ (v); /'dɪskəwnt/ (n) and /dɪs'kəwnt/ (v); /'ɪnstɪŋkt/ (n) and /ɪn'stɪŋkt/ (adj); /'ɪmpərt/ (n) and /ɪm'pərt/ (v); and /səs'pækt/ (v) and /səs'pækt/ (n). But even these pairs are fairly uncommon because in so many pairs where the spelling is the same, an /ə/, /æ/, /ɛ/, or other "full" vowel is reduced to /ə/ or /ɪ/ when the accent is off it: /'prəd(j)ʊws/ [or /'prəwd(j)ʊws/] (n) but /prə'd(j)ʊws/ (v); /'ædɪkt/ (n) but /ə'dɪkt/ (v); /'kændəkt/ (n) but /kən'dəkt/ (v); /pər'fækt/ (v) but /pərfɪkt/; etc.

6.2 In such pairs of words as /'əgəst/ - /ə'gəst/, where the accentuation correlates with the part of speech, the possibilities of its operating distinctively are limited to contexts in which either part of speech would make (syntactic) sense. It is only a half-truth to talk unqualifiedly of "parts of speech being differentiated solely by a difference in accentuation;" for in "real" language, parts of speech exist only in syntactic contexts. A minimal opposition between them in isolation "really" exists only if it can also be made the only opposition in an entire utterance or other meaningful unit. But the true functional yield (in utterances) of accentual distinctions between English parts of speech is an aspect of accentuation that remains to be quantified.

7.1 Secondly, the ordinary spelling of English, irregular though it is in patches, is more regular than is sometimes recognised. One of the ways in which it is meaningful (and therefore "right", it seems to me) is that it spells alike (except for the endings)

⁵ To cite examples from various areas; there is probably nobody for whom they are all true.

⁶ As spelled in Kurath's *Word Geography of the Eastern United States* (Ann Arbor, University of Michigan, 1949), "Glossary", p. 82.

such groups of words as *photograph* – *photography* – *photographic* and *democrat* – *democracy* – *democratic*. These are related semantically and morphologically, and the conventional spelling reflects this relatedness, while our phonemic and phonetic transcriptions disguise it: /'fəwtəgræf/ – /fə'tagrəfi/ (U.S., where Southern English has /fə'təgrəfi/, of course); and /'dɛmækɹæt/ – /də'mækɹəsi/ – /dɛmə'krætɪk/, not to mention the further minor differences introduced when we move from phonemics down into the details of phonetics.

8.1 The accentual system is significant in another way for spelling teachers. In the family that includes *photograph*, *photography*, *photographic*, for every unstressed schwa that appears, there is some other member of the family that includes a stressed vowel in that position. One might not be sure how to spell the /ə/ of *photograph*, but the doubt is resolved by the /ɑ/ of *photography* and *photographer*.

8.2 Furthermore, /'ɛmfəsis/ and /ə'm'fætɪk/ between them identify every vowel except at the end. Likewise, one might ask whether the last syllable of /dɪktɛjtər/ is spelled *-er* or *-or*, but the question is answered by reference to the word /dɪktə'tə(w)rɪəl/.

8.3 Trouble arises, however, when there is no other member of the family that has a stressed pronunciation of the vowel in question. For example, /'sɛpəɹət/ has /sɛpə'reɪʃən/ to help identify its third vowel as *a*, but nothing to help with the second vowel. Hence the doubt that leads to the misspelling *seperation*.

8.4 This lack of a related form accounts for spelling difficulties in other words as well. The next example may be controversial, but I believe that this same explanation underlies the schoolboy's tendency to spell *dollar* and *grammar* with *-er* for *-ar*. It is true that *grammar* is related to /grə'mɛ(j)rɪən/ and /grə'mætɪkəl/, but do schoolboys know these words? It may be that one way that increasing our vocabularies improves our spelling is through acquiring words related to those we already know but stressed where the already-known word is not.

9.1 Behind such a family as *photograph* – *photographer* – *photography* – *photographic* and even *photo*, there seems to lie a matrix or mould composed of these elements: fəwtagræf⁷ (√fəwtagræf). To this, various suffixes are added, such as *-ic*, *-y* and *zero* forming the ordinary word *photograph*. Now, if we consider *-ic* as a suffix – and accentually we have not, because it changes the accentuation of the word – (see § 3.1) then the suffix consists not merely of some phoneme(s) added to the end of the matrix, plus a main accent⁸, but also a rhythmical pattern⁹ that establishes where the secondary accents will fall and, even more strikingly, the realisation of intervening

⁷ Since this is a form that may never actually be realised as a word (though for some examples, it is), I avoid enclosing it in the type of bracket that implies a word or morpheme in its actual realisation, whether phonemically or phonetically considered. Perhaps the unrealised nature of this matrix will be conveyed if we mark it with √ .

⁸ That accentuation is sometimes correlated with word-endings is a conclusion that was also reached independently and with somewhat different results during the same years the present study was in progress, by Roger Kingdon, *The Groundwork of English Stress* (London, Longmans, 1958).

⁹ The rhythmical patterns of English words are described by Gordon F. Arnold, *Stress in English Words* (Amsterdam, North-Holland Publishing Co., 1957), and *Lingua*, 6 (1956-7), 221-267, 397-441.

syllables as unstressed instead of with the full vowel of the matrix. There are two big differences between the matrix $\sqrt{f\acute{o}wtagr\grave{a}f}$ and the word $/^{\acute{f}\acute{o}wt\grave{a}gr\acute{a}f/$. One is the "addition" of a primary accent and a secondary one; the other is this substitution of $/\acute{a}/$ for \sqrt{a} ; third, there is the addition of a zero suffix, but somehow this is less prominent than the other two differences!

10.1 The last point is one that I must confess has occurred to me too recently for full checking and digestion of the implication, but it looks as if English spelling may be morphemic rather than phonemic. (-God save us all from phonetic spelling if it is literally phonetic!) The theoretical difficulty with classifying English spelling as morphemic is that, as we have just seen, it represents the matrix $\sqrt{f\acute{o}wtagr\grave{a}f}$ rather than the morpheme $/^{\acute{f}\acute{o}wt\grave{a}gr\acute{a}f/$ (noun or verb). There are two conclusions: (1) Recognise that the spelling represents matrices instead of morphemes, and change the label from "morphemic spelling" to "matrical spelling" (with due apologies for spawning jargon).¹⁰ (2) Recognise that the morpheme $/^{\acute{f}\acute{o}wt\grave{a}gr\acute{a}f/$ consists of not only the constituent phonemes and the accents, but also a zero suffix, and that these accents together with the lack of accent on the unaccented syllables constitutes a rhythm within the word that is part of it as a pair of morphemes. This may be wearily obvious, but it needs emphasis because such distinguished people refer to the main accent in English as a phoneme. In the effective system of English accentuation, it is no phoneme, for it has only a very marginal distinctive ("phonemic") function; the extreme difficulty of finding minimal pairs of words distinguished only by their accentuation seems to me a very faithful measure of this marginality.

10.2 The spelling does seem to reflect the matrices (without suffix or accentual rhythm) rather than the morphemes (complete with these). I should awfully like to be able to proclaim, "English spelling is not phonetic but morphemic," but it isn't quite that, and it is rather less stirring to hear, "English spelling is matrical", while it would be downright silly to trumpet, "English spelling is radical". I shall therefore take advantage of this impasse to retire and yield the floor to the next discussant.

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¹⁰ It is pronounced [$^{\acute{m}\acute{a}t\acute{r}\acute{ɪ}k\acute{ə}l$]. See the *New English Dictionary*, "matrical", def. 2.

THE NASAL VOWEL PHONEMES OF FRENCH: A CASE HISTORY

ERNEST F. HADEN

Abstract

A synchronic analysis of Modern French phonology sets up nasality /~/ as a distinctive feature in such pairs as: bat, bas/banc; fait/feint, faim, fin; faute/source, etc. Furthermore the nasal vowels contrast with each other: lent/lin/long/l'un (the last one for some speakers, at least).

Diachronically viewed, the nasal vowels acquired phonemic status over a period of some 500 years, and emerged in the following order: /ã/ (11th c.), /ɛ̃/ (13th c.), /ɔ̃/ (14th c.), /œ̃/ (15th–16th c.).

The notion of structuralization – parallel to grammaticalization – is pertinent to the process by which nasalization (phonetic) evolves into nasality (distinctive feature), to the readjustments which occurred from time to time within the vocalic system, and to the historic phenomenon of denasalization.

In Modern French, the oral vowel phonemes /i, y, u/ have no nasal counterparts. Earlier *[i] opened, to be incorporated eventually into /ɛ̃/, along with the reflexes of [ejn > ɛ̃j] e.g. plein, and of [ajn > ɛ̃j] e.g. main. Similarly *[ũ] shifted to the range of [ɛwn > ɛ̃w̃] so that *brun* rhymed with *jeun*, i.e. [brũ : ʒɛ̃w̃]. Following a development parallel to the previous [ew > œ] we now get [ɛ̃w̃ > œ̃].

The emergence of each of the modern nasal vowel phonemes followed by approximately a century the establishment in the system of the corresponding oral phoneme.

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SPANISH *LL*, *Y* AND *RR* IN BUENOS AIRES AND CORRIENTES

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Differences in the phonological systems of the Spanish of Buenos Aires (called *Porteño*) and that of the northwestern Province of Corrientes pose both certain questions of learning to spell and problems of a standardized Argentinian pronunciation.

The consonantal system of General Latin American Spanish has a single palatal reflex /j/ for two former entities – palatal [ʎ] (spelled *ll*) and prevocalic [j] = orthographic *y*.

p	t	č	k
b	d		g
f	s	j	x
m	n	ñ	
	l		
	r		
	ř		

In *Porteño*, a voiced fricative /ʒ/ stands in place of GLAS /j/. In Corrientes, however, a palatal /ɲ/ still exists, while prevocalic *y* has become the affricated /j/ and the *ř* of GLAS and *Porteño* has become a sound I represent as *ž* for reasons given below.

Thus:

orthographic	GLAS	Porteño	Correntino
<i>l ll</i>	l	l	l ɻ
	j	ž	
<i>ch y</i>	č	č	č j
<i>r rr</i>	r ř	r ř	r ž

E.g. *un caballo bayo* “a bay horse” GLAS [uŋ kaβajo βajo]
 Port. [uŋ kaβažo βažo]
 Corr. [uŋ kaβaʎo βaʎo];

Goyo es de Goya, yo soy de Corrientes.

Port. [gožo es de goža žo soi de kořientes]

Corr. [gožo es de goja ʎo soi de ko.rienteh].

We might note here that Correntino students of English have no trouble with the initial sounds of [jən, jæk, jein], but it is harder for them to master the pronunciation

of French [žak, žã, žoli], for they tend to substitute the affricate for the fricative. The difficulty is reversed for Porteño students who are learning English and French, for the voiced affricate is absent from their own pronunciation.

The /r̄/ of GLAS and Porteño corresponds to the /ž/ of Corrientes, which has phonetic variants [ɾ ~ ř ~ ž]. Elena Vidal de Battini (*El español en la Argentina*, pp. 69, 71) states that this sound is “erre asibilada”, viz. *ɾ*. My Porteño ears hear it as a fricative of a less dental nature than *ɾ* and more palatal than ř, and I therefore represent it as ž. It is surely the palatal quality which makes possible such puns as this joke: Porteño: ¿*Qué es bayo?* [kes βažo?] (“What is a bay horse?”) Correntino (understanding *barro* “mud”): *Tierra con agua*. [tjeřa konagwa] (“Earth and water.”) Porteño: “No! It’s a reddish-brown horse.”

I decided to make tests in Corrientes to see what light could be thrown (a) on the phonemic problem that the ž pronounced by Porteños is not associated by the Correntinos with their own ř but with their own /ž/ = ɾ ~ ř ~ ž and (b) on the related graphemic problem that the phonetic βežo and žuβia heard in Buenos Aires are spelled *bello* and *lluvia* and mean “beautiful, rain”, while the same phonetic words uttered in Corrientes have quite different meanings – “water cress, blonde” – and would be spelled *berro*, *rubia*. I made about 300 tests, in the city of Corrientes and in the towns of P. de la Patria, Itatí, Gral. Paz, Saladas and Chavarría to the north of the Corrientes river, and in Ituzaingo, Mercedes, Guaviravi, Yapeyú and P. de los Libres in the south-eastern half of the province.

Informants of different levels were given oral and written tests. Children in morning classes normally enjoy a higher social status, while those in afternoon classes generally earn their living doing household chores for families of a higher social standing. Young people or adults attending night classes have for the most part not had a chance to learn how to read and write in their childhood, being on the whole children of illiterates. Further, I tested some young students from Normal Schools and Colleges.

The graphemic problem was tested by my reading aloud in my Porteño pronunciation some words whose meanings were unknown to most of the informants and other words which had to be interpreted purely on the basis of the sound. Some were incorporated into sentences.

	Porteño (is phonetically equivalent to) Correntino			
‘beautiful’	<i>bello</i>	[βežo]	<i>berro</i>	“water cress”
‘bay horse’	<i>bayo</i>	[βažo]	<i>barro</i>	“mud”
‘noise’	<i>bullá</i>	[βuža]	<i>burra</i>	“female donkey”
‘native of N. plateau’	<i>coya</i>	[koža]	<i>corra</i>	“runs, run!”
‘corn’	<i>callo</i>	[kažo]	<i>carro</i>	“cart”
‘rain’	<i>lluvia</i>	[žuβja]	<i>rubia</i>	“blonde (f.)”
‘What rain!’	¿ <i>Qué lluvia!</i>	[kéžuβja]	¿ <i>Qué rubia!</i>	“What a blonde!”
‘What a noise!’	¿ <i>Qué bullá!</i>	[kéβuža]	¿ <i>Qué burra!</i>	“What a donkey!”
‘What a bay!’	¿ <i>Qué bayo!</i>	[kéβažo]	¿ <i>Qué barro!</i>	“What mud!”
‘He stepped on my corn.’	<i>Me pisó un callo.</i>	[me pisó unkažo]	<i>Me pisó un carro.</i>	“I was run over by a cart.”

In 236 tests administered to children aged 11-16, my /ž/ was spelled *rr* 188 times, e.i. 79.66 %. Thus it is clear that a Correntino will, in the light of his own phonemic system, interpret our Porteño rendering of /ž/ as equivalent to orthographic *rr*.

The Porteño has a different graphemic problem – to write *ll* or *y* correctly when he hears /ž/. In 90 tests given to children of ten to fourteen years of age, 77 % of the pertinent cases showed *ll* for *y* or vice versa.

The oral tests had to be varied somewhat for (a) pupils still unable to read (in evening courses), (b) those who could read but not yet write well (e.g. in Itatí), and (c) those with both skills.

In the first case certain words were elicited orally by means of a questionnaire, given here with a typical set of answers:

- | | | |
|---|-------------------|-----------|
| 1. What is the name of a salad vegetable that grows in shallow waters by the river? | (<i>berro</i>) | no answer |
| 2. What do you call the mixture of earth and water? | (<i>barro</i>) | /bažo/ |
| 3. What do you call the female of the animal that brays? | (<i>burra</i>) | /buža/ |
| 4. How would you urge somebody to escape from danger? | (<i>corra</i>) | /koža/ |
| 5. What do you call a four-wheeler drawn by horses? | (<i>carro</i>) | /kažo/ |
| 6. What is the opposite of brunette? | (<i>rubia</i>) | /žuβia/ |
| 7. What other word do you know for very beautiful? | (<i>bello</i>) | /beło/ |
| 8. What do you call a light reddish-brown horse? | (<i>bayo</i>) | no answer |
| 9. What other word do you know for noise? | (<i>bulla</i>) | no answer |
| 10. What do you call a native of the Northern plateau? | (<i>coya</i>) | no answer |
| 11. What do you call the water that pours from the sky on a stormy day? | (<i>lluvia</i>) | /luβia/ |

The results showed that orthographic *rr* represents /ž/ [ɾ ~ ř ~ ž], *ll* is /l̄/, and *y* is /j/ (although the particular respondent given here failed to answer questions 8 and 10).

For the second group, a series of examples was written on the board and read off by each student out of earshot of the others, so that the pronunciation of one might not influence the next.

¡Qué rubia!	¡Qué lluvia!	bello – berro	callo – carro
¡Qué burra!	¡Qué bulla!	bayo – barro	coya – corra
¡Qué barro!	¡Qué bayo!	bulla – burra	lluvia – rubia
harina	pera – perra	El burro está en al barro, está embarradito.	
arena	pero – perro	Goyo es de Goya, yo soy de Itatí.	
aroma	para – parra		
araña	Cora – corra		

Again the same results: orthographic *rr* is /ž/ [ɾ ~ ř ~ ž], *ll* is /l̄/, prevocalic *y* is /j/.

Besides these three tests, another was given to a group of teenagers (16-17) who were studying English in the last course of the Commercial School (Escuela Nacional de Comercio): *ya* – *yes* – *ya*, *yeso* – *jet*, *ya* – *Jack*, *yo* – *job*. The same results obtained for Correntino Spanish /ž, l̄, j/ and showed that in English words *j* was equated with /j/ and *y* with /j/.

To sum up: I find that in Porteño Spanish

/r/ is realized as [r̄] before pause or consonant (*amor*, *parte*) and

[r] between vowels or after plosive (*aro*, *tropa*);

/r̄/ is realized as [r̄] initially or between vowels (*risa*, *arre*).

DISCUSSION

LUNT:

The Correntino has two sets of problems related to orthography and orthoepy: he must relate his own speech (1) to standard spelling, and (2) to the idealized pronunciation, and then he must match it against the prestigious Porteño speech of Buenos Aires which differs both from Correntino and the ideal. Surely he can easily learn that his /j/ = *y* (+ vowel-letter), /l/ = *ll*, and /ʒ/ = *rr* between vowels and *r* in initial position. Left alone, it would probably be simple enough to teach him that his [j] is "good" but his [j] "should" become [j] and his [ʒ] a [r̄]. Here, however, he doubtless becomes confused by the pronunciation of teachers who use, very likely without consistency, the Porteño [ʒ] and the ideal [j] for both *y* and *ll*. It is really the teacher who must become aware of the various layers in this complex of problems and then proceed methodically to work out techniques of handling them.

ENGLISH SYNTAX PROBLEMS OF FILIPINOS AND THE PRINCIPLES OF LINGUISTIC RELATIVITY

CEFERINA I. C. ESTACIO

This paper will present a few major grammatical problems observed in the learning of English by Filipinos, then briefly attempt to make inferences for non-linguistic data. In effect, the second part seeks to discover parallels and correlations between the linguistic behavior of a specific human community through what Whorf has called "fashions of speaking", on the one hand, and other behavioral aspects of culture, on the other.

The effective teaching of English in the Philippines poses a continuing challenge, since all instruction, except for recent changes introduced in the lower grades, is conducted through it. In the years following the end of World War II, there has been, in general, a marked inadequacy in the English language skills of Filipino college students and graduates. This situation, traceable to many causes, has made change advisable but no suggestion for change has gained acceptance.

As one effort to improve teaching and learning, the survey test which provides the basis for the problems enumerated below was conducted at the opening of the school year in 1960 and 1961. The subjects, numbering about 10,000, were college freshmen representing the various dialect groups of the country. For predictive purposes, a structural comparison would obviously have sufficed, but what was desired here was to determine the degree of achievement or mastery so that provision for areas of weakness could be made in the new syllabi under preparation for the basic courses in English. Items tested included grammatical categories which certain contrastive analyses had predicted as likely to offer problems, a prediction which has been confirmed by observations in the classroom and out of school. Selection, reconstruction, and transformational units were used in the tests, and productive rather than receptive control was the main concern. Because of necessary overlaps between syntax and morphology, as well as unavoidable semantic considerations in several instances, there are constant references to these two items.

The problems revealed by the test and corroborated by classroom performance as frequent and persistent errors involve three general types: taxemes of selection, taxemes of order, and substitution. The first group will now be considered:

1. Note the following sentences:

Singular	Plural
English: The dog barks.	The dogs bark.
Pilipino: ¹ <i>Ang aso ay kumakahól.</i>	<i>Ang mga aso ay kumakahól.</i>

Here we observe two differences: the device for showing plurality and the presence or absence of concordance between a primary and secondary word. English uses the -s morpheme to show plurality but Pilipino uses the function word "mga" /m'ŋa/ before the substantive. Moreover, English attaches the same morpheme to the finite verb expression to show the singular, but omits it for the plural. Pilipino, on the other hand, although having this specialized taxeme to show correlation between a plural subject and the verb, does not use it as an essential or required form.²

This non-obligatory use causes two confusions in English production: on the one hand, the frequent ignoring of the -s morpheme for the plural substantive, resulting in (a) the dropping of the /s, z, ɪz/ in substantives which are always plural in form (headquarters, trousers, pants, scissors), and (b) the use of zero alternants for nouns even when more than one is meant,³ on the one hand, and (b₁) the use of the -s morpheme for nouns which have zero alternants for the plural, such as "furniture", "fun", "clothing", "traffic", "poetry", and "graft", on the other. Infrequently, too, the -s or -es ending is used instead of vocalic or other spelling change for nouns like "mouse" and "focus", for instance. This phenomenon whereby pattern and its utilization do not always follow each other may be explained, as Sapir has pointed out, by the fact that "all languages evince a curious instinct for the development of one or more particular grammatical processes at the expense of others, tending always to lose sight of any explicit functional value that the process may have had in the first instance, delighting, as it would seem, in the sheer play of its means of expression". Consequently, because English has multiple expressions of an identical function, i.e., here to show the basic concept of plurality, while Pilipino has only one essential pattern, much difficulty arises.

1a. Related to the expression of singular and plural of verbs is the selective problem of numerative and identificational relations. In this category is the more complex syntactic construction known as character-substance, which is a type of phrase resulting from the combination of two or more forms, with a noun expression at the head and an adjective expression as attribute. Examples are *warm day* and *any time*,

¹ *Pilipino* is the name by which the national language of the Philippines is now designated. Tagalog /Ta'ga ləg/, which Bloomfield and Lopez discuss in their books, is the language of the southern and southwestern provinces of Luzon, and by and large, of very wide use in Manila. It is generally accepted as the major basis of the national language, and does not differ radically in sound and structure from other Philippine dialects.

² Jespersen, on number concord, says the rule "is really superfluous, as the notion of plurality belongs logically to the primary word alone; it is no wonder that many languages more or less consistently have given up the indication of number in secondary words" (*Philosophy of Grammar*, p. 207).

³ This tendency is usually increased, or usually goes with, the failure to pronounce sibilants in final position, a distortion induced by the native sound patterns.

the first of which falls under the subdivision of quality-substance and the second under that of limitation-substance.

Errors involving the second type, i.e., limitation-substance together with problems of other forms in a sentence, occur more frequently than errors of quality-substance. These errors involve limiting adjectives, both determiners and numeratives, and may be classified into five groups:

aa. The often complete absence of the determiners *a*, *an* or *the* before the noun head, especially at the beginning of a sentence. Examples are in cases of definition, as "University is. . .", and other cases like "The story give lesson", where there is an error in concordance besides, and "I came from office".

The dispensability of these determiners in the native language patterns, together with special taxemes for sub-classes in English⁴ aggravates the problem.

bb. The use of the indefinite determiner with mass nouns that never take -s. These occur in such phrases as "a scenery", "a baggage", and "an applause", showing a tendency to treat mass nouns as if they were countable. Infrequent cases showing, moreover, a lack of feeling for the euphonious arrangement of phonemes, occur in such examples as "a equipment", "a ammunition".

cc. A closely related error is the prevalent use of the definite determiner even if particularity is not intended, as in "I went to *the* movie", or "Did you see *the* man pass by?"

dd. The use of the numerative determiner "many" with the singular of bounded nouns, defined as "that species of objects occurring in more than one specimen, such that the specimens cannot be subdivided or merged". One therefore hears "many candy", and the like.

ee. The use, with the plural substantive, of 1. a singular determiner, as in "a children", or 2. a singular limiting adjective, as in "all *this* years", "*another* days".

1b. Errors caused by class-cleavage of lexical forms. "Paper" is a mass noun, and may be plural only in the specialized meaning designated by expressions like "pieces of", or "reams of", but by class-cleavage, "papers" would mean "documents" or "finished written work". Other examples by class-cleavage which are misused are "instructions", "hairs", "properties" and "advices". A very usual error here is the phrase "many works", used to mean "chores" or "tasks".

1c. The use of the singular after the phrase "one of the". Examples are: "one of my friend", "one of the student".

2. Errors of the Substitution Type:

Problems of this group represent a major difficulty, for though, as Bloomfield has said, "some forms of simple anaphoric substitution seem to occur in every language, there are great differences of detail".

2a. One problem is that of anaphoric replacement. An *anaphoric substitute* has been defined as a linguistic form or grammatical feature which, under certain con-

⁴ Examples of these special taxemes are the rules for names of institutions, rivers, etc., as: *the* University of the Philippines, but *Harvard* University; *the* Pasig River; plays *the* piano.

ventional circumstances replaces a recently-uttered linguistic form, called an *antecedent*. Two kinds of difficulties concern one, the absence of substitution where its use would be more natural,⁵ and another, treating a transitive verb as intransitive by repeating it in answer to a Yes-No question but dropping the object. Examples of these are:

Question: "Did you hear the lecture?"

Answer: "Yes, I heard the lecture", or the non-English construction "Yes, I heard" instead of "Yes, I did".

Also: Question: Did you see the accident?

Answer: Yes, I saw.

And, Question: How did you like the party?

Answer: I enjoyed very much.

2b. Another very usual error, involving cultural content besides, is the combination of the affirmative and the negative, as in:

(If someone says): Do not do that again.

Answer: Yes (But meaning "No").

Or: You were not absent, were you?

Answer: Yes. *Or* Yes, I was not absent.

And another: Would you mind closing the window?

Answer: (trying to be courteous) Yes. *Or* Yes, of course.

3. Problems of Word Order:

In Pilipino, the "favorite" type of construction is the Action-Actor, as opposed to the English Actor-Action. Note the following:

Kumain si Nena. (Ate-Nena) – Nena ate:

and the equational sentence

Malakí ang bahá: (Big-the-flood) – The flood is big.

These sentences have actor action equivalents (*Si Nena'y kumain, Ang baha'y malakí*), but they are not commonly used, nor do they sound natural.

3a. Reversals pose a problem too. English changes statements to questions by adding an auxiliary (*do* or *have*) before the subject of sentences which may be answered by Yes or No, but Pilipino does it through intonation and/or the addition of the particles "ba", "ga", or "baga", depending on the region. Examples are:

Statement: *Pupunta ka sa bahay.* You will come to the house.

Question: *Pupunta ka ba sa bahay?* Will you come to the house?

or, in the actor-action order, *Ikaw ay pupunta sa bahay*, it becomes *Ikaw ba ay* (sandhi form ba'y) *pupunta sa bahay?*

⁵ This error has often been worsened by the usual insistence, especially in past years, to have children always answer in complete sentences. Intended to develop sentence sense and word order, the practice has caused stilted constructions at times.

aa. The addition of the helper "do" or "have" or their other forms creates the problem of "discontinuous constituents" and causes errors both of linguistic time and number. Samples of these errors are: "Do he go there?", the reversal for "He goes there", and more commonly, the double preterite form, as in "Did he went?", the reversal for "He went".

bb. Likewise, modifiers are frequently placed immediately after the verb, as in the constructions "I like *very much* your bag", or "You ask *first* the permission of your mother".

From the preceding discussion, it is evident that the problems of structural transfer start from two causes, namely, the use of similar devices to signal structural relations, but in different forms and distributions, and the use of entirely different signalling devices. The apparently greater flexibility of Filipino in its juxtaposition of form-class to form-class increases the possibility of errors in English constructions.

So much for syntax problems.

Let us turn to the inferences for non-linguistic behavior, which are here offered with no illusions about their precise evaluation, considering the present still speculative character of inferences of this kind. They may or may not be verifiable, but they do come from premises arising not from mere information but from an intimate knowledge of the language and culture of the people who speak the second language. Support is borrowed from Hockett's statement that "the practical task of learning or teaching a foreign language cannot be successfully performed in an ethnographic vacuum", that "ethnography without linguistics is blind", that "linguistics without ethnography is dead". The speculations could serve one or both purposes, namely, as a beginning characterization of the Filipino "Weltanschauung" or "thought world", and through this beginning, point up a potential value of the linguistic relativity principle for cross-cultural communication, and/or, of more immediate implication, to find in the grammatical and semantic categories investigated possibilities for improving teaching and learning in a second or foreign language.

The hypothesis from which the deductions that follow start assumes "that users of markedly different grammars are pointed by their grammars toward different types of observation, and hence are not equivalent as observers but must arrive at somewhat different views of the world".⁶ Sapir, before Whorf, postulated that "we see and hear and otherwise experience largely as we do because the language habits of our community predispose certain choices of interpretation". In seeking to apply this hypothesis to the problems enumerated, this paper goes one remove further by implying that the structure of the Philippine dialects leads the Filipino to a certain view of the world, a view he unconsciously projects to a non-native language, often inhibiting his use of the patterns of that language.

⁶ Benjamin Whorf, *Language, Thought and Reality*. Further to this: "We cut up and organize the spread and flow of events as we do largely because, through our mother tongue, we are parties to an agreement to do so, not because nature itself is segmented in exactly that way for all to see."

One of the problems noted concerns the non-obligatory use of the specialized taxeme of concord or congruence with verbs. This could indicate, on a rather high order of correlation, a Filipino response or tendency to synthesize, to view an object or event above its components, as contrasted with the usually analytical view taken by native speakers of English. This response is, in this writer's opinion, outstandingly illustrated by the view a Filipino takes of his family: no real boundary limits the possible coverage of his kinship system, for it includes not alone relations by consanguinity and affinity carried through numerous collateral branches, but also relations through religious ritual.⁷ Within this flexible and constantly expanding boundary of kinship may be found an integral system of social security, whereby personal misfortunes generally become more bearable through family sharing, illustrating the tradition of "damayan", a concept of sharing in a more-than-mere-friendly attitude ("pakikisama"), motivated by strong feelings of sympathy and mercy ("Kaawa-awanaman") towards someone less fortunate. This seem to be an extension of the "bayanihan" system, a concrete manifestation of sharing in physical tasks, as in building or moving a house, or in planting and harvesting. Sociology provides the descriptive term "Gemeinschaft", whereby emotion, rather than the objective criteria of organization and efficiency in "Gesellschaft", help in formulating decisions.

Again, if the linguistic relativity premise is valid, the Filipino thought-world might be evaluated as event-dominated (the action or result coming ahead of the doer), an assumption borne out, or reflected in, a parallel in his non-linguistic behavior, i.e. his faith in the presence and power of a Divine Being that guides him. On a lower order of correlation, and involving not only word order but non-essential number differentiation, is the Filipino trait of courtesy and consideration for others.⁸ Modesty, self-effacement, a disinclination to oppose or disagree outright so as not to offend, are other manifestations. Furthermore, if language indeed be "the guide to social reality", one might find a real correlation between the event-dominated favorite sentence-type and the Filipino national trait known as "bahala na", a combination of a deep and abiding religious faith and a supreme concept of fatalism. Engendered by a complete belief in a God even from pre-Spanish times, this attitude was further strengthened by the centuries of Spanish colonial experience, taking the defense posture of apathy, which was mistakenly labelled indolence.

In summary, it may be said that the Filipino, despite over sixty years of being educated through the medium of English, has not become truly bilingual. And even those who use English with native-like fluency and skill remain, quite expectedly, Filipino in

⁷ Most outstanding of this type is the "compadre" system, whereby godparents at a child's baptism and confirmation stand in a special relation not only to the child but to the family and with one another. Feelings generated by this relationship extend to other contexts and situations, as in business transactions and employment.

⁸ On this, E. B. Rodriguez provides this item of Filipiniana: "... the spirit of consideration towards their neighbors was the factor, above all, which moved the early Filipinos to action" (*This Week*, Sunday Mag. of the *Manila Chronicle*, date and issue no. torn off).

non-linguistic response, i.e., not fully acculturated. A wide sampling of errors other than those noted, such as of tense and gender, presents more opportunities for exploration of the Sapir-Whorfian thesis, but would be too long to discuss here. We cannot treat here methodology for lack of space. But be it as it may, it is apparent that structural and cultural interferences provide not only interesting but obviously useful possibilities for investigation both by linguistics and related disciplines, especially if mutations of the second language, as in the present case, are neither generally welcome nor acceptable.

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DISCUSSION

MEYERSTEIN:

Some of the problems are not typically those of Tagalog speakers learning English, but are typical of students of other native language backgrounds, or indeed the result of regularization which children effect in learning their own language; in other words, some of the problems mentioned are not specifically *bilingual* but also *intralingual*.

It is interesting also to notice similarities of Tagalog and French noun plural formation and these similarities might favorably affect the learning process from Tagalog to French or French to Tagalog.

ANSRE:

Many of the problems cited by Miss Estacio are found in the English of West Africans, where we find deviations on all levels – in phonology, morphology, syntax, and semantics. A particularly troublesome one in semantics is the response to negative questions. For example, “Did you not clean the room?” “Yes”. The meaning “yes, I did *not* clean the room” is normal in most West African languages, but this is not the normal English response.

I'd like to stress the need of a thorough linguistic study of the first language of students before any introduction of a second. Only thus can we avoid misunderstandings and unnecessary waste of time.

LEARNING PROBLEMS INVOLVING ITALIAN [s], [z] AND ENGLISH /s/, /z/

ROBERT J. DI PIETRO

It has been observed by Professor Haugen and Professor Weinreich, among others, that language learners systematically interpret the structure of a foreign language in terms of the structure of their native language.¹ A close correlation is seen to exist between ease in acquiring fluency and the proximity of linguistic structures. The importance of contrastive analysis as a preliminary step to understanding the range of transfer from one linguistic structure to another has been recognized by many analysts working in the field of language learning.² It is within this frame of reference that we examine and compare the phonemic status and patterning of Italian [s], [z] and English /s/, /z/.

1. *The Units.* In the first stage of our investigation we are concerned with individual phones in each language and their phonemic arrangements. In English the voiceless sibilant [s] and the voiced sibilant [z] are clearly allophones of separate phonemes. Contrasts are readily found as evidenced by minimal pairs like /sɪŋ/ : /zɪŋ/, /sɪp/ : /zɪp/, /raɪs/ : /raɪz/, /lúws/ : /lúwz/. The Italian situation involving comparable phones is somewhat problematic, with the largest amount of data indicating lack of contrast.³ Taking the breath-group as our distributional frame, we observe that the voiceless phone [s] occurs initially (as in ['se:-ra]), finally (as in ['la:-pis]), and before [t] (as in ['stel-la]). The voiced phone [z] occurs before voiced consonants, e.g., ['zdeŋ-ŋo], ['zbaλ-λo]. The problem area is between vowels where some speakers have [s] only (as in ['rə:-sa]), some have [z] only (['rə:-za]), while others have both voiced and voiceless phones distributed unpredictably among various lexical items. Such persons would say: [ri-sa-'li:-re] "to re-ascend" but [ri-zor-ŋi-'men-to] "new arising", or ['rə:-za] "rose" but ['ro:-sa] "gnawed". Most important for our analysis, however, is that very few phonemic contrasts are provided by intervocalic [s] and [z].

¹ Specific reference is made to: Einer Haugen, *The Norwegian Language in America* (Philadelphia, 1953); Uriel Weinreich, *Languages in Contact* (New York, 1953); Robert Lado, *Linguistics Across Cultures* (Ann Arbor, 1957); the project of contrastive studies presently being undertaken by the Center for Applied Linguistics, Washington, D. C. under contract to the U. S. Office of Education, Department of Health, Education and Welfare. Research for the present study was done as part of the Center's Italian-English study.

² For a fairly extensive list of contrastive studies see William W. Gage, *Contrastive Studies in Linguistics: A Bibliographical Checklist* (Washington, D.C., 1961).

³ Robert A. Hall, Jr., "Italian [z] and the converse of the archiphoneme", *Lingua*, IX, 2 (June 1960), pp. 194-7.

As of yet, only near-minimal pairs of the type exemplified by [ˈrə:-za] and [ˈro:-sa] can be found.

2. *Patterning.* Italian has fourteen two-member clusters with the phones [s] and [z] as first member: [sp] as in [ˈspes-so], [st] as in [ˈstel-la], [sk] as in [ˈskujə:-la], [sf] as in [ˈsfi:-da], [zb] as in [ˈzbaλ-lo], [zd] as in [ˈzdeɲ-ɲo], [zg] as in [ˈzgar-ˈba:-to], [zv] as in [zva-ˈni:-re], [zǰ] as in [ˈzǰe:-lo], [zm] as in [ˈzma:-ni̯a], [zn] as in [ˈznɛl-lo], [zɲ] as in [zɲau-ˈla:-re], [zl] as in [ˈzla:-vo], [zr] as in [zra-di-ˈka:-re]. We note that such clusters occur in both syllable onset position and across syllable boundaries.

Excluding for the moment possibilities across syllable boundaries, English has seven two-member clusters with /s/ as first member: /sp st sk sf sm sn sl/. Three, /sp st sk/, occur in both onset and coda positions of the syllable, e.g., /sp/ in /spil/ and /lisp/, /st/ in /stíl/ and /list/, and /sk/ in /skæt/ and /tásk/. There is one cluster with /z/ as first member which is limited to coda position, i.e., /zd/ as in /háwzd/.

If we include di-syllabic clusters, then English matches all Italian clusters except [zɲ], which is lacking principally because English does not have a phone [ɲ]. Examples with /z/ as first member are: /zb/ as in /θézbijən/, /zd/ as in /mázdə/, /zg/ as in /krézgiy/, /zv/ as in /rówzvàyn/, /zǰ/ as in /fázǰiyn/, /zm/ as in /ǰázmin/, /zn/ as in /bíznis/, /zl/ as in /ízləm/, /zr/ as in /éZRə/.

3. *Transference.* Having described the units and their arrangements in both Italian and English, we are prepared to interpret the learning problems experienced both by Italian-speaking learners of English and English-speaking learners of Italian. In order to simplify this presentation, I shall use the term “source” in referring to the native language of the learner and “target” in reference to the language he is learning.

3.1. *Source: English, Target: Italian.* Making English the source and Italian the target, it is observed that the learner tends to unvoice the first member of the clusters [zm zn zl], and adjust them to the English clusters /sm sn sl/, producing something like [ˈsma-niyə] for [ˈzma:-ni̯a], [ˈsnel-low] for [ˈznɛl-lo] and [ˈsla-vow] for [ˈzla:-vo]. For the clusters [zb zd zg zv] either a “support” vowel is inserted, e.g., [zə-ˈden-nyow] for [ˈzdeɲ-ɲo], etc. or *both* members of the cluster are unvoiced, e.g., [ˈspal-lyow] for [ˈzbaλ-lo], etc. In the case of [zr], the first element is unvoiced and the second element is reshaped to English [r], e.g., [sra-di-ˈka:-rey] for [zra-di-ˈka:-re]. The clusters [zɲ] and [zǰ] present special problems for the English-speaking student. In regard to [zɲ], his closest comparable sequence of phones is [səny-] or [zəny-]. Italian [zɲau-ˈla:-re] is consequently reshaped to something like [sənyaw-ˈla-rey] or [zənijaw-ˈla-rey]. In addition to [zəǰ-] and [səǰ-], [zǰ] is reshaped [sk] or [ǰ], e.g., [ˈskey-low] or [ˈǰey-low] for [ˈzǰe:-lo].

3.2. *Source: Italian, Target: English.* Reversing the poles of our analysis and taking Italian as source and English as target, we observe that the phonemic contrast between /s/ and /z/ is not maintained, especially in initial and final positions. The clusters /sm sn sl/ are interpreted as [zm zn zl], as in [ˈzmaɪl], [ˈznoʊ], [ˈzled]. Support vowels [a] or [e] occur to avoid syllable final clusters, e.g., [ˈesk-a] “ask”, [ˈlis-ta] “list”, [ˈtes-ke] “task”.

4. *Concluding Points.* In view of the data presented in this paper, the following conclusions are drawn:

4.1. It is evident that statements of distribution are as important in the contrastive analysis of two languages as is the inventory of comparable phones. Both Italian and English have a pair of phones [s] and [z] involving no outstanding difficulties in articulation for speakers of either language. Yet, learning problems exist in controlling the clusters into which they enter.

4.2. The concept of the phoneme has a reality for the language learner as well as for the analyst. The Italian-speaking learner needs to be aware of the phonemic contrast conveyed by the phones [s] and [z] in English, which is lacking in his native language.

4.3. A contrastive analysis makes it possible to predict the range of error-making. However, consistent accuracy in prediction is impossible in many cases – due to the inequality of alternatives in either source or target language. An outstanding example is the possible transference of four English alternatives for Italian [zǰ]: /sǰ/, /zǰ/, /sk/, /ǰ/.

4.4. As for the preparation of teaching materials, those texts which simply list phonemes (or, more likely, phones) run the risk of obscuring important differences in phonological structure.

4.5. A thorough-going contrastive analysis seems to be the necessary initial step in the preparation of any successful text or course of instruction.

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DISCUSSION

W. R. LEE:

The value of error-prediction based either on a comparison of L. 1 and L. 2 or an analysis of systematically made collections of errors varies from one type of user to another, and is of most use to those who are going to teach an L.2 in an L.1 area with which they are unfamiliar. Even those, however, who know from teaching experience in a given L.1 area what the L.2 learning errors are likely to be, should be able to make use (for course-planning, etc.) of well founded and well set-out statements of probable errors.

It seems doubtful whether wholly reliable prediction of errors can be based on an L.1/L.2 comparison alone: an error which might be expected does not always occur, no doubt owing to flaws in the descriptions or the comparison. Basing prediction thus, moreover, is to adopt a roundabout means of arriving at the information wanted. Collection and provisional analysis of the errors themselves would appear to offer a safe and useful short cut. In addition, if errors are collected at *various* stages of pupil-achievement, the guidance afforded to teachers etc. can be much more detailed than it would otherwise be.

The value of systematic L.1/L.2 comparison here, it seems to me, lies partly in the way it can *illuminate* mistakes, revealing why they were made and thus encouraging the teacher to be more sympathetic towards his pupils – for where one fails to understand the reason for a blunder or shortcoming one is inclined to attribute it to stupidity and become irritated. Such comparisons, themselves based on methodical descriptions of the L.1 and L.2 can also help to “true up” the statements arrived at by rough analyses of errors *collections*.

ON THE USE OF ZERO IN MORPHEMICS

SOL SAPORTA

In the most complete study to date on the use of zero in linguistics, W. Haas posits two conditions for establishing zero as a morphological element: (1) zero must alternate with an overt form, and (2) zero must contrast with an overt form.¹ It is the purpose of this paper to ask whether indeed these conditions are both necessary and sufficient.

First, an assumption: we adopt Item and Arrangement as a model of analysis, i.e., phonemic stretches are segmented into morphs and morphs assigned to morphemes.² Accordingly, we reject replacive and subtractive morphs.

We return now to the two conditions proposed by Haas. The first is that zero must alternate with an overt form, i.e., zero morphs may be acceptable, but zero morphemes are not, since a morpheme may not always occur as zero. Thus, English adjectives like *rich*, *small*, etc. cannot be said to have zero morphemes expressing positive degree in contrast to comparatives and superlatives *richer*, *richest*, *smaller*, *smallest*. We merely say that *rich* is monomorphemic, *richer* is bimorphemic.

The second condition is that zero must contrast with an overt form. Contrary to many analyses, this precludes establishing a zero plural for English *sheep*, since such a zero would contrast only with its own absence. *I see the sheep* is ambiguous. It seems pointless to pretend that it is not and to imply that *sheep* represents two morphemically distinct sequences which differ only by the presence or absence of a zero.

A form which satisfies both conditions is Spanish *señor* "gentleman", analyzed as *señor* plus a zero masculine, expressed elsewhere as /-o/ as in *hermano* "brother". The masculine in both cases contrasts with an overt form /-a/ meaning feminine: *señor-a* "lady", *herman-a* "sister". In other words, *señor* may be viewed as occupying one corner of a square, where all other corners are occupied by forms which are clearly bimorphemic.

We ask now whether indeed we are obliged to establish zeros when the conditions are met, and conversely, whether there may not be cases where we might want to relax the conditions.

Consider English nouns like *boy*, *pan*, etc. Shall we say they have a zero singular?

¹ Haas, W., "Zero in linguistic description", *Studies in Linguistic Analysis* (Oxford, 1957), 33-53.

² The validity of this and similar assumptions has been questioned, most seriously by Noam Chomsky, *Syntactic Structures* (The Hague, 1957), 49-60.

Such a zero contrasts with an overt plural. Does it alternate with an overt form? That is, are there forms where the singular is bimorphemic? Forms like *datum*, *data* might be considered as evidence for positing a singular morpheme. So, for that matter, might forms like *man*, *men*. But most linguists argue that *man* must be monomorphemic because *pan* is. The circularity is clearly vicious: *man* is monomorphemic because *pan* is; and *pan* is monomorphemic because *man* is. Now we may try to modify the first condition so that zero must not only alternate with an overt form, but that this overt form must be productive, or at least less marginal than the alleged singular in *man*. Accordingly *boy* has no zero singular.

However, the notion of productivity may have another aspect. For example, the Spanish plural normally has allomorphs /-s/ after vowels and /-es/ after consonants as in *hombre/hombres* "man/men" and *mujer/mujeres* "woman/women". But forms ending in unstressed vowel plus /-s/ have no overt marker for the plural; thus, *el lunes* "Monday" *los lunes* "Mondays" *el paraguas* "the umbrella" *los paraguas* "the umbrellas". In such a case, the proposed zero allomorph of the plural would be phonologically conditioned and all new forms would have predictable zeros for the plural. But this zero fails to satisfy the second condition. Since there is no singular morpheme, the form *paraguas* is ambiguous, and the zero does not meet the requirement of contrasting with an overt form. We must either refuse to recognize a zero plural in *los paraguas* or modify our conditions to permit a productive, phonologically conditioned zero even in the absence of contrast with an overt form.

The above example suggests that there may be cases where the conditions of contrast and alternation preclude establishing a zero element, even if such an element could be accounted for by very general morphophonemic rules. Similarly, there are cases where we would presumably want not to establish morphological zero elements even though both conditions are apparently fulfilled. Consider English *He came home*. A zero could be said to alternate with the *to* in *He came to school*. Notice that in both cases there is a contrast with a form like *from*: *He came from home/He came from school*. The zero then would alternate with an overt form *to* and contrast with an overt form *from*. And yet there is considerable reluctance to posit such a zero. Most linguists apparently prefer to assign *home* to two classes, one including *school*, *church*, etc. as in *I see my home*, *I see my school*, *I see my church*, and the other including *downtown*, and perhaps *west*, etc. as in *He came downtown*, *He came west*. I do not wish to argue for positing a zero allomorph of *to* in *He came home*; on the contrary, such an analysis seems to obscure differences which should be pointed out. However, it is clear that the two conditions of alternation and contrast with overt forms are not sufficient. Another modification is required so that word-like units may not have zero alternates. Indeed, we seem most willing to accept zeros for affixes which are clearly inflectional, but the basis for such a hierarchical preference is rarely made explicit. Consider the form *fast* in *A fast driver* and *He drives fast* compared to other adjective-derived adverbs with *-ly*, *slow/slowly*, *quick/quickly*, etc. We are perfectly willing to allow monomorphemic adverbs which are identical with

adjectives rather than positing zero allomorphs of {-ly} in adverbial forms like *fast* in *He drives fast* and *long* in *Has he been gone long?*, etc. The morpheme {-ly} though clearly more bound than the preposition *to* is nevertheless not an inflectional suffix of the kind which is added to a large, general class such as the class of all adjectives and zero is usually restricted to such pervasive categories.

We have tried to suggest, then, that the usual conditions of alternation and contrast with overt forms, proposed for positing zero as a morphological element may be neither necessary nor sufficient. The conditions as stated do not normally provide for the relevance of the productivity or pervasiveness of either the zero or its alternants. Zero then is a device which has the effect of combining syntactically similar sub-classes which are formally different, thus facilitating statements of greater generality. The fact is, however, that no matter how many conditions we may make justifying the use of zero, we shall not have answered what is perhaps the crucial question, namely, what conditions compel the use of zero?

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DISCUSSION

HOUSEHOLDER:

Mr. Saporta has posed his question rather as a psychological query about the behavior of linguists than as a query about linguistic theory. His example of *paraguas* is quite similar to the English situation with the possessive ('s), which is predictably zero after a plural morph ending in s or z but elsewhere has an overt shape (s, z, iz or iz). Looked at from the point of view of syntax, it is obviously necessary to set up a gender morpheme for every Spanish noun, whether overt or not, and a plural morpheme (in both English and Spanish). Saporta's further cases are more dubious; "home" is most naturally listed in expansions of an allative phrase symbol, hence no zero needed, and English adverbs in -ly (and analogous constructions in other languages) constitute a very peculiar problem. In general we must consider a verb (say "depart") basic and the noun ("departure") transformationally derived; on the other hand we should like to make the adjective (as in "quick departure") basic, and the adverb (as in "depart quickly") transformationally derived. How can we do both?

ROBINS:

The search for universally valid conditions, either on the one hand precluding the positing of zero elements in linguistic analysis at any level or on the other hand compelling it, is likely to prove a vain one. It would be difficult to imagine any set of linguistic phenomena which could in no way be analyzed formally without recourse to zero, though, of course, there are many sets of which the most satisfactory analysis does make use of zero elements at some point.

The positing of zero elements is one of the means available to the linguists to

smooth out in his analysis the awkward corners of languages in the interests of general symmetry or congruence of linguistic statement. Actual obligation on its use or avoidance only arises in a particular language after it has once been adopted (or avoided) elsewhere in the language with regard to comparable material (under the general scientific requirement of consistency). Thus in English if *sheep* plural is analyzed as /ʃi:p/+/ø/, then *aircraft* plural must be similarly analyzed as /'ækra:ft/+/ø/.

While no rules of general validity are to be sought in this, relative frequencies of the forms involved is often made a guide to the desirability of using zero elements in an analysis. The more such elements are used, the weaker is their power of bringing the phenomena to which they are applied within symmetrical and elegant analytic statements. Thus the positing of zero as a plural marker in English *sheep* etc., which are in contrast with vastly more numerous regular forms such as *lamb*, *lambs*, is much more easily justifiable than it would be in Chinese (Mandarin), where the nouns with an overt plural suffix *-men* (*ta¹mên*, they; *hsüeh²shêng¹mên*, students, etc.) are only a small subclass of nouns, the great majority not having a formal marker of plurality.

General statements on the use of analytic devices like zero elements can only take the form of suggested lines of linguistic conduct or of retrospective summaries of observed tendencies in previous work. Such summaries may reveal areas of agreement in the way such devices are in fact used, and may in consequence serve as guides for future work; but they can hardly claim the status of prescriptive, universally valid rules.

THE FALLACY OF A UNIVERSAL LIST OF BASIC VOCABULARY

SAUL LEVIN

Glottochronology has been accorded a mixed reception since it was first developed about ten years ago. The proponents, particularly Morris Swadesh, have always conceded – somewhat disarmingly – that the method has yet to be perfected and that the results so far obtained from it need to be handled with caution. Meanwhile various researchers have continued to use the method, each modifying it a little and resolving its ambiguities in accord with his own judgement. On the other hand it has often received scathing criticism. The controversy has clarified a great deal of linguistic thought, and the glottochronologists deserve credit for stimulating the whole profession – their critics as well as their adherents.¹ The critics have mostly brought up objections that could hardly have been clear to Swadesh and his early collaborators from the limited material which went into their computations of vocabulary changes in certain languages with a long recorded history. I must point out, however, that the original material itself – had they considered it more carefully – ought to have suggested to them that they were attempting to measure the unmeasurable. The part of the material I am going to cite belongs to what would seem the most accessible pairs of an ancient and a modern language – Latin and French, Spanish, and Italian, those of the Romance family that have enjoyed the widest international currency. Latin and the major Romance languages must be common knowledge at any university, though the United States perhaps is not so happy as other countries in this one respect.

Linguists are prone to look wistfully at those sciences which have made great advances through statistical techniques; but at least for the present period of linguistic research, as in the past, statistics are no substitute for exact knowledge of particular languages. The rule that basic vocabulary is changed at the approximate rate of 19% every thousand years was obtained by a comparison of about two hundred items in ancient (or early medieval) languages and their later descendants:

¹ See the long review article, with copious bibliography, by D. H. Hymes, "Lexicostatistics So Far", to which are appended comments by seven other linguists – *Current Anthropology*, I (1960) 3-44. So thorough is Hymes's report that it leaves me feeling authorized to skip over most aspects of the glottochronology controversy, omit mention of many noteworthy articles, and limit my view here to a small number that bear strongly on my immediate and particular concern. In this paper I am much indebted to conversations with Dr. Michael M. Horowitz, assistant professor of anthropology at Harpur College.

- | | |
|-------------------------------------|-------------------------------------|
| (1) Old and Modern English | (8) Ancient and Modern Chinese |
| (2) Latin and Spanish | (9) Old Norse and Modern Swedish |
| (3) Latin and French | (10) Latin and Italian (Tuscan) |
| (4) Old and Modern High German | (11) Latin and Portuguese |
| (5) Middle Egyptian and Coptic | (12) Latin and Rumanian |
| (6) Koine and Modern Greek (Athens) | (13) Latin and Catalan ² |
| (7) Koine and Modern Greek (Cyprus) | |

Later Swadesh obtained a new average of 14.6%. The main cause for the difference was that he revised the list, cutting it down to 92 items and then adding eight to make an even hundred.³ He also excluded Spanish, Italian, Portuguese, Catalan, and the Greek of Cyprus from his averaging, because part of their evolution was not separate from French or the Greek of Athens; he excluded Coptic too, because it may not be a linear descendant of Middle Egyptian. That Swadesh's lists, or any likely modification of them, will not do justice to the plain facts of shifts in vocabulary, ought to be clear if you stop to scrutinize just his first few items, whether arranged alphabetically or by a certain classification.

For instance, the Latin word for "all" is reported to survive in French; evidently *tōtum* is accepted as the Latin word for "all".⁴ Latin does indeed employ this word in expressions such as *tōta Italia* (Cicero, *In Verrem* 5.8, etc.) where English might render it "all Italy", and the like. But another Latin word *omne* corresponds to "all" at least as often as *tōtum* does. *Omne* is gone from French; its semantic sphere has been largely taken over by *tout*, though not completely.⁵ In Italian part of that sphere is still covered by *ogni* – e.g., *ogni ragione* continues *omnem ratiōnem* "every reason". The Romance descendants of *tōtum* have thus gained ground, but unequally. The method of glottochronology, however, permits only a yes-or-no relation: What was

² The procedure is explained by Swadesh particularly in two articles, "Salish Internal Relationships", *IJAL* 16 (1950), 157-167, and "Lexico-statistic Dating of Prehistoric Ethnic Contacts", *Proceedings of the American Philosophical Society*, 96 (1952), 452-463; and by Robert B. Lees, "The Basis of Glottochronology", *Language*, 29 (1953), 113-127. In the interest of greater accuracy Lees, p. 118, specified "Old English of 900-1000 A.D. : Modern English ...; Plautine Latin of 200 B.C.: early Modern Spanish of 1600 A.D. ...", etc.

³ "Towards Greater Accuracy in Lexicostatistic Dating", *IJAL*, 21 (1955), 121-137. For brevity and fairness I will not take up any items used by Swadesh at first but later demoted by him as less suitable than the ones he retained.

⁴ *IJAL*, 21, 133. The reports of Swadesh and Lees save space by listing only the modern English word, not the one identified in each language – ancient and modern – as its equivalent. Knut Bergsland and Hans Vogt, in their very recent and devastating critique, "On the Validity of Glottochronology", *Current Anthropology*, 3 (1926), 129, call for the publication of the Latin-Romance and other lists. Swadesh in his comment on their article (p. 144) remarks that microfilm copies of the lists have been available on request. For the most part they are not needed by persons who know the languages; from a + or – in the table you can figure out which word in both the earlier and the later language must have been identified with a certain English gloss. Yet sometimes we are left wondering; e.g., if "fat (grease)" persisted in French but was lost in the other Romance languages, and if "walk" persisted in Spanish and Rumanian but not in the others, what are these words?

⁵ *Chaque* has moved into some of it.

the one word for "all" in Latin? What is it in each Romance language, the same word (apart from phonetic changes and – in some items – an added suffix) or a different word? The equation of *tōtum* "all" is the more surprising because Swadesh had earlier contrasted the Latin *omnēs* with the Spanish *todos*.⁶

The first item in the non-alphabetic table (*IJAL* 21.132) is the pronoun "I", which persists 100% in the test languages. As Latin and French are included, the method must have overlooked the point that while *ego* is phonetically the ancestor of *je*, the functions of *ego* have been almost entirely taken over by *moi* (from *mē*).⁷ Semantically *je* answers not to *ego* but to the first person singular ending of Latin verbs. It will hardly do to proceed as though the meaning of *ego* were still in *je* because both can be translated "I"; an intelligent oral translation of Latin and French into English would give "I" more stress when it represents *ego*.⁸

The oversimplification that runs through glottochronology can be pinpointed in the last word of this statement of Swadesh on method: "A simple clear-cut criterion of what constitutes a change could be set up, namely, the substitution of a new element from whatever source as the most usual everyday expression of a given notion."⁹ He has proceeded as though "all" or "I" or any other item on his list were not just an English gloss but a "given notion", and as though the same given notion could be expected to recur in other languages. So in a comparison between any two languages the notion would be unchanged and permanent; the sole question would be whether the sequence of sounds for expressing the notion in one language is related to the sequence of sounds used in the other. But only a superficial study could be content to treat the items of a vocabulary list as universal and unchanging notions. With a little probing Harry Hoijer found how hard it was to translate thirty-nine of Swadesh's hundred items into Navaho and other Athapaskan languages.¹⁰ Hoijer is to be commended for trying the list out on these American Indian languages; but difficulties just like the ones he revealed ought to have been pointed out by anyone who took the trouble to juxtapose Latin and Romance. When the table states that the Latin word for "woman" persists in French but not Spanish, the method has evidently forbidden the investigators to allow that Latin has two very common words, *fēmina* (accusative *fēminam*) and *mulier* (accusative *mulierem*), from which are derived, respect-

⁶ *Proceedings of the American Philosophical Society*, 96, 455. Quite apart from debatable matters of judgement, I am afraid that the tables, at least as published, were not checked well enough to catch mere mistakes which nobody ever intended.

⁷ One error which raises a smile is that the Old English word for "thou" persists in Modern English.

⁸ The glottochronologists have not given special treatment to the considerable number of French words in their lists which have phonetically diverged so far from Latin and Romance that their status as cognates would be unrecognizable if not for studies in historical and comparative grammar: [ʒ] from *ego*, [ʃjē] from *canem* "dog", [œj] from *oculum* "eye", [ʃvø] from *capillum* "hair", [œ] from *ūnum* "one", [o] from *aqua(m)* "water", and others.

⁹ *Proceedings of the American Philosophical Society*, 96, 455.

¹⁰ "Lexicostatistics: A Critique", *Language*, 32 (1956), 53-58. He noted, however, "no other investigator has reported so large a number of items difficult to translate with precision".

ively, the French *femme* and the Spanish *mujer*. The two Latin words are not simply interchangeable. *Mulier* is less respectful; wherever it would be possible in English to say either *woman* or *lady*, the Latin equivalent could only be *fēmina*. Yet *fēmina* is also the word for a female animal (hence the Spanish *hembra*). Unless an ample context is given, no one can rationally settle upon the Latin word for "woman".¹¹ A little further reflection brings out that etiquette plays a large but indefinite and therefore unmeasurable role in the usage of basic vocabulary, as in other facets of social behavior. The persistence or change in the etiquette or associations of certain words is always a fascinating study; but lexicostatistics seems bound to ignore all such subtleties. Its incapacity to cope with them raises doubt whether it can give any trustworthy results.

Shifts in vocabulary, including basic vocabulary, are cultural changes, not arbitrary substitutions within an autonomous network of vocal symbols. The glottochronologists, like any other linguists, know this of course; but the operation of their method apparently obliges them to overlook it. The trouble is well illustrated by the item "bird"; the Latin word (*avis*, accusative *auem*) is reported to survive in French and Italian but not Spanish. Not *avis* itself (which is feminine) but its masculine diminutive *aucellus* (accusative *aucellum*) from Late Latin is the source of *oiseau* and *uccello*. Little remains phonetically of the Latin *au*- "bird"; the accented diminutive suffix is much more prominent and is the symptom of a semantic change, parallel at bottom to what happened in Spanish. For there *ave*, while it still signifies "bird", is less frequent than *pájaro*, from *passer* "sparrow". It was right to disregard *ave* as irrelevant to the basic vocabulary. But the present use of *pájaro* is only a partial change from Latin, because the Spaniard says it most often in reference to the kind of bird called *passer* in Latin.¹² In spite of what all birds have in common, one kind differs much from another; and the primary interest or experience of a language community may be focused upon certain kinds more than others. The Romans (and other ancient peoples) paid most attention to large, soaring birds which would appear ominously against the sky when men were tense and wavering in their deliberations. As ancient civilization declined, such experiences became rarer or less important. The birds most in evidence now in the Romance countries are small, chirping, fluttering denizens of

¹¹ In an oral comment at the session Professor E. Adelaide Hahn remarked that *fēmina* is to *mulier* as *uir* is to *homō*: *homō* is the less respectful word for "man", *mulier* for "woman". Her point can be supported by much Latin usage. However, instances turn up where the opposition of sex is expressed to *uir*: *mulier*; e.g., in the very early "Senatus consultum de Bacchanalibus" HOMINES · PLOVS · V · OINVORSEI · VIREI · ATQVE · MVLIERES "more than five persons in all, men and women" (*Corpus Inscriptionum Latinarum* 1². 581. 19).

¹² The "sparrow" is now specifically *gorrión* in Spanish. However, if we were to set up a typical context – one person asking an other about something that just darted past them – it might well go thus:

Latin A: *Quid fuit istud?* ("What was that?") B: *Passer.*

Spanish B: *¿Qué fué esto?*

B: *Un pájaro.*

A Roman did not limit *passer* to just one species and would probably not have said *avis* in reference to such an insignificant thing.

a man-made environment. So it is Procrustean to summarize the evolution of terms for "bird" as in Swadesh's table: French +, Italian +, Spanish -¹³.

It does not appear that Swadesh himself ever grappled much with such semantic problems in the Romance or the other "control" languages. His own research has taken up a great many ill-documented American languages. In such studies the obtainable material may consist of nothing but a brief vocabulary compiled in haste by an anthropologist or out of curiosity by a missionary, trader, or other unprofessional visitor. One practical advantage of a list like Swadesh's is that even from a language no longer spoken, but not yet forgotten by one or two old people, you can elicit these few dozen words. But the more a linguist knows of how the words in a certain language are really used, the less offhand he will be in identifying them semantically, whether with words in languages known or supposed to be cognate to it, or with English glosses. The changes of vocabulary that we are in the best position to study are too intricate to be quantified without grave distortion. Nor can we expect to reestablish Swadesh's constant rate of change by arguing that with closer attention to empirical facts such as I have brought out, we would allow in some cases for changes not recognized by him but would balance them by disallowing or lessening other changes which he admitted. His list – or any conceivable modification of it – admits only of loose handling. If you try to rid it of imperfections, you have nothing left to work with. For as soon as you reflect closely upon identifications and upon decisions as to retention or loss, you confront something inescapable: no word refers to an objective reality unalloyed. Therefore changes in the semantic content of words, insofar as it is non-objective, can only be sensed intuitively or sympathetically, but not formulated numerically, either in whole numbers or in fractions. The attempt to mathematicize semantic changes cannot help being arbitrary and falsifying the material.

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¹³ With a plus sign to indicate persistence, the glottochronologists lump together words that have undergone a major morphological process and words that have suffered purely phonetic changes. So from the Latin *cor* "heart" the Spanish derivative *corazón* is put on the same footing as the Italian *cuore* and the French *cœur*. The method equates what is obviously unequal. But what mathematical terms could do justice to a partial deviation such as *cor* : *corazón*?

GLOTTOCHRONOLOGY WITH RETENTION-RATE INHOMOGENEITY

MARTIN JOOS

Abstract

The current mathematical theory of glottochronology can be adequate only if the members of the test list are equally likely to vanish in time. But it is *a priori* far more reasonable to assume that some are hardier than others, and some less hardy. Their hardinesses (retention-rates) are probably spread out fairly smoothly from nearly complete retention down to the neighborhood of about 50 per cent retention (rate 0.50), with a skewed distribution to which the following is a practical approximation:

words:	2	7	17	24	24	17	7	2	--- total 100
retention:	0.96	0.93	0.89	0.84	0.78	0.71	0.63	0.54	per 1000 years
retained:	1.92	6.51	15.13	20.16	18.72	12.07	4.41	1.08	--- total 80.00

Over a period of exactly 1000 years, this gives the same result as 100 words each with retention-rate 0.80. Consequences include (a) that the discrepancy could not be discovered during calibration, and (b) that the discrepancy would very badly spoil the argument for periods of several thousand years, such as are often deduced in glottochronology. A further discrepancy in the same direction would result from increased hardiness of surviving words resulting from their incorporation in idioms. Tables and curves will be included, suggesting improved procedures.

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ON THE VALIDITY OF COMPARATIVE LEXICOSTATISTICS

ISIDORE DYEN

The central question in comparative lexicostatistics today is whether it is valid.¹ By comparative lexicostatistics we mean those procedures of applying statistics to the genetic comparison of the lexicons, particularly the basic vocabularies of related languages. If comparative lexicostatistics is valid, there is a likelihood that it can be used for glottochronological purposes. If it is not valid, then obviously comparative lexicostatistics is useless not only for glottochronological purposes, but also for anything else.

In comparative lexicostatistics at present our chief tool is a standard basic meaning list. Such lists as the 200-word Swadesh list, the 215-word Swadesh list and the 100-word Swadesh list are examples of such basic meaning lists. We collect a gloss-list for each language according to the basic meaning list. The percentage of plausible cognates in a pair of lists is the lexicostatistical percentage of that pair of lists or the pair of languages represented by the lists.

We now have strong evidence that there is a factor in these lexicostatistical percentages which is independent of the language-family involved. As conceived, this factor is a culture-free phenomenon, thus constituting a language universal. To this extent it could be said to establish comparative lexicostatistics on a rigorous scientific basis. The factor concerned is evidenced, among other things, in what Swadesh called the constant rate of change of basic vocabulary.²

For our purpose the basic vocabulary of a language is its gloss-list. We now believe that a gloss-list tends to change in time at a regular rate. This is so because for any given time interval each meaning has associated with it a probability of change. Although these probabilities vary for different meanings, the collective effect of the varying probabilities is to produce the tendency toward a regular rate of change. We do not have these probabilities for each meaning as yet, but the evidence to be presented suggests they are constant.

For a given time-interval each meaning has associated with it a probability of

¹ The work on which this paper is based was supported in various phases by grants from the Tri-Institutional Pacific Program, the National Science Foundation, and the Department of Health, Education and Welfare.

² Cf. M. Swadesh, "Lexico-statistic Dating of Prehistoric Ethnic Contacts", *Proc. of the Am. Phil. Soc.*, 96 (1952), 452-463.

replacement; that is, a probability that the ordinary word with that meaning will be replaced. We have evidence that indicates that the relative frequency of replacement in meanings is so similar for different language families that we can define a replacement constant as the probability that a word in a given meaning will be replaced within a given time. A list replacement constant is then the collective effect of all the meaning replacement constants in the list. It is the effect of the list replacement constant that was observed by Swadesh in his constant rate of change phenomena.

However, it is quite clear that other factors than the replacement constant, such as taboo,³ affect the lexicostatistical percentages. The rate of change of basic vocabulary should be viewed as being constant if other factors are non-operative. There are, however, other factors which speed up or slow down the actual rate of change. The evidence seems to point to the fact that at least for certain meanings the probability of a replacement remains constant throughout time.

We can now proceed to the evidence.

Of the meanings of the 200-word Swadesh list 196 were ranked by the frequency of cognate-pairs in each meaning as they appear in 89 Malayopolynesian lists.⁴ (The meanings excluded were "freeze", "ice", "snow", and "that".) Thus if the frequency of cognate-pairs occurring in the meaning "nose" is greater than the frequency of cognate-pairs meaning "liver" the ranking of "nose" is higher than that of "liver". The resultant ranking is called the Malayopolynesian Cognate-Pair Frequency Ranking (CFRMP).

The lists of Malayopolynesian languages were selected chiefly because at the time when the ranking was developed these lists constituted all of the Malayopolynesian lists with 195 entries or more and seemed to be sufficiently different to be regarded as lists from at least highly divergent dialects if not different languages. The list is admittedly neither as large nor as representative as it might be.

There are two types of indeterminacy that must be taken into account in establishing the ranking. These are (1) the indeterminacy introduced by blank pairs, i.e. pairs in which at least one entry is blank and (2) the indeterminacy introduced by doubtful cognate-pairs. A doubtful cognate pair is a pair of words whose relationship is in doubt. Thus, for example, English *feather* and Russian *pero* constitute a doubtful cognate pair.

If there were no doubtful cognate-pairs, it would not be difficult to establish the ranking. The number of cognate-pairs in a set of cognates can be obtained by the formula for the number of different pairs in a set of objects if order is not distinctive: namely $n(n-1)/2$. The percentage of cognate-pairs in the maximum possible number

³ Cf. K. Bergsland and H. Vogt, "On the Validity of Glottochronology", *Current Anthropology*, 5 (1962), 115-129, particularly pp. 126 f.; and I. Dyen, "Lexicostatistically Determined Borrowing and Taboo", *Language*, 39 (1963), 60-66.

⁴ These lists are given in Appendix II.

of cognate-pairs determines the ranking of a meaning. The use of the percentage of pairs rather than the raw number of pairs takes account of blank pairs.

The indeterminacy introduced by doubtful cognation was taken into account in the following way: two sums were taken for each meaning and these two sums were then averaged to obtain the score for the given meaning. The first sum was reached by counting all doubtful cognations as *positive*; in this way the *largest* possible number of cognate-pairs (exclusive of the effects of blanks) in each meaning was obtained. A second sum was obtained by counting all doubtful cognations as *negative*; in this way the *smallest* possible number of cognate-pairs (exclusive of the effects of blanks) was obtained. The average of the two sums is taken to be the closest available approximation to the sum of cognate-pairs which would be obtained under optimum conditions.

The ranking (= CFRMP) obtained is used in presenting the Lexicostatistical Comparison of Lithuanian and Russian in Appendix I.

If the meanings, and thus the homosemantic pairs, in a comparison of two Malayopolynesian gloss-lists are arranged in the CFRMP, it is a natural consequence that the cognate pairs tend to be concentrated in the higher ranking meanings. Thus if the ranking is divided into thirds, the first third of the ranking should normally have a higher percentage of cognate pairs than the second, and the second third a higher percentage than the third third. The convention is followed of using R for "percentage" and roman numerals for the thirds and to divide the CFRMP in the following way: I — meanings 1-65; II — meanings 66-130; III — meanings 131-196. Thus we expect $R_I > R_{II} > R_{III}$.

It is not of course a necessary consequence of the CFRMP that every pair of MP languages will have their thirds arranged as above. For example, where the overall percentage of cognates is extremely high, the differences, being small in number, have a better chance of appearing unexpectedly to be larger in the second third than in the third third; or, possibly, even larger in the third third than in the first or second third. Similarly where the overall percentage is very low and the number of cognates in the second and third thirds is very small, the chances are improved that the percentage in the third third may be larger than that of the second third. Thus where the overall percentages are very large or very small the likelihood is increased that the percentages of the thirds will not be in descending order. In the middle ranges of overall percentages, say between 25% and 75%, the likelihood of conformity is greatest.

Yet, as we have said, it would not be surprising if the cognate percentages in the respective thirds of a Malayopolynesian language pair were to show a descending relation. It *would*, however, be surprising if a related pair of non-Malayopolynesian languages were to show the same effect when the meanings are arranged in the CFRMP.

If a single related pair of non-Malayopolynesian languages were to show this effect it can be calculated that there is a one in six chance of having this happen by

TABLE I
*Cognate-Pair Percentages of Four Pairs
of Non-Malayopolynesian Languages in Thirds of the CFRMP*

	+	—	%
Lithuanian – Russian			
1 – 65	30	34	46.9
66 – 130	23	41	35.9
131 – 196	14	50	21.9
Total	67	125	34.9
Tobelorese – Loda			
1 – 65	26	39	40.0
66 – 130	20	26	35.7
131 – 196	14	38	26.9
Total	60	113	34.7
Bahnar – Mnonggar			
1 – 65	33	31	51.6
66 – 130	19	41	31.7
131 – 196	12	46	20.7
Total	64	118	35.2
Nonivia – Nambakaenga			
1 – 65	28	28	50.0
66 – 130	14	32	30.4
131 – 196	10	38	20.8
Total	52	98	34.7

chance. There are six ways in which the percentages of plausible cognates in the individual thirds could be related to each other, ties being ignored. They are as follows, the conforming relation being (1):

- (1) $R_I > R_{II} > R_{III}$
- (2) $R_I > R_{III} > R_{II}$
- (3) $R_{II} > R_I > R_{III}$
- (4) $R_{II} > R_{III} > R_I$
- (5) $R_{III} > R_I > R_{II}$
- (6) $R_{III} > R_{II} > R_I$

There is thus one chance in six that a pair of non-Malayopolynesian languages would show thirds in the conforming relation. There is then one chance in thirty-six ($= 6^2$) that two pairs of non-Malayopolynesian languages would show the conforming relation. We have tested the CFRMP on four pairs of non-Malayopolynesian languages. All four pairs showed conformity. There was one chance in 1296 ($= 6^4$)

that such an event could occur by chance. We conclude that it is more likely that this event was brought about by the CFRMP. Since the four pairs of non-Malayopolynesian languages are pairs which are related to each other and not to the other pairs, it is clear that we have far transcended the bounds of a single language family. This is evidence for a hypothesis that the CFRMP approximates a CFR which applies to all language families, that is, to a universal CFR or CFRU.

I have presented in Appendix I a full comparison of Lithuanian and Russian as an example of the procedure followed.⁵

The percentages of cognate-pairs in the CFRMP thirds of the four pairs of non-MP languages are given in Table I.

In addition to the comparison of (1) Lithuanian and Russian there are those of: (2) Tobelorese and Loda — two related non-Malayopolynesian languages of north Halmahera in Indonesia; ⁶ (3) Bahnar and Mnonggar — two Mon-Khmer languages of Vietnam; ⁷ (4) Nonivia and Nambakaenga — two related non-Malayopolynesian languages of the Santa Cruz Islands of Melanesia.⁸

This much for our first bit of evidence.

A CFR was drawn up for Indoeuropean languages (CFRIE) on the basis of 39 lists. Many of these lists were drawn from the well-known book by C. D. Buck, *A Dictionary of Selected Synonyms in the Principal Indo-European Languages*. Only the modern languages were collected. These amount to eighteen. To these 21 other lists were added, collected from dictionaries, colleagues, and native speakers.⁹ In order to utilize the lists from Buck the 200-word list was reduced to 154 meanings well-covered in Buck and occurring in the CFRMP.

The frequency of cognate-pairs in each meaning was calculated as for the CFRMP. The following is the cognate-pair frequency ranking of 154 basic meanings reached by using Indoeuropean material (CFRIE):

1. three, 2. name, 3. new, 4. tongue, 5. sun, 6. heart, 7. wind, 8. to give, 9. eye, 10. tooth, 11. mother, 12. to die, 13. egg, 14. night, 15. to sit, 16. star, 17. salt, 18. one, 19. ear, 20. day, 21. to live, 22. nose, 23. know, 24. long, 25. to drink, 26. father, 27. to stand, 28. smoke, 29. yellow, 30. water, 31. foot, 32. to sew, 33. seed, 34. worm, 35. thin, 36. to spit, 37. to sleep, 38. dry, 39. short, 40. to suck, 41. feather, 42. warm, 43. white, 44. meat, 45. sea, 46. bone, 47. narrow, 48. fire, 49. dog, 50. root, 51. green, 52. to eat, 53. fish, 54. flower, 55. man, 56. woman, 57. to hear, 58. red, 59. wife, 60. to lie, 61. to blow, 62. head, 63. to tie, 64. fog, 65. hand, 66. to kill, 67. to come, 68. to float, 69. leaf, 70. husband, 71. to say, 72. right (side), 73. to sing, 74. lake, 75. skin, 76. neck, 77. to see, 78. tree, 79. to fly, 80. stone, 81. sky, 82. to flow, 83. earth, 84. all, 85. heavy, 86. wide, 87. bird, 88. to dig,

⁵ The Lithuanian was given by Alfred Senn, the Russian by Alexander Schenker.

⁶ Tobelorese from A. Huetting, *Tobeloreesch-Hollandsch Woordenboek* (Den Haag, 1908). Loda data from M. J. Van Baarda, *Woordenlijst: Galélareesch-Hollandsch* (Den Haag, 1895); and by the same author, "Het Lòda'sch", *Bijd. t. d. Taal-, Land-, en Volkenkunde*, 56 (1904), 317-496.

⁷ Bahnar list from R. Pittman, Mnonggar from Ziemer.

⁸ Both lists from D. A. Rawcliffe via George Grace.

⁹ The sources of the Indoeuropean lists are given in Appendix III.

89. to swim, 90. blood, 91. smooth, 92. ashes, 93. hair, 94. animal, 95. straight, 96. to wash, 97. black, 98. to cut, 99. to walk, 100. sharp, 101. rub, 102. to fall, 103. small, 104. snake, 105. to laugh, 106. to breathe, 107. mountain, 108. wing, 109. sand, 110. to burn, 111. liver, 112. hold, 113. few, 114. to bite, 115. year, 116. old, 117. cold, 118. many, 119. good, 120. near, 121. to hunt, 122. to split, 123. grass, 124. to play, 125. woods, 126. to hit, 127. leg, 128. dust, 129. mouth, 130. child, 131. to fight, 132. to think, 134. wet, 135. rope, 136. dull, 137. to pull, 138. thick, 139. road, 140. belly, 141. big, 142. left (side), 143. far, 144. cloud, 145. back, 146. to vomit, 147. fruit, 148. to push, 149. to smell, 150. bad, 151. tail, 152. dirty, 153. to throw, 154. to turn.

A selection of the 154 meanings of the CFRIE was made from the CFRMP, keeping their CFRMP order. Each ranking was then divided into thirds in the following way: I — meanings 1-52; II — meanings 53-104; III — meanings 105-154. A three-by-three table was constructed, presented in Table II:

TABLE II
*Distribution of 154 Meanings of Swadesh 200-Word List
in the Respective Thirds of CFRMP and CFRIE*

CFRMP → CFRIE ↓	1 - 52	53 - 104	105 - 154	T
1 - 52	26	18	8	52
53 - 104	19	20	13	52
105 - 154	7	14	29	50
T	52	52	50	154

The 52 meanings in the first third of the Indoeuropean ranking were distributed over the top three squares. In the first square to the left were counted those which also occurred in the first third of the Malayopolynesian ranking. In the middle square those in the second third of the Malayopolynesian ranking and in the upper right hand square those which are in the third third of the Malayopolynesian ranking.

The meanings in the second third of the Indoeuropean ranking were similarly distributed over the middle row of squares and those in the third third of the Indoeuropean ranking were distributed over the lowest row of squares.

It is easy to see that the upper left hand corner consists of a goodly collection of meanings occurring in the upper third of both rankings and that the lower right hand corner has an even stronger collection of meanings occurring in the lower third of both rankings. What is most interesting is the fact that a X^2 -test for the significance of the difference between the three rows produces a result of over 25, a number which could have been produced by a chance relation less than once in a thousand

times.¹⁰ The difference between the three rows is the effect of the similarity of the two rankings.

This concludes the second bit of evidence. Both bits of evidence agree and can be said to be independent of each other in the main. There was less than one chance in a million that all of these events could have occurred by chance. It is thus more likely that the cognate-pair frequency rankings reflect a factor which is independent of the language family. This factor, at least for the present, we call the replacement constant. It has yet to be determined for each meaning and for the list as a whole. As calibrated with time it can be used for glottochronological purposes. Work is now going on to determine the value of the replacement constants for each meaning. In any case, there is every reason to believe that the validity of comparative lexicostatistics has been established beyond reasonable doubt.

APPENDIX I

Lexicostatistical Comparison of Lithuanian and Russian
(Meanings in CFRMP)

	Lithuanian	Russian			Lithuanian	Russian	
1. five	penki	pjat'	+	27. blood	kraūjas	krov'	+
2. two	dù (m.), dvi (f.)	dva	+	28. he	jis	on (jemu)	+
3. eye	akis	glaz	—	29. kill	užmūšti	ubivat'	—
4. we	mēs	my	+	30. leaf	lāpas	list	—
5. louse	utėlė	voš'	—	31. tongue	liežuvis	jazyk	—
6. father	tėvas	otec	—	32. sky	dangūs	nebo	—
7. to die	miřti	umirat'	+	33. road	kėlias	doroga	—
8. to eat	vālgyti	est'	—	34. to vomit	vėmti	rvat'	—
9. mother	mótina	mat'	+	35. fish	žuvis	ryba	—
10. four	keturi	četyre	+	36. tooth	dantis	zub	—
11. three	trīs	tri	+	37. thin	plónas	tonkij	—
12. one	vėnas	odin	+	38. bird	paūkštis	ptica	+
13. stone	akmuo	kamen'	+	39. hand	rankà	ruka	+
14. nose	nósis	nos	+	40. to fear	bijóti	bojat'sja	+
15. to hear	girdėti	slyřat'	—	41. what?	kàs	čto	+
16. new	naūjas	novyj	+	42. they	jì	oni	+
17. thou	tù	ty	+	43. far	tólimas	daleko	—
18. ye	jūs	vy	?	44. egg	kiaušinis	jajco	—
19. fruit	vaīsius	plod	—	45. to come	ateiti	priři, priřodit'	+
20. name	vařdas	imja	—	46. fire	ugnis	ogon'	+
21. ear	ausis	uxo	+	47. bone	káulas	kost'	—
22. liver	kėpenos, kėpenys	pečen'	+	48. head	galvà	golova	+
23. tree	mėdis	derevo	—	49. in	loc., ĩ + acc. v		+
24. to drink	gėrti	pit'	—	50. right (hand)	deřinys	pravyyj	—
25. ashes	pelenai	zola	—	51. I	àř	ja	+
26. to rain	lyti	dožd' idet	—	52. who?	kàs	kto	+
				53. feather	plūnksna	pero	—
				54. woman	žmonà	ženščina	—

¹⁰ The probability of $X^2 = 18.467$ with four degrees of freedom is .001: R. A. Fisher and F. Yates, *Statistical Tables* (New York, 1957), p. 45.

	Lithuanian	Russian		Lithuanian	Russian		
55. heavy	sunkùs	tjaželyj	—	107. dog	šuõ	sobaka	—
56. day	dienà	den'	+	108. animal	gyvulỹs	životnoe	+
57. stick	pagalỹs	palka	—	109. to blow	pũsti	dut'	—
58. bark (tree)	mẽdžio žievẽ	kora	—	110. red	raudõnas	krasnyj	—
59. water	vanduõ	voda	+	111. salt	druskà	sol'	—
60. root	šaknìs	koren'	—	112. child	vaikas	rebenok	—
61. to dig	kàsti	ryt'	—	113. person	žmogùs	čelovek	—
62. to sleep	miegõti	spat'	—	114. rotten	supũvęs	gniloj	—
63. hair	plàukas	volosy	—	115. snake	gyvãtẽ, žaltys	zmeja	—
64. to bite	kãsti	kusat'	—	116. night	naktis	noč'	+
65. smoke	dũmai	dym	+	117. wide	platùs	širokij	—
66. wind	vẽjas	veter	+	118. long	ilgas	dlinnyj	—
67. star	žvaigždẽ	zvezda	+	119. if	jẽi, jẽigu, kãd	esli	—
68. man	výras	mužčina	—	120. guts	viduriaĩ,	kiški	—
69. tail	uodegà	xvost	—		žãrnas		
70. skin	õda	koža	—	121. black	júodas	černyj	—
71. and	iř	i	?	122. because	už tai kãd,	potomu čto	—
72. to suck	čiul̃pti	sosat'	—		todẽl kãd		
73. at	priẽ	v, u	—	123. lake	ẽžeras	ozero	+
74. sun	sáulẽ	solnce	+	124. belly	piĩvas	život	—
75. wet	šlãpias	mokryj	—	125. dry	sausas	suxoj	+
76. to live	gyvẽnti	žit'	+	126. thick	stõras	tolstyj	—
77. sharp	ařtrũs	ostryj	+	127. to float	plaũkti	plavat'	+
78. to stand	stovẽti	stojat'	+	128. to burn	dẽgti	goret'	—
79. rope	viřvẽ	verevka	+	129. yellow	geltõnas	žẽltyj	+
80. fog	miqlà	tuman	—	130. to swell	pũsti	puxnut'	+
81. left (hand)	kairỹs	levyj	—	131. to see	matýti	videt'	—
82. white	bãltas	belyj	+	132. foot	kõja	noga	—
83. sea	mãrẽs	more	+	133. to breathe	alsúoti,	dyřat'	—
84. where?	kuř	gde	+		kvẽpúoti		
85. heart	řirdis	serdce	+	134. husband	výras	muž	—
86. to flow	tekẽti, bẽgti	teč'	+	135. warm	řil̃tas	teplyj	—
87. here	čià	zdes'	—	136. to smell	užúosti,	čuvstvovat'	—
88. scratch	kasyti	česat'sja	—		mãn kvẽpia		
89. earth	žẽmẽ	zemlja	+	137. to walk	vãikřčioti	xodit'	—
90. neck	kãklas	řeja	—	138. wing	spařnas	krylo	—
91. wife	žmonà	žena	—	139. not	nẽ	ne	+
92. year	mẽtai	god	—	140. narrow	siaũras,	uzkij	+
93. seed	sẽkla	zerno	—		aĩkřtas		
94. fat	taukaĩ	žir	—	141. mouth	burnà	rot	—
95. to sew	siũti	řit'	+	142. old	sẽnas	staryj	—
96. dust	dũlkẽs	pyl'	—	143. mountain	kãlnas	gora	—
97. leg	kõja	noga	—	144. other	kitas	drugoj	—
98. sand	smẽlis,	pesok	—	145. to laugh	juõktis	smejat'sja	—
	smẽlỹs			146. to push	stũmti	tolkat'	—
99. to swim	plaũkti	plyt'	+	147. this	řis, řitas	ẽtot	?
100. to count	skaičiũoti	sčitat'	—	148. big	didelis	bol'řoj	—
101. flower	gelẽ	cvet	—	149. near	ařtimas	blizko	—
102. know	žinõti	znat'	+	150. to wash	mazgõti,	myt'	—
103. bad	blõgas,	ploxoj	—		skal̃bti		
	negẽras			151. back	nũgara	spinà	—
104. few	nedaĩg	malo	—	152. green	žãlias	zelenyj	+
105. straight	tiesũs	prjamoj	—	153. to give	dũoti	davat'	+
106. to sing	dainũoti	pet'	—	154. to fly	skristi, lẽkti	letet'	?

	Lithuanian	Russian			Lithuanian	Russian	
155. when?	kadà	kogda	+	177. worm	sliekas, kirmėlė	červ'	—
156. wipe	šluostyti	vytirat'	—	178. small	māžas	malen'kij	—
157. rub	trinti	teret'	+	179. to pull	tráukti	tjanut'	—
158. cloud	debesis	tuča	—	180. all	visi	vse	+
159. with	sù	s	+	181. to sit	sédėti	sidet'	+
160. to tie	rišti	vjazat'	—	182. dirty	nešvarùs	grjaznyj	—
161. to hit	mùšti	bit'	—	183. cold	šáltas	xolodnyj	+
162. to lie	gulėti	ležat'	—	184. to cut	piáuuti	rezat'	—
163. dull	bùkas, neaštrùs, atšipęs	tupoj	—	185. woods	miškas	les	—
164. to fight	kovóti	drat'sja	—	186. to stab	dùrti	zakalyvat'	—
165. many	daūg	mnogo	—	187. to think	galvóti, mąstyti	dumat'	—
166. meat	mésà	mjaso	+	188. some	keli, kėletas	nemnogo	—
167. to hunt	medžióti	oxotit'sja	—	189. there	teñ, tenai	tam	+
168. to turn	krỹpti, sùkti	povoračivat'	—	190. to squeeze	suspáusti	sžimat'	—
169. good	gėras	xorošij	—	191. smooth	švelnùs, nešiurkštùs	gladkij	—
170. river	ùpė	rcka	—	192. to fall	(nu) kristi, nupùlti	padat'	—
171. grass	žolė	trava	—	193. hold	laikýti	deržat'	—
172. to spit	spiáuuti	plevat'	—	194. to say	sakýti	govorit'	—
173. to throw	mėsti	brosat'	—	195. how	kaip	kak	+
174. short	trūmpas	korotkij	—	196. to play	žaisti, žaisti	igrat'	—
175. right (correct)	taisyklingas	pravil'nyj	—				
176. to split	skėlti	raskalyvat'	+				

APPENDIX II

*The 89 Malayopolynesian Lists Used in Establishing
CFRMP and Their Sources*

Achinese: Teuku Jacob. **Aneityum:** G. Grace from J. Inglis, *A Dictionary of the Aneityumese Language* (London, 1882). **Atok:** E. Wolfenden and W. Oates. **Babátana:** Nathan Sipuda. **Balinese:** Muter Anak Agung. **Baranago:** John Qoqoni. **Bareke:** Misake Aleve. **Bikol:** Dr. Obiyas. **Bilaan:** J. and G. Dean. **Buginese:** Muhammad Said. **Buli:** G. Maan, "Boelisch-Nederlandsche Woordenlijst", *Verh. v. h. Bat. Gen. v. K. en W.*, 74 (1940), pt. 3. **Buru:** H. Hendriks, *Het Burusch Van Masarété* ('s-Gravenhage, 1897). **Cebuano:** G. Svelmoe. **Chamorro:** G. Grace from E. R. v. Preissig, *Dictionary and Grammar of the Chamorro Language of the Island of Guam* (Washington, 1918); and G. Fritz, *Chamorro Wörterbuch* (Berlin, 1908) (*Archiv f. Studium deutscher Kolonialsprachen*, B. 2). **Dibabaon:** Jannette Forster. **Dobuan:** Rev. R. V. Grant. **Duke:** Ihaka. **Fiji:** G. Grace from A. Capell, *A New Fijian Dictionary* (Sydney, 1941). **Gaddang:** E. Wolfenden. **Gayo:** G. A. J. Hazeu, *Gajosch-Nederlandsch Woordenboek* (Batavia, 1907). **Gorontalo:** Idrak Jassin. **Halia** (or **Hanahan**): Fr. Peter Tatamas. **Havunese:** M. C. Radja Haba. **Hoava:** Samuel Kuku, Stephen Buka, and Nehemiah Tupiti. **Ibanag:** E. Wolfenden and W. Oates. **Inibaloy:** E. Wolfenden and T. Lyman. **Isinai:** L. Newell. **Isneg of Kabugao, of Malaweg, and of Nabwangan:** E. Wolfenden and W. Oates. **Karo-Batak:** M. Joustra, *Nederlandsch-Karosche Woordenlijst* (Leiden, 1922). **Keheraran:** Rev. R. V. Grant. **Kerebutu:** Gideon Lea. **Kerintji:** Jakub Isman. **Kubokota:** Michael Domeni. **Lampung:** M. Charles. **Lones:** Samuel Kunubi. **Luqa:** Mark Sepi. **Macassarese:** Muhammad Said. **Madegugusu:** Scrivin Tuke. **Madurese:** Hassan Shadily. **Malay:** I. Dyen. **Manmanua:** G. Svelmoe. **Marovo:** Elijah Iputu. **Marshallese:** G. Grace from *Marshallese-English and English-Marshallese Dictionary*, District Naval Intelligence Office, 14th Naval District, in cooperation with Commander Marshalls-Gilberts Area, U.S. Pacific Fleet and Pacific Ocean Areas, 1945 (anon.; mimeog.). **Merina:** P. Vėrin. **Minangkabau:** M. Charles from Alidin Doeana. **Mono:** Elijah Hoala. **Motu:** W. H. Goodenough. **Nengone:**

Rev. Père Marie-Joseph Dubois. **Palau:** H. K. Uyehara. **Panayati:** Rev. F. S. Kemp. **Petats:** Samuel Tousala. **Piggattan:** E. Wolfenden and W. Oates. **Ririo:** Cornelius Vulumu. **Saposa:** Paul Koroaj. **Sumoun:** Joram Kout. **Sundanese:** Achmat Hajat. **Tagacola** of Kalagan: F. and J. Dawson. **Tagalog:** H. C. Conklin. **Tahitian:** Mrs. Nordman-Salmon. **Teop:** Peter Vakute. **Tinata Runa:** S. B. Rodger. **To'abaita:** Jotham Ausuta and R. B. Vance. **Toba Batak:** I. Dyen. **Trukese:** G. Grace from S. H. Elbert, *Trukese-English and English-Trukese Dictionary* (1947). **Vagua:** John Kamavae, Z. Keqa, L. Pitaköe. **Vangunu:** John Lilivae and Raymond Sibe. **Varisi:** Nathan Kure. **Wedauan:** A. P. Jennings. **West Nakanai:** W. H. Goodenough and A. Chowning. **Yogad:** L. Newell. **Dehu, Fwagumwäk, Hameha, Kusage, Kusaiean, Male, Mokilese, Nadubea, Nalik, Nauru, Nukuoro, Paici, Pingelapese, Ponapean, Pwamei, Roviana and Wen Yai** are all from G. Grace.

APPENDIX III

The 39 Indo-European Lists Used in Establishing CFRIE and their Sources

Albanian: Gustav Weigand, *Albanesisch-deutsches und deutsch-albanesisches Wörterbuch* (Leipzig, 1914). **Armenian:** Knut Bergsland and Hans Vogt, "On the Validity of Glottochronology", *Current Anthropology*, 5 (1962), 123 f. **Bengali:** Frank Southworth. **Bulgarian:** Kamen Ganchev. **Byelorussian:** Eugene Protas. **East Czech:** A. A. Duffek. **English (Modern).** **Gujarati:** via G. Cardona. **Hindi:** F. Southworth. **Icelandic (Modern):** Bergsland and Vogt, *Current Anthropology*, 5 (1962), 117-9. **Khaskura (= Nepali):** G. W. P. Money, *Gurkhali Manual* (Bombay, 1942). **Macedonian:** H. G. Lunt, *A Grammar of the Macedonian Literary Language* (Skopje, 1952). **Marathi:** F. Southworth. **Norwegian (Riksmål):** Bergsland and Vogt, *Current Anthropology*, 5 (1962), 117-9. **Ossetic:** V. I. Abaev, *Russko-Osetinski Slovar'* (Moscow, 1950). **Panjabi:** F. Southworth. **Persian:** P. Kazemzadeh. **Portuguese:** H. Michaelis, *A New Dictionary of the Portuguese and English Languages* (London, 1893). **Tadžik:** D. Arzumanov and X. K. Karimov, *Russko-Tadžikski Slovar'* (Moscow, 1957). **Ukrainian:** W. Luciw. **Waziri (= Waziri Pashto):** J. G. Lorimer, *Grammar and Vocabulary of Waziri Pashto* (Calcutta, 1902).

Breton, Czech, Danish, Dutch, French, German, Greek (Modern), Irish, Italian, Latvian, Lithuanian, Polish, Rumanian, Russian, Serbocroatian, Spanish, Swedish, and Welsh all from C. D. Buck, *A Dictionary of Selected Synonyms in the Principal Indo-European Languages* (Chicago, 1949).

DISCUSSION

LUNT:

It is regrettable that nearly all of the discussants on the papers concerning lexicostatistics (and/or glottochronology) failed to record their comments for this volume. I am taking it upon myself to bring up some of the major questions, for it seems to me that the advocates of lexicostatistics are dangerously close to a doctrinaire position of sheer belief that overrides the substantial and crucial objections that have been repeatedly raised and not, to my taste, answered effectively. Dyen's paper is a case in point.

Dyen asks, "Is lexicostatistics valid?" He then utterly ignores the question, turning aside to produce several tables with numerals, accompanied by rather broad and resounding generalizations. At the end he surprisingly claims to have established the validity of comparative lexicostatistics beyond reasonable doubt! All this with hardly a gesture toward answering the real objections to lexicostatistics as practiced so far.

Dyen's figures look impressive and the magic of the chi-square should ward off

dissent, yet there is no demonstrated relation of regular rates of *change* and the frequency of cognate pairs within a language group. I cannot see that the result of the statistical significance test is really a positive support to the theory. When we look more closely, the whole procedure collapses into the usual subjectivism of practically all lexicostatistical work so far. Just *what* is being counted? The numbers were obtained by comparing pairs of gloss-lists. The validity of the numbers (and therefore necessarily of any operations performed with them) depends on two questions: (1) how were the glosses selected, and (2) on what basis were the plus/minus decisions made?

Let us touch on the second question first. Dyen's Lithuanian-Russian gloss-lists, like any pair of lists of kindred languages, contain some clearly unrelated pairs (akis/glaz "eye"), some fairly obviously related pairs (nosis/nos "nose", galva/golova "head"), and an indeterminate number of uncertain ones (tolimas/daleko "far" [Dyen says -], kepenos/pečen' "liver" [Dyen says +]). Now, in IE we have an enormous body of etymological knowledge, so that Lith. *i* "in" is clearly the same as R. *v*, although surely the identity has not been recognizable to speakers of the two languages since about 500 AD at the latest. Yet our knowledge is faulty, and a great many judgements depend on complex assumptions which may be controversial; that is, there *is* no absolute yes or no – the judgements are subjective. Scholars divide, for example, on tolimas/daleko. A question arises as to whose decisions Dyen accepted. The over-all lists were done out of Buck's *Dictionary of Selected Synonyms*. Thus *tongue* achieves its very high position (4) because Buck believes, among other things, that *liežuvis* and *jazyk* are cognate – but Dyen places a minus. He disagrees with Buck on other items too (e.g. spit, worm). This is not a criticism of Dyen's decisions here; it is merely one more demonstration of the honest difference of opinion on such matters which exists even among specialists. Dyen, while admitting the possibility of "doubtful cognate pairs", in fact fails (at least in the Lith./R. comparison) to be at all realistic about the extent of doubt entertained by careful comparatists. The problem is not a simple *yes vs. no vs. doubtful*, but includes several (how many?) degrees of doubt. Surely it would be possible to set up some sort of criteria for classifying these degrees and elaborating mathematical ways to handle the resulting complexity of numbers to be dealt with. Yet in practice Dyen, like his fellow-believers, shoves the material into the Procrustean plus/minus either/or and rushes on to play with the numbers so obtained. This is one of the reasons that lexicostatistics as practiced so far is not valid as linguistics.

(The only suggestions – very tentative at that – toward more realistic evaluation that I have seen are in the meticulous essay by I. Fodor, "The Validity of Glottochronology on the Basis of the Slavonic Languages", *Studia Slavica* 7.296-346, Budapest, 1961. Fodor repeated his conclusions – sharply negative – on lexicostatistics so far and outlined a few of his suggestions for improvement in the 1962 discussion in *Current Anthropology*. It is unfortunate that that discussion was not in a more specifically linguistic journal, for too many linguists have failed to read it.)

A more serious difficulty is in deciding at what point a modified form of an ancient word is to be considered "not the same". Dyen considers as equivalents *kas/kto* "who?", *kas/čto* "what?", *kur/gde* "where?", *kada/kogda* "when?" and *kaip/kak* "how?". Within Slavic, it is common to point out the different development of **k^wid* "what?" as **čb* (without reinforcement), surviving chiefly in a type of Serbo-croatian, opposed to the common **čbto* or the northwestern **čbso*. What has the Slavic suffixoid *-de* (not only in *кѣде* "where" but in related forms like *сьде* "here", *въсьде* "everywhere") to do with the Lith. *kur*? Even though one might agree to put a plus for both *kas/kto* and *kas/čto* on grounds of the total paradigms, it is hard to justify the three adverbs on any meaningful etymological basis other than the bare IE root **k^w*. If Dyen gives them all plusses, why does he have doubts about "and", *ir/i*? Discussions of Slavic dialects often make much of far-reaching regional preferences for different suffixes attached to the same roots; an extreme example is western *pət-ak-č* (m.) "bird" vs. eastern and southern *pət-ic-a* (f.). If Lith. *paukštis* is from the "same" root (and I agree with most etymologists, against Buck and Dyen, that it is not), is it to be marked with a plus in a comparison to a Czech dialect *fták* as well as to a Serbian *tica*? And without our store of comparative, and above all historical, data, how could we equate the latter two forms, whose only common point is a single consonant? Dyen's answer would presumably be a plus, cf. *i/v* "in", where Russian has nothing at all left of the original syllable. Again, I cannot believe that yes-no-maybe is in any way an adequate schema for handling such questions. More complex and realistic criteria are needed. Is it really meaningful to compare the numerical sums obtained by comparing languages where we can confidently equate *four* to *tésares* and *čtyři* with languages for which we have little more than a gloss-list taken ad hoc from a passing informant? These are more reasons why lexicostatistics as practiced so far is invalid.

Dyen states that for his purpose "the basic vocabulary of a language is its gloss-list". Now, can we really consider *any* 200-item list a "basic vocabulary" in any sense meaningful to linguistic science? Is the list sufficient to furnish even an outline grammar (with phonology, morphology, syntax) of *any* language? Moreover, it turns out in operation that this "basic" list is variable according to quite unlinguistic criteria. Dyen starts with a 200-word list for Malayo-Polynesian, and lops off 2% without explanation. Naturally we can guess that *snow*, *ice* and *freeze* are culturally foreign to these languages, but what about *that*? Do these languages have only one demonstrative pronoun, or is there some other reason for parting with such an obviously fundamental word? No matter, Dyen can sacrifice another 21% of the original list in order to make comparisons with the extraordinarily well documented IE languages, merely because it is convenient to use Buck's *Dictionary*. Words like *five*, *two*, *we*, *four*, *he*, *in*, *who*, are expendable – perhaps, one might guess, less basic than *straight* or *ashes*? At any rate it is clear that the size of the gloss-list is not very important for the lexicostatistical believer.

Let us look again at some of the words and conclusions. Dyen suggests that it is a

semantic law (although he never uses such a mentalistic term) that governs the rate at which individual items wear out. Yet the ordering of the IE list makes it seem a good deal more likely that many of the items owe their high frequency to the extraordinary persistence of dental consonants (plus our fortunately full knowledge of ancient dialects). *Nose*, for example (IE 22, MP 14/9 [i.e. 14 in 196-item list, 9 in 154-item selection]), is discarded by David Cohen in making a new list useful for Semitic comparisons (cf. p. 492). *Snow* would surely stand very high indeed in IE, although unknown to the MP languages, while *rain* (26 in MP) would be very low. Doubtless the IE word had a phonological shape that was subject to such distorting historical developments that it was replaced. We need explanations for the contrasts in the lists such as: IE 147 fruit MP 19/14, 139 road 33/24, 146 vomit 34/25, 143 far 43/31, 151 tail 69/52, 8 give 153/120 (Cohen discards it for Semitic), 36 spit 172/136, 39 short 174/138, 34 worm 177/140, 15 sit 181/141. Or others of the words Cohen discards: 24 long 118/92, 31 foot 132/102, 53 fish 35/26, 43 white 65/48. All this argues against the likelihood that Dyen's contentions are significant, the chi-square notwithstanding.

Part of the divergencies (and the concomitant low places of the items on the frequency lists) may come from the difficulty of translating the English list. For example, in three versions of the Russian (Schenker, Zvegincev, Fodor) the variants are greater at the end (cf. 131 fight 164/128 *drat'sja*, *sražat'sja*, *borot'sja*; 122 split 176/139 *raskalyvat'*, *rascepljat'*, *lomit'*; 186 *zakalyvat'*, *vonzat'*, *kolot'*). And here we are back to the first basic question I asked earlier, *how* are gloss-lists translated from English? Or even, how compiled *in* English?

The underlying assumption, unfortunately so tacit as to be unknown to many lexicostatisticians, is that there *is* a universal list of approximately 200 (or is it 100?) concepts for each of which every individual language *must* possess one specific morpheme. This is by no means the generally accepted notion that any concept *can be somehow* expressed in any language; it is the special unspoken prerequisite for making a lexicostatistical gloss-list that these select concepts *are* always provided with specific labels. Nowadays alternate glosses are sometimes admitted (cf. Dyen's Lith./R. list, and note that no notice is taken of the changed probabilities when comparing multiple possibilities), but the assumption is overriding – and crippling. It is responsible for the difficulties repeatedly reported by investigators trying to find *the* right word for many of the items when compiling these “basic” lists for languages they know well. Accounts of such difficulties by critics of the method have been brushed aside by the believers; Dyen makes no reference to this basic problem in his paper here. The classic case is Hoijer, with Navaho, but the Congress has now the specific comments of Cross, D. Cohen, and S. Levin. The believers continue to base their work on this absurd and essentially anti-linguistic assumption, and this makes lexicostatistics, as practiced so far, completely invalid as a branch of linguistic science.

The lexicon obviously is a part of the system of a language, albeit more superficial than phonology, morphology, and syntax. And surely it has its own structure(s), as do

the other more easily described phonological and morphological subsystems of the language. If not, we must abandon our definitions of language as a system or else exclude the lexicon from language. Some of the lexical subsystems in certain languages are fairly clear, and they cause trouble when one constructs gloss-lists. Kinship terms like *brother* and *uncle* are likely to be totally misinterpreted unless the whole matrix of the kinship is included – and the difficulty is recognized by removing most kinship terms from lexicostatistical consideration. Yet surely it is unrealistic to compare items from a two-way *this-that* pronominal system (e.g. Russian *ètot-tot*) with items from a three-way *this-that-yon* system (e.g. Serbocroatian *ovaj-taj-onaj*, Lith. *šis-tas-anas*). It becomes particularly absurd when it turns out that both R. *ètot* and *tot* are cognates of SC *taj* (and Lith. *tas*). The solution of the lexicostatistician is to ignore the potential *yon* (and, in Dyen's case, to refuse to recognize the clear difference between Lith. *šis*, cognate with obsolete *sej*, and *ètot*, cf. his # 147). A more complex example: is it valid to compare items from a system like (I am surely oversimplifying) *human-person-man-husband-spouse-wife-woman* with (R) *čelovek-osoba-lico-mužčina-muž-suprug-supruga-žena-ženščina* or (SC) *čovjek-osoba-lice-muškarac-muž-suprug-supruga-žena* or *Mensch-Person-Mann-Gatte-(Ehegatte?-) Gattin-Frau-Weib (-Ehegatte, Ehefrau?)*? Schenker's Russian equivalent for *person* is *čelovek*, but Zvegincev and Fodor give *lico*. The choice is subjective and context-determined, not right or wrong. – Or how can one pretend that items like the *come* and *walk* (without the really general *go!*) of English, where the semantically related verbs are represented by utterly separate morphemes, are immediately comparable with R. *prixodit'-xodit'* which are combinations of one base morpheme with variable added morphemes? Dyen gives a simple unqualified plus for the comparison Lith *ateiti* vs. R. *priiti, prixodit'*. Let us not ask what he would have done if using Fodor's R. list, which gives only *prixodit'*, nor query whether the difference of prefix (*and* suffix: L. *ei-* vs. R. *i + d -*) should not enter into our decision: is it not, in any case, difficult to make any comparison of the suppletive morphemes of the Russian "come" (*pri + id|š#d|xod*) to any language where there is no suppletivism or else a different set (e.g. SC *pri + id|š#d|laz*)?

There is no point in continuing. The lexicostatisticians, crippled by an untenable basic assumption, plagued by the unrealistic need to set plusses or minuses as answers to questions that do not admit an unqualified yes or no, seek refuge in a simplistic manipulation of numbers. But since the numbers do not derive from any linguistic reality, the fancy formulas and elaborate tables are meaningless – simplicism has become simple-mindedness. It is regrettable that so much time and effort has been consumed in such idle delusion.

Lexicostatistics as practiced so far is not valid. Yet we need not despair. Recognizing that the lexicon has its own structure, we can set up new models to explore this structure for individual languages and then compare those things which are comparable in other languages. Surely we must operate with more realistic amounts of "basic" vocabulary (is fewer than two thousand items possible?), even though this

makes comparisons with extinct and poorly-documented languages difficult or impossible. Above all there must be clear and unambiguous agreement as to how each gloss-list is compiled, and as to what is to be regarded as "the same" in comparisons. It may very well be a long and difficult struggle, with many setbacks and disappointments, before meaningful results emerge. Yet this complex task should not be put off in favor of more of the showy but meaningless number games which have been the only fare so far offered by the lexicostatisticians.

LE RENOUVELLEMENT DES MÉTHODES EN LINGUISTIQUE GÉOGRAPHIQUE

GEORGES REDARD

Pour J. Gilliéron, qui a posé la théorie moderne de la linguistique géographique, chaque élément de la langue a son histoire individuelle, qu'il appartient au dialectologue d'établir dans le détail. La réunion préalable des matériaux – c'est-à-dire l'élaboration d'un atlas linguistique – est soumise, pour l'auteur de l'ALF à quatre principes fondamentaux: 1) enquêteur unique, 2) enquête directe (sur place, et non par correspondance), 3) transcription phonétique non régularisée, 4) publication intégrale des matériaux.

La publication de l'ALF date de plus d'un demi-siècle (1902-1909). Les principes de Gilliéron sont-ils encore valables aujourd'hui? Ils ont été en général suivis et lorsqu'on s'en est écarté, c'est plus pour des raisons pratiques, matérielles que théoriques. Les objections qu'on y peut faire sont de nature différente et d'importance inégale. De fait, trois ordres de données nouvelles légitiment la confrontation: I. Domaine sur lequel porte l'enquête; II. Technique de l'enquête; III. Technique et objet de la description.

I. DOMAINE SUR LEQUEL PORTE L'ENQUÊTE

Les enquêtes dialectologiques ont porté d'abord sur des parlers germaniques et romans. Puis ont été étudiés en tout ou partie, les domaines celtique, slave, finno-ougrien, berbère, bantou, arabe, chinois, japonais, etc. – si on ne compte pas les 21 tomes du *Linguistic Survey of India* (1894-1902) dont les matériaux, réunis par correspondance et assortis d'une vaste collection de phonogrammes, constituent un Thesaurus, non un atlas. Cette extension a emporté des problèmes que ne pouvait prévoir Gilliéron et avec la solution desquels ses principes étaient incompatibles. Ainsi A. Basset, explorant le berbère, dut renoncer à un atlas unique – vu l'étendue du domaine, et à un questionnaire unique, vu sa différenciation; il lui fallut recourir souvent à un interprète, tenir compte du nomadisme, etc. L. de Boeck pour le bantou, J. Cantineau dans le Hauran, Lieou Fou, Tch'en K'i-sieng, F. Giet, W. A. Grootaers pour le chinois, ce dernier maintenant au Japon, tous firent, de force, des expériences analogues qui exigeaient des décisions nouvelles, le plus souvent des dérogations. Par exemple, il devint impossible de confier la récolte des matériaux à un seul enquêteur. Déjà l'AIS du Jud-Jaberg en avait eu trois, comme l'Atlas des parlers de la Suisse alémanique dont R. Hotzenköcherle vient de publier le premier volume; l'Atlas russe – dont le 1er volume est

également paru – en a occupé une quantité. D’abord pratique, cette multiplication a entraîné un problème théorique : les sources étant plus diverses (enquêteurs nombreux et, à travers eux, pour chaque point, plusieurs informateurs), il devient plus aisé d’apprécier le degré d’unité ou de désintégration d’un dialecte, de reconnaître les aires qui manifestent une relative stabilité, celles dont l’évolution est rapide, etc. En revanche la réaction de l’oreille est individuelle et les notations par conséquent divergentes. Mais l’utilisation du magnétophone permet le contrôle, même longtemps après le relevé. L’avantage apparaît ainsi plus réel que l’inconvénient : là où plusieurs enquêteurs sont possibles ou nécessaires, on ne saurait hésiter à modifier le principe de Gilliéron.

II. TECHNIQUE DE L’ENQUÊTE

Le second principe de Gilliéron – enquête directe – garde, lui, toute sa valeur, qu’aucun dialectologue ne met en doute. Il faut néanmoins le rappeler, car trop de relevés encore sont faits hors du lieu de résidence habituel de l’informateur, ou publiés avec des localisations insuffisantes.

L’enquête elle-même est naturellement toujours conduite à l’aide d’un questionnaire, toujours criticable, et qui, selon la boutade de Gilliéron, serait meilleur si on pouvait l’établir une fois l’enquête achevée. Mais il faut aussi enregistrer des matériaux “spontanés”, obtenus hors du jeu régulier des questions et des réponses. M. Hotzenköcherle en a montré l’intérêt. Certes, il ne s’agit pas d’extorquer des mots rares, obsolètes en vue d’expliquer un précédent état de langue,¹ mais d’actualiser le vocabulaire latent du témoin et de le recueillir dans sa libre expression. C’est en général la seule façon de se renseigner sur la syntaxe, la modulation du discours, la phonétique combinatoire et, pour le lexique, sur les notions de la vie intellectuelle et affective. Le magnétophone est ici d’un grand secours, à condition que l’enregistrement soit doublé d’une notation phonétique directe et que l’appareil utilisé soit de qualité professionnelle (le niveau sonore et la vitesse d’enregistrement des appareils d’amateur accusent des variations qui réduisent considérablement la valeur du témoignage recueilli²). Le principe de Gilliéron n’est donc pas mis en cause, mais sa réalisation est améliorée par la récolte des matériaux spontanés et l’utilisation du magnétophone.

III. TECHNIQUE ET OBJET DE LA DESCRIPTION

Les principes 3 (transcription phonétique non régularisée) et 4 (publication intégrale des matériaux) nourrissent la controverse depuis plusieurs années. Faut-il schématiser

¹ Sur cette tendance, cf. A. Martinet, *Romance Philology*, 8 (1954-55), p. 2. Cp. aussi E. Bérésine, *Recherches sur les dialectes persans*, I (Casan, 1853), p. 2 : “Ce n’est qu’en recueillant les mots des dialectes persans qu’on peut ressusciter la langue ancienne des Akhamaniens [Achéménides].”

² Cf. en dernier lieu W. Kallenbach et H.-J. Schroeder, *Phonetica*, 7 (1961), p. 95-108.

les notations phonétiques? En termes plus larges, y a-t-il une dialectologie structurale qui s'opposerait à la dialectologie "traditionnelle"? La question a été posée à plus d'une reprise et les réponses qu'on y a rendues sont très diverses.³ Le temps manque pour les détailler ici. La plupart ont ceci de commun qu'elles négligent la pratique de l'enquête ou ne la considèrent que dans ses conditions les plus favorables: territoire restreint, localités aisément accessibles, témoins plus ou moins instruits. Or, ces conditions peuvent être tout autres – ainsi sur le domaine iranien qui nous est familier. Surtout, on oublie trop que le travail dialectologique connaît trois phases, à chacune desquelles s'applique une méthode propre:

a) *Phase de l'enquête.*

L'enquêteur a pour tâche de noter la parole aussi soigneusement que possible, donc de collecter des faits individuels. Il doit se garder de toute schématisation, et cela d'autant plus qu'il connaît mieux le parler qu'il explore. Tant qu'il ne dispose pas d'un matériel suffisant ni du temps nécessaire pour en étudier la distribution phonétique, morphologique et sémantique, la discrimination est impossible, donc dangereuse. Il peut, certes, s'apercevoir rapidement que, dans tel parler, l'accent est phonologique, mais les faits immédiatement clairs sont rares: le plus souvent apparaissent ensemble des contrastes pertinents et des variantes combinatoires; seule une confrontation minutieuse permettra d'inventorier les oppositions fondamentales d'une part (échelon phonologique), les oppositions à fonction non diacritique d'autre part (échelon subphonologique). Les faits morphologiques et sémantiques obéissent à un conditionnement parallèle, et leur identification est également prématurée à ce stade du travail. Enfin, passant d'un dialecte à un autre qui est voisin, l'enquêteur qui schématise croira y reconnaître des faits identiques, par une généralisation souvent fallacieuse, car ce qui est ici contraste pertinent peut n'être là que variante combinatoire et vice-versa (le phénomène est bien connu en diachronie – en latin, la quantité vocalique, d'abord pertinente, est tombée au niveau subphonologique au bénéfice du timbre qui n'était qu'accessoire –, il est fréquent aussi, naturellement, lorsqu'on considère, en synchronie, deux systèmes voisins).

b) *Le dialectologue élabore la publication des matériaux réunis au cours de la première phase.*

En possession de l'ensemble des matériaux, le dialectologue est à même d'identifier les facteurs conditionnants et les faits conditionnés; il peut répondre aux deux questions formulées par Trubetzkoy: tel phonème se présente-t-il dans tel dialecte? Si oui, dans quelles positions se trouve-t-il employé?⁴ Dès lors il négligera certaines des précisions

³ Voir notamment U. Weinreich, *Word*, 10 (1954), p. 388-400; J. Fourquet, *Festgabe Frings* (Berlin, 1956), p. 192 ss.; L. Heilmann, *Quaderni dell' Istituto di Glottologia (Bologne)*, 4 (1959), p. 45-54; R. P. Stockwell, *American Speech*, 34 (1959), p. 258-268; R. Grosse, *Biuletyn Fonograficzny*, 3 (1960), p. 89-101; R. I. McDavid Jr., *Orbis*, 10 (1961), p. 35-46. Rappelons aussi les intéressantes considérations de G. Tuaillon, *Revue de linguistique romane*, 22 (1958), p. 293-316.

⁴ *TCLP*, 4 (1936), p. 228-234 = *Principes*, trad. Cantineau (Paris, 1949), p. 343-350.

apportées par l'enquêteur dans ses relevés impressionnistes, corrigeant ainsi des disparates évidemment secondaires. Mais l'inventaire une fois établi, la fonction délimitée, les traits phonétiques isolés des faits phonologiques, la carte publiée devra néanmoins représenter la réalité de l'enquête et offrir à chacun la possibilité de l'interpréter. De plus, tous les matériaux doivent être publiés, comme l'exigeait Gilliéron, car toute sélection est arbitraire et peut mener à des conclusions arbitraires. Le récent volume I de l'atlas russe illustre bien cette double nécessité. Couvrant, à l'est de Moscou, une aire de 660×380 km (la densité est grande : 938 localités, ce qui représente un point par 225 km^2), il a été préparé par une rédaction centrale qui a unifié les données procurées par de nombreux enquêteurs. Des 279 cartes qu'il comporte, les $\frac{3}{4}$ ont trait à des questions grammaticales, le $\frac{1}{4}$ au lexique. Elles présentent les différents types, figurés à l'aide de symboles géométriques. Le détail des formes manque, de sorte qu'il est impossible de reconstituer le travail des rédacteurs. D'autre part, on a éliminé tous les mots qui apparaissent partout sous la même forme. L'absence d'un mot sur la carte signifie donc ou bien que ce mot n'existe pas dans tel dialecte, ou bien qu'il y est d'usage courant. Cette carence doit être compensée par la publication de volumes supplémentaires. Jusqu'à au moins, aucune vérification n'est possible. Sous prétexte de structuralisme, par souci d'être "moderne", on empêche en réalité l'analyse structurale ; le schème proposé interdit de remettre en cause les délimitations mutuelles et les relations des éléments engagés. Si cela est, dans une certaine mesure, légitime lorsqu'il s'agit d'atlas particuliers,⁵ on ne saurait l'admettre pour un atlas général. La seule méthode, même si elle paraît démodée, consiste à mettre quiconque en possession de toutes les pièces du dossier, à l'aide de quoi il pourra éventuellement reprendre le procès sur nouveaux frais.

Cela ne signifie aucunement la négation d'une dialectologie structurale. Mais celle-ci appartient à la troisième phase.

c) *Le dialectologue travaille sur les cartes publiées.*

Les cartes présentent des faits de parole, atomisés. La confrontation d'une série de cartes ouvre l'accès à la langue. Elle permet principalement de dégager la structure d'un système et de comparer les structures respectives de systèmes différents :

1) la structure d'un système : le parler de tel point est considéré pour lui-même ; chacun des éléments est défini en fonction du système auquel il appartient. On reprend ainsi le travail commencé dans la phase précédente, mais en poussant cette fois l'analyse jusque dans le détail.

2) les structures de systèmes différents : par la comparaison des systèmes analysés, on établit les différences qui les distinguent au point de vue phonologique (différences d'inventaire et de fonction), phonétique (réalisation dans la prononciation), morphologique (distribution et utilisation des morphèmes) et sémantique. L'étude des limites d'extension aboutit à tracer des aires dialectales ; aires à contours nets pour ce qui est de la

⁵ Cf. p.ex. E. Bagby Atwood, *A Survey of Verb Forms in the Eastern United States* (Univ. of Michigan, 1953).

phonologie (puisqu'un phonème existe on n'existe pas), aires à contours moins nets pour ce qui est de la phonétique et de la sémantique (puisque le passage d'un type de réalisation à un autre est graduel, qu'on observe des points où plusieurs réalisations du même phonème sont également possibles et facultatives, et que, d'autre part, entre deux domaines excluant la bissémie peut s'instituer – et s'institue généralement – une zone de confusion).

Aux renseignements des cartes s'ajoutent ceux du commentaire de l'enquête et des causalités extra-linguistiques connues (facteurs historiques, d'économie et de civilisation). De l'étude, statique, de la structure, on passe à celle, dynamique, de la différenciation et de ses causes internes et externes. Le mouvement d'un dialecte apparaît, les parlers se caractérisent – suivant le procès défini par M. Martinet⁶ – comme divergents ou convergents; l'appartenance dialectale se laisse définir à partir d'un ensemble, non plus d'un fait isolé comme l'existence d'un mot particulier, d'une construction singulière, etc. Enfin sera mieux éclairé le cas des emprunts, qui tient si grande place dans tous les domaines où une langue commune, par le prestige de sa littérature et la force de l'administration, pousse les dialectes vers l'uniformisation, c'est-à-dire la disparition.

On peut ainsi dire, et c'est à peine un paradoxe, que le travail commence au moment où un atlas est publié. L'idéal serait, bien sûr, que l'éditeur l'entreprenne lui-même, autrement dit que l'atlas offre les cartes des matériaux bruts (ou presque), puis les cartes structurales, enfin les cartes exposant les différenciations et leur procès. Mais c'est là presque toujours une utopie. Qui a oeuvré longtemps pour récolter les matériaux et les publier sait qu'il ne pourra guère dépasser le plat principal; le dessert sera pour d'autres. Il est seulement curieux de voir, lorsque ce dessert est à portée de bouche, combien peu en profitent. Qui donc a songé à tirer parti de la masse de matériaux d'un atlas comme celui de Jud et Jaberg? Et pourquoi n'y pense-t-on pas? K. Jaberg, à qui je posais un jour la question, me répondit: "C'est que les atlas sont passés de mode, nous ne sommes plus à la page." Mais cette réflexion désabusée ne vise que les caprices de la mode. Une fois la récolte faite, le grain peut attendre et l'on reconnaîtra bien un jour, selon une très vieille expérience humaine, qu'il est utile de le moudre.

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DISCUSSION

LOMBARD:

Je voudrais retenir ici un détail. Gilliéron avait certainement raison, comme l'a rappelé M. Redard, de souligner combien il est important que l'enquêteur recueille sa documentation directement, sur place, qu'il écoute lui-même; rien ne saurait rem-

² *Romance Philology*, 8, p. 10 s.

placer une présence, une oreille. Mais je crois qu'il ne faudrait pas exagérer: l'oreille peut dans certains cas être remplacée par la lecture sans que l'enquête en souffre – et cela dans le domaine de la morphologie, de la syntaxe et du lexique plus que dans celui de la phonétique. (On sait que les atlas linguistiques modernes accordent en général beaucoup plus d'intérêt à ces trois derniers aspects du langage que ne le fait celui de Gilliéron.) Mettons qu'il s'agisse par ex. de savoir si le sujet, ou le groupe qu'il représente, dit je *m'assieds* ou je *m'assois* ou s'il dit *gifle* ou *claque* ou même *taloche*; la question dont le but est d'éclairer un tel détail, peut fort bien – à condition que la personne interrogée s'y prête, et qu'il ne s'agisse que de compléter, avec une dépense minimale de temps, une enquête ouverte sur place – être confiée à la poste. Telle est du moins une des expériences que j'ai pu faire au cours de mes enquêtes sur la morphologie du roumain.

NEW METHODS TO INTERPRET LINGUISTIC MAPS

WILLEM A. GROOTAERS

Abstract

The Linguistic Atlas of Itoigawa (Niigata Prefecture, Japan), the first regional atlas done in Japan, is based upon three successive surveys (1957; 1959; 1961) done by the three same linguists (Shibata, Grootaers, Tokugawa), in the same localities and with the same informants. Some of the methodological findings¹:

- a) divergences between maps of the same words gathered in different years are non-relevant for the reading of the maps;
- b) differences between active and passive usage of dialect words is nearly nonexistent;
- c) words belonging to childhood preserve in their distribution the boundaries of school districts which have now disappeared.

Tokyo City University

DISCUSSION

REDARD:

Le parallélisme entre fait dialectal et organisation scolaire est saisissant (mais s'agit-il vraiment de faits dialectaux et la différence constatée est-elle partie d'un ensemble?) En revanche, le déplacement, en quelques années, de limites dialectales ne pourrait être apprécié que si l'on disposait de précisions: situation et âge des témoins conditions de l'enquête, etc. Les différences établies lors d'enquêtes successives pourraient être relevées lors de la même enquête (réponses divergents de plusieurs témoins).

¹ Some earlier methodological findings have been published in *Orbis*, VIII, 2, and in First Dialectological Congress, Louvain, 1960.

LA TEORIA CROCIANA E LA LINGUISTICA STRUTTURALE

GIULIO BONFANTE

*Abstract**

The Crocean or esthetic theory of language can and must be integrated with the structural theory, which is in itself quite incomplete and cannot explain even the working of language itself, let alone linguistic change. Special attention must now be devoted to the "peripheral" zone of language – slang, jargon, affective and expressive terms, child language, onomatopoeia, interjection – which does not enter, or only with force, in the "structure" of language as it is usually conceived.

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* *The only copy of Professor Bonfante's significant paper was lost by a secretary who was to have typed it, and the author did not wish to reconstruct his presentation. The Editor expresses his most sincere apologies to Professor Bonfante and regrets profoundly that the readers of this volume have been deprived of an excellent statement of Crocean principles as elaborated by Bonfante and cogently and challengingly compared with other current theories and methodologies.*

H. G. LUNT

SIGN, MEANING, SOCIETY

FERENC KOVACS*

Language is a system of signs.

This is a commonplace, but nevertheless we are compelled to start out from this thesis, for it is extremely problematical despite its banal character. Both the notions "language" and "system of signs" are problematical.

"Sign" has already been explained, and not without result, from a physiological point of view (conditionalism); similarly, it has been applied with great success as "information" by means of technical devices (cybernetics).

"Language" has been frequently and ingeniously defined, abstracted of linguistic factors, as an artificial product applicable to linguistic research on the one hand, and as a necessary criterion of "society", on the other. Language remained a "system of signs" in both cases, but though it conformed to the methods of research, it could not be approached — as a real and living language — by any of them.

"Language" being frequently and excellently defined between the triangulation points of "sign", "meaning" and "society" has remained problematical regarding both its content and its methodological accessibility. Both forms of conditionalism (the original Pavlovianism and behaviorism) consistently strive to apply the methods of the natural sciences to "language", though it turns out time after time that these methods are not applicable to this subject any more. At the same time this implies that cybernetics, using an extremely wide scale of technical possibilities, determines its own limits in "information", the "meaning" of which — due to its nature — originates from programming.

The triangulation points of "sign", "meaning" and "society" are worthy of being taken into consideration again only if the real "language", being practically effective and actually functioning, as well as the "sign" bearing meaning within human "society", become actually approachable. To achieve this we have only one possibility according to which a person produces the "sign" in order to make himself understood by another person; the other person listens to the "sign" in order to understand his partner; "society" is brought to existence by the connection of at least two individuals — the speaker and the hearer.

* In cooperation with Dr. György Koutra, Chair of Biology, Central Institute for Pedagogical Research, Budapest.

In such a conception our three classical notions must be explained as the intellectual relation of human psyches correlated with each other.

Accordingly, "language" is the intellectual function of mental relations, the acoustic and material manifestation of which is the "sign"; its outcome being, so to say, inconceivable but nevertheless having a real effect is "meaning"; its successful realization is the actual community, that is "society".

Summing up: "language" cannot be successfully examined by means of the methods of the natural sciences; it may be examined only on the basis of a methodology which is in accord with the nature of "language".

The first steps toward such a methodology were made by W. Wundt, but the detailed elaboration has not yet been carried out. This is one of the tasks to be solved in the future.

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ON SOME TRACES OF THE UNKNOWN LANGUAGE-SUBSTRATUM IN THE NORTHERN URALIC LANGUAGES

B. A. SEREBRENNIKOV

One of the distinctive features of the historical development of the vowels of the first syllable in the Samoyed language is the clearly expressed tendency to form the *a* vowel.

Many words of the Samoyed language compared with etymologically related words of the Finno-Ugric languages have in the first syllable the vowel *a*, for ex. Sam. *jaha* "river", Lap. *jokkâ* ~ *jogâ*, Mord. *E jov* (the name of the river Moksha), Zir. *ju*, Ost. *jogæn* or *johan*, Hung. *jo* (in hydronymics);¹ Sam. *hala* "fish", Lap. *guolle* ~ *guole*, Mord. *kal*, Cher. *kol*, Vog. *kul*, *hul*, Ost. *kul*, *hul*, *hut*, Hung. *hal*²; Sam. *hădy* "Norway spruce, *Picea albis*", Fin. *kuusi*, Lap. *guossâ* ~ *guosâ*, Mord. *kuz*, Cher. *kož*, Vot. *kyz*, Zir. *koz*, Vog. *howt*, Ost. *hut*³; Sam. *hăsuj* "dry", Zir. *koš*, Vot. *kwaš* "shallow (water)", mord. *koške* "dry, cher. *kukšo* "dry", *koškaš* "to dry", Kola Lap. *goške* "dry"⁴; Sam. *hăda* "nail, claw, hoof", Fin. *kynsi* ~ *kynte*, Lap. *gâžžâ*, Kola Lap. *gânžâ*, Mord. *kenže*, Cher. *E küč*, Vot. *gyžy*, Zir. *gyž* Vog. *künš*⁵; Sam. *ńamü* "tongue", Hung. *nyelv* "tongue, language", Lap. *njalbme* ~ *njalme* "mouth" Cher. *jəlme* "tongue", Ost. *ńäləm*, *nătəm*⁶; Sam. *ńawa* "hare", Mord. *numolo*, Zir. (*Jažva*), *ńimal*, Hung. *nyúl*, Lap. *njoammel*⁶; Sam. *wadaš* "drag, haul", Mord. *E veřams*, Cher. *E wüdaš*, Hung. *vezetni*; Sam. *tajo* "birch bark", Vot. *tuj* "birch bark"; Sam. *masaš* "to wash oneself", Zir. *myškyny*, Mord. *muškems*, Hung. *mosni*, Cher. *muškas* "to wash"; Sam. *řa* "tree", Fin. *puu*, Cher. *pu*, Zir. *pu*.

This peculiarity of the Samoyed language was mentioned first by B. Collinder in his *Comparative Grammar of the Uralic Languages*: "Sam. N displays a tendency to widen all the narrow and halfwide vowels: *o > a*, *u > o*, *y > o*, *a*, *e > ä*?"⁷

The comparison of the etymologically related words of the dialects of Samoyed and the languages closely related to Samoyed-Selkup and Kamassian- which are spoken in other areas clearly shows that the tendency to widen narrow vowels of the first syllable exists only in the northern dialect of Samoyed usually called Yurak dialect. It

¹ B. Collinder, *Fenno-Ugric Vocabulary* (Uppsala, 1955), p. 18, 19.

² *Ibid.*, p. 21.

³ *Ibid.*, p. 30

⁴ *Ibid.*, p. 13.

⁵ *Ibid.*, p. 30.

⁶ *Ibid.*, p. 43.

⁷ B. Collinder, *Comparative Grammar of the Uralic Languages* (Uppsala, 1960), p. 186.

was not original, for in the first syllable of the corresponding words in other Samoyed dialects the *a* vowel is often missing, cf. Yurak *hādy* “spruce”, Kam. *ḡod*; Yurak *jaha* “river”, Yenisei Sam. *joha*; Yurak *har* “knife”, Yenisei Sam. *koru*; Yurak *hała* “fish,” Kam. *ḡola*, Tawgi Sam. *kole*, Yurak *ńawa* “hare”, Selk. *ńoma*, *ńewa*; Yurak *jamb* “long”, Selk. *čump*, Yurak *sawa* “good”, Selk. *soma*; Yurak *mat* “six”, Selk. *muqyt*, Yurak *ja* “earth, country”, Kam. *đu*, Yurak *jala* “day”, Selk. *čely*, Yenisei Sam. *jere*, Yurak *jabto* “goose”, Yenisei Sam. *jotto*, Yurak *pa* “tree”, Selkup *pō*.

Hence a conclusion can be drawn that the origin of the tendency to widen the narrow vowels of the first syllable in the Yurak dialect goes back to the time when the ancestors of the modern Yuraks migrated from Siberia to northern regions of the European part of Russia.

It is interesting to note that a similar tendency exists in the northern dialect of the Vogul language (river Sośva and the upper stream of the river Ložva).

Protovogul *ā* has changed into *a* in Sośva dialect, cf. Tavda dial. *āmp* “dog”, Sośva dial. *āmp*, Tavda dial. *sāt* “seven”, Pelym d. *soat*, Sośva d. *sat*.⁸ Protovogul *ē* has changed in Sośva d. to *ā*, cf. Middle Konda d. and Lower Ložva d. *kel* “birch,” Sośva d. *hāl*; Pelym d. *kēr* “male of the reindeer”, Sośva d. *hār*; Pelym and Middle Konda d. *wēt* “to gather”, Sośva d. *wat*;⁹ Middle Konda d. *jē* “river”, Pelym d. *jē*, Sośva d. *jā*; Middle Konda d. *ńēḡ* “arrow”, Upper Konda d. *ńēl*, Sośva d. *ńāl*; Pelym d. *tēriḡ* “crane”, Sośva d. *tāriḡ*.¹⁰ Protovogul *ē* in the northern dialect of Vogul has changed into *a*, cf. Tavda d. *eli* “kills,” Sośva d. *ali*¹¹; Tavda d. *pēt* “to fall”, Sośva d. *pat*; Tavda d. *šem* “eye”, Sośva d. *sam*; Tavda d. *et* “five”, Sośva d. *at*.¹²

Protovogul short *e* has changed in the northern dialect into short *a*, cf. Tavda d. *kep* “hill”, Sośva d. *kap*¹³, Tavda d. *pent* “to shut, close”, Sośva d. *pant*.¹⁴

Protovogul *ī* has widened in the northern dialect to *ē*, cf. Tavda d. *kīt* “to send”, Sośva d. *kēt*; Tavda d. *pīš* “old”, Sośva d. *pēs*, Tavda d. *pīt* “to boil, cook”, Sośva d. *pēt*.¹⁵

The tendency to widen the original narrow vowels of the first syllable was not alien to the Lappish language.

Protouralic short *e* has changed in Lappish into *a*, cf. Fin. *keski* “middle”, Lap. *gâska*, Fin. *vene* “boat”, Lap. *fânâs*, Hung. *nyelve* “language”, Lap. *njalbme* ~ *njalme* “tongue”.

Lappish *a* corresponds usually to the Finnish *ü*, cf. Lap. *barggat* “to work”, Fin. *pyrkiä* “to strive”, Lap. *šaddat* “to grow”, Fin. *syntyä* “to be born”, Lap. *čâkčâ* “autumn”, Fin. *syksy* etc.

⁸ W. Steinitz, *Geschichte des Wogulischen Vokalismus* (Berlin, 1955), p. 176.

⁹ *Ibid.*, p. 187.

¹⁰ *Ibid.*, p. 188.

¹¹ *Ibid.*, p. 203.

¹² *Ibid.*, p. 204.

¹³ *Ibid.*, p. 206.

¹⁴ *Ibid.*, p. 267.

¹⁵ *Ibid.*, p. 216.

The vowel *i* has apparently changed in Lappish into *a*, cf. Lap. *namma* "name", Fin. *nimi*, Lap. *albme* "heaven", Fin. *ilma* "air", Lap. *balva*, "cloud", Fin. *pilve*.

The Lappish *a* corresponds also to the narrow vowels in the etymologically related words of the Permian languages, cf. Lap. *baddat* "to swell", Zir. *bydmyny* "to grow", Lap. *gassag* "rough, coarse", Zir. *kyz thik*.

The vowel *ä* has changed in Lappish apparently into *a*, cf. Fin. *äima* "needle", Lap. *aibme*, Fin. *hähnä* "woodpecker", Lap. *čaihne*.

The tendency to widen the original narrow vowels of the first syllable display simultaneously three Uralic languages Lappish and the northern dialects of Samoyed and Vogul. This fact cannot be treated as an occasional phenomenon. The origin of this fact is due to a certain external impulse. Lappish and northern dialects of Samoyed and Vogul were influenced by an unknown language substratum in the arctic zone of the European part of Russia.

We can suppose that the language of Sirt'as who in the Yurak historical tales and legends are regarded as ancestors of modern European Samoyeds or Yuraks belonged to this language-substratum.

The results of the influence of this substratum could not be equal in all cases because the quality of the narrow vowels in Lappish, Samoyed and Vogul was not absolutely equal.

Some narrow vowels of Samoyed remained unchanged, cf. Yurak *pide* "nest", Fin. *pesä* Mord. E. *piže*, Hung. *feszek*, Vog. *piti*; Yurak *sew* "eye", Fin. *silmä*, Mord. *šelme*, Zir. *šin*; Yurak *pilüc* "to fear", Fin. *pelko* "fear", Hung. *fel* "to fear", Mord. *peľems*; Yur. *peľa* "half", Cher. *pel*, Ost. *pelök*, Hung. *fel*.

The cause of this phenomenon is to be looked for in a peculiar quality of these vowels.

The widening of the original narrow vowels in Lappish and northern Vogul has their own distinctive features.

The change of *o* into *a* which is so typical of Samoyed is non-typical of Lappish and Vogul. The change of various kinds of *e* or *ä* into *a* is more typical for these languages.

At the present time it is impossible to determine exactly the territorial boundaries of this substratum. The main zone of this language was apparently the north-eastern part of Russia and partly the northern Siberian slopes of the Urals.

There are some other traces of the substratum influence, for example the change of original *k* into *h* or *x* in the Yurak dialect of Samoyed and partly in some dialects of Vogul and Ostyak, cf. Yurak *hala* "fish", kamas. *kola*, in Vogul and Ostyak dialects *hul*, *hut*, Yurak *hädý* "spruce", kam. *kod* etc., the tendency to replace original *š* with *s* is traced in Yurak and partly in Vogul (Sošva d.), a tendency to change the affricate *č* into *ć* or *s* in Samoyed (cf. also the absence of this affricate in Vogul and Ostyak).

However these results of this influence are not traced in Lappish, which is due to the heterogeneity of the language-substratum. It had apparently eastern and western groups of dialects.

LES NIVEAUX DE L'ANALYSE LINGUISTIQUE

EMILE BENVENISTE

Quand on étudie dans un esprit scientifique un objet tel que le langage, il apparaît bien vite que toutes les questions se posent à la fois à propos de chaque fait linguistique, et qu'elles se posent d'abord relativement à ce que l'on doit admettre comme *fait*, c'est-à-dire aux critères qui le définissent tel. Le grand changement survenu en linguistique tient précisément en ceci : on a reconnu que le langage devait être décrit comme une structure formelle, mais que cette description exigeait au préalable l'établissement de procédures et de critères adéquats, et qu'en somme la réalité de l'objet n'était pas séparable de la méthode propre à le définir. On doit donc, devant l'extrême complexité du langage, viser à poser une ordonnance à la fois dans les phénomènes étudiés, de manière à les classer selon un principe rationnel, et dans les méthodes d'analyse, pour construire une description cohérente, agencée selon les mêmes concepts et les mêmes critères.

La notion de *niveau* nous paraît essentielle dans la détermination de la procédure d'analyse. Elle seule est propre à faire justice à la nature *articulée* du langage et au caractère *discret* de ses éléments ; elle seule peut nous faire retrouver, dans la complexité des formes, l'architecture singulière des parties et du tout. Le domaine où nous l'étudierons est celui de la langue comme système organique de signes linguistiques.

La procédure entière de l'analyse tend à délimiter les *éléments* à travers les *relations* qui les unissent. Cette analyse consiste en deux opérations qui se commandent l'une l'autre et dont toutes les autres dépendent : 1) la segmentation ; 2) la substitution.

Quelle que soit l'étendue du texte considéré, il faut d'abord le segmenter en portions de plus en plus réduites jusqu'aux éléments non décomposables. Parallèlement on identifie ces éléments par les substitutions qu'ils admettent. On aboutit par exemple à segmenter fr. *raison* en [r] – [ɛ] – [z] – [ø], où l'on peut opérer les substitutions : [s] à la place de [r] (=saison) ; [a] au lieu de [ɛ] (=rasons) ; [y] au lieu de [z] (rayon) ; [ẽ] au lieu de [ø] (raisin). Ces substitutions peuvent être recensées : la classe des substituts possibles de [r] dans [rezø] comprend [b], [s], [m], [t], [v]. Appliquant à chacun des trois autres éléments de [rezø] la même procédure, on dresse ainsi un répertoire de toutes les substitutions recevables, chacune d'elles dégagant à son tour un segment identifiable dans d'autres signes. Progressivement, d'un signe à l'autre, c'est la totalité des éléments qui sont dégagés et pour chacun d'eux la totalité des substitutions

possibles. Telle est en bref la méthode de distribution : elle consiste à définir chaque élément par l'ensemble des environnements où il se présente, et au moyen d'une double relation, relation de l'élément avec les autres éléments simultanément présents dans la même portion de l'énoncé (relation syntagmatique); relation de l'élément avec les autres éléments mutuellement substituables (relation paradigmatique).

Observons tout de suite une différence entre les deux opérations dans le champ de leur application. Segmentation et substitution n'ont pas la même étendue. Des éléments sont identifiés par rapport à d'autres segments avec lesquels ils sont en relation de substituabilité. Mais la substitution peut opérer aussi sur des éléments non segmentables. Si les éléments segmentables minimaux sont identifiés comme *phonèmes*, l'analyse peut aller au delà et isoler à l'intérieur du phonème des *traits distinctifs*. Mais ces traits distinctifs du phonème ne sont plus segmentables, quoique identifiables et substituables. Dans [d'] on reconnaît quatre traits distinctifs : occlusion, dentalité, sonorité, aspiration. Aucun d'eux ne peut être réalisé pour lui-même hors de l'articulation phonétique où il se présente. On ne peut non plus leur assigner un ordre syntagmatique ; l'occlusion est inséparable de la dentalité, et le souffle de la sonorité. Chacun d'eux admet néanmoins une substitution. L'occlusion peut être remplacée par une friction ; la dentalité par la labialité ; l'aspiration par la glottalité, etc. On aboutit ainsi à distinguer deux classes d'éléments minimaux : ceux qui sont à la fois segmentables et substituables, les phonèmes ; et ceux qui sont seulement substituables, les traits distinctifs des phonèmes. Du fait qu'ils ne sont pas segmentables, les traits distinctifs ne peuvent constituer de classes syntagmatiques ; mais du fait qu'ils sont substituables, ils constituent des classes paradigmatiques. L'analyse peut donc reconnaître et distinguer un niveau phonématique, où les deux opérations de segmentation et de substitution sont pratiquées, et un niveau hypo-phonématique, celui des traits distinctifs, non segmentables, relevant seulement de la substitution. Là s'arrête l'analyse linguistique. Au delà, les données fournies par les techniques instrumentales récentes appartiennent à la physiologie ou à l'acoustique, elles sont infra-linguistiques.

Nous atteignons ainsi, par les procédés décrits, les deux niveaux inférieurs de l'analyse, celui des entités segmentables minimales, les phonèmes, le niveau *phonématique*, et celui des traits distinctifs, que nous proposons d'appeler *merismes* (gr. *merisma*, *-atos* "délimitation"), le niveau *merismatique*.

Nous définissons empiriquement leur relation d'après leur position mutuelle, comme celle de deux niveaux atteints successivement, la combinaison des merismes produisant le phonème ou le phonème se décomposant en merismes. Mais quelle est la condition *linguistique* de cette relation ? Nous la trouverons si nous portons l'analyse plus loin, et puisque nous ne pouvons plus descendre, en visant le niveau supérieur. Il nous faut alors opérer sur des portions de textes plus longues et chercher comment réaliser les opérations de segmentation et de substitution quand il ne s'agit plus d'obtenir les plus petites unités possibles, mais des unités plus étendues.

Supposons que dans une chaîne angl. [li:vɪŋθɪŋz] "leaving things (as they are)" nous

ayons identifié à différentes places les trois unités phonématiques [i], [θ], [ɲ]. Nous tentons de voir si ces unités nous permettent de délimiter une unité supérieure qui les contiendrait. Procédant par exhaustion logique, nous envisageons les six combinaisons possibles de ces trois unités: [iθɲ], [iɲθ], [θiɲ], [θɲi], [ɲiθ], [ɲθi]. Nous voyons alors que deux de ces combinaisons sont effectivement présentes dans la chaîne, mais réalisées de telle manière qu'elles ont deux phonèmes en commun, et que nous devons choisir l'une et exclure l'autre: dans [li:viɲθiɲz] ce sera ou bien [ɲθi] ou bien [θiɲ]. La réponse ne fait pas de doute: on rejettera [ɲθi] et on élira [θiɲ] au rang de nouvelle unité /θiɲ/. D'où vient l'autorité de cette décision? De la condition linguistique du *sens* auquel doit satisfaire la délimitation de la nouvelle unité de niveau supérieur: [θiɲ] a un sens, [ɲθi] n'en a pas. A quoi s'ajoute le critère distributionnel que nous obtenons à un point ou à un autre de l'analyse dans sa phase présente si elle porte sur un nombre suffisant de textes étendus: [ɲ] n'est pas admis en position initiale et la séquence [ɲθ] est impossible, alors que [ɲ] fait partie de la classe des phonèmes finaux et que [θi] et [iɲ] sont également admis.

Le *sens* est en effet la condition fondamentale que doit remplir toute unité de tout niveau pour obtenir statut linguistique. Nous disons bien de tout niveau: le phonème n'a de valeur que comme discriminateur de signes linguistiques, et le trait distinctif, à son tour, comme discriminateur des phonèmes. La langue ne pourrait fonctionner autrement. Toutes les opérations qu'on doit pratiquer au sein de cette chaîne supposent la même condition. La portion [ɲθi] n'est recevable à aucun niveau; elle ne peut ni être remplacée par aucune autre ni en remplacer aucune, ni être reconnue comme forme libre, ni être posée en relation syntagmatique complémentaire aux autres portions de l'énoncé; et ce qu'on vient de dire de [ɲθi] vaut aussi pour une portion découpée dans ce qui le précède, par exemple [i:vi] ou ce qui le suit, [ɲz]. Ni segmentation ni substitution ne sont possibles. Au contraire l'analyse guidée par le sens dégagera deux unités dans [θiɲz], l'une signe libre /θiɲ/, l'autre [z] à reconnaître ultérieurement comme variante du signe conjoint /-s/. Plutôt que de biaiser avec le "sens", et d'imaginer des procédés compliqués – et inopérants – pour le laisser hors de jeu en retenant seulement les traits formels, mieux vaut reconnaître franchement qu'il est une condition indispensable de l'analyse linguistique.

Il faut seulement voir comment le sens intervient dans nos démarches et de quel niveau d'analyse il relève.

Il ressort de ces analyses sommaires que segmentation et substitution ne peuvent pas s'appliquer à des portions quelconques de la chaîne parlée. En fait, rien ne permettrait de définir la distribution d'un phonème, ses latitudes combinatoires de l'ordre syntagmatique et paradigmatic, donc la réalité même d'un phonème, si l'on ne se référait toujours à une *unité particulière* du niveau supérieur qui le contient. C'est là une condition essentielle, dont la portée sera indiquée plus loin. On voit alors que ce niveau n'est pas quelque chose d'extérieur à l'analyse; il est *dans* l'analyse; le niveau est un opérateur. Si le phonème se définit, c'est comme constituant d'une unité plus

haute, le morphème. La fonction discriminatrice du phonème a pour fondement son inclusion dans une unité particulière, qui, du fait qu'elle inclut le phonème, relève d'un niveau supérieur.

Soulignons donc ceci: une unité linguistique ne sera reçue telle que si on peut l'identifier *dans* une unité plus haute. La technique de l'analyse distributionnelle ne met pas en évidence ce type de relation entre niveaux différents.

Du phonème on passe ainsi au niveau du *signe*, celui-ci s'identifiant selon le cas à une forme libre ou à une forme conjointe (morphème). Pour la commodité de notre analyse, nous pouvons négliger cette différence, et classer les signes comme une seule espèce, qui coïncidera pratiquement avec le *mot*. Qu'on nous permette, toujours pour la commodité, de conserver ce terme décrié – et irremplaçable.

Le mot a une position fonctionnelle intermédiaire qui tient à sa nature double. D'une part il se décompose en unités phonématiques qui sont de niveau inférieur; de l'autre il entre, à titre d'unité signifiante et avec d'autres unités signifiantes, dans une unité de niveau supérieur. Ces deux propriétés doivent être quelque peu précisées.

En disant que le mot se décompose en unités phonématiques, nous devons souligner que cette décomposition s'accomplit même quand le mot est monophonématique. Par exemple, il se trouve qu'en français tous les phonèmes vocaliques coïncident matériellement avec un signe autonome de la langue. Disons mieux: certains signifiants du français se réalisent dans un phonème unique qui est une voyelle. L'analyse de ces signifiants n'en donnera pas moins lieu à une décomposition: c'est l'opération nécessaire pour accéder à une unité de niveau inférieur. Donc fr. *a*, ou *à* s'analyse en $/a/$; – fr. *est* s'analyse en $/e/$; – fr. *ait*, en $/\varepsilon/$; – fr. *y*, *hie*, en $/i/$; – fr. *eau*, en $/o/$; fr. *eu*, en $/y/$; – fr. *où* en $/u/$; – fr. *eux*, en $/\emptyset/$. De même en russe, où des unités ont un signifiant monophonématique, qui peut être vocalique ou consonantique: les conjonctions *a*, *i*; les prépositions *o*; *u* et *k*; *s*; *v*.

Les relations sont moins aisées à définir dans la situation inverse, entre le mot et l'unité de niveau supérieur. Car cette unité n'est pas un mot plus long ou plus complexe: elle relève d'un autre ordre de notions, c'est une phrase. La phrase se réalise en mots, mais les mots n'en sont pas simplement les segments. Une phrase constitue un tout, qui ne se réduit pas à la somme de ses parties; le sens inhérent à ce tout est réparti sur l'ensemble des constituants. Le mot est un constituant de la phrase, il en effectue la signification; mais il n'apparaît pas nécessairement dans la phrase avec le sens qu'il a comme unité autonome. Le mot peut donc se définir comme la plus petite unité signifiante libre susceptible d'effectuer une phrase, et d'être elle-même effectuée par des phonèmes. Dans la pratique, le mot est envisagé surtout comme élément syntagmatique, constituant d'énoncés empiriques. Les relations paradigmatiques comptent moins, en tant qu'il s'agit du mot, par rapport à la phrase. Il en va autrement quand le mot est étudié comme lexème, à l'état isolé. On doit alors inclure dans une unité toutes les formes flexionnelles, etc.

Toujours pour préciser la nature des relations entre le mot et la phrase, il sera nécessaire de poser une distinction entre mots *autonomes*, fonctionnant comme consti-

tuants de phrases (c'est la grande majorité), et mots *synnomes* qui ne peuvent entrer dans des phrases que joints à d'autres mots: ainsi fr. *le* (*la...*), *ce* (*cette...*); *mon* (*ton...*), ou *de*, *à*, *dans*, *chez*; mais non toutes les prépositions: cf. fr. pop. *c'est fait pour*; *je travaille avec*; *je pars sans*. Cette distinction entre "mots autonomes" et "mots synnomes" ne coïncide pas avec celle qui est faite depuis Marty entre "autosémantiques" et "synsémantiques". Dans les "synsémantiques" se trouvent rangés par exemple les verbes auxiliaires, qui sont pour nous "autonomes", déjà en tant qu'ils sont des verbes et surtout qu'ils entrent directement dans la constitution des phrases.

Avec les mots, puis avec des groupes de mots, nous formons des *phrases*; c'est la constatation empirique du niveau ultérieur, atteint dans une progression qui semble linéaire. En fait une situation toute différente va se présenter ici.

Pour mieux comprendre la nature du changement qui a lieu quand du mot nous passons à la phrase, il faut voir comment sont articulées les unités selon leurs niveaux et expliciter plusieurs conséquences importantes des rapports qu'elles entretiennent. La transition d'un niveau au suivant met en jeu des propriétés singulières et inaperçues. Du fait que les entités linguistiques sont discrètes, elles admettent deux espèces de relation: entre éléments de même niveau ou entre éléments de niveaux différents. Ces relations doivent être bien distinguées. Entre les éléments de même niveau, les relations sont *distributionnelles*; entre éléments de niveau différent, elles sont *intégratives*. Ces dernières seules ont besoin d'être commentées.

Quand on décompose une unité, on obtient non pas des unités de niveau inférieur, mais des segments formels de l'unité en question. Si on ramène fr. /*ɔm*/ *homme* à [ɔ] – [m], on n'a encore que deux segments. Rien ne nous assure encore que [ɔ] et [m] sont des unités phonématiques. Pour en être certain, il faudra recourir à /*ɔt*/ *hotte*, /*ɔs*/ *os* d'une part, à /*om*/ *heaume*, /*ym*/ *hume* de l'autre. Voilà deux opérations complémentaires de sens opposé. Un signe est matériellement fonction de ses éléments constitutifs, mais le seul moyen de définir ces éléments comme constitutifs est de les identifier à l'intérieur d'une unité déterminée où ils remplissent une fonction *intégrative*. Une unité sera reconnue comme distinctive à un niveau donné si elle peut être identifiée comme "partie intégrante" de l'unité de niveau supérieur, dont elle devient l'*intégrant*. Ainsi /*s*/ a le statut d'un phonème parce qu'il fonctionne comme intégrant de /*-al*/ dans *salle*, de /*-o*/ dans *seau*, de /*-ivil*/ dans *civil*, etc. En vertu de la même relation transposée au niveau supérieur, /*sal*/ est un signe parce qu'il fonctionne comme intégrant de: – *à manger*; – *de bains...* /*so*/ est un signe parce qu'il fonctionne comme intégrant de: – *à charbon*; un – *d'eau*; et /*sivil*/ est un signe parce qu'il fonctionne comme intégrant de: – *ou militaire*; *état* –; *guerre* –. Le modèle de la "relation intégrante" est celui de la "fonction propositionnelle" de Russell.¹

¹ B. Russell, *Introduction à la Philosophie mathématique*, trad. fr., p. 188: "Une 'fonction propositionnelle' est une expression contenant un ou plusieurs constituants indéterminés, tels que, lorsque des valeurs leur sont assignées, l'expression devient une proposition... 'x est humain' est une fonction propositionnelle; tant que x reste indéterminé, elle n'est ni vraie ni fausse; mais dès que l'on assigne un sens à x, elle devient une proposition vraie ou fausse".

Quel est, dans le système des signes de la langue, l'étendue de cette distinction entre constituant et intégrant? Elle joue entre deux limites. La limite supérieure est tracée par la phrase, qui comporte des constituants, mais qui, comme on le montre plus loin, ne peut intégrer aucune unité plus haute. La limite inférieure est celle du "merisme", qui, trait distinctif de phonème, ne comporte lui-même aucun constituant de nature linguistique. Donc la phrase ne se définit que par ses constituants; le merisme ne se définit que comme intégrant. Entre les deux un niveau intermédiaire se dégage clairement, celui des signes, autonomes ou synnômes, mots ou morphèmes, qui à la fois contiennent des constituants et fonctionnent comme intégrants. Telle est la structure de ces relations.

Quelle est finalement la fonction assignable à cette distinction entre constituant et intégrant? C'est une fonction d'importance fondamentale. Nous pensons trouver ici le principe rationnel qui gouverne, dans les unités des différents niveaux, la relation de la FORME et du SENS.

Voici que surgit le problème qui hante toute la linguistique moderne, le rapport forme: sens que maints linguistes voudraient réduire à la seule notion de la forme, mais sans parvenir à se délivrer de son corrélat, le sens. Que n'a-t-on tenté pour éviter, ignorer, ou expulser le sens? On aura beau faire: cette tête de Méduse est toujours là, au centre de la langue, fascinant ceux qui la contemplant.

Forme et sens doivent se définir l'un par l'autre et ils doivent ensemble s'articuler dans toute l'étendue de la langue. Leurs rapports nous paraissent impliqués dans la structure même des niveaux et dans celle des fonctions qui y répondent, que nous désignons ici comme "constituant" et "intégrant".

Quand nous ramenons une unité à ses constituants, nous la ramenons à ses éléments *formels*. Comme il a été dit plus haut, l'analyse d'une unité ne livre pas automatiquement d'autres unités. Même dans l'unité la plus haute, la phrase, la dissociation en constituants ne fait apparaître qu'une structure formelle, comme il arrive chaque fois qu'un tout est fractionné en ses parties. On peut trouver quelque chose d'analogue dans l'écriture, qui nous aide à former cette représentation. Par rapport à l'unité du mot écrit, les lettres qui le composent, prises une à une, ne sont que des segments matériels, qui ne retiennent aucune portion de l'unité. Si nous composons SAMEDI par l'assemblage de six cubes portant chacun une lettre, le cube M, le cube A, etc... ne seront porteurs ni du sixième ni d'une fraction quelconque du *mot* comme tel. Ainsi en opérant une analyse d'unités linguistiques, nous y isolons des constituants seulement formels.

Que faut-il pour que dans ces constituants formels nous reconnaissons, s'il y a lieu, des unités d'un niveau défini? Il faut pratiquer l'opération en sens inverse et voir si ces constituants ont fonction intégrante au niveau supérieur. Tout est là: la dissociation nous livre la constitution formelle; l'intégration nous livre des unités signifiantes. Le phonème, discriminateur, est l'intégrant, avec d'autres phonèmes, d'unités signifiantes qui le contiennent. Ces signes à leur tour vont s'inclure comme intégrants dans des unités plus hautes qui sont informées de signification. Les démarches de l'analyse

vont, en directions opposées, à la rencontre ou de la forme ou du sens dans les mêmes entités linguistiques.

Nous pouvons donc formuler les définitions suivantes :

La *forme* d'une unité linguistique se définit comme sa capacité de se dissocier en constituants de niveau inférieur.

Le *sens* d'une unité linguistique se définit comme sa capacité d'intégrer une unité de niveau supérieur.

Forme et sens apparaissent ainsi comme des propriétés conjointes, données nécessairement et simultanément, inséparables dans le fonctionnement de la langue.² Leurs rapports mutuels se dévoilent dans la structure des niveaux linguistiques, parcourus par les opérations descendantes et ascendantes de l'analyse, et grâce à la nature articulée du langage.

Mais la notion de sens a encore un autre aspect. Peut-être est-ce parce qu'on ne les a pas distingués que le problème du sens a pris une opacité aussi épaisse.

Dans la langue organisée en signes, le sens d'une unité est le fait qu'elle a un sens, qu'elle est signifiante. Ce qui équivaut à l'identifier par sa capacité de remplir une "fonction propositionnelle". C'est la condition nécessaire et suffisante pour que nous reconnaissons cette unité comme signifiante. Dans une analyse plus exigeante, on aurait à énumérer les "fonctions" que cette unité est apte à remplir, et – à la limite – on devrait les citer toutes. Un tel inventaire serait assez limité pour *meson* ou *chryso-prase*, immense pour *chose* ou *un*, peu importe; il obéirait toujours au même principe d'identification par la capacité d'intégration. Dans tous les cas on serait en mesure de dire si tel segment de la langue "a un sens" ou non.

Un tout autre problème serait de demander: *quel* est ce sens? Ici "sens" est pris en une acception complètement différente.

Quand on dit que tel élément de la langue, court ou étendu, a un sens, on entend par là une propriété que cet élément possède en tant que signifiant, de constituer une unité distinctive, oppositive, délimitée par d'autres unités, et identifiable pour les locuteurs natifs, de qui cette langue est *la* langue. Ce "sens" est implicite, inhérent au système linguistique et à ses parties. Mais en même temps le langage porte référence au monde des objets, à la fois globalement, dans ses énoncés complets, sous forme de phrases, qui se rapportent à des situations concrètes et spécifiques, et sous forme d'unités inférieures qui se rapportent à des "objets" généraux ou particuliers, pris dans l'expérience ou forgés par la convention linguistique. Chaque énoncé, et chaque terme de l'énoncé, a ainsi un référend, dont la connaissance est impliquée par l'usage natif

² F. de Saussure semble avoir conçu aussi le "sens" comme une composante interne de la forme linguistique, bien qu'il ne s'exprime que par une comparaison destinée à réfuter une autre comparaison: "On a souvent comparé cette unité à deux faces [l'association du signifiant et du signifié] avec l'unité de la personne humaine, composée du corps et de l'âme. Le rapprochement est peu satisfaisant. On pourrait penser plus justement à un composé chimique, l'eau par exemple; c'est une combinaison d'hydrogène et d'oxygène; pris à part, chacun de ces éléments n'a aucune des propriétés de l'eau" (*Cours*, 2e éd., p. 145).

de la langue. Or, dire *quel* est le référend, le décrire, le caractériser spécifiquement est une tâche distincte, souvent difficile, qui n'a rien de commun avec le maniement correct de la langue. Nous ne pouvons nous étendre ici sur toutes les conséquences que porte cette distinction. Il suffit de l'avoir posée pour délimiter la notion du "sens", en tant qu'il diffère de la "désignation". L'un et l'autre sont nécessaires. Nous les retrouvons, distincts mais associés, au niveau de la *phrase*.

C'est là le dernier niveau que notre analyse atteigne, celui de la *phrase*, dont nous avons dit ci-dessus qu'il ne représentait pas simplement un degré de plus dans l'étendue du segment considéré. Avec la phrase une limite est franchie, nous entrons dans un nouveau domaine.

Ce qui est nouveau ici, tout d'abord, est le critère dont relève ce type d'énoncé. Nous pouvons segmenter la phrase, nous ne pouvons pas l'employer à intégrer. Il n'y a pas de fonction propositionnelle qu'une proposition puisse remplir. Une phrase ne peut donc pas servir d'intégrant à un autre type d'unité. Cela tient avant tout au caractère distinctif entre tous, inhérent à la phrase, d'être un *prédicat*. Tous les autres caractères qu'on peut lui reconnaître viennent en second par rapport à celui-ci. Le nombre de signes entrant dans une phrase est indifférent: on sait qu'un seul signe suffit à constituer un prédicat. De même la présence d'un "sujet" auprès d'un prédicat n'est pas indispensable: le terme prédicatif de la proposition se suffit à lui-même puisqu'il est en réalité le déterminant du "sujet". La "syntaxe" de la proposition n'est que le code grammatical qui en organise l'arrangement. Les variétés d'intonation n'ont pas valeur universelle et restent d'appréciation subjective. Seul le caractère prédicatif de la proposition peut donc valoir comme critère. On situera la proposition au niveau *catégorématique*.³

Mais que trouvons-nous à ce niveau? Jusqu'ici la dénomination du niveau se rapportait à l'unité linguistique relevante. Le niveau phonématique est celui du phonème; il existe en effet des phonèmes concrets, qui peuvent être isolés, combinés, dénombrés. Mais les catégorèmes? Existe-t-il des catégorèmes? Le prédicat est une propriété fondamentale de la phrase, ce n'est pas une unité de phrase. Il n'y a pas plusieurs variétés de prédication. Et rien ne serait changé à cette constatation si l'on remplaçait "catégorème" par "phrasème".⁴ La phrase n'est pas une classe formelle qui aurait pour unités des "phrasèmes" délimités *et opposables entre eux*. Les types de phrases qu'on pourrait distinguer se ramènent tous à un seul, la proposition prédicative, et il n'y a pas de phrase hors de la prédication. Il faut donc reconnaître que le niveau catégorématique comporte seulement une forme spécifique d'énoncé linguistique, la proposition; celle-ci ne constitue pas une classe d'unités distinctives. C'est pourquoi la proposition ne peut entrer comme partie dans une totalité de rang plus élevé. Une proposition peut seulement précéder ou suivre une autre proposition, dans un rapport de consécution. Un groupe de propositions ne constitue pas une unité d'un ordre

³ Gr. *kategorema* = lat. *praedicatum*.

⁴ Puisqu'on a fait *lexème* sur gr. *lexis*, rien n'empêcherait de faire *phrasème* sur gr. *phrasis* "phrase".

supérieur à la proposition. Il n'y a pas de niveau linguistique au delà du niveau catégorématique.

Du fait que la phrase ne constitue pas une classe d'unités distinctives, qui seraient membres virtuels d'unités supérieures, comme le sont les phonèmes ou les morphèmes, elle se distingue foncièrement des autres entités linguistiques. Le fondement de cette différence est que la phrase contient des signes, mais n'est pas elle-même un signe. Une fois ceci reconnu, le contraste apparaît clairement entre les ensembles de signes que nous avons rencontrés aux niveaux inférieurs et les entités du présent niveau.

Les phonèmes, les morphèmes, les mots (lexèmes) peuvent être comptés. Ils sont en nombre fini. Les phrases, non.

Les phonèmes, les morphèmes, les mots (lexèmes) ont une distribution à leur niveau respectif, un emploi au niveau supérieur. Les phrases n'ont ni distribution ni emploi.

Un inventaire des emplois d'un mot pourrait ne pas finir; un inventaire des emplois d'une phrase ne pourrait même pas commencer.

La phrase, création indéfinie, variété sans limite, est la vie même du langage en action. Nous en concluons qu'avec la phrase on quitte le domaine de la langue comme système de signes, et l'on entre dans un autre univers, celui de la langue comme instrument de communication, dont l'expression est le discours.

Ce sont là vraiment deux univers différents, bien qu'ils embrassent la même réalité, et ils donnent lieu à deux linguistiques différentes, bien que leurs chemins se croisent à tout moment. Il y a d'un côté la langue, ensemble de signes formels, dégagés par des procédures rigoureuses, étagés en classes, combinés en structures et en systèmes, de l'autre, la manifestation de la langue dans la communication vivante.

La phrase appartient bien au discours. C'est même par là qu'on peut la définir: la phrase est l'unité du discours. Nous en trouvons confirmation dans les modalités dont la phrase est susceptible: on reconnaît partout qu'il y a des propositions assertives, des propositions interrogatives, des propositions impératives, distinguées par des traits spécifiques de syntaxe et de grammaire, tout en reposant identiquement sur la prédication. Or ces trois modalités ne font que refléter les trois comportements fondamentaux de l'homme parlant et agissant par le discours sur son interlocuteur: il veut lui transmettre un élément de connaissance, ou obtenir de lui une information, ou lui intimer un ordre. Ce sont les trois fonctions interhumaines du discours qui s'impriment dans les trois modalités de l'unité de phrase, chacune correspondant à une attitude du locuteur.

La phrase est une unité, en ce qu'elle est un segment de discours, et non en tant qu'elle pourrait être distinctive par rapport à d'autres unités de même niveau, ce qu'elle n'est pas, comme on l'a vu. Mais c'est une unité complète, qui porte à la fois sens et référence: sens parce qu'elle est informée de signification, et référence parce qu'elle se réfère à une situation donnée. Ceux qui communiquent ont justement ceci en commun, une certaine référence de situation, à défaut de quoi la communication comme telle ne s'opère pas, le "sens" étant intelligible, mais la "référence" demeurant inconnue.

Nous voyons dans cette double propriété de la phrase la condition qui la rend analysable pour le locuteur même, depuis l'apprentissage qu'il fait du discours quand il apprend à parler et par l'exercice incessant de son activité de langage en toute situation. Ce qui lui devient plus ou moins sensible est la diversité infinie des contenus transmis, contrastant avec le petit nombre d'éléments employés. De là, il dégagera inconsciemment, à mesure que le système lui devient familier, une notion tout empirique du signe, qu'on pourrait définir ainsi, au sein de la phrase: le signe est l'unité minimale de la phrase susceptible d'être reconnue comme identique dans un environnement différent, ou d'être remplacée par une unité différente dans un environnement identique.

Le locuteur peut ne pas aller plus loin; il a pris conscience du signe sous l'espèce du "mot". Il a fait un début d'analyse linguistique à partir de la phrase et dans l'exercice du discours. Quand le linguiste essaie pour sa part de reconnaître les niveaux de l'analyse, il est amené par une démarche inverse, partant des unités élémentaires, à fixer dans la phrase le niveau ultime. C'est dans le discours, actualisé en phrases, que la langue se forme et se configure. Là commence le langage. On pourrait dire, calquant une formule classique: nihil est in *lingua* quod non prius fuerit in *oratione*.

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DISCUSSION

BUYSENS:

A la page 266, M. Benveniste mentionne les deux opérations par lesquelles nous analysons le discours. Je reprends son opposition entre *raison* et *saison*, qui permet d'opposer *r* et *s*; dans ce processus d'analyse, il y a une troisième opération; lorsque M. Benveniste a rapproché ces deux mots, il a implicitement admis que ce qui suit les phonèmes *r* ou *s* est identique; il a donc admis une identification en même temps qu'une opposition. Pour la segmentation, l'opposition est évidemment plus importante que l'identification; mais pour la suite de l'étude linguistique, l'identification est très importante; par exemple, il s'agira de prouver que le *r* de *raison* est le même phonème que le *r* de *perron* ou de *départ*. Certains linguistes se sont demandés par quelle méthode cette identification pouvait se faire; ils oubliaient qu'ils s'étaient basés dès le début sur de telles identifications. J'ajouterai que lorsqu'on oppose *raison* et *saison*, on lie l'opposition *r/s* à une opposition de signifiés; mais lorsqu'on identifie le segment *aison* des deux signifiants on n'identifie pas leurs signifiés; il y a là un fait qui doit nous faire réfléchir.

A la page 269 je lis: "Un mot peut donc se définir comme la plus petite unité signifiante libre susceptible d'effectuer une phrase." Que signifie le mot *libre*? Par la suite il est question de mot autonome et de mot synnôme; si *libre* était synonyme de *autonome*, le mot synnôme ne serait pas un mot; ce n'est donc pas possible.

D'autre part, que signifie *susceptible d'effectuer une phrase*? Si M. Benveniste veut dire que tout mot peut constituer une phrase à lui seul, le mot synonyme n'est pas un mot. Et d'ailleurs tous les mots autonomes ne peuvent pas former à eux seuls une phrase. Par exemple, pour employer le mot *table* tout seul dans une conversation, il faut des circonstances tellement exceptionnelles que l'on peut en faire autant pour des parties de mots. Vous savez que bon nombre de Français prononcent de même le futur *je ferai* et le conditionnel *je ferais*; supposons que je n'aie pas compris si mon interlocuteur a dit "J'y assisterai" ou "J'y assisterais"; je puis lui demander "rai ou rais"; ou si je m'étonne qu'il emploie le conditionnel, je puis lui demander simplement "rais"? Le mot ne peut donc pas être défini comme la plus petite unité pouvant constituer une phrase. J'en viens dès lors à me demander si dans le passage auquel je faisais allusion, M. Benveniste a réellement cherché à donner une définition. Car à la fin de son rapport il donne une véritable définition du signe: "le signe est l'unité minimale de la phrase susceptible d'être reconnue comme identique dans un environnement différent, ou d'être remplacée par une unité différente dans un environnement identique." Je n'ai évidemment aucun reproche à faire à cette définition.

Passons à la notion de prédicat. Page 273 je lis "le prédicat est une propriété fondamentale de la phrase, ce n'est pas une unité de phrase", et "Il n'y a pas de phrase hors de la prédication." J'ai cherché en vain dans le rapport ce qu'il faut entendre par prédicat. Tout ce que j'ai découvert, c'est qu'il ne s'agit pas du prédicat des logiciens, ni du prédicat tel que je l'ai défini moi-même. Il me semble que M. Benveniste se sert de la notion de prédicat pour définir la phrase; il faudrait donc définir le prédicat sans faire allusion à la notion de phrase.

Page 273-274 je trouve deux phrases qui semblent se contredire. D'abord M. Benveniste dit que "un groupe de propositions ne constitue pas une unité d'un ordre supérieur à la proposition. Il n'y a pas de niveau linguistique au delà du niveau catégorématique." Mais plus loin: "La phrase est l'unité du discours.... La phrase est une unité en ce qu'elle est un segment du discours." Selon cette dernière déclaration, il y aurait un niveau du discours. D'autre part, je suis étonné de lire que, selon M. Benveniste, la phrase n'est pas distinctive par rapport à d'autres unités de même niveau; il me semble pourtant qu'il y a opposition entre "Je le connais" et "Je ne le connais pas" et que l'insertion de l'une ou de l'autre phrase dans un discours modifie la signification du discours, J'admets évidemment que le lien entre les phrases d'un discours n'est pas le même que le lien entre les mots d'une phrase; mais s'il faut tenir compte de cette différence, il faut alors aussi admettre que le lien entre les mots d'une phrase n'est pas le même que celui entre les phonèmes d'un mot; et il faut cesser de parler de valeur distinctive indifféremment dans les deux cas.

Enfin je trouve page 274 que M. Benveniste n'admet que trois sortes de phrases: assertive, interrogative, impérative; il y a encore les phrases optatives. Certes, elles ressemblent aux impératives, mais je ne vois pas le moyen de les y assimiler; car les phrases impératives demandent à l'auditeur d'agir; par exemple, si on m'annonce un visiteur et que je réponde "Qu'il entre", je demande à mon interlocuteur de trans-

mettre mon message. Mais si je dis "Puisse-t-il faire beau temps", je ne demande pas à mon interlocuteur d'agir; la phrase optative est celle où se marque le moins le désir d'influencer l'auditeur.

GARVIN:

This concerns the remark that the identification of structural units requires the use of units of the next higher level as frames. There is an apparent contradiction: how can higher-level units be used in the analysis, when these have themselves not yet been defined? The contradiction is resolved when one keeps in mind that the higher-level units used as frames will not be analytically defined units, but behavioral units such as informant words or informant sentences, obtained by elicitation under controlled conditions. A detailed discussion of this is contained in my "A Study of Inductive Method in Syntax", *Word*, 18 (1962), 107-120.

WINTER:

M. Benveniste lists as one of the distinctive properties of the sentence vis-à-vis the phoneme, the morpheme, and the word that there is a finite number to the lower-level features, whereas sentences cannot be counted. The question is whether this assessment is fully justified. One example will suffice: just as the class of sentences is open-ended in German or in English, so is the class of words – witness the unlimited freedom in the generation of compounds. To a lesser extent, the same applies to even lower levels of analysis: the number of morphs can be increased at any time by as simple a process as borrowing; thus even the morphs of a language as a functioning system are not countable. The same openness to borrowing applies again to the phonemic level once we decide not to exclude borrowed material from our consideration. What we are faced with are relative degrees of openness and closedness of a subsystem, not as Benveniste seems to suggest, absolute closedness on some levels vs. absolute openness on another. If this observation is correct, much of the suggested uniqueness of the status of the sentence is eliminated, and the various levels turn out to be more similar to one another than indicated by Benveniste's treatment.

CARNOCHAN:

The concept of levels is now a commonplace in modern linguistic analysis, although different writers make different uses of it. Even Professor Firth, whose contribution to linguistics included a powerful development of this concept, was not always consistent in the number of levels he recognized. In the present paper, I cannot see what advantages Professor Benveniste obtains by the introduction of his new terminology; his terms seem only to cover the most traditional levels of phonetics, phonology, morphemics and syntax.

While agreeing with all he says about the importance of regarding the sentence – used technically – as the unit of discourse, it appears to me that his analysis places a very heavy load on this sentence level; and it may well be argued that it is not useful to

regard this as a “level” at all. It would appear preferable to regard such units as word, phrase, group, clause and sentence – all defined technically for the particular language under analysis – as stages or ranks rather than as levels, since grammatical, phonological and phonetic statements can be made about each of them at the appropriate time, and it would indeed appear that such statements are the ones that are made at different levels.

The presentation in proceeding from the smallest units, the differential features, to the largest, the sentence, seems to be that for levels of synthesis rather than for levels of analysis, even though the one presupposes the other. It is surely often useful to go in both directions, from one level to another, up and down, provided that one is aware at which level one is operating for particular parts of the description. To “squint” at one level from another is frequently deplored. I entirely agree. To squint is useless. It is necessary to take a thorough look, not to squint, and to see how one’s statement at one level affects those at all the others.

It may be that the *niveau phonématique* corresponds to the more traditional level of phonology, but if so, it seems to me as a follower of Firth that it is a retrograde step to ignore all the work that has been done showing the advantages of considering phonological elements other than phonemes, and to exclude them from mention. I would prefer at this level not only phonematic elements – those that have a linear place in structure and an order, but also prosodic elements of phonological structure, those which have phonetic exponents relateable to larger pieces, at least as extensive as the syllable; remembering that at this level, we are dealing not with noises or sounds at all, but with abstractions functioning both syntagmatically as elements of structure in the “piece” and paradigmatically as terms in systems set up for the relevant places in structure. Exponential reference to these elements is made at the phonetic level, which corresponds, it may be presumed with Professor Benveniste’s *niveau mérisématique*.

HATTORI:

On page 272, Professor Benveniste formulates the following:

La forme d’une unité linguistique se définit comme sa capacité de se dissocier en constituants de niveau inférieur.

Le sens d’une unité linguistique se définit comme sa capacité d’intégrer une unité de niveau supérieur.

In his definitions, *la phrase* is also *une unité (la plus haute)* (see p. 271, l. 26; p. 274, l. 25), and the word (*θiŋz*) (= *things*) can be analyzed into *deux unités* which have meanings (see p. 268, l. 27), but phonemes are *unités* but not *unités signifiantes*. Therefore, we probably have to exclude the phonemes, distinctive features, etc., from the *unités linguistiques* in the definitions quoted above.

Now, in my definition, an *utterance* is a unique event which has both sound and meaning. Even the same speaker cannot repeat the same utterance. What he can repeat is the *sentence(s)* which is or are contained in the so-called “same” utterances.

An utterance contains one or more sentences which is or are the recurrent features (including the distinctive features) due to social linguistic habits. Non-recurrent features and those features which are recurrent but due to individual habits are excluded from the sentence. In Hjelmslev's terms, we could say that an utterance is *substance* while the sentence is *form*. I will call the sound and meaning of a sentence its *shape* and *sense*. Shape and sense are the form of *expression* and *content*, respectively, in Hjelmslev's terminology. (See my article "Analysis of Meaning" in *For Roman Jakobson*, and "Can We Understand Foreigners?" in my *Methods in Linguistics*, Iwanami, Tokyo, 1960, pp. 795).

It seems to me that Benveniste's *phrase* means the *utterance* (which contains *one* sentence) rather than the *sentence*, because he says that ... sous forme de phrases, qui se rapportent à des situations concrètes et spécifiques (p. 272, l. 31).

The sound and meaning of an utterance cannot be analyzed (*dissocié*) into units. Its sound is an indivisible continuum. What we can analyze into units and features is the shape of a sentence, not the sound of an utterance containing it. Although we cannot analyze the meaning of an utterance into units, I think, we can analyze the sense of a sentence into constituent units and features.

If we call the sense of a *word* (= *mot*) its *sememe*, the sense of a sentence is evidently not the sum of the sememes of the words which it contains; it is something more than that. However, we have to pay attention to the fact that the shape of a sentence is also something more than the sum of the shapes of the words which it contains.

The *expressor* of a sentence (as distinguished from the *utterer* of an utterance [See my book *Methods in Linguistics*, p. 797 ff.]), who has been in the background during the process of the sentence, comes to the surface at the end of it, and endorses the whole sense of the sentence. Corresponding to this feature of the sense of a sentence, its shape has a marked feature which denotes the end of it, followed by a pause.

The intonation, which is a constituent of the shape of a sentence and is superimposed like an envelope upon a sequence of the shapes of words, has its corresponding sense which does not belong to any word or construction contained in the sentence.

The sense of a construction is something more than the sum of the sememes of words which it contains. We can easily understand this point, when we have an opposition. Thus, a Japanese construction *sewa-suru rôzin* (*sewa-suru* means «to take care of»; *rôzin* means «old man») can mean two things:

- 1) an old man who takes care of (somebody)
- 2) an old man into is (to be) taken care of (by somebody)

This difference can be revealed also by means of the so-called transformation:

- 1) is the transform of *rôzin-ga* [nominative] *sewa-suru*
- 2) is the transform of *rôzin-o* [accusative] *sewa-suru*.

Corresponding to this, the shape of a construction is something more than the sum of the shapes of words which it contains. Thus, the sentence *sewa-suru rôzin-wa arimasen* («I have no old man who would take care of [my children]» or «I have no old man whom I have to take care of») can be pronounced:

Sewa-suru rôzin-wa / arimasen.

But it cannot be pronounced:

Sewa-suru / rôzin-wa arimasen.

This fact leads me to assume that we have here some features of the shape which are superimposed like envelopes upon the shapes of the sequences of words: *sewa-suru rôzin-wa* and *arimasen*, respectively. We could call these features of the shape with their senses the *tagmemes*. The two tagmemes above, which are superimposed upon *sewa-suru rôzin* and have different senses, are homonymous, because there is no difference in their shapes.

When Mr. Susumu Kuno, who is engaged at Harvard in the work of machine translation, put the following type-written sentence in the machine:

He did it as fast as he could,

it gave two answers. The difference in meaning of the two interpretations can be paraphrased as the following:

1) He did it in his fastest possible way,

2) He did it as fast (=fast of breakfast) because he was able to. Although we usually interpret this type-written sentence as having the first sense, a sentence with the second sense is also not altogether impossible theoretically. In their spoken forms, the two sentences have different shapes, that is, they are not homonymous but differ in the shapes of tagmemes, intonation, and therefore in the possible pauses which can occur between words. In their type-written forms, however, there is no clue at all as to their structural difference. The sentence is usually uttered in the shape (1) and is understood in the sense (1), and therefore we understand in the sense (1) the type-written sentence as well. However, the machine is right in giving two answers, because there is no clue, in the script, as to the single decision.

The shape of a word can be analyzed into constituent units and features, i.e. syllable(s), phonemes and distinctive and other features. In my opinion, however, a prosodeme is necessary to complete the shape of an autonomous word (*mot autonome*). It is superimposed like an envelope upon the syllable or the sequence of syllables which constitutes the shape of the word. The shape of a *synonymous* word (*mot synnyme*) has a secondary prosodeme or shares a prosodeme, with the autonomous word to which it is affixed.

On the other hand, the sememe of a word, which is neither compound, derived nor inflected, but is simple and primary (e.g. a Japanese noun), can be analyzed into the constituent features, i.e. the grammatical feature(s) and the lexico-semantic features.

Accordingly, in place of the first definition of Professor Benveniste, I would say that *the shape and sense of a linguistic unit have the capacity to be analyzed into lower level constituents.*

As for the second definition, I should like to mention several points. Inasmuch as a phoneme does not have any sense of its own, its shape must be its capacity to integrate a unit of some higher level. On the other hand, a linguistic unit, i.e. in this case a word or a construction, has both the shape and the sense. Is its shape also not its

capacity to integrate a unit of a higher level? At the same time, I want to call for attention to the fact that the sememe of a word has a peculiar power to restrict the environment in which it may occur. For instance, the sememes of English words *pie* and *road* have the same grammatical features, but their lexico-semantic features differ, and we see reflections of this fact in the following cases:

the constructions *a delicious pie* and *a delicious road* have the same tagmeme and both are grammatical, because the sense of the tagmeme, which can be described as something like «modifier (predicate) + modified (subject)», matches with the grammatical features of the sememes of *pie* and *road*. From the semantic point of view, however, the former construction is adequate, whereas the latter is inadequate, because the lexico-semantic features of the sememe of *pie* matches with those of *delicious*, but those of *road* do not.

If we call the *form* (in Hjelmslev's term) of an utterance the *linguistic production*, it may consist of more than one sentence. The sentences contained in one linguistic production are grammatically independent from each other, but semantically dependent upon each other, making a contextual continuity. Therefore, we could say that their senses integrate the sense of the linguistic production. This fact cannot be overlooked, as well.

In conclusion, I should like to emphasize the following point: in the study of the shape and sense, i.e. the *form* of a language, it is not only at the lowest and highest levels, but also at all the intermediate levels, that we have to be confronted with the *substance*. (See my comments to Hans Glinz's and F. G. Lounsbury's papers in this volume.)

HERDAN:

The distinction of levels of linguistic analysis as given by Professor Benveniste is quite in accordance with that used in mathematical linguistics under the name of the Type-Token duality. What belongs to the category of Type in one given level, e.g. the phoneme as the appropriate unit on the phonemic level of a language, becomes the Token on the next higher level, the morphemic one, when it is used as part of the morpheme; or the word which represents the Type on the vocabulary level becomes the Token on the next higher level as part of the sentence.

But I shall not elaborate the general agreement between Professor Benveniste's view of levels of linguistic analysis and mathematical analysis of language which, after all, is only to be expected considering that mathematical linguistics can be conceived as the quantification of de Saussurian linguistics. Instead I should like to draw attention to a shift in the dividing line which has taken place between the two extreme levels of analysis, each taken as an integral whole: Linguistics in the strict sense of the term, on the one hand, and literary criticism on the other. In the time at my disposal I cannot do better than point to an analogous shift of the dividing line between two levels of analysis which has in more recent times taken place in physics.

The trend I am referring to is known as the *geometrization* of physics. The main –

and profound – difference between classical and modern, i.e. Einsteinian physics is this. According to the former, the mathematician was to confirm and analyse the properties of space and time, building up the primary sciences of geometry and kinematics; then, when the stage had thus been prepared, the physicist was to come along with the *dramatis personae* – material bodies, magnets, electric charges, light and so forth – and the play was to begin. The geometry was that of Euclid as the only possible one, and the behaviour of the *dramatis personae* had to be in conformity with it, no matter what the difficulties and contradictions were in which one got involved.

In Einstein's revolutionary conception of general relativity, it was the characters which created the stage as they walked about on it; geometry was no longer antecedent to physics, nor was it any longer exclusive, but was indissolubly fused with physics into a single discipline. Euclidian geometry was deposed from its old position of propriety, if not uniqueness, and from being accepted as a valid representation of space.

The transition from classical to modern physics appeared to turn on the question whether we prefer to have an easily intelligible geometry with complicated physical laws, different according to the branch of physical science, or a less intelligible geometry with simple physical laws, unified for the various fields of the physical universe. The concensus of opinion now is that geometry should be regarded as part of physics, and, therefore, our system of geometry should be one in which the rest of physics can be expressed as simply as possible: it is this consideration which led ultimately to the curved space of general relativity.

Generally speaking, the development consisted in realising that much of what was regarded as physics, or space and time content, could be conceived as geometry, and this also characterises the transition from classical to structural linguistics. The three-fold system of phonemics, vocabulary and grammar of a particular language was for classical linguistics what Euclidian geometry was for classical physics: the form or the stage on which the ideas as the *dramatis personae* appeared. Differences between works of literature in a given language could only be accounted for in terms which had nothing to do with the general stage of language.

The development towards structural linguistics consists in reclaiming more and more linguistic features as part of the stage, just as more and more physical features were claimed to be part of geometry. The advantage this has in linguistics is again parallel to that in physics. We may have simple or easily intelligible linguistics of the old type, but with rather complex laws – if any – governing the actual linguistics events in *la parole*, or we may have the less intelligible and more complex structural linguistics of today, but with rather simple laws for the relation of literary events. The concensus of opinion is now very much in favour of the latter.

According to the classical, i.e. pre-structural view, the departments of the linguists and of the literary scholars were quite different. By linguistics one understood the general laws of language structure; but the use of language in literary works was regarded as being a purely individual achievement, and therefore not amenable to any general treatment. In the light of the results of mathematical linguistics, such a

view can no longer be upheld, because mathematics has succeeded in establishing some of the general laws of language in use. This new discipline is now wedged between linguistics and literary studies, and since it deals with general laws of language, really belongs to linguistics proper.

MISRA:

I add that in the Paninian theory of language, it was the sentence which was accepted as the most natural level to start with, in linguistic analysis. The lower levels are artificial abstractions; their validity lies in their being representations of the sentence, which is itself the only integral whole in a language. This is the theory known as *vākya-sphoṭa* (representation of a sentence).

PIKE:

I support Prof. Benveniste in his attention given to units, and in the places where he includes distribution in a level higher than a unit as relevant to the distribution of that unit.

But I disagree when he treats the sentence as itself the highest level, without distribution in a still higher relevant structure. In recent theoretical and empirical work of the Summer Institute of Linguistics we have been forced to deal with units larger than the sentence. In order to be able to analyze the difference between independent versus dependent sentence types empirically, for example, we have frequently needed to utilize contrastive occurrence in a unit comprising an utterance slot followed by a response slot.

In theory, furthermore, we treat a unit – any and every unit of purposive human behavior – as being well-defined only if its distribution in a larger unit is specified. This requires that a sentence, also, have this distribution made explicit.

In a number of recent studies in American Indian languages my colleagues are specifying paragraph structures, antecedent relationships, and the like.

In field work, furthermore, we find it impossible either to work exclusively from lowest to highest levels, or from highest to lowest. Rather, we start with a total but small “circle” of data-simultaneously lexical, phonological, grammatical. This, grasped in a behavioral context, can then be expanded in all directions.

CHATTERJI:

The present divergence of view in linguistic theory and methodology which has been posed by the title of the subject for this morning's plenary session, *Levels of Linguistic Analysis*, has been exercising linguistic scholars in both Europe and America, and it has in some of its obvious or noteworthy forms attracted the attention of some of the few top-ranking linguistic scholars in India also. Exponents of the new method and approach, mostly trained in America, are now working in the study of different Indian languages, and they are but vaguely conscious of this diversity, but some of them are inclined to ignore or brush aside as old-fashioned

whatever does not conform to the canons and to the terminology of the new method. The present speaker has had his life's work done in the atmosphere of the tradition which has been built up in India and Europe, and again as a reflex in Modern India of what has been established in Europe. As by training he has not been a "scientific technician" in linguistics, and as he lacks a knowledge of psychology and mathematics and the physical sciences at their higher levels, he is not competent to say whether by too much application of these physical sciences and of mathematical and psychological concepts in the new methodology, we are not removing linguistics from its own particular sphere, and making it "the caricature of some other discipline", as de Saussure warned us not to do. Economics, I am told, has now become almost a succursal of statistics and mathematics.

I have therefore not ventured to plunge into the subject with all its implications and its technological paraphernalia. I have taken in this gathering of specialists — and I crave your indulgence for that — the position of a lay student of language for whom language is primarily an all-round social phenomenon in which the individual man participates. Linguistics is a human science, a science dealing with man as a living and thinking organism circumscribed by actualities, and consequently, as he feels, a detached, dry-as-dust as well as exclusive application of the physical sciences has to be taken with a little caution and circumspection. Language is a continuity, a dynamic process; at the same time, at a given space-time context, it is within its dynamism, in admissible theory at least, something static. Both these aspects have to be taken note of.

The levels of linguistic analysis as treated in India presents a long history. We at least in India are now at a confluence of the old and the new. The situation from the point of view of the Old World tradition, which began in India and which took its final shape in Europe, has been discussed in some very suggestive papers by the late Professor J. R. Firth of London and some of his British colleagues (in *Studies in Linguistic Analysis*, Oxford, 1957), and these papers have sought to clarify the position. In the following observations, without going into details of illustrative examples, I have sought to look at the matter from what may be called the Indian point of view — with its uncompromising objective description and analysis of the phonetic and morphophonemic structure of Sanskrit as a static thing, and its gradually developing and almost overwhelming sense of the historical and the comparative for the latter phases, and its want of a new approach in tagging on the human science of linguistics to other scientific disciplines in both the material or physical and the metaphysical planes; — and it may perhaps be permissible to present this before the Congress, for aught it is worth, as being something, at least partially, within the orbit of the study of the levels of linguistic analysis.

The beginnings of grammar and the grammatical science in the Sanskrit language in India go back to the Vedic period (sometime before 1000 B.C.), and serious studies appear to have started from the early centuries of the 1st millennium B.C.

The greatest advance of these studies on the phonetic side we find in the works known as the *Prātiśākhya*s, which started to be composed probably from the middle of the 1st millennium B.C.; and on the side of both phonetics and morphology as well as syntax — in morphophonemics and morpheme relations — the sage and grammarian Pāṇini, living in the extreme North-West of India (near the present town of Attock or Aṭak on the Indus, now in Pakistan) in the 5th century B.C., presents the peak for the age, and for any age for the matter of that, in the study of Sanskrit. Pāṇini was at the end of a long series of grammarians whom he has utilised and at times quoted, and at the same time he stands at the head of another long line of grammarians, who came after him, for some 2400 years, right down to the 20th century.

The Sanskrit grammatical system as perfected by Pāṇini presents one of the highest achievements of man in the field of linguistic analysis. In both the objective or scientific study of the sounds of the living speech, of the sounds in themselves and in their relation to each other as well as to their significance in linguistic communication, and also in the study of the morphemes or the significant elements which by combination give rise to expressive words and sentences, this Linguistic Analysis presents something at a very high and comprehensive level, making, in the first instance, Phonetics the basis of all analysis. This can very well be taken note of in the latest development of the linguistic science in the West in what has been called *descriptive linguistics*.

In the first instance, the word *Vyākaraṇa* in Sanskrit, which means “grammar”, etymologically has the sense of “explanation” and “analysis” (prefix *vi* + prefix *ā* + root *kr* “to do” + nominal suffix *-ana*: “to do i.e. to open up fully”). In the *Taittirīya Saṃhitā* of the Black Yajur Veda, one of the earliest Vedic texts, it is said that speech was originally unexplained or unanalysed, and the Gods spoke to Indra (the King of the Gods): “analyse this speech for us”; and Indra went into the middle of speech and analysed it (*vāg vai parācy avyākṛtā vadaḥ: te devā Indram abrūvan – imāṃ no vācaṃ vyākur'iti... tāṃ Indro madhyato'vakramya vyākarot*: TS., VI, 4, 7, 3). This analysis of “unsplit speech” was from both the levels of sounds and of significs — of the functions of the phonetic elements in expressing ideas and relations. Later commentators have called this analysis “cutting asunder” (*vi-ccid*). The study of the sounds, on which is based the arrangement of the letters in the Indian system of writing, has been, considering the age, on the highest scientific level: modern phonetics is but corroborating with the help of instruments the most remarkable discoveries of the ancient Indian phoneticians. Thus they had discovered in ancient India that the opening (*vivāra*) of the glottal passage produced breath or breathed sounds (*śvāsa* or *aghoṣa*), and its closure (*saṃvāra*) resulted in the production of voice and voiced sounds (*nāda*, *ghoṣa*); and there were “pressed down” or checked consonants i.e. unexploded stops (*nipīḍita*, *sannatara*) both finally and in the middle of the word in consonant combinations.

In the matter of morphological analysis, the discovery was first made of the three

basic categories in the analysis of the Sanskrit speech — *nāman* or the noun, i.e. the subject, *ākhyāta* or the predicate i.e. the verb, and *nipāta* or particles i.e. help-words. The concept of the *root* as a fundamental or basic significant element in speech, which has dominated our ideas of linguistic analysis for over 2500 years (in Sanskrit, and then in Greek and Latin, and in Arabic and Hebrew, and this has been extended to other languages) was also arrived at in India before 500 B.C., when the word *dhātu* came to be established in the sense of the abstract “verb-root”, as opposed to *ākhyāta* meaning “the finite verb”. The word *dhātu* is from the verb-root *dhā*, which is the equivalent in Sanskrit of the English verb *do*, and it originally meant “layer, constituent element, basic element”; and then was specialised by grammarians in the sense of “verb-root”. Linguistic analysis was well on its way from the time that in some of the older Vedic texts the poets were becoming conscious of the fact that the words of the language they were using were capable of being split up into two or more components, the basic and the accidental — *prakṛti* or root and *pratyaya* (“that which goes to the basic”) or *udaya* (“the coming up or out”) i.e. the affix.

Science is both analysis and synthesis, with the indication of a sequence of cause and effect. The linguistic analysis was in full swing. Pāṇini was fully conscious of the two levels of language — the actual living or spoken language as current in the world of men and women round about him (*laukika*) and the “high” or archaic language of old or traditional poetry (*chāndasa*). A most comprehensive description of the two he has given in his great work the *Aṣṭādhyāyī* or “Eight Books” consisting of nearly 4000 *sūtras* or aphorisms (rules or descriptions within the briefest terms), touching all points of phonemes, morphemes and word-relations.

The *sūtras* form a marvel of concentration in propounding speech analysis from all aspects, such as they were understood 2500 years ago among the Aryans in India. It was discovered that the same morpheme had different phonetic forms, and that the use of the same morphophoneme brought about different phonetic or significant effects, either in the outward acoustic appearance or in some slight nuance in meaning, or in both. Thus the various morphemes were in the first instance isolated as, e.g., *-a*, *-ta* (with a variant *-na*), *-i*, *-aka* etc. But these affixes, within the general orbit of their significance, had at times special nuances, and brought about modifications of various kinds in the form of the completed word (*pada*) as used in connected speech (*vākya*). Generally the affixes are indicated by themselves — the affixes give their own names, but occasionally an affix has a separate name for itself. Thus each consonant sound in the language has its name consisting of the sound plus the vowel *ā* (*ka*, *kha*, *ṇa*, *ca*, *pa*, *sa* etc. for *k*, *kh*, *ṇ*, *p*, *c*, *s* etc.), but in the solitary case of *r*, it has a special name, *repha*, beside its common name *ra*. With regard to the affixes, a few examples will make clear how this comprehensive analysis was made in these *sūtras*.

Take, for example, the very common Sanskrit primary affix *-ā*, which is found added to the root to form a large number of nouns and adjectives, indicating some-

times the doer and sometimes the deed. The root changes in various ways when the *-a* affix comes to modify it. Thus —

(1) In certain roots with the vowels *i u r l*, and in the roots *jñā, prī, kṛ* (really *jñ, pri, kir*) the affix *-a* is added at the end, and there is no modification of the root, and the resultant noun is agentive. Thus *vid + a = vida* “a knower”; *budh + a = budha* “a wise man”; *nṛt + a = nṛta* “a dancer”; *jñā = jñ + a = jñā* “a knower”; *prī = pri + y + a = priya* “one who pleases”; *kṛ = kir + a = kira* “scatterer”. This *-a* has been augmented by adding the sound *k-* to it — it is named as *k-a*, or *ka*, where *k-* is a symbol signifying that the verb-root remains unchanged.

(2) In certain roots, with some special words, prefixes or prepositions added to them initially, the addition of the *-a* affix to form an agentive noun is accompanied by the loss of the vowel, and if the root ends in a nasal, by the loss of both the vowel and the nasal, before the *-a* affix is added. Thus, root *gam* becomes simply *g* before the affix *-a* can be added to it: *sarva-ga, para-ga, kha-ga, dur-ga; dasyu + han = dasuy-ha; padma + jan = padma-ja*. To indicate this kind modification brought about by the affix *-a*, in this particular case the affix has been given the symbolical letter or sound *ḍ* before it, and this *-a* is called *ḍ-a*, or *ḍa*.

(3) The *-a* affix in certain cases is pronounced in the *udātta* or high pitch or acute accent, as *á*; while the root vowel is modified by *guṇa* i.e. extension of the root vowel by *-a-*: e.g. *jī, j-a-i = jai + a = jaia, jayā*. This *-á* has been given the name of *ac*, i.e. *a-c*, by adding *-c* at the end, and the *-c* signifies the advent of the high pitch (*udātta*) on the affix.

(4) In certain roots, when they are compounded with a noun, a nasal comes after the noun just before the root, and the affix has the high (*udātta*) pitch: thus *priya + root vad + á = priya-ṃ-vad-á*, “one who speaks pleasantly”. The *-a* affix here is called *kh-a-c*, the addition *kh-* meaning the presence of the nasal, and *-c* meaning pitch accent on the affix itself, as in the case of (3) above.

(5) In the above cases there is the intrusion of the nasal between the prefixed word and the root, but when the accent falls on the root vowel and not on the suffix *-a*, it is called *kh-a-ś* (or *khaś*): e.g. *jana-m-éjaya* (root *ejī*), *vātam-ája*: (root *aj*), etc.

(6) In a number of other words, the *-a* affix is added, but the expected nasal intrusion is not there according to another rule, the high pitch is on the root-vowel, and the sense of the word is “action that is being done”; then this *-a* has been named as *kh-a-l* (or *khal*): e.g. *īṣat-kar-a, duṣ-kar-a*.

(7) Root *pac* + nominal suffix *-a* gives the form *pāka*. This *-a-* is called *ghañ* or *gh-a-ñ*. Here we have lengthening of the root vowel (*-ā-* to *-ā-*), and this lengthening here is symbolised by the letter *-ñ*. Then, the palatal *-c* ending the root is changed to the guttural *-k*, and the letter or sound *gh-* indicates this function of the suffix *-a*. So *ghañ* (*gh-a-ñ*) standing for this *-a* expresses in a tabloid form how the root is modified when this *-a* is added.

In this way, to express analytically the modifications indicated by the nominal

affix *-a* and its various functions, there are various extensions of it by adding indicative sounds or letters before or after it — a sort of an algebraical compression of analytic description. These algebraical additions are known as *anubandhas* in Sanskrit (lit. “on or after bindings”); and, in the actual formation of the word, these have no place — they are omitted (in the technical language of Sanskrit grammar, “these *anubandha* letters then undergo *it* or extinction”). Vowel and Consonant sounds like *u, k, kh, ñ, c, ñ, t, d, ñ, t, n, p, y, r, l, v, ś, ṣ* etc. are among such *anubandhas*. So in an analytic description of the various functions of the nominal primary affix *-a*, according to its behaviour or action on the root and prefixed word, it is given such a diversity of names — *a, a-ñ, a-c, a-ñ, a-p, k-a, k-a-ñ, kh-a-c, kh-a-l, kh-a-ś, gh-a, gh-a-ñ, t-a, t-a-k, t-a-c, d-a, ñ-a, ś-a*, etc. In this way, the explanatory analysis of various formative affixes has been denoted by a kind of algebraic method.

In some cases, the affix has been indicated, not by itself as in the case of *-a*, but by an arbitrarily created name. Thus the affix *-aka* has been called under various circumstances *vu-n, ñ-vu-l, vu-ñ, ṣ-vu-n*. The present-participle affixes *-ant-* and *-āna-* or *-māna-* have been respectively named *śatṛ* and *śānac*; the affix *-ana-* has been variously named as *yu-c, l-yu, l-yu-ṭ*.

Verb-roots have similarly been prefixed and suffixed with *anubandha* letters to indicate descriptive analysis of the phonemic and morphemic modifications which are operative. (See Otto Böhtlingk’s *Panini’s Grammatik*, Leipzig, 1887, and works by other Western authorities like Bruno Liebich, Paul Thieme, Louis Renou; also works by Indian authors, like K. C. Chatterji’s *Technical Terms and Technique of Sanskrit Grammar*, Calcutta, 1948; K. V. Abhyankar’s *Dictionary of Sanskrit Grammar*, Baroda, 1961; and G. B. Palasule’s work *The Sanskrit Dhātupāṭhas*, Poona, 1961, Chapter III: Groupings, *Anubandhas* and other Technical Devices used in the *Dhātupāṭhas*, pp. 57-88).

As a linguistic exercise, of the nature of a *plaisanterie*, it would be interesting to analyse the morphophonemes of, say, the modern English strong verbs, indicating by means of a well-thought out and systematic series of letters like *Anubandhas* added to the basic affixes (e.g. *-ed, -t* for the past, as in the regular verb) the various changes in the root vowel of the verb and in the past-participle and other forms.

This kind of linguistic analysis is something unique, and it has held the field in India all through in orthodox or old-fashioned Sanskrit grammar, and it has been extended to Pali also. This method considered language, from the point of view of descriptive grammar, as something static, which existed *in se*, and there was not the slightest attempt in this method towards a historical approach; and of course it was too early for the comparative method, as the comparative study of languages was a thing not yet discovered. There was some complication in the above algebraical analysis of the affixes. Frequently the use of the same letter-symbol for different morphophonemic functions brought in some ambiguity and even confusion. Again, the use of different letter-symbols, often for the same purposes by Pāṇini himself, who took them over from his predecessors, also gave rise to complications

which had to be steered clear of. Finally, later grammarians often used some of their own creations in this line, side by side with a good many taken over from Pāṇini, and this gave rise to different systems of *anubandhas* or morphophonemic symbols.

Here we have a noteworthy attempt at linguistic analysis both in sounds and significant morphs at a static and descriptive level. The grammatical conscience of India became saturated with this kind of outlook or attitude: find out the root and the morphophoneme and dissect the action of the morphophoneme, and indicate the whole thing in a lump as it were. Of course, this kind of analysis was applicable primarily to Sanskrit, about which the ancient and mediaeval Indian grammarians thought that it was a language which came out as a perfect speech and was not capable of development. The close study of the earlier Vedic and the later Epic Sanskrit did not help to bring about the idea of a historic development; except that, in the case of the Middle Indo-Aryan or Prakrit dialects, it was conceded that in the bulk they were but a modification, a debasement in a way, of Sanskrit; and although the phonemes and morphemes of the Prakrit dialects were to some extent analysed, they were generally, together with the bulk of the roots and words, looked upon as derived from Sanskrit through the working of some phonetic laws which were fairly accurately observed and described. The Prakrit morphemes generally were equated in their function or application with those of Sanskrit, with the implication that they were phonological developments of the latter.

This was then the line of linguistic analysis. We have thus a rigid description of the Sanskrit language (the earlier Vedic forms were treated as variants only), in both its phones and phonemes, morphs and morphemes, as well as in its morphophonology, these being considered in themselves, without any thought of historical development; and this description analysed to the fullest the sounds and forms and their functions separate or conjoint, and presented this analysis in series of succinct algebraic formulae, as explained above. Then for the Prakrits, there was the general admission that the Prakrits represented a phonological and morphological development or modification of Sanskrit. Sanskrit was behind Prakrit: and this great fact was always looming in the background of Indian linguistic thought.

This attitude was passed on as a natural corollary or inheritance when the New or Modern Indo-Aryan Languages come to be considered. The attitude of the Indian scholars of Sanskrit towards the New Indo-Aryan languages — the *Bhāshās* — was the same as that towards the Prakrits; they were but later forms of Prakrit, and were derivative, and could be explained only with the help of Sanskrit. The *Bhāshās*, as much as their earlier forms, the *Apabhraṃśas* and the Prakrits, could, by the application of phonological laws or rules, be easily restored to Sanskrit.

In present-day Indian linguistics, particularly in the study of the New Indo-Aryan speeches, this historical level of analysis is the predominant one. The comparative aspect has now come to extend and complete it. The three great pioneers in Indo-Aryan Linguistics of the present age, who established the study of New Indo-Aryan

during the seventies and eighties of the last century, viz. John Beames, Ramakrishna Gopal Bhandarkar, and A. Rudolf Hoernle, in a way strengthened the traditional Indian line of analysis: Sanskrit, as the static *fons et origo* of Indo-Aryan, giving rise to Prakrit, which changed to the *Bhāshās*. Of course, for Sanskrit itself, the findings of comparative Indo-European Linguistics brought in a new vista, which has now been further extended by the postulating of Indo-Hittite.

The study of languages in India, particularly of the New Indo-Aryan Languages, follows the pattern that developed in Europe during the close of the 19th and beginning of the 20th century, and its basic outlook was historical and comparative. Just as the *Junggrammatiker* of Germany during the last quarter of the 19th century brought in a new outlook and approach to grammar and established the canons of historical and comparative grammar, so during the last thirty years a new approach towards grammar and grammatical analysis has come to the field; and mainly through the labours of a brilliant group of language-investigators in America, it has come to be established, and established at the forefront. One may say that the American methods of modern descriptive grammar and of structural linguistics, with their thorough rehauling of our inheritances in linguistic investigation, and their rather exuberant wealth of new terms and coinings, at times carrying with them the atmosphere of the physical laboratory and of "field-work", and their occasional crossing over into the domains of mathematics and mathematical and symbolical logic and the physical sciences, proved a little disconcerting for the votaries of the traditional school, who had so far found the historical-comparative method with adequate broadbasing on phonetics, even experimental phonetics, enough. The approach to the problems of language was at times on abstract lines — without being facetious it may be opined that it was like *abstract art* — and this seemed to encroach upon a good deal of the terrain. At the Conference of Linguists held in Paris in 1938, under the auspices of the CIPL, as far as I remember, it was Dr. Doke, the Africanist and authority on Bantu from the School of Oriental and African Studies in London, who gave his opinion that the modern levels of linguistic approach often took us high up into a sort of "a linguistic stratosphere, where we felt very much the need of a little oxygen", and that linguistic speculation and classification should always move with its feet firmly set on the soil.

New methods naturally would require new words. But in this matter, too, in the interest of international convenience in the study of a science through the English language, there should be international cooperation and agreement, among the two main groups of English-speakers in the United States and the United Kingdom — America and England, and the wider groups of English-users outside. This view, along with a number of other ones quite pertinent to the question, was expressed by Professor Einar Hagen in a paper contributed to the Journal of the Linguistic Society of America, *Language*, in 1951, where he deplored the widening gulf in linguistic approach and in linguistic terminology between America and Europe.

Just as old fashioned "philologists" at the turn of the 20th century and for a

couple of decades after that felt a little bewildered at the methods of modern phonetics, both theoretical and descriptive as well as experimental, so some of us in India, senior workers and students on the wrong side of 70 and even of 60, feel "the want of a little oxygen" in the methods which are being developed in America and are being quickly disseminated outside America, including India. We are not at all critical of these new methods, simply because we have not been able to follow them through our own inability in keeping abreast of the times. We can see that the level of linguistic analysis, particularly for structural linguistics, is on a different, perhaps more objectively constructed plane. In America itself, we can see how the approach has changed within thirty years, from a work by L. Bloomfield on *Language* (1924), and the current works of linguisticians like Charles F. Hockett, Zellig S. Harris, H. A. Gleason, Kenneth L. Pike, Henry Hoeningwald, Uriel Weinreich and others, some of whom have had the advantage of a training in the "classical" tradition and classical languages also. In India during the last eight years, the new approach and methodology has been slowly introduced, through the various summer and winter linguistic schools held in different universities to which senior linguistic scholars and researchers from all over India repaired for intensive training in language-study and linguistic analysis. These schools were run under the joint auspices of the Linguistic Society of India (Calcutta and Poona), the Deccan College of the University of Poona, and the Rockefeller Foundation of New York (which largely financed it). Professors and lecturers as well as research scholars from American universities and occasionally from British universities came under this scheme, and they took classes and introduced this new method; and young as well as senior linguistic scholars from India also went to study in some of the American universities. In this way, a new generation of Indian linguistic investigators from different parts of India, who are working in both the Aryan and the Dravidian and other non-Aryan languages, have come to the field, and are employing the methods of modern descriptive and structural linguistics in the study of Indian languages.

The current, or rather the old Indian level of analysis professedly relies on the historico-comparative approach, with full consideration of the actual phonetic level. In the Indian tradition, this method has so far given good results, and in what may be called the orthodox method, following Beames, Bhandarkar, Trumpp, Platts, Hoernle, Grierson, Kellogg, Jules Bloch, Grahame Bailey and others, historical grammars of Bengali, Awadhi, Konkani, Assamese, Maithil, Bhojpuri, Panjabi and descriptive grammars of current Bengali, Awadhi (Lakhimpuri), and a number of other dialects have been written, and these have broken new ground in India.

The old or Euro-Indian method and the new or American method are now face to face with each other, and they present two levels of linguistic analysis. I think the American method of looking most closely into the facts of the language under study — in fact, often confining itself into the actualities without much concern for earlier stages, is the result of the linguistic scholarship of America having largely taken in hand the investigation of the Amerindian languages which have preserved

no past records or specimens. The historical method could not be followed in this domain, as there were no materials, except for the speech as it was in front of us. Hence it was more satisfying intellectually and perhaps also more profitable to take up the language as it was objectively — to get at its *tat-tva* or “that-ness”, to use a Sanskrit expression — the exact nature of the speech — the understanding of the *Ding-an-sich*, the thing-in-itself. Perhaps this circumstance supplied one of the most important bases of the American linguistic approach and linguistic science. Perhaps this sometimes manifested itself into an ignoring of, or impatience with the considerations or questions or implications or anxieties with regard to an earlier background, presenting the dynamic aspect of speech.

But in India, there has been some sort or other of a complete linguistic record for some 3000 years continuously. The anomalies or irregularities of the phonemic and morphemic situation in a New Indo-Aryan language are almost always capable of being explained in their historical or sequential setting or development by the earlier records registered in literature or inscriptions or in dialectology. Hence the tendency and the temptation would be to base investigation into a modern Indo-Aryan speech on the pattern or ground-plan of the earlier stages of it: and it would appear so obvious on the face of it. Besides there is ample material for the comparative approach, and for the question of linguistic substrata. The Indian mind, again, is always eager to go into the fundamentals — the base or foundation of a matter to its deepest depth attainable. Hence the acceptance of Primitive Indo-European (and then Indo-Hittite) has been so easy and natural for speech-researchers in India following the modern or European method.

In treating of the basic character of a modern language as it now stands, or for the matter of that of any language at a given stage of its existence, we may of course confine ourselves to its actual facts and to establish a sort of pattern which can be fully correct and true for it. This is what we see in the detailed descriptive grammar of Sanskrit, at a particular point in its history, such as was done by Pāṇini, following an all-inclusive phonemic-morphemic consideration, as indicated above. The modern English verb-system with all its irregularities and its anomalies is the result of a gradual historical development of what we see in Old English, and the Old English system is based on that of Primitive Germanic, of Primitive Indo-European and of Indo-Hittite successively. But still it is possible to take modern English by itself, and draw out a new and a consistent pattern from it for its verb-system, without reference to Old English or Germanic or Indo-European, in the manner done by H. A. Gleason (see, e.g., pp. 102-103 of Gleason's *Introduction to Descriptive Linguistics*, New York, 1955). I have treated the historical development of the verb-system from Old through Middle to New Indo-Aryan, as in Modern Bengali, where each anomaly or irregularity is sought to be explained with reference to what was behind it in the earlier phases of Middle and Old Bengali and of “Prakrit” and “Sanskrit” (in my *Origin and Development of the Bengali Language*, Calcutta, 1926). But in my *Modern Bengali Grammar* written in the Bengali Language for Bengali-speaking

students at high school and college (1945ff.), I have arranged the whole thing in a pattern which holds good for Modern Bengali only.

I think that in linguistic analysis, the proper level should be both descriptive and structural, and historical and comparative. These two levels of approach cannot be cut off from other — they can easily go with each other. For the fundamental thing about language is that it is never static — it is always in a state of flux, it is a continuous historical process. Language is never detached from life, and it will ultimately refuse to be dissected and analysed with modern “tools” upon a laboratory table only. It can be questioned if we could think of language in the abstract when dealing a living speech. Its phonemes and morphemes are redolent with life, and throbbing with heart-beat. This should never be lost sight of in descriptive linguistics, if only as a background, and occasionally a source of light in illumining dark spots in modern languages most easily. A classical training is always a great stand-by in any kind of intellectual work. Then those who confine themselves to the historico-comparative method, particularly in India, should recall the necessity of studying the “thing-in-itself” before we can find out its place in the scheme of things. And the perennial lesson of Pāṇini is there. His grammatical approach was nothing if not rigorously descriptive — his work was the consummation of a great labour in finding out the structure of Sanskrit as it was current in his time, as a living speech, with its literary form in the offing as something which was also a part of it. To revive the spirit of Pāṇini in his level of linguistic analysis as concerned with a living speech independently of its past will once again be doing a great service to the science of linguistics, particularly in India, as a balance to the long period of known historical development for 3000 years.

THE DESCRIPTION OF A POEM

A. WILLEM DE GROOT

In his book on *English Metrists*, which appeared about forty years ago, T.S. Omond raises the question whether we shall ever have a *generally accepted theory of prosody*, and, if so, whether it will be more or less complex than its many predecessors. I may be allowed to quote his own words:

“What then, is the upshot of the whole matter? This, for certain, that as yet we have no established system of prosody. Much analytic enquiry has yielded no synthesis authoritative and generally accepted. It is a strange fact, so late in the history of our literature; Greek metrists would have viewed it with surprise. That the synthesis will come is surely past question. When it does come, I suspect it will be found less and no more complex than its many predecessors.”¹

Since then, forty years have elapsed, but we are faced with exactly the same situation, and the present paper is concerned with essentially the same problem, which, nowadays, we may prefer to formulate as follows: how to describe poems on the basis of an acceptable theory of structural poetics. At the end of my paper I shall come back to the questions raised by Omond: whether a generally accepted theory will ever be forthcoming, and, if so, whether it will be more or less complex than previous ones.

Before enlarging upon our subject, we think it useful to make a few preliminary remarks on a problem which has given rise to some discussion: the place of descriptive poetics and, more generally, aesthetic stylistics, within or outside the limits of what has been called rather inadequately “linguistics proper”.

I submit that the problem is largely, or entirely, a matter of definition. Stylistics may be defined as the theory of the use of a language for aesthetic purposes, including the study of aesthetic language products. Poetics is the study of aesthetic language products of a special kind, called poems, or verse. Once we agree upon the definition of linguistics as the theory of *la langue* and *la parole*, i.e. of languages and the use of languages – or, from a different point of view, the theory of conventional patterns of vocal human behavior, and of this kind of behavior itself – aesthetic stylistics automatically falls under the head of *la parole*, and poetics under the head of stylistics. If, however, we prefer some other definition of the terms linguistics, stylistics, and poetics, the answer to the question may be different. The definition of a term is, of course, arbitrary, and

¹ Omond, T. S., *English Metrists* (Oxford, 1921), 266-267.

a matter of convenience. I may add that I shall use the expression “descriptive poetics” as the opposite of “explanatory poetics”. As usual, however, in matters of this kind, no sharp line of demarcation can be drawn between analysis and description of the phenomena in question on the one hand, and their explanation on the other.

We may add that verse, or poems, are subject-matter of more than one branch of science: linguistics, the theory of literature, psychology – especially gestalt psychology –, and aesthetics. There is no reason to assign different parts of poetics to different sciences: the difference is not in the subject-matter, but in the angle of approach resulting from aim and object of each specific branch of science separately. We are obviously dealing with the common phenomenon of overlapping of various sciences with regard to their subject-matter.

DEFINITION AND STRUCTURE OF A POEM

A *poem* is defined as a text consisting of continuously corresponding units (called “lines”) which are sequences of words, and at the same time variations of a more or less constant auditory theme.

It is one of the merits of Roman Jakobson to have made the statement – about thirty years ago – that the auditory features of a work of literary art as such are exclusively features of the language used by the poet. The statement was intended to eliminate from the definition of a work of literary art as such any external factors of production such as different ways in which the same poem may be recited. The same holds, of course, for calligraphy, and different ways of printing the same text. A given work of art, may, however, be a combination of a work of literary art and some other kind of art at the same time, as, for instance, a song. Roman Jakobson is also of opinion that the auditory features of a poem are only “phonological” ones, *i.e.* independently distinctive (not redundant or concomitant) features of words and sentences. This part of the statement may be doubted, but, anyhow, from the definition of redundant and concomitant features it follows that the auditory linguistic form of a given poem can be described completely in terms of distinctive features.²

Descriptive poetics, as different from descriptive linguistics, is *definitione* concerned with aesthetically relevant features of language products only. In describing a given poem its first task is to distinguish between the aesthetically relevant and the aesthetically non-relevant features. One of the methods applied is to establish the stylized features, especially the linguistic features common to all, or the majority, of the lines of the poem. Actually, the function of a poem, *i.e.* what makes it a work of art, is to create, in the mind of the hearer or reader, a gestalt, comparable to a melody in music. The gestalt of a poem is an organized whole or configuration of aesthetic experiences. Just as a melody is distinct from the separate tones, the gestalt of a poem is different from the sum total of its linguistic features. Each of the aesthetically relevant features

² Jakobson, R., “Über den Versbau der serbokroatischen Volksepen”, *Archives Néerland. de Phonétique Expérimentale*, 8-9 (1933), 135 f.

of the text serves to create a feature of the gestalt of the poem. In this sense the relevant linguistic features may be said to function or operate as *gestalt factors*. According to their function in the structure of the poem, we may distinguish factors of *a* segmentation, *b* dominance, *c* alternation, *d* correspondence, and *e* harmony of each separate line with the constant “undercurrent” theme. This classification claims to be both *universal* and *fundamental*. In principle, it applies to all types of verse in any language. It is intended to represent the general and indispensable frame of descriptive poetics on the formal level, in the sense in which the distinction of words and sentences, or of phonemes, morphemes, words, constructions and sentences, or a similar one, is universal and fundamental in descriptive linguistics. In other words, it is meant to constitute the synthesis which Omond had in mind, on the formal level. Before discussing each of these categories of factors separately, we may make a few preliminary remarks.

First, no sharp line of demarcation can ever be drawn between aesthetically relevant and non-relevant features. All linguistic features of words and sentences play a role, somehow or other, in creating features of the gestalt of the whole poem. There is, however, always – in the poetry in a given language at a given period as well as in a given poem – a *scale of aesthetic relevance* according to which the features should be classified. For practical purposes, however, any description of verse in a given language, or of a given poem, has to draw a more or less arbitrary line between more and less relevant features, and confine itself to the former. For various reasons, however, it would be a fallacy to believe that the most relevant features are those which are most regularly and conspicuously stylized throughout the poem, more especially the features common to all, or the majority, of the lines of the poem. I shall come back to the question when dealing with the factors of correspondence, and with the harmony between an individual line and its theme.

Second, without affecting the fundamental character of the functional distinctions made above, there is, normally, considerable overlapping in the sense of one linguistic feature, or bundle of features, having more than one function. To give just one example: in modern Indo-European languages, rhyme is a functional bundle of linguistic features: similarity of accented vowels at the end of two or more lines. The bundle, however, is at the same time a factor of correspondence of two or more lines, of dominance in each separate line, and of segmentation of successive lines.

Third, the scale of relevance concerning a given structural function may be rather complicated. It is obvious, for instance, that the limit between successive words ranks highest as a factor of segmentation. Different kinds of word limits, however, may be of different rank: 1. limits between sentences, 2. limits between constructions, 3. mere word limits, and even different kinds of limits between constructions may not be equally relevant. The lowest rank is represented by the limit between syllables belonging to the same word, *i. e.* in cases of enjambment, which will be discussed further on.

It may be hardly necessary to draw attention to the fact that descriptive poetics needs a special set of tools, *i. e.* an inventory of concepts and methods of investigation of its own, which are not, or not commonly, used in the description of languages and of non-

aesthetic language products. Some of them may be borrowed from other sciences, such as gestalt and dominance from modern psychology, and theme and variations of a theme from musicology. Thus far, all or most of them have been less precisely described and defined, and are less fully developed in their application to linguistic materials than the concepts and methods of investigation with which most linguists are familiar. An additional difficulty lies in the fact that several of these concepts concerning aesthetic experience and perception, can easily be illustrated, *i. e.* defined by means of "ostensive definitions", but far less easily defined without the use of terms and concepts which themselves are difficult to define.

Finally, it should be kept in mind that the functional distinctions made above comprise the formal auditory functions of linguistic features, not their expressive functions dependent upon concurrent features of the content of the poem. I shall make no attempt at classifying the latter, but I shall make a few remarks upon them when dealing with harmony between a theme and its variations.

THE LINGUISTIC FEATURES AS FACTORS OF THE GESTALT

a. *Factors of segmentation.* According to their rank, we may distinguish primary and secondary factors. Word limits are primary factors *par excellence*, and it has already been remarked that they are of different rank. Enjambment is a very interesting illustration of the fact that a gestalt is a whole which is, in a sense, prior to its parts. A poem consists of lines, but the lines do not make the poem. On the contrary, the poem makes the lines. Secondary factors of segmentation are of various kinds. We only mention regular endings of lines, as, *e. g.*, an accented syllable preceded by an unaccented one at the end of lines in blank verse, and the clausula heroica of the Greek and Latin hexameter. We have already mentioned the bundle of factors called "rhyme" as a factor of segmentation.³

b. *Factors of dominance.* Whereas, apparently, all languages have the same primary factor of segmentation, *viz.* word limits, the primary factors of dominance of a line vary considerably from one language to another. Moreover, there is much difference with regard to the place of the dominant in the line. There is no doubt that both the place of the dominant, and the factors of dominance in poetry in a given language are largely dependent upon the identificational features of words and sentences in the language in question. The study of this dependence (of the relations between language structure and verse structure) belongs to the domain of *explanatory poetics*. A dominant of a unit is defined as a center of attention which is one of the main features of the unit as an organized perceptual whole. In verse, the most important *dominants* are those of the separate lines, and those of the periods of alternation throughout the line. The dominant being the culminating center of an organized whole of perceptual experience, it tends to be the place of maximum aesthetic relevance, and of maximum stylization of line

³ Stutterheim, C. F. P., "Poetry and Prose, their interrelation and transitional forms", *Poetics*, (The Hague-Warsaw, 1961).

and period of alternation. In modern English, German, Dutch, Italian, Spanish, Russian blank verse, the dominant of a line is, normally, the last syllable of the line which has word accent and construction accent, and which, very often, is at the same time the second pitch point of a pitch contour of assertion: *A thing of beauty is a joy for ever*. Consequently, its place is at the end of the line. Its primary phonological factor is loudness, whereas length is a secondary (often redundant or concomitant) factor. In rhyming verse in the same languages the dominant of the line is the rhyming syllable, and combines the same phonological features with the fact that it is a center of attention in that it corresponds with a similar rhyming syllable in the same poem. We may suspect that in Greek and Latin metrical poetry the dominant is the clausula, *i. e.* the highly stylized end of a line. We may likewise suppose that in Old Germanic alliterative verse the dominant of the line is not at the end of a line, but one of the alliterating syllables. Place and factors of the dominant are different again in Japanese blank verse, and in other languages.

c. *Factors of alternation.* In poetics, *alternation* is defined as continuous succession of corresponding periods of syllables throughout the line, each of the periods containing one, and no more than one, syllable which is the dominant of the period. The periods as such are not segments regularly separated by word limits. A line such as *Viele freilich müssen drunten sterben*, is exceptional in the sense that it combines alternation of syllables with segmentation on the level of successive words. On the level of alternation, word limits have no aesthetic relevance. Classical prosodic theory has not clearly developed the concept of a period of syllabic alternation which is not, as such, a segment, and most of modern verse description is still along the same lines. We may distinguish verse with alternation and verse without alternation. The latter is exemplified by modern French poetry, which has no regularly stylized periods of syllables of the kind. The most important classification of types of verse in modern English and other modern Indo-European languages is based upon the distinction of *rhythmic* and *non-rhythmic alternation*. In rhythmic alternation the dominants of the periods are at the same time peaks of rhythm. *Rhythm* is defined as a periodicity which goes with the well-known "rhythm experience" (Rhythmuserlebnis), which is different from mere periodicity. It requires intervals of about 3/4 of a second between successive peaks of periods.⁴ Accordingly, in rhythmic verse the number of syllables between peaks is either regularly two (as, *e. g.*, in so-called anapaestic and dactylic verse), or (as in much so-called "free verse") it varies on a scale which reveals a strong preference for two syllables, usually 2-1-3-0-4-5 etc. Obviously, the degrees of frequency are paralleled by similar degrees of aesthetic relevance. Roughly speaking, we may say that in rhythmic alternation rhythm is stylized in a work of literary art, whereas in non-rhythmic verse it is a matter of reading or reciting the lines. The study of the relations between language structure and verse structure belongs to explanatory poetics. The latter is dependent upon the former in two ways: negatively, and positively, also in the matter of alternation. No phonic

⁴ De Groot, A. W., "Der Rhythmus", *Neophilologus*, 19 (1932), 81-100, 177-179, 341-365.

feature may be used to mark an arsis if it is not an identificational feature of words or sentences in the language used by the poet. Speaking positively, the arsis may be a long syllable, as, *e. g.* in classical Greek and Latin poetry, and it may be a loud syllable, as, *e. g.*, in modern English. It may be doubted if alternation is ever primarily a matter of pitch. The fact that Greek, which has both a free musical word accent, and an opposition of long and short vowels, prefers quantity to pitch, is revealing and instructive. A secondary factor may be the use of "full vowels" in arsi, and the frequent use of "neutral vowels" in thesi, as, *e. g.*, in modern German poetry. The absence of alternation in modern French is obviously due to the fact that the opposition of long and short vowels plays only a secondary, if any, role in its vowel system, that French has no word accent, and that its construction accent is fixed, non-distinctive. Perhaps in all languages, similarity of vowels may be used as a factor of correspondence of successive peaks, but only occasionally: *Der tiefe dienende Levite* (Rilke).

d. *Factors of correspondence of lines.* Continuous correspondence of successive segments, called "lines", is the only constant feature which distinguishes verse from prose. *Correspondence* is defined as harmony by similarity of directly or indirectly successive units. There is no need to sum up the possible factors of correspondence, because any feature of form, and of content, may be used for this purpose. In a given poem, like, *e. g.* the following, the factors of correspondence may be very elusive:

Icicles filled the long window
With barbaric glass
The shadow of the blackbird
Crossed it, to and fro.
The mood
Traced in the shadow
An undecipherable cause.

or:

I know noble accents
And lucid, inescapable rhythms,
But I know, too,
That the blackbird is involved
In what I know.

(Wallace Stevens, *Thirteen Ways of Looking at a Blackbird*)

The example is intended to illustrate that it is a fallacy to assume that there is always a bundle of features, or even one single feature, common to all the lines of a poem. Apart from some rather vague similarities of alternation, difficult, if not impossible, to represent by means of numbers and statistics, the main factor of correspondence may be the fact that each separate line is a variation of a constant, though flexible, theme. The theme itself is equally elusive.

e. *Harmony between theme and lines.* A *theme* is defined as the gestalt of which the gestalt of each separate line is a variation. *Variations* are defined as transformations of a theme. As a gestalt is an organized configuration of experiences, it follows that theme and variations can be investigated directly only by experiencing them, and observing them by means of introspection. The results of introspection may be verified in various ways, *e. g.*, by means of statistics of frequency of gestalt factors, especially by establishing bundles of features which are common to all, or most of the lines of a given poem. If the gestalten cannot be experienced, *e. g.* if the investigator is not thoroughly familiar with the language in question, only indirect methods of research can be applied. A

theme is constant from the beginning to the end of the poem, with two noticeable restrictions. First, it may have *variants*, as, e. g., Virgil's hexameter has two common ways of segmentation of the line, a bimembral, and a trimembral: *Arma virumque cano – Trojae qui primus ab oris*, and *Infandum – regina, iubes – renovare dolorem*. Second, the theme may change more or less gradually, e. g. if the lines of a poem, or a stanza, become gradually longer or shorter.⁵ Variations are "transformations" in the sense that they are experienced in their relations to a basic theme, i. e. as developments or elaborations of the theme. Similar phenomena are wellknown from musicology. The existence of themes and variations is proved, among other things, by different reactions to more and less "regular" lines. Variations may be classified in various ways, according to different criteria; we mention only the following. 1. The level of aesthetic structure: segmentation, dominance, alternation, etc. 2. The linguistic features as gestalt factors on each of these levels, as illustrated, e. g. on the level of correspondence, by differences between isosyllabic, quantitative, and accentual verse. 3. Harmony and conflict with the theme. 4. Purely formal aesthetic function, and expressive function. It may be remarked that harmony and conflict with the theme are not identical with formal "regularity" and "irregularity"; an "irregular" line may be in perfect harmony with the theme, whereas a highly regular line may have a minimum of harmony as a result of its "monotony". It is very strange indeed that deviations from the theme in separate lines (called "irregularities of the line") have been looked upon as deficiencies of the poem by eminent scholars, such as Jespersen and Heussler.⁶ On the contrary, they are indispensable, and have both a formal and expressive function. Harmony is not only a matter of similarity, but also of dissimilarity, and, in good poetry, irregularities of lines are among the most important features of the poem both by their formal and their expressive functions. Actually, the beauty of a poem is less dependent upon the regularities than upon the irregularities of the poem.⁷

CONCLUSIONS

Coming back to Omond's questions, I think we may safely assume that an acceptable synthesis in descriptive poetics will certainly be forthcoming, but that it will be more and not less complex than its predecessors.⁸

Laren, N.H., *The Netherlands*

⁵ Braakhuis, A. P., *De thematische structuur van de versregel (with a summary in English)* (The Hague, 1962).

⁶ Heussler, A., *Deutsche Versgeschichte*, I (1925), 1-85, and "Wege und Irrwege in der neueren Verslehre", *250 Jahre Weidmannsche Buchhandlung* (1930), 38-55. – Jespersen, O., "Notes on Metre", *Linguistica*, 1933, 249-274; or in *Journal de Psychologie*, 30 (1933), 333-338.

⁷ De Groot, A. W., *Algemene Versleer* (The Hague, 1946). Cf. Review with summary in English, by C. F. P. Stutterheim, *Lingua*, I (1947), 104-117.

⁸ De Groot, A. W., "Phonetics in its Relations to Aesthetics", *Manual of Phonetics*, ed. by L. Kaiser (Amsterdam, 1957), 385-400; also *An Introduction to Structural Linguistics* (forthcoming), chapter on Poetics.

DISCUSSION

HAMMERICH:

An important part of the aesthetic attitude towards a poem seems to be the *expectation* of differences of auditive elements with a minimum difference of more versus less. This may be one of the main reasons why we do not have poems based on pitch (except under special conditions). With regard to length and stress we are content with differences based on more versus less, but in pitch, in the field of tones, we observe larger and richer differences. Therefore here the natural expression will generally not be verse, but music.

THE LINGUISTIC STUDY OF LITERARY TEXTS

MICHAEL A. K. HALLIDAY

The starting point could be Jakobson's observation: "Insistence on keeping poetics apart from linguistics is warranted only when the field of linguistics appears to be illicitly restricted."¹ It is part of the task of linguistics to describe texts; and all texts, including those, prose and verse, which fall within any definition of "literature", are accessible to analysis by the existing methods of linguistics. In talking of "the linguistic study" of literary texts we mean, of course, not "the study of the language" but "the study (of the language) by the theories and methods of linguistics". There is a crucial difference between the *ad hoc*, personal and arbitrarily selective statements offered, frequently in support of a preformulated literary thesis, as "textual" or "linguistic" statements about literature, and an analysis founded on general linguistic theory and descriptive linguistics. It is the latter that may reasonably be called "linguistic stylistics".

It is a prerequisite of such a study that both the theory and the description should be those used in the analysis of the language as a whole. Linguistic stylistics must be an application, not an extension, of linguistics; this is the only way to ensure the theoretical validity of the statements made. The justification for using linguistic methods in literary analysis is that existing grammatical, lexical, phonological and phonetic theory is already valid and relevant for the purpose. At the same time the descriptive statements made about a literary text are meaningful only in relation to the total "pure" description of the language concerned: if the linguist hopes to contribute to the analysis of English literature he must first have made a comprehensive description of the English of the period at all levels. (It can presumably be taken for granted, in 1962, that the categories of such a description will be formally defined, and that the description will not be restricted to below the rank of the sentence.) If for example all clauses of a particular poem are shown to have the same structure, it is essential to know whether or not this is the only permitted clause structure in the language; and if not, what its relative frequency is in a large sample representative of "the language in general".

Moreover, a text is meaningful not only in virtue of what it is but also in virtue of what it might have been. The most relevant exponent of the "might have been" of

¹ See Thomas A. Sebeok (ed.): *Style in Language* (New York, Technology Press of M.I.T. and Wiley, 1960), p. 352.

a work of literature is another work of literature. Linguistic stylistics is thus essentially a comparative study. To pursue the example above, we also need to know the relative frequency of this clause structure in other works of the same period and the same genre. The more texts are studied, the more anything said about any one text becomes interesting and relevant.

We can therefore define linguistic stylistics as the description of literary texts, by methods derived from general linguistic theory, using the categories of the description of the language as a whole; and the comparison of each text with others, by the same and by different authors, in the same and in different genres.

While insisting that stylistic studies use the same methods and categories as non-literary descriptions, we must make the proviso that such studies may require new alignments or groupings of descriptive categories, through which the special properties of a text may be recognized. This may include the bringing together of categories and items described at different levels as well as those scattered throughout the description of any one level. An example of such a grouping, in which various grammatical and lexical features are brought together, is "cohesion".²

The principal categories subsumed under cohesion are:

- A. Grammatical
 - 1. Structural (clauses in sentence structure)
 - (a) Dependence
 - (b) Linking
 - 2. Non-structural
 - (a) Anaphora
 - (i) deictics and submodifiers
 - (ii) pronouns
 - (b) Substitution
 - (i) verbal
 - (ii) nominal
- B. Lexical
 - 1. Repetition of item
 - 2. Occurrence of item from same lexical set

The grammatical categories are drawn from a comprehensive description of the grammar of Modern English. A brief account of the underlying theory is given in my paper "Categories of the theory of grammar".³ The description itself, however, has not yet been published, and therefore some commentary on these categories is needed here.

Cohesion is of course a syntagmatic relation and, insofar as it is grammatical, it is partly accounted for by structure. Structure is the ordered arrangement of one or more items of the same rank to form an item of the rank above: in English, the ways

² Another example is "involvement"; see Angus McIntosh, "'As You Like It': a grammatical clue to character", *Review of English Literature*, 4, April, 1963.

³ *Word*, 17, December 1961.

in which a sentence can be made up of clauses, a clause of groups, a group of words and a word of morphemes. All structure is thus in the broadest sense cohesive. But with the smaller units there is little consistent variation between texts. A more delicate treatment of cohesion would certainly include at least some relations in clause or group structure, for example apposition and "rank shift"; but in the first instance structural cohesion can be limited to the relations between clauses in sentence structure. These take various forms, of which the most significant for literary texts are "dependence" and "linking". Very roughly, these can be glossed in traditional terms as "subordination" and "co-ordination", the former including "non-defining" but not "defining" "relative" clauses.⁴

Structure, however, is not the only cohesive factor operating at the level of grammar. There are certain grammatical categories whose exponents cohere with other items in the text, items to which they do not stand in a fixed structural relation or indeed necessarily in any structural relation at all. Principal among these are the anaphoric items in the nominal and adverbial group: deictics, submodifiers and adverbs, of which the most frequent are "the", "this", "that", the personal possessives, "such", "so", "there" and "then"; and the (personal) pronouns. These items are regarded as cohesive only in their anaphoric use; this accounts for the majority of occurrences of all except "the", which is most frequently cataphoric. Deictics and pronouns used cataphorically, pointing forward to a modifier or qualifier as in "the tall man", "the man who came to dinner", "he who hesitates", "it is useful to ask", are not cohesive; nor is the homophoric "the" in "the moon". Secondary in importance to anaphora, because much less frequent in written English, is substitution: the use of "do" as lexical item in the verbal group and "one" as head of the nominal group, as in "he might have done" and "a long one".

Lexical cohesion in its clearest form is carried by two or more occurrences, in close proximity, of the same lexical item, or of items paradigmatically related in the sense that they may belong to the same lexical set. For example, in a passage by Leslie Stephen one paragraph ends "I took leave, and turned to the ascent of the peak"; the next paragraph begins "The climb is perfectly easy". Thus in the new paragraph the first lexical item, "climb", coheres with "ascent"; later occur "mountain" and "summit" cohering with "peak". The lexical set is identified by privilege of occurrence in collocation, just as the grammatical system is identified by privilege of occurrence in structure; the set is a grouping of items with similar tendencies of collocation. The occurrence of a high frequency collocation, like "ascent . . . peak", is not here regarded as being itself a cohesive feature, since there seems no reason for assuming that such a collocation is any more cohesive than one of low frequency: too many variables are

⁴ A more correct theoretical statement of structural cohesion is that it is presupposition at the rank of the sentence. Presupposition is the special relation between elements of a non-chain-exhausting structure that have as their exponents terms in a non-choice-exhausting system. Thus in "I'll come if you want me" the structural relation of "conditioning" clause and "conditioned" clause, which is a type of dependence, is one of presupposition.

involved, such as the lexical power of the items and their grammatical relations. But in any case a valid assessment of lexical cohesion depends on the study of collocations in very large samples of text, this being necessary to the recognition of lexical sets; work of this kind on English texts is only just beginning.

The features outlined in the last three paragraphs may be regarded as the main types of cohesion in modern written English.⁵ Frequently, of course, cohesion is lexico-grammatical, as in "the climb", quoted above, with anaphoric deictic. It includes also other features, not listed here but required by a more delicate analysis: for example, lexical variation within a constant grammatical frame, and vice versa.

But it must not be thought that all statements in linguistic stylistics require special alignments of categories. On the contrary, a straightforward linguistic description of a literary text, in which the text is treated in exactly the same way as any other text that is being subjected to linguistic analysis, reveals a great deal both about that text in particular and about literary language in general. To quote Jakobson again⁶: "The set (*Einstellung*) toward the MESSAGE as such, focus on the message for its own sake, is the POETIC function of language." It is this "set toward the message" that determines the particular type of linguistic patterning that is characteristic of literature.

If we keep the word "pattern" as a general, non-technical name for all the organization, at all levels, that is a crucial property of language as such, then the special property of literary language is the patterning of the variability of these patterns. In other words, the creative writer finds and exploits the *irregularity* that the patterns allow, and in doing so superimposes a further *regularity*. It is this "regularity", as we may reasonably call it provided we avoid giving the term an arithmetical interpretation, that marks the "focus on the message". This is clearly displayed by any good linguistic description of a text — provided there exists already a good description, textual or otherwise, of that language.

It is difficult to cite examples briefly, since only an account of a number of interacting features in a text is really illuminating, quite apart from the need to contrast these with other texts. Two points must suffice: the "cline of verbality", and the structure of the nominal groups, in W.B. Yeats' "Leda and the Swan".

There are 15 verbal groups in this poem, and a further 4 verb words outside verbal groups, operating directly in the structure of nominal groups. Table I shows the items involved,⁷ classified according to value in structure. Appended to it, for purposes of comparison, is a similar table for fifteen lines of Tennyson's "Morte d'Arthur", lines beginning "Then quickly rose Sir Bedivere, and ran . . .". The table goes from

⁵ I omit here phonological cohesion: that with grammatical categories expounded directly by phonology, for example (British English) tone 4 in anaphoric use.

⁶ *Op. cit.*, p. 356.

⁷ The table shows the *lexical item*, which is not necessarily coextensive, either paradigmatically or syntagmatically, with either (class) verb (of unit) word or (class) verbal (of unit) group. Thus in "did she put on", "being caught up"; the *verbal groups* are "did put on", "being caught up"; the *verb words* are "put", "caught", the *lexical items* are "put on", "catch up".

TABLE I

	Items in verbal group (i.e. operating at "predicator" in clause structure)					Items in nominal group (i.e. not operating at P.)
Clause class (system: status)	Independent	Dependent		Rankshifted		} (irrelevant)
Group class (system: finiteness)	Finite	Finite	Nonfinite	Finite	Nonfinite	
"Leda & the Swan"	hold push feel engender put on 5	lie let 2	drop catch up master 3	—	beat (2) caress catch lay 5	stagger loosen burn break 4
"Morte d'Arthur"	rose (2) ran plung'd clutch'd wheel'd threw made shot flash'd fell caught brandish'd drew went 15	shock dipt 2	leaping flashing whirl'd 3	—	seen cloth'd 2	—

"most verbal" on the left to "least verbal" on the right. "Leda and the Swan" shows a high proportion of verb words in the less verbal structural positions. Moreover, the more lexically powerful (collocationally restricted) the verb item, the less verbal its use: compare "hold", "push", "feel", "put on", "lie" with "stagger", "loosen", "caress". In both these features this poem stands in sharp contrast to the passage of Tennyson.

There are 25 nominal groups in the poem, of which 17 contain modifying (pre-head non-deictic) or qualifying (post-head) items, or both: for example "the staggering girl", "a shudder in the loins", "the brute blood of the air". In English, when a *potentially* cataphoric deictic occurs in such nominal groups it normally *is* cataphoric: qualifiers and lexical modifiers are shown in this way to be "defining". 15 out of these 17 nominal groups have in fact a potentially cataphoric deictic: "her" (3), "that/those" (2) and "the" (10). Yet only in one instance, "the brute blood of the air", is the normal structure followed: in all other instances the modifying/qualifying items

are "non-defining" and the deictic is not cataphoric. What then is the deictic? The personal possessives, "her loosening thighs" and so on, are clearly anaphoric to the title of the poem, as in those cases where there is no modifying/qualifying item, like "her nape". The "the" and "that" are also best regarded as anaphoric, but in the non-textual sense; the identification is situational.

The greater part of "Leda and the Swan" is made up of nominal groups: these nominal groups contain 46 out of the 56 lexical items in the poem. Of the mass of lexical material in the modifiers and qualifiers, almost none is defining; this in spite of a high frequency of those very deictics, especially "the", whose normal function is precisely to identify such material as defining. We do not need linguistics to point out that Yeats' treatment of an event here is very different from Tennyson's. But we do need linguistics if we want to state accurately the differences between the two texts.

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DISCUSSION

HAHN:

It seems to me that the essentially poetic quality in "Leda and the Swan" lies not in any preponderance of verbal groups over nominal groups, but in two particular phrases, *feathered glory* and *white rush*, in both of which we have a transposition of what would have been much more commonplace and prosaic forms of expression, *glorious feathers* and *rushing whiteness*. The quality-noun *glory* and the verbal noun *rush* alike are such an essential part of the swan that they are used as standing for him, just as Swinburne in "Atalanta in Corydon" says "*over the splendor and speed of thy feet*" instead of *over thy splendid and speedy feet*, or as Poe in "To Helen" says "*the glory that was Greece and the grandeur that was Rome*" instead of *glorious Greece and grand Rome*. Such hypallage is particularly common in Vergil; he talks for instance about *seizing the huge weight of the belt* (*rapiens immania pondera baltei*) instead of about *seizing the belt of huge weight* or *seizing the very heavy belt*.

POETRY AND GRAMMATICALNESS

SAMUEL R. LEVIN

In order that a grammar be adequate to a language, it must generate all and only the grammatical sentences of that language. It follows that a grammar may fail to be adequate for one of two reasons: it may generate some sentences which are not grammatical, or it may fail to generate some that are. We may call these two types of inadequacy *overgeneration* and *undergeneration* — compositely, *dysgeneration*. Since the question of grammaticalness is closely bound up with the question of dysgeneration, we shall discuss grammaticalness from the point of view, first, of overgeneration, then from the point of view of undergeneration. The latter discussion will lead us into the question of poetry.

Although in theory a generative grammar should enumerate all and only the grammatical sentences of a language, in actual practice the results can never be so clear-cut. The requirement that the grammar project, from the corpus of observed sentences underlying its construction, an infinite number of sentences beyond the corpus, implies that the rules, aside from being iterative, must have a certain measure of generality. This same generality entails the enumeration — by the same set of grammatical rules — of a large number of sentences whose grammaticalness is sometimes open to question. We are not talking here of anarchic outputs like **if go ninth John as*. Outputs of this kind would clearly invalidate the grammar. The problem lies rather with outputs that lie more along the margin of grammatical sentences, sequences like **argumentative windows cook with their destinies*, for example. Outputs of the latter type can be precluded by imposing restrictions on the grammatical rules, but a very large number of such restrictions would have to be imposed if all sentences of the above type were to be precluded, and the greater the number of restrictions so imposed, the less general would the grammatical rules become. The problem thus seems to resolve itself into the question of whether greater generality is desired of the grammar, or whether greater grammaticalness is required of it. But there does not seem to be any obvious way to decide what the optimum decision might be.

Of course, one can say that everything and only what the grammar generates is grammatical by definition. According to this view, if the grammar succeeds in generating a large body of clearly grammatical sentences and if, inversely, it does not generate any clearly ungrammatical sentences, then we accept as being also grammatical whatever sentences of a marginal nature the grammar additionally generates, and

no marginal sentences which it does not. Some such solution must, no doubt, be adopted if the question of adequacy is to be coped with at all. But it does not follow from this view, even though all the sentences which the grammar generates are grammatical, that they are all *equally* grammatical. Such an argument would obliterate by fiat a difference that is interesting in a fundamental way, and would leave us with no motivation for investigating the differences between those sentences of which, presystematically, some are grammatical, some are only semi-grammatical, and some are ungrammatical.

As a matter of fact, most of the literature on the subject holds the question of degrees of grammaticalness to be important; it is held to be highly desirable to investigate the relative structures of grammatical, semi-grammatical, and ungrammatical sentences so as to ascertain, if possible, in what relation such sentences stand to the grammatical rules. A priori we might expect those sentences that are semi-grammatical to stand in some different relation to those rules than do those sentences that are grammatical and ungrammatical. We might even expect to find different relations to those rules among sentences that seem semi-grammatical in different ways. These different relations might then serve to explicate the reactions of native speakers, those reactions, that is, on the basis of which we make the distinctions in the first place.

The complementary side to the problem of a grammar's fit to the language arises when it generates not too many sentences, but too few, i.e., when it undergenerates.¹ This situation may result if, either there are no general rules² for generating a particular sequence, or if there is a general rule, but it has been so constrained as to prevent it from generating a particular sequence. There are no general rules in the grammar, for example, that would enable us to generate **if go ninth John as*; obviously, we do not want any such rules. On the other hand, while there is a set of general rules that will generate a string of the form A N V P T N, these rules are presumably constrained so as to prevent the generation of the sequence . . . *seven oceans answer from their dream*. Now, prima facie, it might seem that we would not want the grammar to generate the preceding sentence any more than we would want it to generate **argumentative windows cook with their destinies*. But it is precisely sentences of the former kind that we encounter in poetry (this one is from Hart Crane's *The Bridge*). Sentences like that of Crane's, of course, raise the question of what part or parts of the language the grammar is supposed to be adequate to. This is not the place to go into that question.³

¹ On the question of fit, see F. W. Harwood, "Axiomatic syntax: the construction and evaluation of a syntactic calculus", *Language*, 31 (1955), 409-413.

² By general rules we mean rules whose constituents (ignoring possible constants) are highest-order, that is, have no constraints on them; e.g., $S \rightarrow NP + VP$ or $NP \rightarrow T + N$.

³ It may be mentioned, however, that selecting for examination sequences that have occurred in poetry undercuts the question of what "could" occur given certain circumstances. These sentences *have* occurred; what is more, their occurrence in poetry might legitimately be held to evidence what kinds of constructions the grammar of the language will tolerate when a strain is put on it. It is, in any case, more desirable to deal with actual than with putative sentences. Of course, as we have said,

What interests us here is that, just as not all the sentences which the grammar generates are equally grammatical, so not all the sentences which it does not generate are equally ungrammatical. Indeed, it is not obvious that they all are ungrammatical.

Chomsky has recently discussed the question of assigning degrees of grammaticalness to sentences.⁴ His method, admittedly adumbrative, consists essentially of setting up a hierarchy of categories, as a supplement to the grammar, constructed in such a way that any sequence of words can be represented on each level of the hierarchy. Chomsky's account describes only three such levels and, depending on the depth in the hierarchy at which a given sequence is still grammatically well-formed, it is labeled (1) grammatical (if it is well-formed down through level three), (2) semi-grammatical (if it is well-formed down through level two, but not three), and (3) ungrammatical (if it is well-formed at level one only, on which level there is only one category, the class of all words). An interesting feature of this method is that it assigns degrees of grammaticalness to sequences which the grammar does not generate. Similarly, in this paper we shall deal with sequences which, presumably, a grammar of English would not generate, sequences, thus, that are either ungrammatical or semi-grammatical. As we have pointed out, however, the sequences will come from poetry. In what follows then, we shall discuss the grammaticalness of such sequences, and we shall introduce a procedure which, though different in operation, yields results which are consistent with the results given by Chomsky's formulation.

In judging the degree of grammaticalness of such sentences, we proceed in the following manner: we assume, first of all, that the grammar will not generate them. (If it turns out that a grammar devised for English will, in fact, generate them, that fact is of no consequence to the procedure; other examples are available.) We then ask how the grammatical rules can be fixed so as to generate the sentence in question and, finally, we ask what the consequences of such fixing are to the grammar — in terms of what sentences other than the sentence in question would additionally be generated by the revised rule(s). The degree of grammaticalness of each of the tested sentences is then a function of the number of unwanted consequences (i.e., those sentences beyond the one in question) that the revised rule generates: the greater the number of such unwanted consequences entrained, the less grammatical is the sentence in question; the fewer such unwanted consequences the revised rule generates, the more grammatical is the sentence in question.

As examples to work with, we take the two lines *he danced his did*, from Cummings'

the fact that these actual sentences come from poetry raises the question of what part or parts of the language the grammar is supposed to be adequate to. But we are not directly concerned with that question here. One could perhaps obtain similar examples from the casual language, in which case the question would not arise. But if one cannot obtain such examples from the casual language, then it remains desirable to consider actual sentences — from whatever part of the language they may come. We should notice that putative sentences do not become actual just because a linguist uses them. Such sentences do not belong to the corpus of the language, but rather to the linguist's metalanguage.

⁴ Noam Chomsky, "Some Methodological Remarks on Generative Grammar", *Word*, 17, 235ff.

“Anyone lived in a pretty how town” and *a grief ago*, from Thomas’ poem of that name.⁵ We assume that these are deviant sentences, in the sense that the grammar will not generate them. Presystematically, it appears, further, that though both are deviant, they are deviant in different ways — which suggests that they should have different degrees of grammaticalness. We then ask how the grammar can be fixed so as to generate them. Now as a general rule, a grammar may be fixed to generate a new sentence in one of two ways: a new rule may be introduced; or items may be shifted from class to class.⁶ Both our deviant constructions could be handled by either of these two procedures. Thus, to generate *he danced his did*, we could introduce a new rule, $NP \rightarrow T + V$ (we may ignore the morphophonemic question raised by *did*). The sentence could then be generated by the rules:

- (1) S \rightarrow NP₁ + VP
- (2) NP₁ \rightarrow (T) + N
- (3) N \rightarrow *he*
- (4) VP \rightarrow V + NP₂
- (5) V \rightarrow *danced, did*
- (6) NP₂ \rightarrow T + V
- (7) T \rightarrow *his*

Alternatively, we could shift *did* from the class V to the class N; viz., $N \rightarrow did$. Then the sentence could be generated in due course from the rule $S \rightarrow NP + VP$, where the NP of the predicate is rewritten $T + N$, a rule already in the grammar. Whichever alternative we select to generate this sentence, the consequences in terms of unwanted sentences are great. If we select the first alternative, the new rule $NP \rightarrow T + V$, will generate, in addition to *his did*, all kinds of things like *my had, the went*, etc. If we select the second alternative, the shift of *did* to N will entrain as values of $A + N$, to take just one construction, things like *tall did, enthusiastic did*, etc. Since, on the one hand, the members of V are in the thousands and, on the other hand, the members of A are similarly numerous, thousands of unwanted sentences would be generated by the grammar if it were fixed so as to generate *he danced his did*.

We could of course reduce the number of unwanted consequences if we introduced into the new rule, instead of the general class V, some subclass of V. It is not clear, however, just how we would go about selecting such a subclass. Subclasses are set up on the basis of restricted co-occurrence privileges in many clear instances of grammatical sentences; such would be the division of the class V into V transitive and V intransitive, for one example. In the case of *he danced his did*, however, we ob-

⁵ The fact that the latter sequence is not a sentence is of little consequence; it is part of one. Dealing with the phrase alone merely simplifies the discussion.

⁶ In generative grammar, of course, class assignments are also represented by rules, viz., $N \rightarrow man$, so that a shift from one class to another is also an instance of a new rule. These are rules for lexical selection, however, and it seems desirable to keep them separate in our discussion from the grammatical rules. We therefore speak of shifting items from class to class.

viously have no clearly grammatical instances on the basis of which we could decide to what subclass of V *did* should be assigned. Nor would it do to use the subclass to which *do* belongs in the regular grammatical rules, since the clear cases on the basis of which *do* is assigned to that subclass have no obvious relation to the sentence under consideration here.⁷ For reasons similar to those discussed above, there is no obvious way to select a subclass of N if we adopt the alternative procedure of shifting *did* from V to N.

Now let us consider our second sequence, *a grief ago*. There is a rule, or series of rules, which enables us to generate such sequences as *some time ago*, *a while back*, *a year ago*, etc., i.e. $T_x N_y D_z$. Presumably, the N of this construction is a subclass containing only temporal nouns. To generate *a grief ago*, we lift the restriction from the rule in question so as to include in this N subclass also nouns indicating, say, states of mind, a subclass including *grief*. The revised rule will now generate *a grief ago*, and, in addition, sequences like *a happiness ago*, *some sorrow back*, *a disappointment ago*, etc. Alternatively, we shift *grief* from the subclass N comprising nouns indicating states of mind to the temporal subclass (retaining it of course in the original subclass). The grammar will now generate *a grief ago* and, additionally, such sequences as *a grief back*, *some grief ago*, etc. Now the consequences of fixing the grammar — either way — so as to generate *a grief ago*, seem not nearly as serious as the consequences of fixing it so as to generate *he danced his did*. But since any additional sentences that we might obtain from fixing the grammar so as to generate our two sentences are bound to be of the same grammatical form as the original sentences, our reactions to the new sentences merely duplicate — they do not explain — our reactions to the original sentences.

There is, however, an underlying structural reason for our different reactions, for thinking, that is, that the two sentences and their congeners have different degrees of grammaticalness. The underlying structural difference lies in the fact that there is a much higher functional yield when the grammar is fixed to generate *he danced his did* than there is when it is fixed to generate *a grief ago*. This different yield results from the fact that the constituents involved in the adjustments for *he danced his did* comprise many more members than do the constituents involved in the adjustments for *a grief ago*. To generate *he danced his did* we introduced as part of a new rule a general class, namely $V: NP \rightarrow T + V$. Alternatively, we shifted *did* from one general class to another — V to N. To generate *a grief ago*, on the other hand, the new rule amounted to lifting a restriction on an already existing rule. Even though the new class — comprising temporal and state of mind nouns — is more general than it was previously, it is still much less general than N or V, for example. Alternatively, the shift of *grief* was from one subclass, not general class, to another.

The preceding discussion explains how the degree of grammaticalness of any deviant

⁷ That the selection of a subclass of V for *did* is not feasible is borne out by the occurrence in the same poem of the two sentences: *they sowed their isn't* and *(they) went their came*, where the two verbs *be* and *come* belong to different subclasses of verbs than does *do*.

sentence can be interpreted as a function of the number of unwanted consequences that the revised rule generates: the rules will generate any and all combinations of the members of the constituents constituting the rule; the more members in the constituents, the greater the number of unwanted consequences from the revised rule. The degree of grammaticalness of any sentence not directly generated by the grammar is thus in inverse proportion to the number of unwanted sentences which the revised rule generates.⁸

A sequence like *he danced his did* is not very common in poetry, whereas a sequence like *a grief ago* is quite common: in fact, the latter may be said to be typical of a good deal of diction that is characteristic of poetry. Now, as Chomsky has pointed out, "Given a grammatically deviant utterance, we attempt to impose an interpretation on it, exploiting whatever features of grammatical structure it preserves and whatever analogies we can construct with perfectly well-formed utterances."⁹ In performing these operations on *he danced his did*, however, it is quite difficult to know what the proper analogies with well-formed utterances might be, since there are no utterances of this form generated anywhere in the grammar. When we attempt to adjust the grammar (i.e., make analogies) so as to generate utterances of the form *he danced his did*, a very great number of new constructions is entrained, comprising elements that are only remotely, if at all, connected with the sequence in question. The effect of this line is therefore one of diffusion, and not that fusion which we ordinarily associate with poetic language.

It is quite different with *a grief ago*. There the attempt to impose a structure on the sequence is met with success. In analogizing to well-formed utterances of its form, we entertain such sequences as *a while back*, *some time ago*, *a grief back*, etc. If we certify *a grief ago* by lifting the restrictions on the rules that yield the string $T_x N_y D_z$, then *grief* is associated, by the analogizing process, with the subclass of temporal nouns (*time*, *while*, *year*, etc.).¹⁰ The process of fitting *grief* into this construction thus induces us to regard *grief* as implying time. Since it already implies state of mind (as being a member of that subclass of nouns), there is a fusion here of those two meanings. It is the act of analogizing then that produces the effect of richness which such poetic sequences produce. If we adopt the alternative procedure of shifting *grief* to the subclass N comprising temporal nouns, the same result is produced.

It is not merely paradigmatically, however, that *grief* is associated with temporal meaning. Adjusting the grammar either way so as to generate *a grief ago* causes *grief* to be associated with not only temporal nouns, but also with the temporal adverbs that

⁸ This suggests that the degree of grammaticalness of those sentences that *are* generated by the grammar is in *direct* proportion to the number of sentences which the rule(s) in question generate. We shall not go into this question here. For a discussion of some aspects of the general problem, see Sol Saporta, "The application of linguistics to the study of poetic language", in *Style in language*, ed. Thomas A. Sebeok (M.I.T. Press, 1960), esp. pp. 84, 91f.

⁹ Chomsky, *Word*, 17, 234.

¹⁰ In this procedure, *grief* also brings in its train all the other nouns of its subclass, the state of mind nouns like *sorrow*, *happiness*, *disappointment*, etc.

occur in construction with such nouns. *Grief* thus becomes associated with notions of time syntagmatically as well as paradigmatically.

The important fact about sequences like *a grief ago* is that the grammar limits the framework within which the attempts to render the sequence grammatical must take place. This fact has two important effects: it makes feasible the grammaticalizing of the sequence, and it brings into association with the element(s) in the sequence a group of forms with narrow, well-defined meanings. This latter type of confrontation probably lies behind all metaphor.

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DISCUSSION

QUIRK:

Mr. Levin investigates "how the grammatical rules can be fixed so as to generate" two specific deviant utterances. In the one case ("He danced his did"), Mr. Levin's careful argument leads irresistably to the conclusion that any fixing of grammatical rules – i.e. any generalisation of the Cummings deviation – will yield all sorts of absurd and unwanted sequences, and this suggests that there is something wrong with the exercise itself. The suggestion is endorsed by the generalisation evolved to cover Thomas's "a grief ago" – namely "to include in this N sub-class also nouns indicating ... states of mind". Whereas in the Cummings case the adjusted rule admits too many sequences, in the Thomas case it admits too few: it will not generate "The installation was destroyed two attacks ago" or "I bought this carpet two apartments ago", either of which may have to be accommodated at the turn of a page. We may seriously doubt whether it is the task of a grammar to generate every conceivable word-sequence that users of a language may on occasion produce. On the other hand, it *is* the task of the grammar to "degenerate" (so to speak) any sequence that occurs: that is, to account for an ad hoc sequence in terms of the structural norms on which it is based; our task with poetic usage is not to generate it but describe it. Thus, by reason of "a amonth go", "a week ago", etc., our grammar can state than any noun appearing in place of *month*, *week* etc. will be grammatically determined as a unit of time, regardless of what other lexical sets it may belong to. Alternatively, if our grammar is transformative-generative and contains Mr. Levin's rule $T_x N_y D_z$, such that N_y is a sub-class of temporal nouns and D_z subsumes *back*, *past*, *ago*, this is enough. It will permit the generation of 'some time back' but it will also permit the poet or the wise-cracker his whim, precisely within the language's restrictions as stated in the grammar.

BUYSENS:

Mr. Levin seems to believe that grammaticalness is the only body of rules governing linguistic usage. If I consider his example "Argumentative windows cook with their

destinies", I can't find any grammatical fault with it; it is the meaning that is incorrect. Which particular grammatical rule prevents the generation of that sentence?

Mr. Levin measures the degree of ungrammaticalness by the number of unwanted consequences. How is this "unwantedness" to be ascertained? Is this unwantedness an objective fact. If I were to use sentences like "He drank his did" or "I slept my did", what would permit me to tell whether they are unwanted or not; it is quite possible that such phrases would be liked and imitated. We already have such shiftings as a *has-been*, a *would-be* scholar; the possibility of shifting is included in English grammar.

FIDELHOLTZ:

Professor Levin says: "The degree of grammaticalness of any sentence not directly generated by the grammar is thus in inverse proportion to the number of unwanted sentences which the revised rule generates." I anticipate that difficulty would arise when certain word groups or classes contain smaller numbers of entries than other classes.

Consider the sentence "The man walks the dog", which is exemplary of a large class of sentences ((D) N V_{transitive} (D) N). Now, of the two word classes Adj (e.g., "beautiful", "tall", etc.) and V_{linking} (e.g., "seems", "is", etc.), the class V_{linking} is certainly much smaller.

Suppose we change the rules to allow the generation of sentences of the following two types:

I. (D) N V_{transitive} (D) N Adj (e.g., "The man walks the dog beautiful.");

II. (D) N V_{trans} (D) N V_{link} (e.g., "The man walks the dog seem (or is)"). But I think most of us would agree that the former type (I) is much more nearly grammatical than the latter type (II), although there are probably several hundred times as many sentences of type I as of type II.

Perhaps a way out would be to assign numerical values to each class of words, depending upon the number of words in it, and to assign values to sentence or phrase types depending either inversely on the sum of the values of the "disturbed" classes, or perhaps on the result of subtraction of this sum from some predetermined basal value, to give a numerical value for the degree of grammaticalness.

Again, perhaps there will be advanced a quantitative hierarchical classification of word classes, in which case a quantitative representation of degree of grammaticalness should be feasible, using the structural properties of this classification together with the above-mentioned considerations. This is an interesting problem deserving of much consideration.

THE STYLISTIC FUNCTION

MICHAEL RIFFATERRE

Stylistics studies those features of linguistic utterances that are utilized to impose the encoder's way of thinking on the decoder, i.e. it studies the act of communication not as merely producing a verbal chain, but as bearing the imprint of the speaker's personality, and as compelling the addressee's attention. In short, it studies the ways of linguistic efficiency (expressiveness) in carrying a high load of information. The more complex techniques of expressiveness can be considered — with or without esthetic intentions on the author's part — as verbal art, and stylistics thus investigates literary style.

The conventional study of literature is inadequate to describe literary style per se, because (1) there is no immediate connection between the history of literary ideas and the forms in which they are manifest; (2) critics are misled in trying to use formal analysis only to confirm or infirm their esthetic evaluations — what is needed is a statement of existence, not a value judgment; (3) the intuitive perception of the relevant components of a literary utterance is insufficient to obtain a linguistically definable segmentation of the verbal sequence. For perceptions and value judgments depend upon the infinitely variable psychologies of the readers; they are also influenced by a conflict peculiar to literary utterances, that is, that these do not change, while the reader's linguistic code of reference does. On the other hand, linguistic analysis alone can not discern in the elements of a sequence which linguistic features are stylistic units as well. The traditional procedure which attempts to define style as an entity opposed to language, or as an abnormal system opposed to the linguistic norm is an artificial dichotomy: there *are* oppositions, productive of effects, between stylistically marked and unmarked poles; but these oppositions are given within the language structure; stylisticians' attempts to place marked and unmarked elements in different structures result from a static view of language and from our inability to conceive oppositions except with the help of a two-level spatial model.

Reality, however, is one linguistic message. But it is possible to discern in language various structures according to the chosen viewpoint, and to construct various types of linguistic analysis to fit them. The task of stylistics is therefore to study language from the decoder's viewpoint, since his reactions, his hypotheses as to the encoder's intentions, and his value judgments are so many responses to stimuli encoded in the verbal sequence. Stylistics will be a linguistics of the effects of the message, of

the output of the act of communication, of its attention-compelling function.

Our first step should be to place this function among the other functions of language. Roman Jakobson has proposed to expand Karl Bühler's triadic model to six functions: referential (centered on the verbal or verbalizable context to which the message refers), emotive (centered on the addresser), conative (on the receiver), phatic (on maintaining contact between encoder and decoder), metalingual (on the code common to both), and poetic ("the set, *Einstellung*, toward the message as such. . . This function, by promoting the palpability of signs, deepens the fundamental dichotomy of signs and objects").¹

Poetic function corresponds obviously to the aspect of language described by stylistics. But although "poetic" is an improvement on the "esthetic" used at first by Jakobson and the Prague Circle (for the poetic fact is within the linguistic structure, while esthetics is metalinguistic), it still limits the function's range to verbal art. Jakobson did specify that, "when dealing with poetic function, linguistics cannot limit itself to the field of poetry" and that poetic function is a constituent of all other verbal activities. Even so, the emphasis is on versified poetry at the expense of the "prosaic variety of verbal art",² seen as a transitional form (to be sure, metric forms lend themselves more easily than prose to analysis). But the basic objection is that, when we speak of verbal art, we presuppose that the object of analysis will be chosen according to esthetic judgments, i. e. to variants (they evolve with the linguistic code and with literary taste). Even if this variation did not affect it, analysis would still be limited to the more complex style structures. Such a limitation does not do justice to Jakobson's own initial definition.

It seems to me more appropriate to call the function "stylistic", as this would cover the simpler forms mentioned at the beginning.³

I should like now to outline some of the consequences of this enlarged definition. Form cannot attract attention per se, without being specific; that is, without being repeatable, memorizable, quotable as is. Otherwise, the contents would be the primary object of attention, and could be repeated in other equivalent phrasings. Form is pre-eminent because the message and its content would lose their identifiable, inescapable specificity if the number, order and structure of the verbal items were changed.

Linguistics may analyze any kind of messages, but, as we see, stylistics deals only with structures that do not admit any substitutability; it concerns itself only with those combinatory rules that prevent the decoder from using the minimal decoding sufficient for comprehension, from substituting his choice of what is important in the content for the encoder's. For SF manifests itself in the aspect of the encoding process that limits the freedom of perception during the decoding (and the freedom of performance in the delivery of a literary work).

The set (*Einstellung*) toward the form of the message is actualized when the decoder

¹ R. Jakobson, in Sebeok, *Style in Language* (1960), esp. 350 ff., 336.

² *Ibid.*, 356, 374.

³ Abbreviation: SF.

must take into account every variation that characterizes the sequence structure. Obviously these variations cannot be meaningful without a pattern, which they modify. Jakobson sees the general principle of these variations as defeated expectancy. It seems to me that this should not be construed as the deviation from the norm, which stylisticians are prone to invoke. I have proposed elsewhere the following model to account for defeated expectancy: in a verbal chain, the stimulus of the style effect (contrast) consists of low-predictability elements encoded in one or more immediate constituents; the other constituents, the pattern of which makes the contrast possible, form the context. This concept of context has, over the norm, the advantage of being automatically relevant: it varies for each style effect. Only this variability can explain why the same linguistic item acquires, modifies or loses its stylistic effect according to its position (and also why deviation from the norm does not necessarily coincide with style). It is by low previsibility that the decoding is slowed down with the result of compelling attention to forms.⁴ You will note that this model does not rest on a census of all known style effects: consequently, it cannot be upset by the future developments of verbal art, especially by the creation of ungrammatical forms.

As far as the pattern-disrupting elements are concerned, SF operates mostly at the limits of the code (the use of more "normal" elements requires a special type of context, as we shall see).

The genesis of the most frequent formations must be assigned to a larger range of substitutability: instead of higher-predictability items, words foreign to the *état de langue* (neologisms, archaisms, borrowings, etc.) will be used, or items belonging to a grammatical category different from the one allowed by the sentence structure (periphrasis instead of a single word, substantive where an adjective would have the highest probability of occurrence, etc.). Another type of formation puts in contiguity mutually exclusive items: e.g. *figura etymologica* (ex. "dormez votre sommeil", where the syntactic progress conflicts with a static tautology), strings of synonyms, incompatible meanings etc.

These formations alone, however, have little to tell us on the bipolarity of the stylistic contrasts, for they can be annulled as they saturate a context, thus becoming too predictable. The real agent of defeated expectancy is an *increased* expectancy prior to the occurrence of the low-predictability event. This increase in expectancy results from a stronger patterning of the context. Thus, we can posit that SF tends to develop verbal sequences along the lines of highest or lowest probability.

In short contexts, the strong pattern may be preexistent as such in the code, i.e. a stereotype; this accounts for the renewals of clichés (ex.: Diderot's substitution, "Le linceul ne fait pas le mort", in the "Le /.../ ne fait pas le /.../" context provided by the proverb "L'habit ne fait pas le moine"), for symmetric structures such as syllepsis, chiasmus, etc., and for variations of rhythm on a metric model. Or it may be made immediately perceptible by a density of features that makes up for the brevity of the

⁴ See my "Stylistic Context", *Word*, 16, 207-218, and also 17, 318-344, p. 336 ff.

sentence span (e.g. monostichs): each stylistic device functions as a context for another device following immediately.

In wider contexts, "normal" patterning remains loose so long as the probabilities of occurrence are not restricted by more than the grammatical structure: informants tend to identify such contexts with their empirical idea of the norm. But further restrictions may be added to those of the Markov chain and of grammar (graphic devices, verse, saturation of the utterance with tropes, special vocabulary excluding certain words and therefore increasing the frequency of others, etc.) as the conventional signals of a literary genre, for instance. Such a specialized context has thus permanent features, recurring beyond sentence limits, and to which the verbal chain reverts after each contrast (whereas in "normal" contexts, it is not uncommon that a contrast sets up a chain of similar events, which become in turn a context for contrasts of another type). SF is in such complex cases characterized by (1) the superposition of two contexts: the "normal" one, made of a random succession of patterns, and, contrasting with it as a whole, the context with a constant overall pattern (the normal context need not be verbalized: poetic language, for ex., is commonly identified by opposition to usage; the context is verbalized in the case of poetry or poetic prose mixed with prose); (2) the valorization of words or phrases that would not compel attention (literary recording of casual speech in writing, standard or substandard words in a poetic context, etc.) except for the fact that they break the special pattern (a reversal of the contrast within a single context, where the contrasting element is the "marked" one); (3) a psychological conditioning which emphasizes the discrete character of linguistic signs (as opposed to the continuity of the spoken chain). The isolation of the contrasts in a pattern causes the addressee to believe in some intrinsic, permanent value of the signs, irrelevant to any oppositional system (archaisms, neologisms, "harmonious" words, etc.; thus SF develops the awareness of diachrony and of social dialect levels in language, and also of expressive relations between sound and meaning). The actual structure of style is dimmed because the stresses it permits monopolize attention. This needs to be explored: it would probably explain the appearance of catalogs of rhetorical devices, the "atomization" of traditional literary criticism, and common notions as "felicitous expression", "mot propre", etc.

Emphasis on discrete signs has a corollary in a sort of "paradigmatic" memory. Addressees react to a given word by isolating it from its context and comparing it mentally to a set of synonyms (e.g. "fugitivité de l'eau" will be compared to the more predictable "eau fuyante"). This comparison with the code functions as if it diminished the arbitrariness of the sign. Or, if you prefer, SF increases the motivation of the sign: the choice of words is oriented by the desire to reinforce expression by secondary associations (words of similar sound, semantic families, etc). The best example is the one contrived by Hockett in his amusing revision of Longfellow's poetry to gain greater morphophonemic efficiency:⁵ if "banish the thoughts of day" remains superior

⁵ C. F. Hockett, *A Course in Modern Linguistics* (1958), 293 ff.

to “skal the thoughts of day” and if the case is the same for “the beauty of thy voice” as opposed to the hypothetical “the sugg of thy voice”, it is not because of some intrinsic inappropriateness of the sound /sǎg/ for the sense “beauty”, as most readers would rationalize. It is because of the secondary associations set up by the acoustic similarity of /sǎg/ to words like “plug, mug, jug, ugly, tug, sag, suck”: they overpower any effort we make to accept the proper primary association with the assigned meaning. Contrariwise, “skal” lacks the secondary association which “banish” enjoys with “vanish”.

The following devices all respond to the need felt for an increased motivation: sound symbolism; changes in sentence structures destined to evoke a change in the referent, e.g. infinitive of narration plus asyndeton to suggest an accelerated tempo in the narrated events; self-explanatory neologisms, i.e. compounds whose components are separately meaningful (the arbitrariness of the sign is twice removed, if I may say so); continued metaphors, where the initial semantic transfer is sustained by the systematic coupling of corresponding constituents in the “vehicle” and the tenor). In the addressee’s secondary responses, increased motivation is further rationalized as “adequation of form to content” or “faithfulness to the author’s intention”.

Again we come across psychological constructions, that modify the interpretation, if not the perception of the message. Their existence alone would suffice to justify a separate linguistics of the decoder, because the metalinguistic superstructure they constitute differs notably from the objective reality of the utterances. A particularly striking instance is when a word gets its stylistic value from its frequency of occurrence. It is not the actual frequency, but the apparent frequency that plays a role, for the threshold of perception varies with the nature of the recurrent item. The recurrence of a word that is already compelling attention is perceived faster than a word that receives its value from repetition only; in Czech poetry, if word boundaries are actualized by semantic and syntactic devices, the rhythm, although “mounted” on a regular metre, appears less regular than in “vers libre”.⁶ This constant discrepancy⁷ reminds us that stylostatics should be used with caution. The discrepancy between the actual message and the message as it is perceived can completely alter the act of communication: so with what I call the illusion of realism. Authors often use exotic or technical words as the vehicles of comparisons or as descriptive tools (e.g. botanical words in Rousseau or Chateaubriand, to illustrate rustic descriptions); but they are words beyond the understanding of the average man, and thus do not explain or “show” anything; they literally open on a semantic void; the addressee fills this void, since he immediately replaces the unknown referent with an appropriate figment of his imagination. This occurs with better known words, the referent of which remains nevertheless foreign to the addressee’s actual experience: when Flaubert, describing Africa, speaks of “chameaux. . . couchés sur le ventre, à la manière des autruches”,

⁶ J. Mukařovský, “La phonologie et la poétique”, *TCLP*, 4, 278-88, p. 287.

⁷ Cf. H. W. Hake — R. Hyman, *J. Exp. Psych.*, 45, 64-74, p. 72-3.

his reader's ideation needs no objective referent, but only a referential phrase. The apparent reference to reality masks a veritable semantic autarchy of the message.

We are close to that formalized manifestation of language, to that poetry of grammar, which Jakobson sees prevailing over the referential function.⁸ For him, the structure of a message depends on its dominant function and on the relative importance of a blend of the other functions. But I would object that two functions only are always present — the stylistic and the referential — and that SF is the only one centered on the message while the others have this in common, that they are oriented toward points exterior to it, and that they organize speech around coder, decoder and content. It seems therefore more satisfactory to say that communication is given structure by the five directional functions, and that its intensity (from expressiveness to verbal art) is regulated by SF.

Thus SF prevails consistently over referential function. Even though a message may be ideally oriented towards the objective referent, its cognitive or denotative effectiveness depends on the effect of the sign on the addressee, on the information it carries, not on the completeness or fidelity of the recording of reality. When "neutral", this recording still depends on the encoder's attention and apprehension, which in turn will generate expressiveness and control the decoding. The addressee's own knowledge of the referent tends to be cancelled by the simpler and immutable structure style has built of it. In fiction, the referent tends to become purely verbal, and "obscure" poetry is the end of this evolution: from the moment when ambiguities are maintained instead of being solved, referential function ceases, since it aims at preserving a link between the object and its representation, and SF rules, since a maximum strain is put on the decoder, a maximum check imposed on his escapism. The message gains full autonomy as an object.

SF regulates metalingual function as well, which enables addresser and addressee to check on whether they are using the same code. Such checking is oriented towards the message, since the *actualization* of the code, and its possible ambiguities, are what matters; in noncasual speech, especially in writing, glossing or asking about the code is rarely a real need: the addresser has ample opportunity to remove any obscurity from his actualization of the code; metalingual function then becomes just another procedure of emphasis. The same can be said a fortiori of the phatic function: any signal to start or sustain communication works also as an emphasis on the message.

As for the emotive and conative functions, they both work by forcing attention. The first one stresses by adding emotive to cognitive information. In the second, the direct appeal to the addressee is in itself a device to prepare him for a fuller perception.

The speaker's attitude towards his subject matter being the very principle of style, the physiognomic features of language are dependent on SF. Many linguists would assign them to message delivery rather than to the message itself, but Jakobson

⁸ R. Jakobson, "Poetry of Grammar...", *Poetics* (The Hague-Warsaw, 1961), pp. 937ff.

showed that these features are conventional and coded like the distinctive features.⁹ It is in written speech that their stylistic nature appears most clearly. Feigned or true, emotion is there a system, because its direct, purely indicative expression is transferred to a symbolic representation. Writing, however, does not offer much for representing expressive features (italics, capitals, /!/). SF supplements so fragmentary a code by what I labelled "compensation". The verbal chain being a sequence of information-carrying monemes, compensation makes it also a kind of musical score of delivery designs. A substandard word in noncasual speech, for ex., creates a contrast *and* notes as on a score an expressive intonation, which, whether actually performed or not, is an encoded stylistic sign. The Yiddish rise-fall sentence has been adopted in American English to convey irony: in written incredulous questions, the untransposed interrogative word-order typical of Yiddish ("This is summer?" = "Is that what you call summer?") is both a contrast and a delivery design.¹⁰ If the design is actualized, the Yiddish intonational contour, contrasting with its English intonational context, produces a slangy delivery instance that completes and emphasizes the contrast in the written score. Compensation uses distinctive features twice, as cognitive features, and as devices for encoding physiognomic features. Far from being an impoverished transcription of speech, writing is used by SF to increase the informational output of language.

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DISCUSSION

BUYSENS:

M. Riffaterre seems first to imply that stylistics studies language from the viewpoint of the speaker ("bearing the imprint of the speaker's personality", "the speaker's attitude towards his subject matter being the very principle of style"), yet he also writes: "The task of stylistics is therefore to study language from the decoder's viewpoint."

I do not think that Mr. Riffaterre or specialists in stylistics in general have come to an agreement about the object of their discipline, which can in my opinion be looked at in three ways:

1. To consider only the speaker's viewpoint and to oppose the personal use of language to the social aspect, or "usage".
2. To consider only the addressee's viewpoint. In this case it is impossible to distinguish what the speaker is expressing willingly from what his whole behaviour is manifesting. Pronunciation, for instance, reveals much that can be detected by the addressee, just as a graphologist can discern in one's writing much that the writer did not will to be revealed. But graphology clearly separates this type of significance from

⁹ Jakobson, in Sebeok, p. 354; cf. *Language*, 29.34 ff., esp. p. 35.

¹⁰ U. Weinreich, "Yiddish Rise-Fall Contour". *For Roman Jakobson* (633-43), p. 642-3.

the meaning of the message, whereas I know of nobody in stylistics who seems to think of studying apart those facts that accompany speaking without being part of the meaning. Stylistics is then a mixture of two orders of facts if we adopt the addressee's viewpoint.

3. To consider both speaker's and addressee's points of view, but in opposition to "special languages", e.g. literary language opposed to the languages of lawyers, soldiers, peasants, etc.

HERDAN:

Mr. Riffaterre's paper is very welcome as a contribution to the problem of the relation between style and probability of occurrence of vocabulary and grammar items. This relation, and with it the role of stylostatistics, is still largely misunderstood. A paper by Roger Brown read during the Conference on Style in Language (1961), arranged by Sebeok, speaks of stylostatistics being comparable to the method of fingerprinting for purposes of criminalistic identification. I cannot conceive of a more one-sided view, and one which does less justice to stylostatistics than this. Though I must add that it is quite possible that Mr. Brown did not mean it as his personal opinion, but was quoting it as the conventional view.

Since stylostatistics aims at the determination of individual style differing, or not differing, significantly from that in the language in general (*la parole*), it is at once evident that before trying to describe individual style, the properties of the use of language in general must have been explored. In this way, stylostatistics leads to establishing the general laws for the use of language as a necessary preliminary of the determination of divergencies from these laws in individual style.

To illustrate this: taking the most important stylostatistical parameter, the Characteristic K , or v_m according to my derivation, both can be regarded as sample values of the Repeat Rate of vocabulary items, or of grammatical forms. However, since the Repeat Rate is largely dependent upon the vocabulary in the language in relation to the number of occasions for using words, it is at once clear that K and v_m must be primarily dependent upon, and characteristic of, the language in use in general, or in a particular field of knowledge; only a significant difference in K or v_m from the repeat rate in general can be evaluated as an index of personal style.

Thus, when investigating style we must be intent upon establishing how much of what we call style is truly personal style, and how much is general use of language.

The failure to realize this most important aspect of stylostatistics has marred the presentation of the matter by W. Plath in *Trends of American and European Linguistics 1930-1960* (Utrecht, 1961). Mr. Plath does not see much progress in the matter since Yule, excepting my derivation of the Characteristic. He completely missed the point that in addition to the *algebraically* new derivation of the Characteristic and quite independent from it, I have stressed the need for a new *linguistic* interpretation of that parameter, as being primarily a language constant, evaluating the Type-Token relation in the language in general.

STYLES AS DIALECTS

WERNER WINTER

From a linguist's point of view, references to style as commonly found in literary analyses of texts are often too intuitive and impressionistic to carry much weight. A random example may be cited for an illustration: If a Soviet scholar, D. S. Lixačev, states in a recent edition of Russian diplomatic reports of the 16th and 17th cent. (*Putešestvija russkix poslov XVI-XVII vv.*, Izd. AN SSSR, M.-L., 1954) that the language of these documents contains a blend of the "bookish" and the "colloquial", and that it is "factual" language, then such statements may well sum up our first reactions to the material before us; still, we will feel that the use of such labels is not legitimate unless we can describe in an accurate, objective way what constitutes "bookish", "colloquial" or "factual" language.

The following is a discussion of some recent attempts to discover distinctive properties of certain groups of texts by means of statistical evaluation. The methods used were originally developed for work on German; a presentation of procedures and results has been offered in *Phonetica*, 7 (1962), 193-216. Later on, one of my graduate students, Ludek A. Kozlik, applied the same approach to Russian; his findings are included in his master's thesis, *Some frequency characteristics of Russian styles*, Univ. of Texas (Austin, Texas, 1962). My work has been supported most generously by the U. S. Army Signal Corps project in mechanical translation at the University of Texas.

A style may be said to be characterized by a pattern of recurrent selections from the inventory of optional features of a language. Various types of selection can be found: complete exclusion of an optional element, obligatory inclusion of a feature optional elsewhere, varying degrees of inclusion of a specific variant without complete elimination of competing features.

In these properties, styles agree with dialects, in particular with social dialects; it may even be claimed that styles can be considered special types of social dialects.

The identification of different styles can be undertaken in much the same way as the identification of different dialects of a language: a boundary between two styles can be established on the basis of a bundle of isoglosses. Style boundaries may or may not coincide with boundaries of idiolects; one has the general impression that any speaker of a given language is free to make use of different styles depending very much on circumstances and on choice. However, it seems that there is a certain correlation

between, on the one hand, what one might call (rather inadequately) "erudition" and, on the other, versatility in the use of different styles (or of what we suspect to be identifiable as different styles); use of "poetic" language, for one, seems to presuppose at least previous exposure to this type of diction, use of "scientific" language generally appears to depend on appropriate training.

A complete characterization of a style would require the previous recognition of all distinctive properties of an entire class of utterances. It is not wise to hope and wait for such recognition as we know only too well that exhaustive descriptions of much less complex facets of language still remain desiderata; for all practical purposes it will be sufficient to base a proposed identification on the establishment of some characteristic isoglosses. Any one of the selection types mentioned above can be the source of a differentiation reflected by such an isogloss; only one of them, however, yielded data readily usable in our kind of investigation.

As is the case with other forms of dialect, a section of a language for which we want to use the term "style" cannot be determined by an a-priori decision or even on the basis of comparative evidence. The fact that a special style is used in language A for scientific discourse does not mean that in language B a special style has to exist for the same purpose; the fact that poetic forms are used in a given language and at a given time does not by itself mean that we can postulate the existence of a special poetic style for this language. We have to depend entirely on the discovery of combinations of distinctive properties in any particular section of a language. While this is true, we have to take note of the fact that the native or near-native analyst of spoken and written materials in a given language usually has at least certain rather specific intuitive feelings about "stylistic" properties of his materials and about contrasting styles found in them, just as a native speaker of a language often has a fairly good prescientific notion about dialect distribution.

Ideally of course the identification of contrasting styles should be made without recourse to native reactions (just as one may ideally insist on a phonemic analysis without reference made to meaning); in practice, to heed the common-sense assessment (which after all reflects a thorough exposure to the language and all the reaction conditioning that goes with it) means considerable gains in the efficiency of one's investigations: to pay due attention to native opinion means being kept out of numerous blind alleys. This does not imply that the native speaker is always right; it goes without saying that every conclusion drawn by him from intuitive reaction must be verified by exact checks.

The two approaches, the ideal and the practical, may be briefly illustrated here:

We are interested in the question of word length in Russian (needless to say that our interest was aroused by certain things we observed, and that we have a hunch that we may come upon something significant).

Ideally we would now follow a procedure and a chain of arguments as follows: We decide to check the distribution of long words; by trial and error we detect that a definition of "long word" as a word of nine or more letters yields reasonably clear

results. Finite verb forms we exclude from our investigation since their occurrence is conditioned by obligatory rules throughout the entire language, so that they cannot possibly be used in the determination of styles. We take samples of text, one thousand words each (the sample size has been found appropriate in previous work), from fifty different sources, all by writers of the last two centuries. Wherever possible, we select dialogue passages marked by special punctuation features in our printed texts. (We also search non-dialogue passages occurring alongside dialogue in the same text, but we do not include them in our first set of data.) We find: In 20 of the 50 samples, long words occur with a relative frequency of more than 15%; in 30 samples, this frequency amounts to less than 9%. All instances of low relative frequency occur in dialogue passages. A check of the texts with high relative frequency of long words shows that all are from journalistic and scientific prose (the terms used here to refer to content categories). We conclude that we can use the feature of relative frequency of long words for the purpose of characterizing (in part, to be sure) two forms of speech that are found in dialogue and in argumentative prose, respectively. If the difference found here is confirmed by further independent data, we will revise this statement to say that low frequency of long words is one distinctive property of (simulated) oral style or, once the evidence from fictional narrative is brought in, a distinctive property of non-argumentative prose. If we find the last formulation too involved, we may decide to use its converse, namely, that high relative frequency of long words is one distinctive property of argumentative prose — “long word” here being defined as a word of nine or more letters, “high relative frequency” as a frequency of more than 15%.

The second approach would reflect more closely the actual discovery process: We have noticed that what might be called argumentative prose is full of rather long words: we read the dialogue of stage plays and of novels and find that such is not the case there; we set out to verify our impressions, using the same definition of “long word” as given before. Our impressions are confirmed, but — and this is of course decisive and will offset all inherent weakness of the second approach — the mere impressions are replaced in the process by statements of data, which we now can use with confidence. Even though we did not pretend to start without any notion as to what we would find, our final results are as valid as those obtained by the first approach.

Various aspects of linguistic form are utilized in the identification of dialects: lexicon, phonology, morphology, syntax may all contribute valuable isoglosses. Literary stylistic analysis relies heavily on features of the vocabulary and their distribution.

In our own work, it soon became clear that lexical items, though they may well be distinctive features of specific styles, could not be used for our particular type of investigation. We were interested in determining frequency characteristics of our texts by the evaluation of random selections of constant size, selections which we considered to be representative of the entire text they had been taken from. All but a few (and generally non-optional) features of the lexicon have too low a frequency to draw conclusions from the absence of a particular feature in a given sample or set of

samples; conversely, its occurrence within these samples cannot be taken to reflect similar distribution in the remainder of the text. Conclusions based on lexical items will have to be derived from the entire corpus studied, and typically (as exemplified in the procedures of literary analysis) the fact of the occurrence of a given item is not as significant as the determination under what circumstances it is used. Our analysis provides only for quantitative findings, not for qualitative evaluation; we therefore had to forgo any consideration of lexical units found in our materials.

Instead, we concentrated in our investigation on phenomena of sufficient generality to assure recurrence in our selected data. Thus, we were interested in the relative complexity of word, phrase, clause, sentence; we studied problems of the order of parts of sentences; we considered the question of paratactic versus hypotactic construction within our sentences.

The principal consideration observed in the choice and development of search techniques was that of simplicity. Only through the greatest possible simplicity in our procedures could we hope to be able to cover a rather large sample of text in a moderate amount of time (for a set of questions concerning order of sentence parts in German, 63,093 sentences from 30 different sources were examined); only through simplicity could we assure that our procedures could easily be repeated by other people interested in our problems. Thus, as seen above, the question of word complexity was reduced to a question of word length, and this word length was to be measured in letters, not in phonemes. One may justly label such approach crude; but only by being so unsophisticated we could hope that anybody setting out to check our results with the same texts would arrive at exactly the same figures we had obtained (barring only outright adding mistakes). For practical purposes, the much more refined technique advocated by Greenberg in his typological investigations must be called a failure: neither does it seem possible to arrive at identical results once a group of investigators sets out to duplicate Greenberg's morph count of a sample of words in a language well known, like English, nor is the technique, tedious as it is, feasible for work with really representative bodies of data.

Similarly, to handle a problem like that of clause length in a refined way would have required a great deal of skill and, just as important, a very great amount of time. Again we decided to use a simplified, though indirect and somewhat imprecise, approach to this matter. Taking cognizance of the fact that a finite verb is an obligatory constituent of a clause, and disregarding the fact that some clauses can be joined so that either the verbal or the nonverbal element can become multiple, and finally disregarding the special case (not rare in Russian) that the verb "to be" may be zeroed out, we decided to use the finite verb as an index of clause length: if 10% of all words in a sample were finite verb forms, we interpreted this as indicating an average clause length of ten words. This procedure turned out to be fast and reliable, and it provided simple criteria for the description of such elusive an entity as "nominal style".

What could be studied effectively varied from language to language. In German, the opener of a main clause could be determined at high speed since it involved only

the classification of preverbal material assignable to one part of the sentence. In Russian, the fact that the verb position is not as rigidly fixed as in German created such plotting problems that a study of the clause openers was not made part of the pilot projects included in Mr. Kozlik's work, even though extensive exploratory checks indicated that results not too different from those had in German could be obtained. The distinction between main and subordinate clauses could be made very fast in German, again because of contrastive verb position; in English, a similar search requires a much higher degree of sophistication in text analysis.

In conclusion, a brief resumé of our findings will be presented.

In German, the relative frequency of subject in first position in main clauses was studied. A significant contrast was detected between (simulated) spoken language (of stage plays) and the language of scientific treatises: all samples of the first category had a subject frequency in the upper part (73.6—79.2%) of the total frequency range (54.2—82.2%); all samples of the second category had a low subject frequency (54.2—63.5%).

Conversely, scientific texts showed high frequency of adverbial in first position (30.9—39.7%), while stage dialogue had low frequency values here (15.4—18.6%); the total frequency range for the initial adverbial in main clauses was 15.4—39.7%, so that the two types of language contrasted appear at opposite ends of the scale.

The measurable difference between stage dialogue and scientific prose is noteworthy also in the case of the relative frequency of finite verb forms, our index, as will be remembered, of relative clause length. Here the total range of frequency of finite verb forms per hundred words is 7.4—17.1%; the range for scientific treatises alone is 7.4—11.3%, for stage dialogue, 13.6—15.0%. At this point, values were also obtained for the dialogue passages in fictional prose; here the range was 12.3—17.1%, so that here too we find scientific and dialogue prose at opposite ends of the spectrum.

Not so clear-cut was the distinction when we studied the question of parataxis and hypotaxis in German prose sentences. The stage dialogue showed a strong preponderance of paratactic constructions: in eight out of nine texts studied, 73.6—84.9% of all verbs were found in main clauses; in contrast to this, 18 of 19 scientific texts analyzed belonged to a frequency range of 46.5—68.5%. However, one each of the texts showed the range characteristics of the other group: one stage play had a main verb frequency of only 60.3%, one scientific book, a frequency of 77.0%. Dialogue from novels covers almost the entire frequency range: we find values from 50.8 to 83.1%. If in the previous examples we can say that we are dealing with indications of what we may call a genre style, we must take note here of strong interference with the same genre style; in other discussions of these data (note reference above) I have tried to explain this interference as due to the effects of choice on the part of an author.

We may sum up our findings for German and state that the frequency properties of scientific prose contrast strongly with those of simulated dialogue. If we take the latter as an approximation of the spoken language, then we can say that scientific German shows marked and measurable deviations from the patterns of the spoken language. Once the values obtained for fictional prose are taken into consideration,

we find that this type of language provides a transition between the extremes, not by occupying a well-defined area in the middle between the two, but by covering the entire scale of which the other two form the extremes. This means that, when dialogue style resembles oral German and scientific style represents written German par excellence, fictional German can be made to resemble either more the spoken or more the argumentative style of the language, apparently very much in accordance with the intentions of a writer.

If we come to note a smooth transition from fictional to argumentative German, there is strong evidence for a sharp break between the two genres in Russian.

Reference has been made before to an investigation of the occurrence of long words. In argumentative prose, 15.8—36.6% of all words fell into this category; in dialogue from stage plays and fiction, the range was 2.9—8.9%. The values obtained for fictional narrative were 7.4—15.1%.

Strikingly similar were the findings for a syntactic feature, the use of adnominal genitives. In argumentative prose, 23.4—35.4% of all noun forms were adnominal genitives; in dialogue, the frequencies were 1.2—8.4%. Fictional narrative covered a range of 4.6—17.0%.

The same general pattern is repeated in the figures for clause length. The average number of words per clause (computed on the basis of the frequency of finite verb forms, as pointed out above) is 12.4—20.0 in argumentative prose, 5.8—9.3 in dialogue and 5.7—9.1 in fictional narrative.

Fictional narrative patterns with argumentative prose only in one of our counts (in another, concerned with the frequency of adjectives, fictional narrative provides for a transition between dialogue and scientific discourse, similar to what was found in German): the average number of words per sentence in dialogue is 4.3—8.4, in argumentative prose, 11.5—21.6, and in fictional narrative, 7.9—19.1. A comparison of the values in the last two counts indicates that the internal structure of a sentence in fictional prose tends to be radically different from that of a sentence in scientific prose; it seems warranted to assume that mere sentence length is not as significant a distinctive feature as clause length is.

In contrast to German, the prose of Russian fictional narrative shows great affinity to the prose of the dialogue. If we again assume that the latter can be considered simulated spoken Russian, we note that there is no severe break between spoken and literary Russian, but that a radical difference exists between literary and scientific Russian. This appraisal of the nature of literary Russian seems to agree well with facts of its history: the importance of the spoken language for its development seems very much greater than that of spoken German for the language of a great many eminent writers, witness in particular Goethe.

What has been presented here, are first findings in a field of rather vast scope. The frequency values discussed may eventually turn out not to furnish the most important isoglosses between the style dialects they help define; this is a risk that had to be taken. All findings presented appear to be based on sufficiently broad data (the number of

samples used is 76 in Russian, between 30 and 57 in German; the size of the sample usually is 1,000 or 2,000 of the entities evaluated: words, clauses, sentences, depending somewhat on the limitations imposed by the size of the texts); it is therefore likely that the results obtained are representative.

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DISCUSSION

HERDAN:

Professor Winter is quite justified in using grammatical features as style characteristics. Stylostatisitics is by no means restricted to vocabulary. But apart from this, there is a difference between what we usually call stylostistics and Professor Winter's investigation. In the latter, two obviously different types of style, of the spoken language in dramatic plays and of scientific writing, are compared, and the object of the investigation is to exhibit such grammatical features as would account for the stylistic difference. Thus, it is not a matter of ascertaining *whether* the two styles are different, but knowing them to be so, to ascertain *wherein* the difference consists. In the usual stylostistical investigation, on the other hand, the main problem is to determine *whether* two styles, be it in texts by the same or by different writers, are significantly different. The use of vocabulary and that of selected grammatical features may serve as criterion, and this is how for example P. N. Harrison has tried to establish the non-Pauline authorship of the Pastoral Epistles (Oxford, 1921).

On the whole, I would say that vocabulary frequency is a more recondite characteristic of style than are certain selected grammatical features, but in principle they can be used to the same purpose and it is left to the investigator to decide which features he would regard as being more appropriate for the matter in hand.

The mathematical methods used in stylostistics are, as a rule, of such generality as to be suitable for either characteristic. As a striking instance I should like to mention the use of the concept of *vocabulary connectivity* for the classification of languages according to certain phonological and morphological features. The method of vocabulary connectivity, and the mathematical function of Random Partitioning of vocabulary, were developed originally by dividing a literary text into a number of equal parts and noting how many words were peculiar to each of them, to any two, three, ... all parts. This idea was adapted with great success by Professor P. Franzoli of Florence University to the problem of the classification of Semitic languages, using as criterion the presence or absence of a great number of phonological or morphological features of the Semitic languages. This shows that literary criticism and linguistics proper ought not to be kept in watertight compartments. More precisely, the shift in the borderline between them which has taken place through mathematical linguistics (see my discussion after Prof. Benveniste's lecture on "Levels of Linguistic Analysis") has the effect that part of what is usually attributed to individual style is now recognized as being governed by the laws to which the general use of language is subject.

COMPOUNDING AS A PHONOLOGICAL PROCESS

ILSE LEHISTE

In many languages, words may be considered phonological as well as grammatical entities. The canonical phonological form of such units may be defined in terms of the distribution of segmental phonemes within the unit, in terms of the particular allophones which are selected to represent these segmental phonemes, and in terms of the associated suprasegmental features. Viewed as such phonologically defined entities, words form a level in the phonological hierarchy, which extends from the smallest phonological units (features or phonemes) to progressively larger units of discourse.¹ At the same time, words constitute an intermediate level in the grammatical hierarchy. The study of morphophonemics is concerned with the intersection of the phonological and grammatical hierarchies at the level of phonemes and morphemes. The present paper deals with the interrelationships between words as grammatical units and words as intermediate units in the phonological hierarchy. The language from which the illustrations are chosen is Estonian, a Finno-Ugric language; parallels with other languages are numerous, but space does not permit their presentation in this paper.

The main phonological characteristic of Estonian is a highly developed quantity system. Segmental phonemes — vowels as well as consonants — appear in three contrastive quantities, short, long, and overlong. Three contrastive syllable types occur, whose quantity is not the sum of the segmental quantities of their constituent phonemes; three degrees of syllabic quantity are thus established.² Every stressed syllable appears in one of the three degrees of syllabic quantity. Since syllabic quantity is associated with stress (segmental quantity contrasts may occur also in certain unstressed positions), the distribution of syllabic quantity can best be described with reference to word structure. Disyllabic words have three contrastive quantity patterns. The first syllable of disyllabic words may appear in any one of the three degrees of syllabic quantity. The quantity of the second syllable is determined by the quantity

¹ The concept of the simultaneous structuring of language into phonological, lexical, and grammatical hierarchies is derived from the theory of K. L. Pike as expressed in his book *Language in Relation to a Unified Theory of the Structure of Human Behavior*, Parts I-III (Glendale, 1954, 1955, and 1960).

² The concepts of segmental and syllabic quantity are discussed in more detail in a previous paper. Cf. Ilse Lehiste, "Segmental and syllabic quantity in Estonian", *American Studies in Uralic Linguistics* (1960), pp. 21-82.

of the first syllable, and does not constitute an independent variable. If the first syllable is short, the second syllable contains a so-called half-long vowel, so that the time ratio between the two syllables is $2/3$; if the first syllable is in long syllabic quantity, the second syllable is somewhat shorter, yielding a time ratio of $3/2$. If the first syllable is overlong, the unstressed second syllable is phonetically quite short, and the time ratio between the two syllables is approximately $2/1$. Each of the three word types is further characterized by a different distribution of the intonation contour over the two syllables. Oppositions in syllabic quantity are neutralized in monosyllabic words, all of which resemble phonetically the first syllables of disyllabic words with overlong quantity.

In Estonian, compounding is a frequently used means to create new words. Two monosyllabic words may be combined to form a disyllabic compound. A comparison of these disyllabic compounds with disyllabic non-compounds reveals several structurally significant differences.

In disyllabic noncompounds the second syllable starts with a consonant in first segmental quantity. (Every nonfirst syllable begins with a consonant in first segmental quantity.) In word-initial position, the quantity of a consonant is phonemically non-significant, since no contrasts occur in this position. Phonetically the duration of a word-initial consonant is intermediate between the durations of the same consonant occurring in either first or second segmental quantity. If the second constituent word (i.e. the second syllable) in a disyllabic compound begins with a consonant, this consonant is usually in the intermediate quantity associated with word-initial position. For example, the Estonian word *jääde* 'remnant, scrap' contains a /t/ in first segmental quantity in intervocalic position; the word *jäätee* 'road on ice' contains a word-initial allophone of /t/, which is phonetically longer than the allophone occurring in *jääde*, but shorter than an allophone in second segmental quantity (the so-called short geminate) occurring in *jääte* 'you remain'.³ This is particularly clear in the case of plosives, but has also been observed with other consonants. It will be shown later that the difference in the quantity of the two intervocalic manifestations of /t/ (in *jääde* and *jäätee*) cannot be explained by conditioning by other phonological factors. If the word-status of the second component of a disyllabic compound is not taken into account, a fourth contrastive degree of phonemic quantity should be recognized, which would in turn necessitate a reinterpretation of the total segmental quantity system. If intersection between the phonological and grammatical hierarchies is accepted within the theory, the occurrence of a consonant in this intermediate quantity constitutes a phonological manifestation of a grammatical boundary.

As was mentioned above, the quantity of the second syllable of a noncompound disyllabic word is predictable from the quantity of the first syllable; no contrasts occur in the second syllable. If the first syllable is in overlong quantity, the second syllable

³ The phonetic statements are based on an extensive spectrographic investigation of the speech of five informants. A part of the phonetic data has been reported elsewhere (cf. the reference quoted in footnote 2); a number of new analyses were made for the present paper.

is phonetically short. In disyllabic compounds, both syllables are in overlong quantity. Disyllabic words with an overlong first syllable thus may exhibit a quantity contrast in the second syllable. For example, certain disyllabic infinitives ending in *-ma* differ from disyllabic nouns compounded with *-maa* "land" as second constituent essentially in the duration of the second syllable. This may be seen by comparing the phonological structure of verbs such as *saatma* "to send", *jääma* "to remain", *tootma* "to produce", *looma* "to create" with the structure of compounds such as *raatmaa* "woodland turned into arable land", *jäätmaa* "fallow land", *soomaa* "marshland", *puumaa* "large farm". That such contrasts are not limited to disyllabic words is shown by such words as *kuduma* "to weave", *tagama* "to guarantee" and *kodumaa* "hometown", *tagamaa* "hinterland".

If quantity contrasts in the nuclei of second syllables are considered significant in disyllabic words with overlong first syllables, the two phonetically different quantities occurring in the second syllables of disyllabic words with short and long first syllables should also be considered distinctive. If the special nature of compounds is not accepted as a conditioning factor, it will be necessary to reconsider the phonological status of the quantity of unstressed syllables, and to revise the description of the distribution of syllabic quantity.

The problem of stress in connection with compounding in Estonian is a rather complicated one. Excluding recent unassimilated loanwords, Estonian words are stressed on the first syllable; the placement of secondary stress is not predictable from synchronic data and thus is to be considered significant. The first syllable of the second constituent usually carries secondary stress in a compound. However, secondary stress is not restricted to those nonfirst syllables that were originally first syllables of constituent words in compounds: the third syllables of *tagama* "to guarantee" and *tagamaa* "hinterland" both carry an equal degree of secondary stress. There exist also disyllabic noncompound words whose second syllables carry secondary stress. The segmental structure of these syllables conforms to the phonotactic rules applicable to second syllables of noncompounds, but words of this type are inflected according to the monosyllabic pattern usually associated with unassimilated disyllabic compounds. Compare, for example, the inflection and the phonological shape of the compound *puutikk* "wooden splinter" (from *puu* "wood, tree" + *tikk* "sharp object, splinter"), gen. sg. *puutiku*, part. sg. *puutikku*, part. pl. *puutikke*, with that of the words *puuslik* "idol" and *luustik* "skeleton", gen. sg. *puusliku*, *luustiku*, part. sg. *puuslikku*, *luustikku*, part. pl. *puuslikke*, *luustikke*. All three are inflected like the monosyllabic word *tikk*, gen. sg. *tiku*, part. sg. *tikku*, part. pl. *tikke*. The inflection is not predictable from the phonological shape of the ending, since a word such as *nuustik* "mop" belongs to a different inflectional class and has a gen. sg. *nuustiku*, part. sg. *nuustikut*, part. pl. *nuustikuid*.

If the presence of secondary stress is considered the phonological factor which conditions the occurrence of the features described above as characteristics of constituents of compounds, we might expect these features to be associated with secondary

stress in every instance. This is, however, not the case. Conversely, every occurrence of secondary stress does not signal the presence of the first syllable of a constituent of a compound. If the presence of secondary stress is used as a criterion in constructing rules for determining the morphological structure of words, the same rules that might be applied to segment *puutikke* into *puu* + *tikke* (the original constituents) would segment *puuslikke* into **puus* + *likke* and *luustikke* into **luus* + *tikke*. The word *puuslik* ‘‘idol’’ is effectively a monomorphemic word: *-lik* is a common derivative suffix forming adjectives, but the word *puuslik* is a noun, and its first element can, on semantic grounds, scarcely be identified with the noun *puus* ‘‘hip’’. The word *luustik* is bimorphemic, derived from *luu* ‘‘bone’’ with the suffix *-stik*. While a segmentation **puus* + *likke* would correctly identify the suffix, a segmentation **luus* + *tikke* would be morphemically meaningless.

The presence of compounds in the language thus forces the analyst to choose among two alternatives. Since no phonological conditioning can be demonstrated, analysis at a single level would require that phonemic status be assigned to all those contrastive features which were found to be connected with word boundaries. In Estonian, this would involve recognition of four degrees of segmental quantity, reconsideration of the relationship between syllabic quantity and stress, and the consequent reinterpretation of the total phonological system. The other alternative is to accept morphological criteria in phonological analysis, and to treat the observed features as signals of word boundaries. From this viewpoint, the phenomena described above illustrate the extent to which the grammatical structure of an utterance may determine its manifestation on the phonological level.

However, in Estonian the influence is by no means unidirectional. A tendency may be observed in the language to assimilate compounds to the canonical phonological shape of noncompound words. In the course of this assimilation, the word-initial allophone of the initial consonant of the second constituent of a disyllabic compound is replaced by an allophone in one of the three degrees of segmental quantity, and the syllabic quantity of the second syllable is reduced until it resembles that of the second syllable of a noncompound disyllabic word with overlong first syllable. In addition, these vowels and diphthongs not permitted in the second syllable of noncompounds are replaced by members of a more restricted set of syllable nuclei that may occur in such syllables.⁴ When the process of assimilation is completed, a compound is phonologically indistinguishable from noncompounds. For example, in the word *lauba*, colloquial form of *laupäev* ‘‘Saturday’’, /p/ in first segmental quantity (manifested as a voiceless lenis [p̥]) has been substituted for the initial /p/ of *päev*; /a/ has been substituted for the diphthong /äe/, which may not occur in nonfirst syllables, and the overlong quantity of the second syllable has been reduced. Examples of such assimilated compounds are particularly numerous in place names.

⁴ All nine vowels and 22 diphthongs may occur in monosyllabic words or in the first syllable of a polysyllabic word; only four of the vowels and three of the diphthongs may also occur in non-first syllables of noncompounds.

Very often the assimilatory changes in the phonological structure of a compound word are paralleled by a change in its inflection. In compounds the first element is normally not inflected, and the total word belongs to the inflectional class of the second element (in the case under consideration, a monosyllabic word). After the phonological changes have taken place, the word is inflected according to one of the patterns associated with disyllabic noncompound words. For example, the word *rätsep* "tailor", etymologically a compound of *rätt* "cloth" + *sepp* "smith", presently has a gen. sg. form *rätsepa*, part. sg. *rätsepat*, part. pl. *rätsepaid*. The form *rätsep* is the only form given in *Õigekeelsuse sõnaraamat*, ed. by E. Nurm, E. Raiet, and M. Kindlam (Tallinn, 1960). The dictionary of Wiedemann (F. J. Wiedemann, *Estnisch-deutsches Wörterbuch*, 3rd ed., Tartu, 1923; first edition, St. Petersburg, 1869) contains two parallel forms: *rätsep*, inflected as above, and *rät-sepp*, gen. sg. *-sepa*, part. sg. *-seppa*, where the second element is still inflected as the independent word *sepp*.

Compounding has been described above as a process which modifies the phonological shape of the constituents by assigning to them certain phonological characteristics of noncompound words. The formation of so-called close-knit phrases is a problem that has a certain similarity with compounding, and should be considered in this context. In Estonian, a process similar to that of compounding is used to form phonological units of considerably greater extent than a single lexical word. Some striking examples are provided by sequences where an unstressed word follows a monosyllabic word ending in a cluster /h/ + resonant. In monosyllabic words, /l m n n' v/ have voiceless allophones after /h/: such words as *mahl* "juice", *vihm* "rain", *kahv* "weir for catching crayfish" are pronounced [mah:], [vih:ɱ], [kah:v]. Syllable-initial allophones of /l m n n' v/ are voiced; if an inflectional ending beginning with a vowel is added to these monosyllabic words, the consonant cluster is divided between the syllables and a voiced allophone is used to begin the second syllable. For example, when the comitative ending *-aga* is added, the words are pronounced [mahlaɡa], [vihmaɡa], [kahvaɡa]. The same allophones are used when the words are followed by the word *aga* "but, however" in the same utterance. Such phrases contain no phonologically manifested word boundary.

The quantity patterns of such close-knit phonological phrases resemble those of noncompound words; often similar adaptive changes may be observed in the quantity structure of close-knit phrases as were described in connection with compounds.⁵

The difference between compounds and close-knit phonological phrases is often fluid. Compounds are recognizable on morphological grounds: the first element of the compound is not inflected, and the compound normally belongs to the same form class as its second member. The morphological and syntactic relationships of elements entering into the phonological units here called close-knit phrases are very loose, and new

⁵ Sandhi phenomena in the transitions between words have been described by V. Tauli in "Phonological Tendencies in Estonian", *Historisk-Filologiske Meddelelser udgivet af det Kgl. Danske Videnskabernes Selskab*, Bind 36 (1956-57), pp. 149-151 and passim. However, an exhaustive study of the structure of close-knit phonological phrases remains to be undertaken.

phonological phrases can be formed with the same degree of freedom as new sentences.

The actual phonological manifestation of a word thus depends to a considerable extent upon its position within a phonological phrase, and cannot be predicted without reference to its status as a constituent element of this phonological unit.

Trubetzkoy distinguished two types of languages: those which signal phonologically morpheme boundaries, and those which signal word boundaries.⁶ In recent transformational analysis, phonological boundaries are expressly admitted and expected at morphological boundaries.⁷ The possible existence of phonological boundaries that do not coincide with morphological boundaries appears to be left out of consideration, and by inference may be considered irrelevant to the description.

In tagmemic analysis, interpenetration of phonological and lexical hierarchies is an accepted fact; however, it appears that phonological boundary signals of higher-level units are customarily expected at grammatical boundaries.⁸ In both instances, then, the determining influence appears to proceed from the grammatical hierarchy to the phonological hierarchy.

The present paper suggests that a third language type should be added to the two types postulated by Trubetzkoy: a language in which the relationship between higher-level phonological units and higher-level grammatical units is one of coordination rather than subordination. The case for the independence of syllable boundaries relative to morpheme boundaries was made in a previous paper.⁹ It has been shown above that in Estonian, the status of an element of the grammatical hierarchy as a constituent of a higher-level unit in the phonological hierarchy often determines its phonetic manifestation. In a full description of a language of this type, higher-level morphophonemics will have to be included.

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⁶ N. S. Trubetzkoy, *Grundzüge der Phonologie* (Prague, 1939), pp. 258-261.

⁷ Cf. Condition III in the article "On Accent and Juncture in English", by N. Chomsky, M. Halle, and F. Lukoff, *For Roman Jakobson* (The Hague, 1956), p. 67: "Junctures should be distributed in a manner that is significant on higher levels. Specifically, junctures should appear only at morpheme boundaries, and different junctures should correspond, by and large, to different morphological and syntactical processes." Relationships between higher-level units — "phonological" and "grammatical" words — are defined by M. Halle in *The Sound Pattern of Russian* ('s Gravenhage, 1959), pp. 48-50; a "phonological" word is constituted by one or more morphemes preceded and followed by a word boundary. The postulation of the word boundary is determined by various grammatical considerations. The word boundary, like all other phonological boundaries, may have certain phonetic consequences; its introduction into the representation, however, is not determined by these consequences, but by a set of grammatical conditions. The boundaries of phonological words appear only at junctions between morphemes.

⁸ K. L. Pike, "Interpenetration of Phonology, Morphology, and Syntax", *Proceedings of the VIII International Congress of Linguists* (Oslo, 1958), p. 369: "Borders of high level phonological units such as the stress group may be highly relevant as contrastive characteristics of grammatical units, by indicating the placement of the breaks in the immediate constituents of large grammatical units, and hence constituting one contrastive feature of those units." Cf. also K. L. Pike, *Language in Relation to a Unified Theory of the Structure of Human Behavior*, Part III (Glendale, 1960), pp. 67-68.

⁹ Cf. Ilse Lehiste, "Acoustic Studies of Boundary Signals", *Proceedings of the 4th International Congress of Phonetic Sciences, Helsinki, 1961* (The Hague, 1962), pp. 178-187.

DISCUSSION

GAGE:

Miss Lehiste realizes, I am sure, that not all linguists consider themselves to be caught in quite as tight a bind as she suggests of having either a much more cumbersome phonology or grammatical conditioning. Many American linguists have, of course, been willing to operate on a purely phonological level not only with segments but with operators on segments such as the "plus juncture" postulated in English by Trager and Smith and others.

CARNOCHAN:

Prof. Lehiste's paper is of particular interest in showing two different approaches, one by attempting to make an all-over statement and the other by a polysystemic method, which involves considering the phonology within the grammatical framework. It appears from her presentation that a clearer picture of the analysis of Estonian can be given if this alternative method (as mentioned on p. 334) is followed.

"It has been shown above than in Estonian, the status of an element of the grammatical hierarchy as a constituent of a higher-level unit in the phonological hierarchy often determines its phonetic manifestation." While agreeing with this remark from her last paragraph, I cannot accept, without the implication given below, the conclusion that follows: "In a full description of a language of this type, higher-level morphophonemics will have to be included." It seems to me that since a polysystemic approach is accepted, in that different phonological and phonetic statements are to be given for compound as opposed to non-compound items, the question is clearly raised as to whether morphophonemics is necessary at all. Alternatively, if one is considering the phonological statements made within the grammatical framework, morphophonemics, is there any need for a further phonemic statement? It would appear that it is unnecessary to have both, and of the two, the morphophonemic statements are obviously the more useful, showing as they do the syntagmatic relations of the elements in the language material, and their value paradigmatically in the systems established for the relevant places in structure. Any subsequent phonemic statement would merely be a listing of the elements set up for the total number of systems established in the analysis; for a language of this type any over-all phonemic statement shows a list, not a system.

GRAMMATICAL CATEGORIES AND THEIR PHONETIC EXPONENTS*

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1. It seems often to be an assumption in morphemic analysis that the ideal linguistic situation is one in which each grammatical element can be directly related to a single segment that is phonologically identical in all its occurrences, a situation in which there is a one to one correspondence between morph and morpheme. But such ideal situations (typical only of 'agglutinative' languages)¹ are rare, and modifications in the theory have to be made to account for the realities of language. There would appear to be four stages of thinking about the morpheme:

(i) The morpheme has a single phonological shape. This is the "ideal" situation, e.g., the two morphemes of *dancing* which have identical shapes with those of *dances*, *dancer* and *loving*, *walking* respectively.

(ii) The morpheme has a variety of shapes, but they are in complementary distribution, e.g., the morpheme {plural} in *cats*, *dogs*, *horses*, *oxen*, etc.

(iii) The morpheme has a variety of shapes, but some of them are not susceptible to positive identification (the smallest clearly identifiable phonological segments appearing to belong to more than one morpheme), e.g., the morpheme {past} in *loved*, *baked* and *ran*, *took*.

(iv) The morpheme has no positively identifiable shapes of its own, e.g., the morphemes of person, number, tense, mood, and voice in the Latin verb.

The grammatical categories to which the title refers may be equated with the morphemes in sense (iv). The term is preferred only because of the variety of uses of the term "morpheme". In sense (i) there is, of course, no problem about the relation of the morpheme and its phonetic exponents. Each morpheme is directly relatable to recurrent segments of speech whose differences are all accounted for in phonology. But once (i) is abandoned, the relevance of grammatical patterning is admitted and it becomes reasonable to accept (iv), in which full weight is given to grammatical patterning. Analyses in which (ii) or (iii) are admitted but not (iv) are unhappy compromises.

* I use the terms "grammatical categories" and "phonetic exponents" in what is, in Britain at least, a more traditional sense than that of M.A.K. Halliday, "Categories of the Theory of Grammar", *Word*, 17, 3. My categories are linguistic — stated for a particular language; those of Halliday are metalinguistic — required for his theory. I use "exponent" in what Halliday calls an "absolute" sense — to refer to those features of the utterance that are to be "allotted" to the categories, not as indicating "relative" position on the exponence scale.

¹ Cf. C.E. Bazell, *Linguistic typology* (London, 1958).

It is in sense (iv) only that C.F. Hockett can propose a morpheme {passive} in English,² and it is in this sense only that we can interpret J. R. Firth's "abstract grammatical categories".³

What is important is the recognition that grammatical categories are abstractions *at the grammatical level*. Grammar is not an extension of phonology, and does not depend directly upon the results of phonological analysis. In theory at least grammatical statement is possible without phonology, and yet can remain formal by being directly related to the phonetic exponents.

2. The recognition of the independence of grammar suggests several corollaries.

2.1. First, it is reasonable to demand a phonological statement of a kind that is appropriate to the requirements of grammar for the renewal of connection between the grammatical categories and the language material. Phonology may thus be seen as providing the bridge⁴ between grammar and phonetics, and ought not, therefore to be undertaken without reference to the needs of grammar. This point of view reverses the usual position; instead of grammar being dependent upon phonology, phonology becomes dependent, in part, upon grammar.

Phonemic analysis is not necessarily an ideal form of phonology for this purpose. For it is essentially identificatory. The question it asks is "Is this different from that?", and it is successful when it has provided a means of identifying linguistic elements by writing them down, with economy as one of the most important measures of the degree of its success. It is by its nature, "technique for reducing languages to writing". But an identificatory system is not necessarily a good descriptive system. The number plates of a motor vehicle identify it, but give no indication of its size, colour etc. This analogy is not, of course, really fair since phonemic analysis uses descriptive features for purposes of identification. But since it concentrates on features that differentiate and identify, it is liable to obscure features of grammatical importance. Above all, because of the linear nature of writing it concentrates upon linear segments.

Two simple examples, one from English and the other from Welsh, may be taken to show how phonemic analysis obscures quite simple relations between grammar and phonetics. In English phonemics distinguishes between the morph /s/ of *cats* and the morph /z/ of *dogs* (and then proceeds to a morphophonemic statement to class them together as {s} or {plural}). Yet in a grammatically orientated statement the two would not be distinguished in the first place and there would be no need of morphophonemic restatement. For the features of voicing and voicelessness in syllable final are characteristics of the lexical items "cat" and "dog" (the morphemes {*cat*} and {*dog*}) in both the singular and the plural form. The difference between the singular forms and the plural forms consists only in the presence of a sibilant (not of a voice-

² "Linguistic Elements and their Relations", *Language*, 37.1, 52

³ Passim, especially "A Synopsis of Linguistic Theory 1930-1955", *Studies in Linguistic Analysis* (Special volume of the Philological Society) (Oxford, 1957), 13.

⁴ Cf. my "Linguistic Hierarchy", *Lingua*, 7, 3, 241.

less and a voiced sibilant respectively). The exponent of plurality (the morpheme {plural}) is, then simply sibilance and not /s/ or /z/. I will return to this example in § 3.2. In Welsh a phonemic statement of nasal mutation must deal with the morpho-phonemic alternation of /p/, /b/, /t/, /d/, /k/ and /g/ with /m̥/, /m/, /ŋ/, /n/, /ŋ̥/ and /ŋ/ respectively (examples /pen/ ‘head’, /və men/ ‘my head’, /tad/ ‘father’ /və ŋad/ ‘my father’, etc.). Yet it is obvious that the feature involved is simply that of nasality. From the point of view of articulation the only difference between the paired forms is in the position (raised or lowered) of the soft palate. Nasality and non-nasality ought then to be attributed to the grammatical features with which the mutation is linked (in the examples, nasality with the first person singular possessive pronoun); the lexical items, the morphemes {pen} and {tad} (or would it be better to refer to them as {head} and {father}?) are neutral in respect of nasality and non-nasality; their initial features are to be stated only in terms of labial vs. dental closure.

2.2. Secondly, it will no longer be required that grammar is formal in the sense that all grammatical categorisation and analysis is based upon or determined by the form of the language. All that is demanded here is that every grammatical category shall have phonetic exponents, that it can ultimately be justified by renewal of connection with the linguistic material. There are, it would seem, two versions, a weaker and a stronger, of formal grammar. In the stronger version all grammatical categories are *determined* by form. In the weaker they are merely susceptible of formal *description*. The stronger version is often advocated, but it is certainly never put fully into practice. Indeed a statement that was wholly determined by the formal characteristics of language would be a purely statistical statement of the co-occurrence of formal elements. On such an extreme view there would not even be any dividing line between phonology and grammar, but simply a series of statements about the arrangements of formal elements, taking in larger and larger elements at a multiplicity of size-levels as the analysis proceeded. The weaker view, and one that is implicit in all grammatical analysis though often denied, is that there are a variety of unstated factors determining the grammatical analysis. We do not make explicit, nor often are we aware of the ‘pre-grammatical’⁵ criteria that influence us in handling our material at the grammatical level. The explicit formal criteria of the categories are, indeed, to be clearly stated, but if we believe that these and these alone determine our method of statement we are guilty of self-deception.

It is in the recognition of the distinction between the formal nature of the description of the grammatical categories and the not-wholly-formal criteria that are implicit in the choice of grammatical statement that there is to be found the solution of the relation of grammar and meaning. For while almost all linguists today insist that their analysis is formal, it remains obvious that (i) there is a close relation between grammar categories and meaning, (ii) that a grammar that did not exhibit this close relation (e.g., a statistical statement as envisaged above) would be useless in linguistic

⁵ Cf. Bazell, *Linguistic typology*, 19.

description. For such a description must ultimately be related to language in use as well as to pure form. On the weaker interpretation of formal grammar there is no problem. Grammar is formal in the sense that all its categories have phonetic exponents. These must not, of course, be haphazard, but regularly recurrent features of form — they must involve patterning of some kind. But the criteria of choosing one method of statement rather than another are of a variety of types, and there is no shame in admitting that some of them may be semantic. Meaning is thus to some extent built into the grammar by the linguist himself, though it may also be maintained that the correlations between grammar and semantics are due in part to the place that language occupies in human activity. But if in our analytic procedures we make use (as indeed we all do) of semantic categories, we are less than honest if we dismiss these as mere “heuristic guides”. Some of them may indeed be abandoned if they cannot be justified by the linguistic material, but those that remain must surely have influenced the way in which the material was handled in the final statement.

We are seldom aware of the factors that guide us towards our choice of statement and seldom is there a clear-cut choice between two statements. But one such example may be taken from Tigre (North Ethiopian Semitic). Taking a very limited part of the language one might *prima facie* recognise three grammatical categories in terms of

- (a) unsuffixed forms e.g., /fəḥäm/ “charcoal”, /fanus/ “a lamp”
- (b) forms with suffix /ät/ e.g., /fäḥmät/ “a piece of charcoal”, /fanusät/ “a little lamp”
- (c) forms with double suffix /ätit/ e.g., /fäḥmätit/ “a little piece of charcoal”, /fanusätit/ “a tiny lamp”.

In fact, I have decided to treat /fəḥäm/ but not /fanus/, as “collective”, and /fanus/ and /fäḥmät/ as “singulative”. The terminology is obviously based upon the meaning of the forms, but the formal justification of this analysis is that the singulative forms are the forms used with numerals (all numerals, including “one”). Thus we find /säläs fanus/ “three lamps” and /säläs fäḥmät/ “three pieces of charcoal”, but not */säläs fəḥäm/. This permits a new classification:

collective	/fəḥäm/	—	charcoal;
singulative	/fäḥmät/	/fanus/	a piece of charcoal; a lamp
diminutive	/fäḥmätit/	/fanusät/	a little piece of charcoal; a little lamp
double diminutive	—	/fanusätit/	— a tiny lamp

This second classification is formal, the diminutive being defined in terms of the singulative plus suffix, and the double diminutive as diminutive plus (a further) suffix. It is, moreover, far more useful than the first, in that it fits obvious semantic categories. In this example we know what the alternatives are, but we ought to ask how often we make choices of this kind without being aware of them.

2.3. Thirdly, it seems reasonable to suggest that the exponents of grammatical categories may be stated in terms of the arrangement of pieces of language material as well as the type of material itself. On such a view to some extent at least the

traditional distinction of syntax and morphology, or of Bloomfield's selection and order or even of Hockett's item and arrangement⁶ belong to the exponential statement rather than the grammatical one. We may well decide to disassociate (in Firth's terms) order (grammatical) and sequence (exponential).⁷

This would certainly seem to be very plausible in an attempt to handle such elements of grammatical structure as subject and object. Even if there were no pronominal forms in English we could identify subject and object in terms of position of the relevant elements in the clause. Moreover since position is the exponent of subject and object, there is no reason to retain the order SPO (subject, predicator, object) in categorising an English sentence. Indeed to make quite clear the irrelevance of the order of the grammatical elements it would be better to write SOP, or PSO.

But difficulties arise once we wish to handle together sentences in which there is a difference in the position of the elements we wish to treat together in grammar, and there are degrees of difficulty, as the following cases show.

(i) For many Southern standard English speakers (but not for me) the following are normal: *He gave her the book. He gave the girl it. He gave it her.* We may handle these in terms of a sentence structure SOIP (I = indirect object). But we must state that the exponents of O and I in terms of sequence will depend on whether or not the elements whose position we are considering are both pronouns.

(ii) We may wish to handle together *He sold the rest* and *The rest he sold*. Both may be SPO, but if so we must obviously account somewhere for the difference in sequence and for the prosodic features that accompany it. Are we now to introduce a further grammatical category, or dare we suggest that the differences are so unimportant that we will ignore them in any but the most detailed statement? Alternatively we might decide here to distinguish the two sentences as SPO and OSP, making use of the otherwise irrelevant feature of the order of the grammatical elements to take care of this relatively unimportant feature.⁸

(iii) There is a very good case for attempting to handle together negative and interrogative forms in English. The exponents of both of them must be stated in terms of the occurrence of the auxiliary verbs, but whereas the further exponents of negation may be in terms of the nature of the material (*will* vs. *won't*) the further exponents of interrogation are in terms of relative position of elements (*I will* vs. *Will I?*). It seems very reasonable to recognise negation as a grammatical category, as a morpheme in English, not at all unlike past tense in its exponents (though they are more restricted in their conditions of occurrence). Ought we not equally handle interrogation in a similar way, but with exponents of a different kind?

(iv) Hockett has suggested that we might recognise a morpheme {passive} in English and the treatment of *The body was found by a troop of boy scouts* as the phrase

⁶ "Two Models of Grammatical Description", *Word*, 10, 2-3, 90-114.

⁷ "Synopsis of Linguistic Theory", 17.

⁸ Cf. C.F. Voegelin, "Contrastive and Non-contrastive Syntax", *IJAL*, 27, 4, 287-97. But is syntax ever non-contrastive?

A troop of boy scouts found the body and the morpheme {passive}. But it is not as simple as that. In a thoroughgoing grammatical analysis we must treat the two sentences as having an identical grammatical structure except in respect of the category of voice. (I should prefer a grammatical system of two terms, active and passive, but the problem remains the same if the first is “zeroed out”.) To do this we must establish elements of structure at a higher level of abstraction than subject and object, such that one of these elements corresponds to the subject in the active sentence and to the element preceded by *by* in the passive sentence, and that the other corresponds to the object in the one and the subject in the other. Such an analysis is by no means impossible, but it is clearly complicated.

3. In the previous pages it has become clear that the exponents of grammatical categories may be of two kinds — in terms of the nature of the material and in terms of its position. These may be termed (loosely) morphological and syntactical exponents.

3.1. I have dealt with problems of syntactical exponents in some detail in § 2.3. We are, it would seem, involved in practical choices. How far is it reasonable to handle features involving sequence in terms of abstract categories? Ought they all to be treated rather as of different structures to be related at the grammatical level (in terms of transformation)? The question is not (or ought not to be) “To transform or not to transform?”, but how far we can go before abandoning any attempt to deal with the material in terms of categories of the kind we are discussing here. It seems clear that at least it is probably not worth while having to restate basic categories of the kind *subject* and *object* as suggested in (iv) of § 2.3.

3.2. Morphological exponents are of three kinds.

(i) They may be phonemic or more strictly segmental (since I am not now envisaging a phonemic analysis at the level of phonology), but excluding features of pitch stress, which appear in all languages (even tone languages) never to be completely bound to consonantal and vocalic features. It is only in agglutinative languages that the exponents of grammatical categories are wholly segmental in this sense, and it is the error of early morphemic thinking to suppose that these languages represented the grammatical ideal.

(ii) They are often prosodic — non-segmental features such as voicing, nasality, frontness and backness. Mention has been made already of the English plural forms. A similar statement can be made for the past tense forms /laikt/ and /lʌvd/ where voicing and voicelessness are once again linked not to the exponent of tense, but to the lexical items “love” and “like”. Admittedly we still have to account for /hə:siz/ and /bætɪd/. But this too involves a feature of a more complex kind, but rather similar to that involved in dissimilation, the non-occurrence in immediate proximity of two articulations of the same kind (whereas the others are examples of assimilation in traditional terms). Vowel quality, and even vowel qualities in successive syllables (vowel harmony) is often an exponent of grammatical categories. In Bilin (Cushitic,

Eritrea) for instance, there are two main verb classes, and two main aspects dividing a large number of tenses (roughly corresponding in use to present and past). For one verb class the verbal endings have central vowel quality in one aspect and open quality in the second. For the other they have front quality and open quality respectively (the two classes falling together in this second aspect, there being thus only three types of vowel quality). Examples are (central) /gäbdənəx^w/ “you refused”, (open) /gäbdänäk^w/ “you refuse”, (front) /ğəbdinix^w/ “you bought”, (open) /ğəbdänäk^w/ “you buy”.

It may even be necessary to think again about our phonetic categories. For these are based upon some wide generalisations which are not always wholly applicable to a given language.⁹ For instance, soft mutation in Welsh is describable in terms of the voicing/voicelessness and plosion/friction contrasts. Examples are /pen/ “head”, /ei ben/ “his head”, /brawd/ “brother”, /ei vrawd/ “his brother”. But this involves two types of exponent. Instead they may be interpreted in terms of a single category — degree of pressure. Maximum pressure is obtained by closure at the lips and no obstruction at the larynx. A second degree is obtained by obstruction (voicing) at the larynx. Minimum pressure is obtained if there is obstruction at the larynx and no complete closure at the lips. There has been an attempt to rethink phonetic categories in acoustic terms, but even these are based upon phonemic contrasts, and it is the thesis of this paper that phonemic contrasts are often not directly relevant to the requirements of grammatical description.

(iii) Finally we must recognise that often we can do no more than choose from a list. This is a last resort to be used only when (i) and (ii) are not feasible. But in the case of *took* and *feet* there is no escape from the fact that the lexical items “take” and “foot” have forms that are not (except arbitrarily) phonologically divisible into two parts. We can often make generalised grammatical statements about large numbers of lexical items without being able to ascribe phonetic exponents to each grammatical category; hence the paradigms of the inflected languages. The simplest treatment of these is the traditional one, which lists the forms and gives instructions for the selection of one form or another.

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DISCUSSION

HALLE:

Mr. Palmer is evidently correct in asserting that in general it is impossible to associate different morphological components of an utterance with specific phoneme structures in it. This would have been possible if prefixation, infixation and suffixation were the only morphological processes admitted in a language. We know, however, numerous

⁹ Cf. “Linguistic Hierarchy” and W. Haas, “The Identification and Description of Phonetic Elements”, *TPS*, 1957.

other processes such as umlaut, ablaut, lengthening, reduplication, spirantization, etc. which destroy the one relationship between the morphological representation of a sentence and its phonemic composition. As an example of fairly complex morphological processes of this kind, I should like to discuss here the singular paradigm of two classes of Latvian feminine nouns.¹⁰

N.	māsa	māte
G.	māsas	mātes
D.	māsaj	mātej
L.	māsā	mātē
A.	māsu	māti

Begin by considering the first three cases. The natural analysis suggested is that the final vowel belongs to the stem and that the case markers are zero in the nominative, *s* in the genitive and *j* in the dative. This analysis, moreover, allows us to give a perfectly simple account of the locative and accusative; the former is produced by lengthening the final vowel; the latter by raising (making *diffuse*) this vowel. If, however, we followed the procedure of conventional grammars and described the paradigm as produced by suffixation exclusively we should need separate rules for the two classes of stem:

Nominative	—————→	{	<i>a</i> in env. mās + ———
			<i>e</i> in env. māt + ———
Genitive	—————→	{	<i>as</i> in env. mās + ———
			<i>es</i> in env. māt + ———

In sum, the assumption that the forms are produced by means of suffixation alone leads to complications in the morphophonemics which can be easily avoided if we are willing to allow processes that do not preserve a one-to-one relationship between the morphemes of an utterance and the sequence of its phonemes.

¹⁰ A detailed treatment of the Latvian declension will be found in the forthcoming monograph on the Latvian inflection by M. Halle and V. J. Zeps.

MOHAWK PREFIX GENERATION*

PAUL M. POSTAL

I. INTRODUCTION

Modern descriptive studies of Northern Iroquoian,¹ like similar grammatical studies of other languages, have centered attention on what we may call "segmentive" analyses of sentences and their parts. That is, they have concentrated on first fragmenting sentences into successive, continuous elements, words and morphemes, and then on representing sentence structure in terms of substitution classes of these elements, labelled or not, and sometimes considered to be "immediate constituents". The aim of the present paper is to show that an adequate description of Mohawk sentences is incompatible with this kind of analysis of sentences since a valid theory of Mohawk sentences requires a much more abstract conception of linguistic structure which includes constituents only indirectly related to phonetics and the notion of formal relations between sentence structures. I shall try to show this by considering the way explicit grammatical rules² yield analyses of some sentences of this Northern Iroquoian language, spoken by several thousand speakers in New York State, and Quebec, Canada.³

II. SIMPLE MOHAWK SENTENCES

We shall be primarily interested in the pronominal prefixes of Mohawk verbs. I restrict attention to simple declarative sentences which are of two main types, transitive and intransitive. The latter consists of a subject which is a nominal plus a verb in that

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¹ Cf. Floyd G. Lounsbury, *Oneida Verb Morphology*, (= *Yale University Publications in Anthropology*, 48) (1953); Wallace L. Chafe, "Seneca Morphology I-VIII", *IJAL*, 1960-61.

² The rules used in this paper are two of the types discussed by Noam Chomsky, *Syntactic Structures*² (The Hague, 1962). Phrase structure rules assign to their enumerated output a structure in the form of a labelled tree. Transformations operate on such labelled trees and produce new derived labelled trees. The rules given here are simplified in ways not bearing on the argument.

³ Field work on which this study is based was supported by several grants from the Department of Anthropology of Yale University, and by a grant from the American Philosophical Society, Committee on the Phillips Fund.

order; the former consists of this plus an object nominal which follows the verb (preceded by an automatic particle *ne-* henceforth ignored). A verb in Mohawk consists minimally of a pronominal prefix, a verb base, and a suffix. Each base is either transitive or intransitive, that is, either can or cannot occur with an object nominal. There are some sixty phonologically different pronominal prefixes. All of these occur in transitive verbs, a sub-set of fifteen of these on the regular intransitive verbs. The crucial fact about verb prefixes from the point of view of this paper is that they are strongly determined by the particular nominals with which they co-occur as subject and object (if any). This is illustrated by the following examples with the transitive verb base *nuhweʔ* "to like":

E1	<i>iʔi knuhweʔs ne- kaksəʔa</i>	"I like the child"
E2	<i>iseʔ hsnuhweʔs ne- kaksəʔa</i>	"you like the child"
E3	<i>hra-ouhaʔ hranuhweʔs ne- kaksəʔa</i>	"he likes the child"
E4	<i>ya-ouhaʔ yaonuhweʔs ne- kaksəʔa</i>	"she likes the child"
E5	<i>yaka-ouhaʔ kawawanuhweʔs ne- kaksəʔa</i>	"someone likes the child"

Here the object remains constant and the prefixes of the verb (underlined) change as the subject changes.

E6	<i>hra-ouhaʔ hraknuhweʔs ne- iʔi</i>	"he likes me"
E7	<i>hra-ouhaʔ hyanuhweʔs ne- iseʔ</i>	"he likes you"
E8	<i>hra-ouhaʔ hraonuhweʔs ne- hraksəʔa</i>	"he likes the boy"
E9	<i>hra-ouhaʔ hshakaonuhweʔs ne- yaka-ouhaʔ</i>	"he likes someone"

Here the subject remains constant and the prefix of the verb changes as the object changes.

It is impossible to permute any of the non-equivalent subjects or objects of E1-9. The results of such permutations are *non-sentences* like:

E10	* <i>iʔi knuhweʔs ne- hra-ouhaʔ</i>
E11	* <i>hra-ouhaʔ hshakaonuhweʔs ne- kaksəʔa</i>

Although we do not have the space here,⁴ it is not difficult to show that on a very abstract level the forms in E1-9 must have the following analysis:

Nominals		Verb Prefixes
<i>iʔi</i> = I + pro	<i>k</i> = I + Z	<i>hrak</i> = M + I
<i>iseʔ</i> = II + pro	<i>hs</i> = II + Z	<i>hya</i> = M + II
<i>hra-ouhaʔ</i> = M + pro	<i>hra</i> = M + Z	<i>hrao</i> = M + M
<i>ya-ouhaʔ</i> = Z + pro	<i>yao</i> = Z + Z	<i>hshakao</i> = M + F
<i>yaka-ouhaʔ</i> = F + pro	<i>kawawa</i> = F + Z	
<i>kaksəʔa</i> = Z + ksaʔa		
<i>hraksəʔa</i> = M + ksaʔa		

⁴ All topics of this paper are treated in detail in my doctoral dissertation, *Some Syntactic Rules in Mohawk* (Yale, 1962).

where *I* is the first person morpheme, *II* the second person morpheme, *M* the masculine morpheme, *Z* the zoic morpheme, *F* the feminine-indefinite morpheme, and *pro* the base morpheme of the pronouns.

Any grammar of Mohawk must therefore enumerate the verb prefixes and simultaneously restrict the occurrence of each with respect to the nominals of subject and object. Since verb prefixes are restricted with respect to both subject and object (if one is present), it is necessary that the pronominal prefixes of transitive verbs be represented by two elements, those of intransitive verbs by only one.

III. A PROPOSED GRAMMAR

In so far as the above analyses are correct, and in so far as they may be generalized to other sentences, it is clear that the prefixes of transitive verbs duplicate the prefixes of both their subject and their object in that order. Furthermore, although this has not been shown, the prefixes of intransitive verbs duplicate the prefix of their subject alone. Given these facts, we can account for the relations of prefix to nominal most elegantly with the following set of phrase structure rules:

P1 Sentence \rightarrow Nominal + Verb Phrase

P2 Verb Phrase \rightarrow Verb + (Nominal)

P3 Verb \rightarrow Base + Suffix

P4 Nominal \rightarrow Pre + Noun

P5 Noun \rightarrow $\left\{ \begin{array}{l} \text{pro} \\ \text{ksa?a} \\ \text{etc.} \end{array} \right\}$

P6 Pre \rightarrow Person + (Number)

P7

$$\text{Person} \rightarrow \left\{ \begin{array}{l} \left(\left(\left(\begin{array}{l} \text{F} \\ \text{II} \end{array} \right) + \text{I} \right) \right. \\ \left. \left(\begin{array}{l} \text{1 1} \\ \text{II} \\ \text{III} \end{array} \right) \right. \\ \left. \left. \begin{array}{l} \text{2} \\ \left(\begin{array}{l} \text{I} \\ \text{II} \\ \text{III} \end{array} \right) \\ \text{3 3} \end{array} \right) \right. \\ \left. \left. \left. \begin{array}{l} \text{4} \\ \text{4} \\ \text{4} \\ \text{4} \end{array} \right\} \right\} \text{in} \left\{ \begin{array}{l} \text{---Number} \\ \text{---} \\ \text{---} \\ \text{---} \end{array} \right\} \end{array} \right\}$$

P8 $\text{III} \rightarrow \left\{ \begin{array}{l} \text{M} \\ \text{N} \\ \text{Z} \\ \text{F} \end{array} \right\}$

P9 Number $\rightarrow \left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\}$

P10 Base $\rightarrow \left\{ \begin{array}{l} \text{nuhwe?} \\ \text{etc.} \end{array} \right\}$

Rules P1-6 and P8-10 are straightforward free expansions of constituents into other constituents. Bracketed elements on the right of rules indicate the element on the left may be expanded into any one of the set on the right. Subscripts indicate the order of expansion of brackets (from *n* to 1) and co-occurring brackets with the same sub-

script may be expanded out only line by line. Thus in P7 the sequences F+I and II+I, which represent the exclusive and inclusive we prefixes respectively, occur only in the environment of following number morphemes. II (the second person morpheme) and III (the third person constituent) occur either with or without number morphemes, but I (the first person morpheme) occurs only without following number elements. Parenthesized elements are optional in expansions. Rules P1-10 thus are simply a formal account of the description of simple Mohawk sentences given in paragraph 1 of section II. They add to this, however, a detailed development of the constituent Pre, which represents the prefixes.

It will be noted that these rules produce no prefix at all on the verb. The reason for this is the prefix agreement exemplified in E1-9. If we were to develop the verb prefixes on the verb in the phrase structure, we would have to give a mass of complex context restricted rules to develop verb prefixes in such a way as to yield the agreement equivalences. In fact, we would need an essentially separate statement for each possible prefix type as either subject or object. Since there are twenty such types, it follows that we would need at least forty separate statements. For this and many other equally important reasons, dependent on consideration of more complex parts of the grammar, no prefixes are placed on the verb in the phrase structure. Therefore, in order to account for the facts, we must add the following two ordered transformational rules to the grammar:

<p>$T_{\text{object agreement (1)}}$ Structural Description: Verb, Pre, 1 2 Structural Change: 1, 2, \Rightarrow 2+1, 2</p>	<p>$T_{\text{subject agreement (2)}}$ Structural Description: Pre, Noun, Verb 1 2 3 Structural Change: 1, 2, 3 \Rightarrow 1, 2, 1+3</p>
---	---

Like all transformations, the rules of agreement apply to particular strings of morphemes whose associated phrase structure representations meet the condition stated in the transformation's structural description. The effect of these rules is to place a duplicate of the prefix of the nominal on the verb, one in the case of intransitives since only $T_{\text{subject agreement}}$ will apply, but two in the case of transitives, with subject Pre preceding object Pre because $T_{\text{object agreement}}$ must apply first. These rules thus provide a formalized way of stating the facts of agreement in an intuitively natural way, that is, with a single statement for each type of agreement. The agreement transformations are simply a precise way of stating that "the verb duplicates the prefix of the nominal". Such simple generalizations for equivalences like grammatical agreement are impossible in phrase structure rules. It is thus easy to show that handling verb prefix-nominal relations by these two transformations is overwhelmingly simpler than a treatment of this phenomenon by means of phrase structure rules alone. But this does not really concern us here. Rather we are concerned with some of the sequences which result from the application of the rules of agreement.

Phrase structure rules P1-10 and the two rules of agreement will produce twenty

distinct Pre sequences for intransitive verbs and four hundred distinct Pre=Pre.⁵ sequences on transitive verbs. That is, in the intuitively natural way the rules given thus far permit the free combination of all prefix types as both subject and object. We are now concerned with only the four hundred transitive sequences. These correspond to only fifty-eight actual phonological verb prefixes. Thus our present rules must be radically altered, or else further rules must be added. It is not hard to show that only the latter is a reasonable alternative. For reasons of space we shall be concerned to show this for only forty-six of the four hundred transitive sequences, namely:

$$\begin{array}{l}
 1) \text{ I} = \text{I} \\
 2-5) \text{ I} = \left\{ \begin{array}{l} \text{F} \\ \text{II} \end{array} \right\} + \text{I} + \left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\} \\
 6-9) \left\{ \begin{array}{l} \text{F} \\ \text{II} \end{array} \right\} + \text{I} + \left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\} = \text{I} \\
 10-25) \left\{ \begin{array}{l} \text{F} \\ \text{II} \end{array} \right\} + \text{I} + \left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\} = \left\{ \begin{array}{l} \text{F} \\ \text{II} \end{array} \right\} + \text{I} + \left\{ \begin{array}{l} \text{plural} \\ \text{dual} \end{array} \right\} \\
 26-34) \text{ II} + \left(\left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\} \right) = \text{II} + \left(\left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\} \right) \\
 35-40) \text{ II} + \left(\left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\} \right) = \text{II} + \text{I} + \left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\} \\
 41-46) \text{ II} + \text{I} + \left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\} = \text{II} + \left(\left\{ \begin{array}{l} \text{dual} \\ \text{plural} \end{array} \right\} \right)
 \end{array}$$

Superficially there are no phonological prefixes which correspond to any of these forty-six combinations. And concomitantly it is impossible to find any sentences of the subject + verb + object form which contain such sequences. That is, there are no sentences like:

E12 * i?i Verb ne- i?i

which, if it did exist, would correspond to the abstract structure:

I + pro + I + I + Base + Suffix + I + pro, which *is* produced by rules P1-10 and the rules of agreement. We must account in our grammar for the absence of such sentences and thus simultaneously for the absence of prefixes represented by sequences 1-46. In view of the extreme naturalness and simplicity of P1-10, we wish if possible to do so without changing this group of phrase structure rules. But since these rules generate a superset of sentence structures including the non-existent group, this means accounting for the apparently non-existent sentences by transformation. This course, suggested by simplicity considerations within the phrase structure description of the prefixes proper, would be doubly attractive if there were other grammatical facts which motivated such a decision. And there are such.

IV. THE REFLEXIVE

Many transitive verb bases occur in verbs with prefixes identical to the regular *intransitive* prefixes followed by an element traditionally called the "reflexive" of the shape *atat*. Some examples are:

⁵ We use the symbol "=" as a purely notational marker of the boundary between two successive Pre constituents.

- E13 *i?i katatnuhwe?s* "I like myself"
 E14 *ise? hsatatnuhwe?s* "you like yourself"
 E15 *sawatis hraatatnuhwe?s* "John likes himself"

A key feature of verbs which occur with the reflexive is that they occur in sentences only with subject nominals. No object nominals are permitted with the reflexive. The meaning of reflexive sequences includes reflexive action on "oneself" and where the prefix is plural, action on "each other".

The grammar of Mohawk must thus introduce the reflexive with these transitive verb bases. However, the occurrence of the reflexive is subject to very strong distributional restrictions. Not only must the sentence be without object but the verb base transitive, but the base must be one whose subject and object can be identical (that is, in another sentence without the reflexive). Hence for example a transitive verb base which takes only animate subjects and only neuter objects (such as *yatho* "to plant") cannot occur with the reflexive. It is this latter restriction which shows that the reflexive and the apparently non-existent sentences considered above are related phenomena. If we have the representations provided by rules P1-10 and the rules of agreement (that is, the super-set including those like *I+pro+Verb+I+pro*), we can introduce the reflexive and account for all of its distributional peculiarities and at the same time account for the non-occurrence of sentences containing sequences 1-46 all with a single transformational rule. We need only introduce the reflexive as a substitute for the object Pre constituent of a transitive sentence verb, subject to the condition that *the subject and object of this sentence are equivalent*. As part of this rule we will drop the object Nominal. We will make this transformation optional for third person forms since sentences with identical third person subjects and objects do occur, as for example:

- E16 *sawatis hraonuhwe?s ne- sawatis* "John likes John"

But the rule will be obligatory for nonthird person sequences. Thus a sequence like *I+pro+I+I+Base+Suffix+I+pro* will obligatorily become *I+pro+I+reflexive+Base+Suffix* which accounts for the non-existent sentence E12 above as well as for sentence E13.

It may now be objected that this obligatory introduction of the reflexive in cases of equivalent non-third person subjects and objects is insufficient since most of the non-occurrent sequences in 1-46 do not correspond to complete equivalence. That is, besides cases like *I = I* which are completely identical, there are non-occurrent sequences like *I = F+I+dual*, *II = II+I+plural*, etc. However, note that in these two cases and in fact in all non-occurrent cases the Pre constituents of subject and object do share at least one Person morpheme. That is, the non-occurrent sequences correspond to the set of *Pre = Pre* sequences in which there is a shared first or second person morpheme. There is thus always at least partial equivalence and we can state our transformation in terms of this. Hence the rule which introduces the reflexive into the transitive prefix system is:

$T_{\text{reflexive}}$

Structural Description: Nominal, Pre, Pre, Base, Suffix, Nominal

1 2 3 4 5 6

Structural Change: 1, 2, 3, 4, 5, 6 \Rightarrow 1, 2, reflexive, 4, 5

obligatorily where (a) both 2 and 3 dominate sequences including either I or II and optionally where (b) 1 = 6.

The result of this rule is to substitute the reflexive for the morphemes previously dominated by the object Pre and to drop the object Nominal. Therefore by the general rule of derived phrase structure for substitutions, the reflexive in all cases *is a* Pre. $T_{\text{reflexive}}$ accounts for all the distributional restrictions on the reflexive. It accounts for the fact that the base must be transitive since only transitive bases will occur in a verb preceding an object nominal; it accounts for the fact that the base must be one which can have equivalent subject and object by virtue of the equivalence conditions; and it accounts for the absence of the object nominal by deleting same. It accounts for the presence of intransitive prefixes with the reflexive by reducing Pre = Pre to Pre = reflexive. But a single Pre is the representation which produces intransitive prefixes. At the same time, $T_{\text{reflexive}}$ accounts for the absence of sentences containing sequences 1-46 by replacing the second Pre constituent in all such cases by the reflexive and dropping the object.

The elegant description provided by $T_{\text{reflexive}}$ depends directly on the super-set of abstract sequences generated by rules P1-10 and the rules of agreement. Consider the attempt to explain the non-occurrent sentences without these abstract representations. This means consideration of a phrase structure different from P1-10 which does not produce the unwanted sequences. Note then that one of the chief reasons for the elegance of P1-10 is the fact that the same rules apply to the expansion of both the subject and object Pre so that most of the rules are consequently context-free. Since the context-free rules produce the abstract analysis, it follows that to prevent this we must introduce rules with context restrictions. That is, either subject or object Pre will now have to be developed using the expansions of the other as environment. Hence we will now need different symbols for the Person constituents of subject Pre and object Pre (let us use Person 1 and Person 2) and we will need to replace rule P6 by:

$$\text{P11a} \\ \text{Pre} \rightarrow \left\{ \begin{array}{c} \left\{ \begin{array}{c} \text{Person 2} \\ \text{Person 1} \end{array} \right\} \\ \left\{ \begin{array}{c} 2 \\ 2 \end{array} \right\} \\ 1 \end{array} \right\} + (\text{Number}) \left\{ \text{in} \left\{ \begin{array}{c} \left(\begin{array}{c} \text{Verb} \\ \text{---} \\ \text{---} \end{array} \right) \\ \left\{ \begin{array}{c} 1 \\ 1 \end{array} \right\} \end{array} \right\} \right.$$

Now we can expand either Person 1 or Person 2 but not both in the same way as we expanded Person in P7. Let us choose Person 1 for this purpose. We thus need simply a repetition of P7 with Person 1 on the left hand side. We call this rule P12a but do not bother to write it out:

P12a Person 1 \rightarrow etc.

Verb Base_{transitive} on the *right* of rule P14a are all and only such as can take identical subjects and objects. Thus such a grammar will need special rules to introduce sub-classes of transitive verb bases which can take both animate subjects and objects, both neuter subjects and objects, and so on for many sub-classifications within these classes. Many verb stems will have to be placed in several classes further complicating the situation. It would be difficult to exaggerate the complexities of a full phrase structure description of the sub-classes required for the correct introduction of the reflexive within phrase structure. All of these sub-classes are of course otherwise unmotivated and in fact conflict with the type of sub-class required in general for the description of verb bases and their relations to nominals. With these facts in mind it is of course quite clear that the simplest description is provided by rules P1-10 plus $T_{\text{reflexive}}$, rather than by P1-5, P8-10 and P11a-14a plus the mass of ad hoc sub class rules required for the reflexive. This conclusion can furthermore be greatly strengthened by a consideration of more complex verb bases which consist not of a simple verb stem like *nuhwe?* but of an incorporated noun stem plus a verb stem. Some of these occur with the reflexive and some not. It is possible to show that these differences are an automatic consequence of a grammar which contains $T_{\text{reflexive}}$. Without this rule, however, they must be handled by further ad hoc and complicated special rules.

V. CONCLUSION

Thus on simplicity grounds alone we are led to describe our chosen domain of study with $T_{\text{reflexive}}$. Note, however, that the description thus provided is also far superior in explanatory power. For reflexive sentences are of course understood *transitively* and $T_{\text{reflexive}}$ explains this by deriving them from full transitive sentences. In the grammar of P11a-14a, however, the reflexive sentences must be treated as a variety of *intransitive* structure and the transitivity of reflexive forms is wholly unexplained. The phrase structure introduction of the reflexive provides no more reason for the reflexive element to be understood as reflexively transitive than for it to be understood with a locative, iterative, or any other meaning. Thus the phrase structure introduction of the reflexive fails to account for the intuitive relations between sequences like $I + \text{pro} + I + \text{reflexive} + \text{Base} + \text{Suffix}$ and $I + \text{pro} + \text{Verb} + I + \text{pro}$ and of $M + \text{sawatis} + M + \text{reflexive} + \text{Base} + \text{Suffix}$ to $M + \text{sawatis} + \text{Verb} + M + \text{sawatis}$. $T_{\text{reflexive}}$, on the other hand, correctly reconstructs these relations by deriving the former of each pair from the latter.

We thus see that by pursuing the goal of stating the simplest possible grammar we have been led to the discovery that sentences *with* the reflexive and *without* actual objects are actually slightly deformed versions of transitive sentences with equivalent subjects and objects. But this discovery requires both transformational rules and an abstract analysis of reflexive sentences which includes many elements not directly represented in such sentences. Thus in sentence E13 above the verb is represented not

only as I+reflexive+Base+Suffix, but also at a more abstract level as I+I+Base+Suffix. Furthermore at this same abstract level this "objectless" sentence is represented with an object of the form I+pro. Needless to say, no classification of phonetic parts could ever yield such representations for sentences. Such can only be discovered, and more importantly *justified*, by a consideration of the set of structure assigning rules which enumerate Mohawk sentences and their associated grammatical analyses.

All linguists are agreed that one major task of grammatical study is to provide analyses for as many of the sentences of the language under description as possible. But in both Europe and the United States the idea has developed that the grammatical analysis of a sentence must be identified with some hierarchical classification of its parts. This idea has developed hand in hand with the notion that a grammar may be viewed as an inventory of elements and that general linguistic theory may be identified with the set of classificatory principles which can be applied to the sentences of arbitrary languages to yield the inventories which are their grammars.

This approach to grammar fails to allow for the serious study of the finite set of rules or generative grammar⁷ which accounts for the speaker's ability to produce and understand an infinite class of well-formed utterances, the overwhelming majority of which are wholly novel to his experience. If we actually believe that sentences have a certain structure, and if we take seriously the fact that there are an infinite number of sentences, then a grammar cannot be a list of elements, but instead must be a finite set of explicit rules which can automatically assign a structure to an infinite set of sentences.

As I have tried to show in the body of this paper, instead of a priori classificatory principles, it is the character of these generative rules, in particular their simplicity, which is the chief determinant of the analyses to be assigned to sentences. We can justify a proposed grammatical analysis, regardless of level, only by presenting it in conjunction with a consideration of the way this fits in with the set of rules which must *assign* this analysis, as well as those of an infinite number of other sentences. It is this which studies concentrating on "segmentive" analyses have largely failed to do and one result of this has been the provision of an incorrect analysis for the reflexive in Northern Iroquoian.

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DISCUSSION

STUART:

I should like to comment briefly on certain of Dr. Postal's metatheoretic devices. These devices (by which I mean to imply no disparagement) I shall call "ponentials"

⁷ For a bibliography of this approach to grammar see Chomsky, *Syntactic Structures*.

and "quasi-potentials". The explication of these terms, in relation to linguistic theory and theories generally, I shall leave for some more suitable occasion, at present merely remarking that whereas metatheoretic propositions belong to formal metatheoretic discourse, potentials are informal assertions of metatheory, and quasi-potentials lie, as it were, behind informal assertions, but do so transparently.

As to quasi-potentials in Postal's report, I should like to draw attention to two interesting cases of quotation in his first paragraph; the items in question are "segmentive" and "immediate constituents".

It seems to me that there is in the quotation device an attempt here to imply that such terms are of questionable value, or, possibly, that they are significant terms only if they occur in the context of transformation theory. Without presuming to have exhausted all possible alternatives by this it does seem clear, I think, that some such implication is involved, since Postal is clearly not making simple reference to these terms, i.e., he is not merely mentioning them.

With regard to the notion that these terms are significant only as used in transformation theory, I think we may say that it is impossible to assert in any significant way that these terms (or any others) are so restricted, since this would involve the belief that transformation theory exhausts the totality of conceivable things in linguistics; i.e., it requires our assent to a proposition of the form: 'for all w there is one and only one T such that x and y are significant in w ', which is, empirically speaking, unacceptable.

If, on the other hand, Postal means to imply that these terms are linguistically meaningless, inadequate, or something of that sort (and in view of the last sentence of his report, I take it that this is what he does mean), then I should ask what he thinks expressions like 'M+ksa?a' are if they are not segmentive constructs with respect to some utterance corpus?

If, however, Postal would take the view that these terms play a more intellectually and aesthetically satisfying role in transformation theory than in their occurrences elsewhere in linguistics (assuming Postal's concession that there is some elsewhere in linguistics), then I should feel inclined to say that this is a perfectly acceptable notion; but at the same time I should ask him to say precisely that, or something to the same effect, as being an acceptable basis for discussion.

Which brings me, finally, to a case of potential in Postal's last sentence. Surely Postal cannot really mean to use the word "incorrect"? Even allowing for the possibility that there might be "correct" analyses other than transformational ones, there is here a confusion as to criteria of sufficiency and necessity which I find rather disturbing.

HALLE:

I should like to illustrate with an example from English Mr. Postal's point about the manner in which a properly formulated grammar accounts for our understanding of sentences in specific ways when the sentence itself would seem to allow for several alternative interpretations. Consider the following twelve sentences:

* I help me	I help myself	you help me	* you help myself
I help you	* I help yourself	* you help you	you help yourself
I help him	* I help himself	you help him	* you help himself

The six sentences marked with asterisks are clearly ungrammatical. Note, moreover, that the grammatical and ungrammatical sentences are as it were, in complementary distribution: we can use a reflexive pronoun as object precisely with those subjects for which a nonreflexive pronoun in the object position yields an ungrammatical sentence, and vice versa. If a comparable relationship were observed between two phones, we should surely consider them allophones of a single phoneme and state their distribution by a special rule. If we follow this procedure here, we would derive the sentences with the reflexive pronoun in the object position from sentences with a nonreflexive pronoun by means of the rule:

When the object and the subject refer to the same person, the object noun must be replaced by a reflexive pronoun.

This rule converts the non-existent * *I help me* and * *you help you* into the correct *I help myself* and *you help yourself* respectively.

Observe now that the rule just stated is formally quite analogous to such a well known allophonic rule as that governing the distribution of English velar and palatal stops before back and front vowels respectively. Note, moreover, that the above rule is also supported by the fact that though both *he helps him* and *he helps himself* are grammatical, they are understood differently; i.e., exactly in accordance with the relationship expressed in our rule.

If we now write out the imperative sentences corresponding to the declarative sentences cited above we note immediately that the distribution of grammatical and nongrammatical sentences is identical with those in declarative sentences with a second person subject:

help me	* help myself	you help me	* you help myself
* help you	help yourself	* you help you	you help yourself
help him	* help himself	you help him	* you help himself

The simplest way of obtaining this distribution in the imperative sentence would be by deriving the latter from the corresponding second person declaratives. In other words the rule for the imperative might read:

Form the appropriate second person sentence, then drop the subject and the verbal tense markers.

In fact, this is the rule given in many English textbooks for foreigners, but it is also correct in some deeper sense for it immediately explains why an imperative such as "help me!" is understood as referring to a second person subject though the sentence contains no overt second person morpheme. (This analysis of the imperative was first given by N. Chomsky in his Ph. D. thesis, *The Logical Structure of Linguistic Theory*.)

THEORY AND PRACTICE IN MORPHEME IDENTIFICATION

BARBARA M. H. STRANG

The Atlantic Ocean has many things to answer for. In particular, it is to some extent responsible for two things about this paper which require a word of explanation. Most directly, it was a principal cause of my not seeing before I submitted the abstract of this paper the issue of *Language* containing Professor Hockett's paper on "Linguistic Elements and their Relations". During the intervening months I have been able to modify the paper, but its relationship to the abstract and to its title are somewhat distorted by the changes. But more generally, Hockett's work, with its emphasis on the amount of unrecorded discussion and exploration which has preceded and surrounded published work in American linguistics, brought home to me that, more even than other academic disciplines, linguistics is an iceberg of a subject. The visible peaks of publication are a tiny fraction of the hidden, solid mass of work on which they rest. Any who sail close enough to investigate what they can see are liable to have their keels holed by what they cannot see. For myself, I am ready, in full awareness, to undertake that risk. I hope that those who have seen the development of American linguistics from the inside may see some value in knowing how the published corpus of their work strikes one who is now, as all posterity must be, ignorant of all besides. It is in this hope that I venture to comment on a central problem of theory, and of the relationship of theory to practice, in all current, but especially in American, linguistics.

In general, though not in entirety, the theory of morphemes during the past decade has been dominated by the view crystallised in 1951 in the work of Harris and Trager and Smith, namely that morphemes are to be discovered distributionally, sc. by examination of the patterning of linguistic elements without recourse to meaning. Trager and Smith say, "Inspection of the linguistic material shows immediately that similar sequences or combinations of phonemes keep recurring. ... And from time to time recurrent gaps in distribution are noted. ... The recurring partials, including zero-elements, are the MORPHEMES of a language" (p. 53). This is not to say that morphemes are composed of phonemes, in the sense that Hockett objects to (1961, pp. 29ff.): the relationship of both morphemes and phonemes to the linguistic material is more indirect ("linguistic elements are associated with particular features of the speech behavior in question, and the relations among these elements are studied", Harris, 2.5). But there are difficulties about the statement. Above all, we want to know what kind of statement it is. Although not all its exponents or its critics have realised the fact, it is not an em-

pirical statement. Harris himself acknowledged that it was “impracticable” to obtain a corpus adequate to reveal all the environments of all the morphemes of a language (Appendix to 15.2). I do not believe that such a corpus is logically possible, but that is another matter. The relevant point is that in the view of its chief proponent, distributional theory is a hypothesis, a way of justifying short-cut and intuitive practices by means of appeal to a principle held to be universal and objectively valid, though not in practice capable of application, or, consequently, of verification. The motive for formulating it is clear enough – it is an attempt by linguists to liberate themselves from the vagueness, subjectivity and uncertainty of semantic criteria for the morpheme which had proved itself as a useful analytical notion.

The motivation, therefore, is practical: to improve on the scientific utility of the term *morpheme*, the implicit goal being, apparently, to make the term universally applicable, in the sense that it could apply to forms in every language and apply in such a way that there was a minimum of indeterminacy as to which forms in a language were morphemes and which were the same morpheme. In this connection it should be said that the word *morpheme*, unlike *language*, *speech*, *word*, *sentence*, etc., has no general use outside linguistics; like *phoneme*, it has been invented to do a job and only exists in relation to that job. In a sense, therefore, all discussion about the question *What is a morpheme?* is empty discussion; the real issue, as with all purely technical terms, is how the term can be consistently used with widest application and minimum indeterminacy. It is in this light that the shift from the semantic principle to the distributional principle must be viewed.

I do not wish to spend time on the philosophical shakiness of the distributional principle (because it is unverifiable); its impracticableness is admitted; W. Haas showed in 1954 that even in the hands of Harris it was not what it claimed. I shall look rather to its consequences. Its use as a theoretical justification for heuristic methods should in practice have meant that it gave roughly the same answers as the heuristic (semantic) methods, only more of them – that is, that it reduced indeterminacy of one kind or another. Generally speaking, it was very far from doing this, and the reason lay in what I shall call the two-stage notion of complementary distribution. There is first what I shall call absolute complementary distribution, discovered by overall inspection of the sample (though not quite simply by that, cf. Haas, 1954, pp. 54-60). There is then what I shall call relative complementary distribution, discovered when the sample is inspected for further patterns which only emerge in relation to the already-discovered morphemes; these inspections may be secondary, tertiary or of even remoter orders. In English the morphemes discovered by primary inspection tend to be lexical items, the others tend to be closed-system items of varying degrees of grammaticality. In practical analysis, there turns out to be much more dispute about the secondary items than about the primary ones; this confirms that they are of a different kind, as we would suspect from the fact that they yield to a different procedure. We find this duality from the other end, so to speak, in definitions of the morpheme; they are, for instance, deceptively simple “ors” that occur in Hill’s definition, “A morpheme is a recurrent sequence

of phonemes, or a class of recurrent sequences of phonemes, which contrast with other sequences or classes of sequences" (1958, p. 89) (this is a first definition, and is fined down later, but not in respect of the duality I am considering). So, as is well known, the single distinction between English /kæt/ and English /dɒg/, which does in a sense answer to inspection, is put on a par with what is also taken as a single distinction, that between /heit/ and /heitɪd/, /lʌv/ and /lʌvd/, /sɪp/ and /sɪpt/, /gou/ and /went/, /hit/ and /hɪt/ (where the lack of formal difference between the last pair would by some be interpreted as presence of zero contrasting with its absence, cf. Haas, 1957, p. 36, 1954, p. 77 Note 1). I do not want to suggest that there is anything wrong with this secondary analysis; I only want to insist that it is different from the primary analysis. This being so, there must be some impulse which drives investigators on to take the second step. That impulse, I suggest, lies in the knowledge of what more there is to be discovered – a knowledge of the kinds of patterned distribution there will be. And this knowledge is derived from experience of the kinds of meanings linguistic forms have. So knowledge of meaning does not enter in merely as a labour-saving device in procedure, but as a basic impetus in the formation of theory. In two respects the practical consequences of this are relevant to my argument. The first is that the resultant analyses are not centrally different from Bloomfieldian ones. There is a marginal difference in that such forms as English /gl-/ , /sl-/ , etc. in items like /gli:m/, /glɪmə/, /slɑɪd/, /slɪp/, etc., are not identified as morphemes; but not much more – certainly not the more we were looking for, namely, a reduction in indeterminacy. The second is that certain grammatical contrasts are interpreted as morphemic, regardless of the form through which they function. Certainly the function is in theory identified distributionally, but the impulse to distinguish one set of distributional frames rather than another is determined by what fulfils the same function, what has the same use, in the language. And in terms of a Wittgensteinian analysis of (linguistic) meaning as use it is clear that the distributional theory is a pretty thin disguise. The attempt to do without semantic criteria has led full circle to dependence on them so complete that it prevails in opposition to the direct evidence of form.

I have discussed one way in which analyses resulting from semantic and distributional theories differed otherwise than we should have expected. Quite another difference emerges in some recent work. For, as Hockett points out (1961, p. 38), the principles of morphemic analysis adopted by Hill (1958) would require him to interpret /key+tow/ as consisting of two morphemes, following a principle of phonemic distribution (presence of +) even though it is actually in opposition to the semantic criterion (unitary meaning). Opposition is clearly not the same thing as independence. The dependence of distributional analysis on meaning, in both theory and practice, makes it very odd to adopt a principle of distribution in preference to one of meaning when they clash. This brave attempt to follow a distributional principle through to its logical conclusion only serves to show how close are the ties between distributional and semantic theories, so that they cannot consistently diverge far in practice. What we have not met anywhere is evidence of the positive gain that might have been hoped for from distributional analysis in respect of reduced indeterminacy.

But although the distributional way of thinking has been widespread, it has not been universal. In various publications Hockett, for instance, has avowedly used meaning-criteria, though in a way, and with results, strikingly different from Bloomfield's. In 1958 he defined morphemes as "the smallest individually meaningful elements in the utterances of a language" (14.1). In 1961 he put forward notions of the morpheme which involve us in a fairly drastic re-interpretation of the apparently simple term *element*. It is a morpheme (?sc. {verbness}), programmed into the (phonemic) component *voicedness* in the final phoneme, which distinguishes the English verb /juwz/ from the noun /juws/; it is two morphemes, {first person singular}, {present indicative}, which are programmed into the final phoneme of Latin *amo* (later we are told that it is 'at least' two, but there is no explanation why *first person* and *singular, present* and *indicative*, are regarded as less evidently divisible than *first person singular present indicative*). *Element*, it seems, is not, or is not exclusively, as the chemical metaphor would suggest, a formal or physical component; it is also, or rather, in a somewhat glossematic sort of way, a class-meaning distinguished as having a form to express it. It can be, so to speak, "an iron" as well as "iron". There is a dimension of analysis extra to Bloomfield's. At first sight, Hockett's definition looks very close to Bloomfield's – it looks like little more than a positive transform of "a linguistic form that bears no partial phonetic-semantic resemblance to any other form" (1935, 10.2); yet it produces in application results differing from his more than those of linguists whose theories are quite unlike his.

There is another important difference. An essential feature of Bloomfield's definition is that it cannot leave any indeterminacy; if there is any degree of phonetic-semantic resemblance we are dealing with a, presumably with the same, morpheme. This high measure of determinacy is perhaps obtained at the price of excessive breadth of application; that is, in accordance with some *a priori* notion of what morphemes are one might think Bloomfield regarded too much as morphemic (this has been the case with /gl-/ and /sl-/). But one could not object without such an *a priori* notion, and the status of *a priori* notions about terms which are purely technical is at best uncertain. By contrast with Bloomfield's definition, Hockett's yields a particularly high measure of indeterminacy. That is, once we accept his special use of the term *element*, his definition of *morpheme* will yield a clear answer as to whether an element is or is not a morpheme, but it will be very feeble in indicating which forms are to be regarded as the same morpheme. Consider the revealing *presumably* in his 1958 statement, "*Bear* is presumably the same morpheme in *women bear children* and *I can't bear the pain*" (19.5). I do not accept the presumption, for in kinds of English where *bear*=*have, give birth to* is still current (it is not a spoken form for me), there would certainly be ambiguity if a woman said "I can't bear children" (or, this remark could be made in two very distinct kinds of social situation). But I am not concerned with the rightness of the presumption, only with Hockett's proceeding by way of presumption. Bloomfield's definition would yield a clear resolution (by identifying the two *bears*); I cannot see any reason to assume that study of (purely linguistic) distribution would do so (except that difference

of stress-patterns might distinguish them, though if it did, it would be on lines in conflict with the semantic distinction).

At this point, the argument might head in two directions, and I want to follow it a little in both. In the first place it is clear that the attempts to improve on Bloomfield's definition of the morpheme have failed to produce either workable and consistent procedures or that universal agreement in usage which compensates for the preservation of unrigorous notions. The work of the past thirty years cannot be undone; I am not arguing a return to Bloomfield. But I am inclined to suggest that as a starting-point he has great advantages over his successors simply because what is wrong with the Bloomfieldian morpheme is not indeterminacy but over-inclusiveness. This is a defect which suggests two lines of work which might well bring us ultimately to the same conclusion – an attempt to fine down the definition in the direction of greater exclusiveness, and an investigation of our peculiar conviction that it is over-inclusive.

Instead, and this is my second line of argument, we have tried to replace the Bloomfieldian morpheme by another kind. Since this other kind is open to criticism on so many different counts we may suspect that its appeal lies in some factor or factors not brought into the open. There is first, and openly, a laudable refusal to multiply hypotheses. Language is assumed to have organisation; this qualifies it as a subject for study. But no more than one stratum of organisation need be postulated. Description of the organisation is simplified if we speak in terms of two, conceivably more, kinds of unit, but this measure of descriptive convenience does not compel belief in more than one stratum of organisation. But it is further observable that the structures thus organised are used by members of speech-communities in a way that shows they attach distinct functions to what we may call the second-level variables – functions, that is, in which the second-level variables differ from each other and as a whole from the first-level variables. Since no satisfactory way of classifying this aspect of language has been devised it is for the moment left out of account in description, but it does start a train of thought. As every member of society has the capacity to break in on the knowledge of how second-level variables are used, the clues in every language, the signals learners latch on to in detecting patterns, must be objectively present in the speech-continuum, and must be describable by linguists. In other words, language is seen as a sort of algebraic code which must be breakable; i.e., it must be possible to establish the relationships pertaining between items in it, though it may not yet be possible to establish arithmetic values for them. Now this model of linguistic structure is something that can be tested empirically, and which unfortunately does not stand up to the test. Hockett (1961, p. 46) pointed out that the written codifications of language have never been broken in that way; though I have not space to argue the point fully, I should like to claim that this is not the way in which children break the spoken code of their L_1 , and indeed that there is no reason to believe they would acquire language (i.e., that language would exist) if that was how they had to learn. In effect, to introduce the distributional criterion is to multiply hypotheses; it is to assume that the structure of language is algebraic. From the great volume of work on child-language now available it is clear

that children do not all follow the same course in acquiring language, even within a single speech-community. But it is evident to any observer that language is acquired in social situations where the immediate pressure is to correlate spoken forms with experiences, objects, etc., in the non-linguistic world, and not primarily to contrast them with other spoken forms. The filling out of this rudimentary body of spoken forms is a process in which the linguistic material and the experience of the non-linguistic world are constantly being re-organised, in themselves and in terms of each other, in the child's mind. Whorf's vivid demonstrations of how language bears on the organisation of experience should not be allowed to eclipse completely the older, self-evident observation that experience bears on the organisation of language. There is actually very little kinship between the early process of grasping an L_1 and the later ones of grasping first numbers, then algebra. And though after the age of about six the process of developing interaction between language and experience of the non-linguistic world is greatly slowed, it does not ever entirely stop. No language is ever anything but this changing, developing nexus of systems, mutable in all its mutable users; nor can a language be properly described at any level without reference to its speakers. In restricted circumstances, and by methods of teaching that have not proved themselves the most successful, the acquisition of an L_2 may conform to the algebraic model; the acquisition of an L_1 never. It is a quite delusive sense of having escaped from the problem of describing the meaning, i.e., the use, of linguistic forms that has led to the neglect of this most central problem. At the last meeting of this Congress and elsewhere (1957, p. 102) Professor Randolph Quirk has drawn attention to the need for refining our tools for establishing functional identity; more recently Hockett has stressed the need for attention to semanticity as a universal property of language (1961, p. 45). In reinforcing these pleas I have suggested lines of work of which I believe this is the most valuable, to increase and generalise our knowledge of the acquisition of language, and to re-shape our model of linguistic structure on the basis of this understanding. In making that step forward we should be returning from a quasi-mathematical notion of linguistic study to an authentically behavioural one.

In reading recent published work in linguistics I have sensed a great unease about the essential tools of linguistic analysis and the theory in terms of which they were designed. It seems to be of the first importance that this unease should be fully aired and that the curing of it should be conducted in print in such a way as to win the widest possible agreement about basic techniques and terms. Linguists have complained bitterly throughout this century that the findings of their science have not had a proper impact upon general education or public opinion. Yet in Great Britain, the only country I can speak for, the public thirst for information about language is so far as one can tell insatiable. The trouble is that at the very threshold of the subject beginners have to be plunged into matters at once highly controversial and of the very greatest complexity. For linguists it should be a question of urgent responsibility to resolve these central and pervasive theoretical problems, and it would seem likely that the best hope of doing this would be through co-operative effort. I think, too, that we should do well to be on

the look-out for that point at which unanimity of usage is more important than perfection.

Note. – This paper was submitted before the appearance of the issue of *Word* devoted to “Linguistic Essays on the Occasion of the Ninth International Congress of Linguists” (Volume 18, Nos. 1-2, 1962) and including Professor C. E. Bazell’s article on “Meaning and the Morpheme” (pp. 132-142). There he writes scathingly of the survival of semantic theories of the morpheme. This is not the place for full-length discussion, but it seems appropriate to say that his objections do not apply to those, including myself, who hold a Wittgensteinian use-theory of meaning. Indeed, on p. 136 he explicitly distinguishes between meaning and use (“to attribute the ‘same use’ to a morpheme over a wide range of its distribution does not imply giving it some uniform meaning even over this range”). I do not claim that the adoption of a use-theory brings us to our journey’s end; only that it ensures we are on the right track.

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DISCUSSION

ROSÉN:

It would appear that here the distinction between *langue* and *parole* and between *reference* and *meaning* are not drawn with adequate neatness. This emerges principally from the example that involves the expression *cannot bear children* (p. 361), which, I think, occupies a key position in the entire argument. In one part of the cases, *bear* refers to what may be paraphrased as “being able to tolerate”, in another part of the cases to what may be paraphrased as “giving birth to”. These facts of reference belong to *parole*. The references are differently paraphrasable in non-identical, linguistically definable, environments. The various references (or paraphrases) are complementarily distributed. On the other hand, the largest common denominator of the references in all environments in which *bear* occurs is a fact of *langue*, it is the meaning of the unique morpheme *bear* in all cases. It is neither paraphrasable nor

translatable and has as its single linguistic equivalent the English morpheme *bear*. (See my remarks in *Lingua*, 8, 1959, 264–265.)

W. HAAS:

I find myself in agreement with Mrs. Strang when she says that considerations of distribution, pure and simple, will not yield the required decisions, e.g. about “bear”, and about the ambiguity of “I can’t bear children”.

What seems to be required, over and above information about the distribution of elements within sentential units, is some further information about these units themselves. We shall have to look at facts such as the following: – that while “I can’t bear children” is ambiguous, “I can’t bear *these* children” or “*He* can’t bear children” are not; that “*She wants to* bear children”, is more normal than “He wants to bear children”, and the like. It seems to me that, for morphemic identification, some notion concerning the relative normality of sentences is required, in addition to considerations of distribution within them. Mrs. Strang will probably agree that to learn to distinguish a more normal from a less normal sentence is part of the process of acquiring a language.

JAKOBSON:

Although Wittgenstein’s writings contain many valuable suggestions for the science of language, Peirce’s definition of meaning as translation from one sign into another sign seems to be the most appropriate for the linguistic treatment of semantic problems. The analysis of such equational propositions as “the bachelor is an unmarried man” and, conversely, “the unmarried man is a bachelor” enables us to apply distributional criteria in semantics also. The difference between meaning and reference is a relation between general and contextual meaning. It corresponds to the relationship between phoneme and contextual variants. Both semantic invariants and variables may be handled as an intrinsic linguistic problem, and Bloomfield’s discrimination between central and marginal meanings reflects the first step toward the inquiry into the hierarchical order of semantic variants (the Schoolmen’s *suppositiones*). One can only share with the speaker her astutely outlined semantic approach to the theory and practice in morpheme identification.

SOME ASPECTS OF BILINGUALISM AMONG CULTURED PEOPLE IN CATALONIA

A. M. BADIA-MARGARIT

1. NATURAL BILINGUALISM

In Catalonia it is necessary to make a distinction between natural bilingualism and the so called "environmental" bilingualism (§ 2). The former is found among the children of mixed couples (a Catalan father and a Castilian mother, or vice-versa) and among the children of Castilian couples residing in Catalonia. It originated from the many immigrants who came to Catalonia from other regions looking for work. This immigration can be mainly traced back to the latter part of the 19th century (the period when the great development of mechanized industry took place). It can be inferred from what has been said that working people accounted for the majority of these immigrants. Once in Catalonia, Castilian immigrants generally follow the normal course: they are gradually won over to Catalan, the social language closer to them in their adopted environment. And despite the fact that the new language is more readily assimilated by people in the country than in the cities and that the process of assimilation now proceeds at a slower pace than before 1936 (because the most efficient media for the defense and diffusion of the language are at present missing) (§ 2), the fact remains that immigrants' children are finally won over by Catalan. The situation of natural bilingualism in Catalonia is, then, an unstable one: as individuals, bilingual people are gradually drifting away from bilingualism, but as a social group, due to the constant arrival of new immigrants, there are always people in the stage of transition toward the new language (that becomes firmly established by the second generation).

2. ENVIRONMENTAL BILINGUALISM

Special attention will be given here to another type of bilingualism. While natural bilingualism stems from a sociological fact (immigration, § 1), environmental bilingualism results from a political one: the status of Catalan as a minority language for more than two hundred years, though under harsher circumstances since 1939. Deprived of the influence of schools, newspapers, and radio, Catalan (the language of family and social communication) undergoes the influence of Spanish (the language of teaching and the majority of reading material, whether professional, imaginative or

casual). From what has been said above, it is clear that the influence of the official Spanish language on the natural Catalan one is in direct relation to the education of the individual; thus, in rural circles (where only Catalan is spoken and where public schools are not very effective) there is no environmental bilingualism, while it is very pronounced among those people with a better education or even a university degree. This is not the appropriate opportunity to again refer to what has been said over the last few years with reference to bilingualism or "languages in contact".¹ Apart from our interest in the research and various experiments carried out in connection with specific language contacts,² the underlying purpose of this paper is to show how, with the superimposition of Spanish (the language of culture) on Catalan (the natural language), cultured Catalans cannot generally prevent a series of characteristic features of their natural language from appearing in their "Spanish".

3. DISTURBING EFFECT OF THE OFFICIAL LANGUAGE IN SCHOOL-AGE CHILDREN

In Spain, and therefore in Catalonia, Spanish is the only language used in schools. The same holds true for France (where there are also several linguistic minorities) in respect to French. This criterion is doubtlessly based on the conviction that the unity of the official language is thus consolidated and its diffusion promoted. The opinion of scholars (linguists, psychologists, educators) is divided on this score. Those who have studied the contact between Spanish and Catalan (A. Galí)³ or between other languages under circumstances comparable in one way or another to those prevailing in Spain,⁴ have regarded as very harmful the influence of the "second language". Lately, several linguists, while aware of the inherent difficulties, are inclined not to consider the school situation in such an unfavorable light.⁵ After objectively weighing all available data, specifically in reference to Catalan, we believe that the answer lies in "when" the introduction of the official language occurs: if from kindergarten, its influence is harmful (and the linguistic data on the "Spanish" of cultured Catalans

¹ Uriel Weinreich's book *Languages in Contact* (New York, 1953), is of fundamental importance. See also the *Proceedings of the Eighth International Congress of Linguists* (Oslo, 1958) ("Language Contact", Report by Einar Haugen, 771-785, 798-810; "Research Frontiers in Bilingualism Studies", Report by Uriel Weinreich, 786-797). There is a *Select Bibliography on Bilingualism*, compiled by Lilian Moreland (Capetown, 1948), but it is very deficient. The best bibliography on the subject is to be found in the book by Uriel Weinreich already mentioned.

² Think, for example, in Moses N. H. Hoffman, *The Measurement of Bilingual Background* (New York, 1934); Seth Arsenian, *Bilingualism and Mental Development* (New York, 1937); Werner F. Leopold, *Speech Development of a Bilingual Child*, 4 vols. (Evanston, Ill., 1939-1949).

³ A. Galí, *La mesura objetiva del treball escolar* (Barcelona, 1928); "Comment mesurer l'influence du bilinguisme", in *Le Bilinguisme et l'Education, Travaux de la Conférence Internationale tenue à Luxembourg*, 1928; "Quelques données sur le bilinguisme", in *A New World in the Making* (London, 1933).

⁴ For example, N. Toussaint, *Bilinguisme et Education* (Brussels, 1935).

⁵ See, among others, S. Arsenian, *op. cit.*, 89-123, 132-139; U. Weinreich, *op. cit.*, 73, 116-122.

will prove it, §§ 6-8); if, on the other hand, the official language is introduced in a careful and very measured way during a second stage of the elementary school, the contact can be highly beneficial.⁶ But bilingualism as it is actually practiced in those countries (such as Spain and France) where schools use exclusively the national language, is highly disturbing. Learning a language is a relatively slow process. Psychologists and educators study the phases of this process, one of which is the mastery of vocabulary, and on the basis of the words that the child knows, determine the correlation between his mental and his physical age. It might be convenient to recall the old philosophic principle which states that concepts are not well fixed until they have been expressed or at least until their mental formulation is sufficiently concrete to allow them to be expressed. And this principle cannot be applied (or at least its application is rendered difficult and is delayed) when the child comes prematurely in contact with a second language at a time when all his activity is simply geared to the perception of the outside world. For him it is, for example, a question of learning what "cheese" is (and not that "cheese" in Catalan is called *formatge*): the freer of obstacles his mind is, the easier it will be for him to establish the relationship between the "thing" and its "name" and, therefore, the quicker he will progress and learn what a "buttonhole" is, what a "cloud" is, etc. That is a period for mastering a basic vocabulary (not in English, nor in Spanish or Catalan, but in his "only" language, the language as an instrument of perception, for this reason known as the "mother tongue"). But if simultaneously to his learning that cheese is *formatge* and that buttonhole is *trau* and that cloud is *núvol*, we expect him to grasp the corresponding Spanish word (*queso, ojal, nube*), we make him waste his time and divert him from his basic activity. The mastery of a basic vocabulary in childhood is something much more important than we generally think and the coexistence of a second language does not make the task easier; if nothing else, it forces the child to make an effort often disproportionate to his capacity. We have the testimony of several elementary school supervisors in Barcelona: eight year old Catalan children (whose habitual language is Catalan) have a less rich vocabulary (i.e. they identify less objects) than the children whose parents are not Catalan, for these have been introduced normally to the progressive vocabulary patterns with no outside interference. The contact at a later stage with a second language is, on the other hand, very beneficial to the child (§ 4).

4. PERMANENT EFFECTS OF PREMATURE SCHOOL BILINGUALISM

In the course of several visits to European Interpreters' schools we have repeatedly heard the same remark: a Frenchman (educated in a good monolingual elementary school) can more easily become a good interpreter, or become a better interpreter with the same effort, than one from Luxembourg or the Swiss canton of Bern (who

⁶ The main advantage is learning a third language (U. Weinreich, *op. cit.*, 73).

has been in contact with two languages, French and German, virtually from the cradle). The reason is the same given in (§ 3): that the child has a natural language (his mother tongue) which he is slow in grasping and that trying to impose a second language too soon would amount to forcing the natural process. By introducing it gradually, the disadvantages of the second language (whose influence at an early school age we termed as disturbing) can be turned to assets: the second language (and a third, etc.) if introduced at the proper time, considerably enriches the mind and promotes the agility of language mechanisms. This is the principle applied to education in Switzerland; while the German-Swiss relies in "hochdeutsch" (as all dialects, it is based in the common language) children in Zurich, Bern, Basel, etc., are taught in their own dialect during the first stage of elementary school in order to foster the creation of sound mental habits. They later see their possibilities of expression widened with the introduction of German, French (and even other languages). This is not, however, what concerns us here, but rather the application of the case of the French, Luxemburgian, and Swiss interpreters to that of the cultured Catalans who bear the indelible stamp of a premature contact between the two languages: the Catalan speaker is thus constantly susceptible to the occurrence of the linguistic calque. The remarks made here are of a general character: two individuals with the same Spanish instruction and the same intensity of Catalan in the family can manifest the Catalan substratum in very different ways when speaking Spanish.⁷

5. CONTACT OF CATALAN AND CASTILIAN IN THE SAME INDIVIDUALS

As indicated in the title of this paper, all the examples we will quote have been taken from the speech of cultured people. We admit the difficulty of assessing the value of these examples: they are all habitual expressions among people with a university education; yet there are individuals in whose speech only some of the calques appear (and in some people none are present); this may depend on: (a) how strong the influence of Catalan is in the family life of each individual (and on their relations, reading material, professional environment, etc.); (b) how thorough his Spanish instruction has been (and again, on his social life); and (c) what is commonly known as "facility for languages" (which makes the mastery of the accent of a foreign language easier and faster for one person than for another, with the same amount of effort). Our remarks are restricted to the characteristic features of Catalan manifest in the Spanish spoken by Catalans (the same could be done with the Spanish substratum noticeable in the more colloquial Catalan, but that would be outside the scope of this paper). Both Spanish and Catalan are Romance languages linked by a common

⁷ A linguistic situation very similar to the one studied here (Catalan emerging in Spanish) is that of Provençal emerging in French, studied by Jean Séguy, *Le français parlé à Toulouse* (Toulouse, 1950).

origin, geographical proximity and a close historico-cultural relationship. But they are by no means coincident: that is why they are regarded as independent languages. There are profound differences, both in their basic patterns (of structure) and in the numerous concrete phenomena (of speech). The Catalan origin of a Spanish speaker can be ascertained both by phonetic and grammatical characteristics, as also by his use of words not listed in the dictionary of the Academy (Real Academia de la Lengua Española), and, particularly, by a series of symptomatic hesitations and a general lack of spontaneity. The two latter characteristics could prove (if many others did not) that the structure of both languages is different. Structure calques appear particularly in connection with the sounds (§ 6) and with grammatical functions (§ 8); those of speech appear mainly in connection with the form of words (§ 7) and the vocabulary.⁸ We would again point out that we are offering a general appraisal of the problem based on our personal observation of the phenomena for more than fifteen years; the very general nature of this study necessarily makes it overlook the speech of many individuals.

6. SOUNDS

Many Catalans think that the solution to the problem of their Spanish pronunciation lies in carefully distinguishing between /s/ and /θ/;⁹ this is in general relatively easy to achieve, though it becomes more difficult when /s/ and /θ/ alternate (*esas ascensiones cesan a veces si ceden . . .*). There are, however, other differentiating features in the pronunciation of the two languages. The most widespread is undoubtedly found in the pronunciation of /l/ which in Catalan has a strong velar resonance [ɫ] and constitutes one of the features that most disfigure the pronunciation of Spanish spoken by Catalans.¹⁰ The frequent assimilation of consecutive consonants that occurs in Catalan should also be noted; for example, *atlas* pronounced [adlas] in Spanish often denotes an assimilation when pronounced by Catalans who articulate *tl* just as they

⁸ The review of some vocabulary examples would take up too much space; therefore, it is impossible to deal with this aspect here. For the same reason, more data will be given about sounds and functions than about forms.

⁹ In Catalan, *c* (+ *e, i*) = *s* *cel* [sɛl] "heaven"; no distinction is then made between *cec* "blind" and *séc* "fold". It is thus natural that to the Spanish *z* (+ *a, o, u*) corresponds to the Catalan *s*: Catalan *sofre* Spanish, *azufre* "sulphur". Catalan has adopted several "castilianisms" with *s*: *sambra* (< Spanish *zambra*), *sarabanda* (< Spanish *zarabanda*) and last names ending in *-ez* are pronounced with *s*: *López* [lópəs], (compared with Catalan *Llopis*), etc. No wonder, then, that in common speech uneducated people make no distinction between *s* and *z* when speaking Spanish: *el sielo azul* (for the Spanish *el cielo azul*). See A. Badia-Margarit, *Gramática Histórica Catalana* (Barcelona, 1951), § 35, I, p. 99; also R. Lapesa, *Historia de la Lengua Española*, 4th edition (Madrid, 1959), p. 307.

¹⁰ A. Badia-Margarit, *op. cit.*, §§ 36, VI (p. 102-103), 38, V (p. 110). It is not uncommon to find that many university students do not notice the difference between the Spanish *l* and the Catalan *l* until their third year of studies at the School of Philosophy and Letters, when they begin the scientific study of Spanish phonetics. Many others who pursue other studies will perhaps never notice this difference so pronounced, on the other hand, from the articulatory point of view.

would in Catalan [*allas*].¹¹ The calque is more stable, however, in those cases where the more basic differences of the system are manifested. It is well known that Catalan has a triangular vowel system with four levels, in which /e/ and /ɛ/ and /o/ and /ɔ/ are four distinct phonemes, in contrast with the two Spanish phonemes /e/ and /o/. As a result of this, when speaking Spanish, Catalans tend to open intermediate Spanish vowels too much and pronounce /e/ as /ɛ/ and /o/ as /ɔ/;¹² this is most evident in some "castilianisms" of present-day Catalan: *no hi ha puesto* (for the Spanish *no hay sitio*) "There is no place" pronounced [pwɛstu] (the correct Catalan form is *no hi ha lloc*). Very similar to their application of their vowel system to Spanish pronunciation is their treatment of unstressed vowels; in the speech of Barcelona (and also of a good part of Catalonia) they are reduced to /a/, /i/, /u/. Catalans thus tend to pronounce the Spanish *bola* "ball" as [bɔlə] or the Spanish *duermo* "I sleep" as [dwɛrmu].¹³ With reference to the consonant system, we will only bring out the fact that in the 16th century Spanish lost the voiced sounds /z/, /ʒ/, and /ʒ/ which became fused with their corresponding voiceless sounds. In Catalan the phonemic correlation among the three pairs of voiced and voiceless sounds is still maintained, one of these pairs being /s/ - /z/; thus, Catalan (as, for example, French) voices the final -s sound because of the so-called "liaison". And Catalans do the same when speaking Spanish: *los árboles* "the trees" [loz árboles], and even *deshacer* "to undo" [dezaθer]. This is a widespread characteristic feature of Spanish as spoken in Barcelona.

7. MORPHOLOGY

There are structural characteristics: we have heard *las dientes* for the Spanish *los dientes* "the teeth" (in Catalan the word is feminine, *la dent*). But what is most frequent in morphology is to disregard the particular forms of irregular verbs and to treat them as if they were regular (just as children do): *andaron* for *anduvieron* "they walked" (as from *cantar* > *cantaron*), *reducí* for *reduje* "I reduced" (as *lucir* > *lucí*), etc.

8. SYNTACTICAL CHARACTERISTICS BELONG TO THE REALM OF STRUCTURE

Most structural patterns are common to both Catalan and Spanish and even to other Romance languages. But where the two languages differ, the Catalan speaker tends to construct the Spanish sentence following word for word the characteristic Catalan pattern. We are only reproducing some of the most typical examples: (a) the use of

¹¹ T. Navarro, *Manual de Pronunciación Española*, 5th edition (New York, 1957), § 98, p. 97; A. Badia-Margarit, *op. cit.*, § 35, II, p. 99; R. Lapesa, *op. cit.*, p. 307.

¹² R. Lapesa, *op. cit.*, p. 307

¹³ A. Badia-Margarit, *op. cit.*, § 45, p. 122; R. Lapesa, *op. cit.*, p. 307.

the article with proper nouns (in contrast with the normal absence of the article in Spanish)¹⁴ *la Maria* (for the Spanish *María*); (b) the use of the partitive as in: *tengo tres de colorados, pero de verdes solo había uno* (in Catalan *en tinc tres de vermells però de verds només n'hi havia un*) for the Spanish *tengo tres colorados, verdes sólo había uno* "I have three red ones, but there was only a green one"; (c) the use of the possessive for the personal pronoun (this is normal in Catalan: *davant teu, prop nostre, pintat meu*) which result in: *delante tuyo* for *delante de ti* "before you", *cerca nuestro* for *cerca de nosotros* "near us", *pintado mio* for *pintado por mí* "painted by me"; (d) in Catalan the pronunciation of *pensar en tu* "think of you" and *pensar amb tu* "think with you" (*en una altra persona*) "in someone else" are confused [*pənsantu*] (for phonetic reasons); this, apart from other consequences, makes the Catalan say in Spanish *pensar contigo* (for the Spanish *pensar en ti*);¹⁵ (e) the expression *per això* "therefore" can have in Catalan, when used at the end of a sentence, a definite adversative sense: *ja t'ho podies pensar, per això* "nevertheless, you could already think so"; in Spanish as spoken by Catalans the expression *ya te lo podías pensar, por eso* is frequently heard (while in Spanish *por eso* means only "therefore"); (f) the preposition *sense* "without" can be used in Catalan as an absolute adverb: *m'he quedat sense* "I have remained without"; this brings about the expression (thinking in Catalan) *me he quedado sin* (instead of *sin ello* or *sin billete*, etc., because in Spanish *sin* is only a preposition).¹⁶

9. CONCLUSIONS

The lack of space prevents us from commenting on more examples. We have, nevertheless, endeavored to present some of the most typical ones. Facts prove, then, that among cultured Catalans the contact between Catalan and Spanish results, not in a fusion of their structures, but in a superimposition: Spanish provides the cultural framework while, below it, Catalan strives to emerge and considerably affects the "form" (and for that reason, even the "function") of the language in which the speaker wishes to express himself. Being in contact with two structure systems, it is common to find that cultured Catalans master none of them and, consequently, cannot

¹⁴ S. Gili y Gaya, *Curso Superior de Sintaxis Española*, 8th edition (Barcelona, 1961), § 184, p. 243.

¹⁵ Compare, on the other hand, the prepositional system of Spanish, *soñar contigo* ("soñar a ti") "dream of you".

¹⁶ The list could be considerably lengthened. We have dealt with other cases in more than one occasion: "El subjuntivo de subordinación en las lenguas romances y especialmente en iberorrománico", *Revista de Filología Española*, XXXVII (1953), p. 95-129 (*cuando llegará* for the Spanish *cuando llegue*, compare with Catalan *quan arribarà*); "Los demostrativos y los verbos de movimiento en iberorrománico", *Estudios dedicados a Menéndez Pidal*, III (Madrid, 1952), p. 3-31 (*aquí* for *ahí* "there", *este* for *ese*, *venir* for *ir*, *llevar* for *traer*); we have also studied some differences between Catalan and Spanish from the standpoint of evolution, in *Fisiognómica comparada de las lenguas catalana y castellana* (Barcelona, 1955), pp. 34-40 (sounds), 40-43 (forms), 43-52 (sentence), 52-53 (interpretation).

express themselves easily or spontaneously in either of them: either they use a Catalan contaminated with Spanish or a Spanish patterned after Catalan structure. It is true that on other occasions the calque does not take place: the speaker can overcome the "spontaneous form" on time, but since that obstacle had not been foreseen for the rhythm of speech, a strange delay is noticeable in it: these are the symptomatic hesitations to which we have already referred (§ 5). These hesitations and the calques (and the "roundabout expression" that both entail) suggest the general lack of spontaneity mentioned above. This is the best proof that the two languages in contact are real ones which, though closely linked together by family ties, differ in their structure.

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DISCUSSION

HAUGEN:

The distinction here made between "natural" and "environmental" bilingualism would be better described by other terms. The former is used to characterize the influence of Catalan on Castilian speakers, the latter that of Castilian on Catalan speakers. Both are certainly "environmental", and given the political situation in Spain, both are "natural" enough. The terms seem to be chosen ad hoc and probably reflect the author's pro-Catalan bias. In an international terminology, we might rather speak of "childhood" vs. "adolescent" bilingualism, or thinking of the circumstances of learning, of a "familial" vs. "scholastic" bilingualism. The paper gives many interesting examples of the interference between closely related languages; similar phenomena are found in many countries when dialect speakers are taught a standard language. Perhaps the Catalan situation could be accommodated under Ferguson's recent term "diglossia".

STYLISTIC ASPECTS OF BORROWING

A Stylistic and Comparative View of American Elements in Modern German, and British English

HANS GALINSKY

As a central phenomenon of "languages in contact", borrowing or interference has secured a firm place in linguistics. In the area of English, attention first turned to this language as recipient, later as source language. Results of observation were mostly evaluated as indices to cultural influence. Attempts to find reasons for borrowing have rarely included the factor of style. Nor has general stylistics dealt extensively with the stylistic relevance of borrowings.

Stimulated by linguists and philologists such as Deroy, Erämetsä, Filipović, Ganz, Grootaers, Haugen, Leisi, Marchand, Majut, Møller, Spiess, Ullmann, Weinreich, and by explorers of style like Hatzfeld, Kayser, Ransom, Riffaterre, and, above all, Spitzer, this paper will investigate stylistic considerations promoting or resisting interference.

The example chosen is a field particularly fruitful for modern language contact study, i.e. the impact of American English (AE) on European languages. German (G) will serve as a model for inter-language contact while British English (BE) will represent the intra-language side of the phenomenon. Sources consulted comprise (1) written language: literary artefacts, travel books, scientific prose, newspapers; (2) spoken language: public addresses, public hearings, radio and TV speech, conversation.

Style in this paper is defined as a normative principle, unifying selection or creation, and, consciously or unconsciously, determining or codetermining a writer's or speaker's choice between the following: (1) available possibilities of expression within his native language; (2) these possibilities and those available in one or more foreign tongues he knows; (3) borrowings already imported, self-imported or self-created. The definition embraces levels of usage ("Stilebenen") inasmuch as they help unify utterance, both literary and non-literary. Due to the different degree of linguistic perceptiveness on the part of borrowers, and to the different kind of function which AE exercises in the two types of language contact, the term "American elements" in the Amer.-G contact situation means any element of Engl. spoken or written in America and imported by Germans (sect. I of paper). In the Amer.-Brit. contact situation, which will be treated along comparative lines, the term is restricted to Americanisms (sect. II). The method of arrangement is a gradual advance from

simpler to increasingly subtler traces of stylistic aspects of interference. The terminology used derives from Betz, Haugen and Weinreich.¹

I. 1. The most obvious stylistic function of G borrowings from AE is the impressing of Amer. atmosphere on the Germ. reader's or listener's mind. EX. 1: *Der gute Gott von Manhattan* (1958) by Ingeborg Bachmann is a radio play with a New York setting. Some figures are American, one is European, some are metaphysical powers or agents. Touches of AE are distributed across the play. Low and medium degrees of interference are preferred to avoid spoiling the universal significance of the theme of love vs. the ego. Zero interference is represented by the Amer. girl talking about a "Tanzfest in der Universität" (college dance, mixer). *Party*, one of the most frequent G borrowings from Engl., particularly characteristic of Germ. student talk, is evaded. An interference, minimal, though comprising a whole sentence, emerges in the "loan translation proper" *Grins und ertrag es* (Grin and bear it). The choice of *Grins* is affected by homophony. The idiomatic equivalent "Mach' gute Miene zum bösen Spiel!" is not made use of, as it fails to suggest American atmosphere. The phrasal loan translation *küsst mich zur Guten Nacht* ((he) kisses me good night), once again exploiting cognates, reveals the author's resistance to grammatical interference: the G function word "zur" has been retained. Lexical interference of a low degree shows in the compound *Luftmaschine* (air-conditioner). It presents neither a loan translation proper nor a loan creation such as G *Klimaanlage*, *Klimagerät*, or less habitual *Klimaregler*. It is a newly coined loan rendition. *Luft* renders "air", but *maschine* follows the French-influenced "Schreibmaschine" pattern (machine à écrire) for modern G technical tools.

Interference increases with "Sie müssen zurück um einen ganzen *Block*." The item may be interpreted either as a transfer of an AE near-homophone, belonging in the same semantic field of "building", or as a semantic extension of G *Block*, once again facilitated by field membership (cf. G *Häuserblock*). Medium strength interference enters with *Barmädchen* (barmaid), a loanblend. The first, free, morpheme is transferred; the second is reproduced with the help of G native material whose choice was determined by homophony. *Barmädchen*, however, is less common than "Barfräulein" or more elegant "Bardame". Interference of the same degree marks *Baseballspiel*, another loan blend. Maximal interference is exemplified by the transfer of "cafeteria" and "village". Integration is effected by capitalization and syntax: *Von der Cafeteria aus, bis ins Village*.

I. 2. In borrowing, a desire for truth links the function of suggesting American reality to the function of precision. Both operate jointly in good travel books. EX. 2: *Faust ging nach Amerika* (1958) by Karl Korn, a Frankfurt newspaper editor. "Amerika ist . . . von *cleveren* Köpfen auf seine top-secrets befragt . . . Genau so un senti-

¹ E. Haugen, "The Analysis of Linguistic Borrowing", *Lg.*, 26 (1950), 210-231. U. Weinreich, *Languages in Contact* (Publ. Ling. Circle of N.Y.) (New York, 1953). T. Sebeok, ed., *Style in Language* (Boston-London 1960).

mental, wie man den *Job* wechselt, falls man anderswo mehr verdienen kann, lässt man sich in den Betrieb einspannen." Within the semantic field of "intelligence" terms, *clever* has come to express a shade of meaning lacking in "klug, gescheit, verständig, intelligent, begabt, talentiert, erfahren, gewandt, listig, gewitzt, verschmitzt, pfiffig, schlau, hell (e), durchtrieben, ausgekocht, abgefieimt". Recognition of a high degree of intelligence, yet a certain reserve toward this intelligence characterize the adaptation of *clever* to a native semantic field. More strongly than *clever*, "der *Job*" retains its reference to an attitude, in this case toward paid employment, which Germans suppose to be typically American.

Precision is, of course, indispensable to scientific prose. This is borne out by AE borrowings of G sociologists, psychologists, educators et al. Right down to the level of newspaper reports and summaries of scientific developments, AE interference can be found, genuinely or supposedly adding precision to the G vocabulary. "Late developer" got rendered as *Spätentwickler*. This is a loan translation proper, at the price of syntactic irregularity. The implicitness of reflexive meaning has been borrowed as well. Superficial precision is furnished by the transfer of teen(-)ager, particularly popular in advertizing lingo. *Teenager* would suggest the existence of common "characteristics of persons in their teens". *Clever* and *Job* recur in mass circulation papers and in spoken G, while the vb. derivative *jobben* seems to be restricted to G student slang. The wide diffusion of the two transfers is partly due to the fact that they help recategorize attitudes toward such generally interesting phenomena as intelligence and labor. The fact of connotations having gained precision in written and spoken G could also be illustrated by *Manager*, *managen*, *Managerkrankheit*. In the context of a public hearing by a Bundestag committee, a Defense Ministry official was quoted to the effect (EX. 3): (Schneider bezeichnete Schloss als einen) "selbstbewusst auftretenden *Manager* . . . Die Visitenkarte hinterliess den Eindruck, dass wir es mit einem grösseren *Manager* zu tun hatten". G "Geschäftsmann" and "Geschäftemacher" do not imply being wily and high-pressured; "Manipulierer" lacks the restriction to the commercial and the industrial. Thus they cannot substitute for *Manager*.

I. 3. Precision finds its counterpart in intentional disguise. Its two special domains are: argots meant for communication between initiates; and commercial lingo needing euphemisms. EX. 4: A letter addressed to the editor of the "Frankfurter Allgemeine" by Frau Lüders, former president (by seniority) of the Bundestag: "Trotz des strikten Verböts, Bordelle und bordellähnliche Betriebe zu unterhalten, fahren immer mehr Gemeinden fort, nicht nur beide Augen gegenüber der Einrichtung immer neuer Unternehmen — jetzt unter der unverdächtigen Bezeichnung "*Appartementhaus*" — zuzudrücken . . . sie nehmen ja nicht einmal die sogenannten "*Callgirl*-Betriebe" — unter die strafrechtliche Kuppelei-Lupe. Vielleicht gewährt die Benutzung fremdsprachlicher Bezeichnungen neuerdings Straffreiheit!" *Strip-tease*, a transfer replacing G "Nackttanz" or "Entkleidungsnummer" is another pertinent example.

I. 4. Partly owing to its many syllabic inflections and to the ease with which it can

form multi-morphemic compounds, G is particularly aware of the possibility for style-oriented choice put at its disposal by the transfer of monosyllabic and disyllabic words. As a mixed value, both economic and esthetic, brevity is a natural concomitant of precision. Hence its frequent manifestations in professional speech. EX. 5:

race track: *dopen*, v. "unerlaubtes Präparat (zur Leistungssteigerung) geben"

sociology: *Massenmedien*

cybernetics: das *Bit*, *Bit-Zahl*, *Bit-Folge* "Informationselement"

politics: *Plattform* "Wahlparole, -aufruf, Parteiprogramm"

publicity: *Layout* (*Layouter*) "Gestaltungsskizze, Aufmachung (Layout-Spezialist)"

community life: *Jugendzentrum* "Jugendgemeinschaftshaus"

In professional speech and its radiations into general speech, brevity asserts itself in the growing diffusion of the cognate, agent-indicating suffix -er. It replaces polysyllabic -maschine for determinatum in compounds. EX. 6: air-conditioner *Klimaregler* (or more frequent *Klimaanlage*, *Klimagerät*), dishwasher *Geschirrspüler*, *Geschirrwäscher* (or *Geschirrspülmaschine*), teletyper *Fernschreiber*, computer *Elektronenrechner* (or *Rechenmaschine*). Poetic language even knows how to put transferred suffixations to good use. EX. 7: Bertold Brecht, *Der anachronistische Zug* (1947): "der schwarze Marketier". The phrasal loanblend is preferred to G "Schwarzmarkthändler" which lacks the American connotation and brevity in the expression of the agent.

I. 5. The metaphorical nature of many AE words furnishes yet another appeal in AE-G language contact. Literal transfers are rare, while translations abound, so that foreign origin is often not suspected. That is why in this paper metaphorical items of lexical interference have been reserved as subtler illustrations of how stylistic factors enter into borrowing. Among the travel writers it is Korn who loves *herausgefeuert* (herausgeworfen + fired), whereas Helmut Thielicke, a Hamburg theologian and author of *In Amerika ist alles anders* (1956), is attracted to *Sex-Bombe*, a loanblend using G *Bombe*. But it is the G journalist who as foreign correspondent-translator introduces, and as lead writer, radio and TV commentator introduces and diffuses most metaphorical borrowings. *Gehirnwäsche* (brainwashing), *Gipfelkonferenz* (summit conference) are but a few recent examples.

I. 6. The playful and comic touches inherent in metaphor can become a dominant stylistic reason for borrowing. A playful word formation composed of Engl. morphemes, but not existing as a compound in AE, crops up in Miss Bachmann's New York play. EX. 8: "Gib her! Herr *Missismister*." Thielicke, in the context of a hoax inflicted on two elderly Amer. ladies, one of them a nurse, presents the creation *Baby-Lavator*. His "I have *dislearnt* all my English" (EX. 9) exemplifies how the playful and the realistic combine. The reality of a language contact situation, in which a German coins *dislearn* on the familiar G pattern of "achten — verachten", and its equivalent "respect — disrespect", is made subservient to the function of conveying tone. It was purely playful in *Missismister*, humorous (and suggestive of reality) in *dislearnt*. With *Baby-Lavator*, to which might be added Korn's *Supermobil*, there enters a note of

criticism. Both creations satirize Amer. technology as an activity going to extremes: of size, as in *Supermobil*, of mechanization, as in *Baby-Lavator*.

The target of satire changes from America to "Americanized" Germany in EX. 10: "Bewerbungsschreiben", publ. in "*Darmstädter Echo*" (Oct 22, 1961). "... war ich längere Zeit als *Public-Relations-Chief* tätig, bis mir schliesslich in der gleichen Firma die Stellung des *General-Managers* angeboten wurde. Später arbeitete ich mit grossem Erfolg als *Industrial-Contact-Man*..." The technique of mockery applied to this little piece of newspaper banter is familiar. It consists of an accumulation of borrowed items, resulting in caricature. Borrowing to convey mockery can be best observed in political poetry. EX. 11: Brecht, *Verschollener Ruhm der Riesenstadt New York* (1930) "Was ist das mit den Brücken? Sie verbinden/(Die längsten der Welt!) Schuttplätze jetzt mit Schuttplätzen! /.../ Ihre Maschinen nämlich, heisst es, liegen in riesigen Haufen (den grössten der Welt!) /Und rosten/ Wie die Maschinen der alten Welt (in kleineren Haufen)." Here devices of mockery as far as they stem from AE have changed in two respects: (1) loan translations proper of a grandiloquent cliché "Die längsten der Welt /.../ den grössten der Welt". This cliché has been inserted into a context of contrary value ("in riesigen Haufen /.../ Und rosten"); (2) structural ambiguity. As a frequent potentiality of Engl. syntax and as an actual device of modern Amer. and Brit. poetry, ambiguity may have been borrowed in "Sie verbinden (Die längsten der Welt!) Schuttplätze jetzt mit Schuttplätzen!" The translated, and mockingly reversed cliché "Die längsten der Welt" might refer to the preceding "Sie" (=Brücken) or to the following "Schuttplätze" or to both. Thus borrowings to convey tone have proved to be a rich, though hidden, mine of information about stylistic aspects of interference.

I. 7. More hidden still is the last case of stylistic motivation of borrowing: variation of expression. It shows up in two types. One offers the native stock phrase or word jointly with the borrowed variant. The other omits the native item. Type I prevails in written, type II in spoken language. Type I (EX. 12: Miss Bachmann's radio play): "Die Rhapsoden in den grossen Druckereien griffen in die Setzmaschinen, kündeten die Geschehnisse und *annoncierten* Künftiges." G "kündeten" is varied with the aid of *annoncierten*, which was earlier borrowed from French. Here it renders its Engl. cognate "announced". Miss Bachmann's borrowing from English is but a semantic extension, since *annoncieren* primarily means "advertise".

Type II is illustrated best by *O.K.* It comes no less easy from the lips of Germ. diplomats than from the lips of Germ. teenagers and college freshmen. Its Germ., and global vogue is usually explained in terms of the "prestige motive", the wish of the weaker to identify themselves with the stronger. This probably is not the whole truth. The social contact situation of giving one's approval or endorsement will repeat itself every day. The monotony of such routine situations calls for a compensating variety of expression. G "Ja!, Na ja! Meinetwegen! Schön! Gut so! Schon gut! (Geht) in Ordnung" etc. constitute a fairly small semantic field. *O.K.* furnishes a welcome addition to it.

I. 8. Of these seven functions of G borrowing from AE, function 1 operates mainly in the written, esp. the literary language; 2 serves in written and spoken G, chiefly in professional speech (scientific, political, commercial); 4-6 are most comprehensive, permeating any level of speech; 7 is restricted to literary style and routine situations of colloquial speech.

II. Do these tentative results remain valid if one proceeds from present-day AE-G, inter-lingual, to AE-BE, inter-dialectal, contact? The comparison aimed at necessitates a change of method. Laying all of the evidence before the reader would exceed the space allotted to this paper. Its author, assuming the reader's familiarity with research undertaken by Berg, Foster, Kirchner, and Panten, will confine himself to presenting a few tentative conclusions.

With the exception of comic nonce-word creations and some loan renditions, all of the borrowings discussed had been introduced into G earlier. Stylistic considerations apparently have not caused their introduction, but promoted their diffusion, and perhaps raised their reputation. This result, however, might be dependent on the kind of the source materials used. To what extent stylistic factors determine or co-determine the very first loan or several early loans, cannot be ascertained without rigorous diachronic research. In BE these seven functions operate as well. The BE source materials checked so far also lack completeness. They do not allow us to study the operation of motivations, both stylistic and non-stylistic, during the initial phase of borrowing those items quoted by Berg, Foster, Kirchner, and Panten.

II. 1. Comparative observation will discover that borrowing as an aid to suggesting Amer. atmosphere is in BE and G practically confined to the written language, esp. to literary works of art, travel books and well-written newspaper reports. BE and G naturally differ in the "form of interference". BE as a dialect of Engl. is restricted to transfers: phonic (Isherwood: *gals*), lexical (Auden: *sophomore* year, *mid-western*), morphological (Auden: *unease*), to semantic extensions (*commute*, *cunning*, *make them tick*), and to shift of tone (*quit* "until recently regarded as archaic in Brit., exc. for some stock phrases"). G as a foreign language can vary between transfer, loan translation proper, loan rendition, loan creation, hybrid compounds, and semantic extension.

II. 2. Precision as a stylistic value attracts BE and G writers and speakers, esp. on the professional usage level. Newspaper language, political oratory, commercial, scientific and theatre language are the chief users of AE borrowings, and also their most efficient distributors into general speech. The resulting repatterning (in BE e.g. due to *know-how*, *round trip*, *doodle*, v.) sometimes affects the same semantic field in BE and G (cf. work and age classifications: G *Job*; BE: "This upward movement of 'job' leaves a gap for a new word" (the housewife's daily tasks = *chores*); G *Teenager*, BE *teen-ager*). There is a natural difference. With some borrowings, e.g. *Teenager*, phonic substitution has begun in G, as a sign of diffusion among unilingual speakers.

II. 3. Intentional disguise as facilitated by AE loans seems to be rarely made use of

by BE and G. Techniques of disguise differ all the same. BE imports a newly-coined compound of slightly ambiguous meaning (*boy-friend, girl-friend*); G disguises by way of lexical transfer or loanblend.

II. 4. Brevity as provided by AE interference is manifest in BE (semantic extension: *angle, build up, cut, quiz, rate, release*; shift of tone and usage level: *wed*) and G alike. Lexical brevity is more striking in polysyllabic G. Morphological brevity of the "efficiency grounds" type appears to be more cared for in written BE (scientific prose, officialese) than in written G. G has a strong native development of this type to fall back on.

II. 5. Producing visual sensuousness by metaphor is the most popular function of AE borrowings in both countries. "Mechanisms of lexical interference" show typical differences, though. In BE transfer and semantic extension are the rule (*backlog, brainwashing, bulldozer; break down, put it over, be on the air, make the grade, way of life*). In G loan translation prevails. As to acoustic sensuousness by phonetic symbolism, transfers are unavoidable. Thus BE can respond much more freely than G (cf. BE *blurb, flip-flop, fuddy-duddy, zip*). As to jazz and *quiz*, the G as well as the BE response to sound may have contributed more to the diffusion of the words than to their introduction.

II. 6. Tone-oriented BE and G borrowing prefers comedy. Targets of satire include the process of "Americanization". Political overtones are unmistakable in G literary use. Once again G as a foreign "contact partner" has more varied types of borrowing available on the lexical level. BE as a dialect of Engl. is limited to transfer and semantic extension. Transfer, however, extends to the syntactic feature of the jocular *Can that dame kiss or can she?* type. In both contact situations resistance to borrowing among the older generation assumes the attitude of mock-adoption of borrowings characteristic of the younger.

II. 7. Variation-stimulated borrowing helps break the lingual monotony of routine situations in BE and G, though more widely in BE (*I wouldn't know. Tell me . . . This is the end of the story. Or is it?*). Only to BE can AE furnish Germanic synonyms at places where Romance or Greek terms have been customary: *stand for* ("tolerate"), *stand up to* ("resist"), *breakdown* ("analysis"). AE-borrowed intensifiers (*just that, right away*), popular in BE, are lacking in G.²

II. 8. The question whether BE and G borrowing converge and diverge, not only as to individual stylistic motivations but also as to their patterns of coalescence, must await further study. The same holds true for the socio-cultural, esp. the literary conditions, favoring or resisting the operation of these motivations. One thing has become certain, though: there are stylistic aspects of borrowing. Their variety would have become clearer if this paper had included many more European languages. Partly reversing a wellknown passage from Weinreich's *Languages in Contact*, one might say in conclusion: "It would be most desirable to have a comparative study of

² B. Foster, "Recent Amer. Influence on Stand. Engl.," *Anglia*, 73 (1955), 328-360.

the reactions of all European languages to AE borrowings in the way of style; the fact that the source of the loans is the same — a common denominator in the comparison — would bring to the fore the stylistic criteria which are at play in the integration of loans.”

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DISCUSSION

HAUGEN:

One might question whether the seven stylistic functions enumerated by the author are really exclusive of one another or whether they can be unambiguously established. An example is *strip-tease*, which he classifies as “intentional disguise”, replacing *Nackttanz*. But are these really the same? In Norwegian *nakendans* is associated with primitive tribal dances, *strip-tease* with a particular form of modern entertainment. If this is true in German, perhaps the word belongs equally in the category of “precision”.

THE PROLIFERATION AND EXTENSION OF BANTU PHONEMIC SYSTEMS INFLUENCED BY BUSHMAN AND HOTTENTOT

L. W. LANHAM

1. Contact between languages is a powerful influence in language change but there is little or no evidence that phonemic systems undergo rapid and drastic changes as an immediate result of such contact. No matter how great the flood of loan words, it is possible in most contact situations for these words to be absorbed quite adequately by identifying foreign phonemes with native phonemes. The effects of contact on the sound system of a language have generally been shown to take the form of gap-filling, some redistribution of contrasts and restructuring, without, however, any profound reorganization or extension of the system. There is no known case of two systems converging to produce a third which is virtually the sum of the two. Yet something like this is the case in Xhosa and Zulu, two languages of the Nguni subgroup of Bantu languages which were profoundly influenced by Bushman and Hottentot. In their phonemic systems the influence was confined largely to the consonants and approximately 28 Proto-Nguni consonants are today matched by some 55 in Xhosa, 21 of which are almost entirely confined to the clearly marked Khoisan block in the Xhosa vocabulary. Of the 21, 15 are click consonants and this exceptionally extensive borrowing is possibly attributable to the exotic acoustic quality of these sounds and also to the peculiar social context of bilingualism in Bantu culture (see below).

SOCIAL AND HISTORICAL BACKGROUND TO CONTACT

2. Quite apart from the linguistic evidence, the records of early European travellers in south-eastern Africa, and also tribal history and legend, show the intimacy and extent of the contact with the Khoisan people. Intermarriage with Hottentots and to a lesser degree Bushmen was widespread: Xhosa tribal history tells of the absorption of several Hottentot clans (probably already possessing a strong Xhosa strain) into the extended tribe. Xhosa legend has it that a compassionate royal executioner smuggled witches and other victims intended for his spear through to a nearby Hottentot clan into which they were readily absorbed. (No Hottentot or Bushman survives in Nguni-occupied areas today.)

3. The linguistic evidence of contact is impressive and indisputable. In a dictionary count, about one seventh of Zulu and one sixth of Xhosa words have clicks, a feature clearly pointing to their origin. Non-click consonants of Khoisan origin delimit

another but smaller section of the vocabulary. Despite poor documentation of Khoisan lexicon, a fair proportion of these words can be traced immediately to their source, mainly in Korana Hottentot. (Correspondences in Bushman are more difficult to find.) E.g. H. /|gei-xa/ "magic": Xh. /i-|gíxà/¹ "witch-doctor"; H. /|ã-b/ "grass": Xh. /i|ŋhá/² "grass"; H. /|án/ "split": Z. and Xh. /|ándà/ "split". Attempts to find cognates for Nguni words in the vocabularies of Bantu languages further north uninfluenced by Khoisan, are largely fruitless, although the same languages yield numerous cognates to non-click words. Proto-Bantu roots with clicks are mainly in the *hlonipha* vocabulary – the language of respect used by women in which there is systematically organized substitution of phonemes aimed at avoiding alliteration with the names of senior male relatives, e.g. *hlonipha* /i-|à|à/ for /i-làlà/ "branch".

4. Linguistically and socially the predominating Khoisan influence was that of Hottentot rather than Bushman. This is certainly true for Xhosa (the most southerly Nguni) and probably true for Zulu. It may not be true for Swazi which seems to have a wider Bushman vocabulary than either Xhosa or Zulu, e.g. Sw. /iŋ-|gòvà/ "nose", cf. /auni /|ŋõ/ "nose", which is not shared by Xhosa or Zulu. The Hottentots as cattle keepers were culturally compatible with the Nguni whereas the Bushmen as stone-age foodgatherers were culturally far removed. The Nguni cattle, like those of the early European settlers and the Hottentots, were fair game for the bow and arrow. While initial contacts may have been peaceful there is clear evidence that widespread conflict between Nguni and Bushmen occurred later and that the extermination of the Bushmen is as much attributable to the Bantu as to the Europeans. Nevertheless, the Bantu certainly took Bushmen wives, although the reverse was probably never the case.

5. Bilingualism, which opened the door to Khoisan linguistic influence, was nurtured in a different setting from that of Western culture. In the extended polygamous family of the Nguni there was not one, but several "families", each clustering around one of several wives. The father was an occasional visitor to these families and in the linguistically formative years of the child, the predominating influence was that of the mother. Only later was the influence of the father and the extended family strongly felt. There was, therefore, an aspect of separation in time and place in acquiring the two languages which is not usually part of bilingualism in our culture.

6. The time of first contact must be placed at between 5 and 7 centuries from the present. While the first impact may have involved the Nguni as a relatively homogeneous group, very shortly afterwards separation first between the forerunners of the Xhosa-Zulu and Swazi groups, and later between Xhosa and Zulu took place. The evidence for this is: Of 2400 click words recorded in Xhosa only about 375 are shared with Zulu and some notable semantic differences are connected with this small group.

¹ /i-/ is a Xhosa noun prefix. In the examples given hyphens separate affixes. A brace is placed over digraphs and trigraphs symbolizing unit phonemes. Tonemes in Khoisan, where known, are marked /' /.

² The prefix is /in-/. The phoneme /|ŋh/ lies across a morphologic boundary.

This is to be compared with 81% of certain cognates between Zulu and Xhosa in Swadesh's 200-item test list and about the same percentage in a somewhat larger, culturally orientated word list.

Working with these lists, glottochronologic dating places Xhosa-Zulu separation at between 5 and 6 centuries and, providing the basic theory is sound, this must be taken as a highly reliable figure in view of the shallow time depth and virtual certainty in identifying cognates. Non-linguistic evidence tends to confirm this dating. The hiving off of Swazi was probably a century to 3 centuries earlier.

KHOISAN PHONEMIC SYSTEMS

7. *Bushman vocalic systems.* Most systems have 5 or 6 vowels of high frequency separated by a front: back contrast and three tongue heights. Mid-central /ə/ is reported for most dialects but is only weakly contrastive in comparison with other vowels; it is often predictable and, in many positions, highly prone to elision in colloquial style. Contrasts at syllable peaks include: (a) nasal and non-nasal vocoids; (b) a difference in vocoid length; (c) pharyngealization in a number, but not all dialects. (a) is interpreted segmentally, (b) and (c) are regarded as suprasegmental phonemes, i.e. as /·/ and /~·/. A number of investigators report diphthongs (usually \widehat{ai} , \widehat{au}) as well as vowels in sequence in successive syllables. From recordings available to us, our inclination is to accept only the latter. Sequences of two vowels without intervening consonant(s) are common, longer sequences are rare.

The following systems are typical:

		!hũ·					≠homani			
i	u	ĩ	ũ	i	u	ĩ	ũ			
e (ə)	o	ẽ	õ	e (ə)	o	ẽ	õ			
a	ǝ	ã	õ	a			ã			

8. Syllabic nasals (usually the complete series of nasal resonants) occur quite frequently with a contrastive tone as syllable nuclei, e.g. $||\widehat{ãũ}||\text{pẽ} / \text{pẽm} /$ "eat", $|\neq\eta\grave{\eta}|$ "python"; ≠homani $|\grave{\eta}cũ|$ "lip" (probably "my lip").

9. *Bushman suprasegmental phonemes.* Available evidence points overwhelmingly to three level pitches as the basis of contrast in most Bushman tonemic systems. Each syllable bears one of these pitches and pitch contrasts carry a much higher functional load than in Nguni. Rising and falling glides are reported but are probably best interpreted as clusters. No analysis of intonation is available.

10. /·/ occurs only with vowels. Pharyngealization apparently stretches over syllable peak and margin and possibly over more syllables than one, and is therefore interpreted suprasegmentally.

11. *Bushman consonantal systems.* The clicks constitute the core of the pattern of consonantal contrasts both in frequency of occurrence and in pattern symmetry. Individual dialects usually select 4 click qualities from: bilabial /⊙/, dental /|/,

alveolar /≠/, palatal /!/, lateral /||/. These clicks stand in 4 or more series, the first of which is “unmarked” and each of the remainder “marked” by some release or accompaniment such as voicing, nasality, etc. Clicks predominate over all other consonants in roots. The symmetrical pattern of clicks contrasts with somewhat ragged patterning elsewhere in the consonantal system.

||āũ||ǀǂ has a somewhat fuller but nevertheless typical system (manner-of-articulation differences are described phonetically to give some idea of phoneme representation):

Non-glottalized voiceless		≠	!		t	t̂s	t̂š	k	ǀ
Glottalized voiceless					t̂ǀ	t̂ŝ		k̂ǀ	
Non-nasal voiced	ǂ	≠ǂ	ǃ	ǃǃ	b	d	ǃz	g	
Voiceless spirant					s		š	x	h
Voiced spirant					z		ž		
Nasal resonant	ǁ	≠ǁ	ǃǁ	ǃǃǁ	m	n		ŋ	
Non-nasal resonant					w	r			

Most Bushman dialects have about the same number and type of series as ||āũ||ǀǂ. ≠*homani* of the extreme south adds an order of palatal stops. Two widely separated dialects present a separate order of post-velar stops. The voicing correlation is often weakly exploited, particularly in stops, and the functional load is usually low. The glottalized series is strong in all dialects and affricates predominate.

12. Implicit in our description of consonants is the recognition of 4 types of cluster which are found in most, if not all dialects: (1) *h* clusters (voiced and voiceless aspiration following stops,³ clicks, spirants and affricates), e.g. !hũ · /šǀhĩ/ “thing”, /šha · / “root”, /≠ǀhā · / “arm”; ||āũ||ǀǂ /≠ǁhwáí/ “pangolin”, /≠xhwí/ “tail of an animal”; /t̂shĩ-ǁhwí/ “arrow barb”; ≠*homani* /t̂shú-ǁgōā/ “God”. (2) Velar-spirant clusters ([x] and [ɣ] following stops, clicks, spirants and affricates), e.g. !hũ · /txa/ “shoot”, /≠xǁ/ “cold”, /šxō · / “bump”; ||āũ||ǀǂ /ǁgǁ/ “scorpion”, /auni /|xwà · / “dance”, /txō/ “husband’s brother”; ≠*homani* /cxābā/ “downwards”. The clustering of /k̂ǀ/ with preceding click is common and is identified as a cluster of type 2: !hũ · /|k̂ǀǁ/ “be sick”; ≠*homani* /||k̂ǀǁǁ/ “bark of tree”. (3) *w* clusters (the labio-velar glide following most other consonants and clusters), e.g. !hũ · /||ǀhwā/ “shade”, /|k̂ǀpwā/ “bubble out”; ≠*homani* /ǁǁǁǁǁhwísí/ “millipede”. (4) ǀ clusters (glottal catch following clicks and spirants), e.g. !hũ · /sǀǁ/ “blow out a fire”, /ǁǁǁǁǁ/ “suck out”; ≠*homani* /||ǀǁ/ “tsamma pip”. Glottalized stops and affricates are not counted as clusters incorporating /ǀ/ for several reasons, one being a necessity for recognizing a unique cluster of 5 consonants when the glottalized velar affricate accompanies a click.

It is essential to segment syllable margins in Bushman and our cluster analysis is generally applicable to all dialects as an overall pattern. This does not rule out the

³ Aspirated stops are not a common feature, some Bushman dialects apparently lacking them completely.

possibility, of course, of treating some of our clusters as unit phonemes in some dialects. It is a fact, however, that Bushman consonantal systems become remarkably similar when this analysis is made and click accompaniments and releases, the most prolific and diverse section of consonantal phonology, are greatly simplified in this way.

12. *Bushman phonotactics*. Syllables are defined by a contrastive tone and syllable nuclei are provided by a vowel or a syllabic nasal. Consonants cluster up to 4, usually in the order C-4-2-1-3 (see 11 above) where C = "most other consonants". A minority of syllable margins are unmarked consonantly and a sequence of 2 vowels is not uncommon.

13. *Hottentot vocalic systems*. Striking similarity exists between Hottentot and Bushman, pharyngealization being the only Bushman feature that Hottentot lacks. Korana vowels are:

i	u	ĩ	ũ
e (ə)	o		õ
a			ã

Syllable nuclei comprise a single vowel or a syllabic nasal and bear a contrastive tone. Vowoid length may be weakly contrastive and is part of the nucleus. Two vowels frequently follow each other in sequence without intervening consonants, three vowels in sequence is rare.

14. *Hottentot suprasegmental phonemes*. Contrasts in the Korana tonemic system carry a high functional load and seem to be based on 3 level tones and one rising and one falling glide. 5 different tonemes rather than toneme clusters is probably the best structural interpretation.

15. *Hottentot consonantal systems*. Khoisan systems are closely similar and the clicks again provide the central network of high frequency contrasts. A cluster interpretation for click releases and accompaniments is rejected because non-click consonants do not cluster and all click accompaniments can be identified as features marking unit phonemes, which match manner-of-articulation differences separating non-click phonemes.

Korana consonants:

Voiceless stop	\widehat{p}	$\neq p$	$!p$	$\widehat{\widehat{p}}$	p	t	k ([k ~ c])	ʔ
Glottalized affric.	$\widehat{kxʔ}$	$\neq kxʔ$	$!kxʔ$	$\widehat{\widehat{kxʔ}}$		$\widehat{tsʔ}$	$\widehat{kxʔ}$	
Voiced stop		\neq	!		b	d	g	
Aspirated stop	\widehat{h}	$\neq h$	$!h$	$\widehat{\widehat{h}}$		\widehat{th} ([t ^h ~ ts])	\widehat{kh} ([k ^h ~ kx])	h
Spirant	\widehat{x}	$\neq x$	$!x$	$\widehat{\widehat{x}}$		s	x	
Nasal resonant	$\widehat{\eta}$	$\neq \eta$	$!\eta$	$\widehat{\widehat{\eta}}$	m	n		
Non-nasal reson.						r		

In contrasting stops, voicing carries a low functional load and a good deal of sub-

stitution between a voiceless stop and its voiced cognate occurs (cf. Bushman). The “unmarked” click apparently has a weakly voiced velar stop release (Nama spelling actually writes *g*) and is, therefore, aligned here with the voiced stop series. Clicks with glottal stop accompaniment have been aligned with voiceless stops because of the presence of /ʔ/ in that series. Examples of Hottentot words: /ḵāè/ “dark”, /kḵʔ/áó “seek”, /≠hānú/ “right”, /ḵōá/ “grow gray”, /|óm/ “suck”, /≠ã/ “enter”.

16. *Comparative note.* In Hottentot, click accompaniments and releases (5 in Nama, 6 in Korana) comprise a closed series, this contrasting sharply with Bushman where varying numbers of up to 10 or 11 such accompaniments are reported according to the order and the dialect. For this reason we interpret differently what is more or less the same phenomenon in Bushman and Hottentot. Notice also that (a) the voicing contrast is weak in most Khoisan dialects, (b) the glottalized affricates are a strong series in all dialects.

NGUNI PHONEMIC SYSTEMS BEFORE CONTACT

17. Assuming that the Proto-Nguni level antedated extensive Khoisan contact, our reconstruction of the Proto-Nguni phonemic system is:

Consonants:				Vowels: ǀ ǁ	
Voiced implosive	ǃ		ǂ	i	u
Voiceless stop	p	t	č	k	e
Voiced stop	b	d	j	g	a
Aspirated stop	pʰ	tʰ		kʰ	
Voiceless spirant	f	s	š	h	Tonemes: /' ^ ^/ (/ ^/ is a falling glide)
Voiced spirant	v	z			
Voiceless lateral spir.		ɬ			
Voiced lateral spir.		L		Junctures: /+/ (a length feature)	
Non-nasal resonant	w	l	y		/'/ (open juncture)
Nasal resonant	m	n	ɲ	ŋ	

Notes: (a) Aspirated stops and plain spirants probably still clustered with preceding homorganic nasals. (b) The 2 high front and 2 high back vowels are unlikely to have merged by this time because P-B *ǀj is /zi/ in Xhosa -Zulu but /ti/ in Swazi and the /t/ = /z/ correlation is too divergent to have occurred after *ǀj, a most potent source of phonetic change, had been lost. (c) The palatal order was a post-P-B development but the large number of cognate words in Nguni associated with this order indicate that it was well established in P-N.

NGUNI PHONEMIC SYSTEMS AFTER CONTACT

18. Xhosa is the most influenced by Khoisan and its phonemic system is the most prolific of Nguni dialects:

Vowels: i u
 e o
 a

Tonemes: /' ^ ^/

Junctures: /+ ' /

Consonants:		-	+	-	+	-	+				
1. Implosive	6		A	B	č	k	C	D			
2. V'less stop	p	t	t̂s	t̂y			k̂x		!		
3. Voiced stop	b	d	d̂z*	d̂y	ǰ	g		ĝ	!ĝ	ĝ	
4. Aspirates	p̂h	t̂h	t̂sh*	t̂yh	č̂h	k̂h		ĥ	!ĥ	ĥ	
5. V'less spirant	f	s			š		x			ɬ	h*
6. V'd spirant	v	z					ɣ			L	ɦ
7. Non-nasal res.	w	l		y							
8. Nasal resonant	m	n		ɲ		ŋ		ŋ̂	!ŋ̂	ŋ̂	
9. Nasal aspir.	m̂h*			ɲ̂h				ŋ̂h	!ŋ̂h	ŋ̂h	

(Correlation according to affrication indicated by + -. * = low frequency.)

19. There are 137 contrastively different syllable margins in Xhosa and consonant clusters must be recognized. Consonant clustering is highly stereotyped however, viz.: (a) nC margins in which n = a nasal resonant of the same order as C. With 5 exceptions n combines with all of series 2, 3, 5, 6. (b) Cw and nCw margins in which w = labio-velar glide and C = all other phonemes except those of the labial order and /h, ɦ/.

20. *Xhosa phonotactics*. Syllable nuclei are provided by one vowel or syllabic /m/ and the syllable is defined by a contrastive tone. Vowels do not occur in sequence being always separated by a consonantal margin or /'/, various morphophonemic processes obviate vowel sequences which are created potentially by the grammar. Xhosa words: /úmntù/ "person", /sifàngú/ "shoe".

21. *The impact of Khoisan*. In absorbing the flood of Khoisan loans it was relatively easy for vocalic nuclei (including tones) to be matched, although vowels in sequence created a problem which was usually resolved by elision or consonantalization of one vowel, or coalescence of the 2, e.g. H. /!èi-b/ "head-cloth": Xh. /i-!híyà/ "head-cloth"; H. /|òā/ "wipe": Xh. /|ŋhwálá/ "be cleansed, pure"; H. /||kx?āē-b/ "time": Xh. /i-||á/ "time"; H. /|hāū-b/ "church": Xh. /i-|áwà/ "religious service"; H. /|pai-sen/ "sickness": Xh. /i-|ésinà/ "fever".

22. The exotic quality of clicks was too much for the existing consonantal system, however, and an almost *en bloc* acceptance was afforded to Hottentot clicks except that: (a) The Xhosa, in the same way as early investigators of Khoisan, found difficulty in identifying the alveolar as separate from the dental and palatal clicks and merged 3 Hottentot orders into 2. (b) Two manner-of-articulation contrasts that did not match those of Xhosa were rejected, viz. the velar spirant and glottalized velar affricate

(even though plain /kx̣ʔ/ was readily absorbed). The series with glottal catch accompaniment was heard as the unmarked series and it is interesting that clicks of this series still have the former as occasional free variants in Nguni today. The weakly voiced velar stop release was aligned with the series of voiced stops in Xhosa. The fact that there was imperfect matching in sound feature is indicated by, *inter alia*, a Xhosa aspirated click often corresponding to an unaspirated click in Hottentot.

23. There are no counterparts in Hottentot to Xhosa clicks of series 9 although some Bushman dialects have this type of click accompaniment. Xhosa words with these clicks are not numerous but a high percentage are traceable to Hottentot, which tends to rule out Bushman as the source, e.g. H. /!hũ/ “stamp, pulverise”: Xh. /!ŋhúšà/ “stamp maize”; H. /≠hũi-b/ “willow-tree”: Xh. /úrŋ-|ŋhùnú6è/ “willow tree”. In these source words the click with aspirated velar stop release followed by a nasal vowel, is sufficiently frequent to suggest that the Xhosa linked the nasal quality of the vowel with the click and in this way developed a new series distinguished by: “click quality, nasality, aspiration”.

24. Velar spirants and affricates (box C) were also accepted and not replaced by native phonemes, e.g. H. /kx̣ʔàì/ “laugh”: Xh. /úm-kx̣ákx̣àkx̣à/ “roars of laughter”; H. /xūrú-p/ “powder”: Xh. /i-xúlúwà/ “gunpowder”. Xhosa /kx̣/ still maintains the strong glottalization of Hottentot /kx̣ʔ/ and has extended it to other phonemes of series 2 where it is non-distinctive, but often found in free variants of /p, t, tṣ/, etc. Low frequency /ɣ/ probably resulted from the exploitation of the deeply entrenched voicing correlation in Xhosa after /x/ was absorbed.

25. Of the affricates in box A /tṣ/ is the only high frequency phoneme and has a very probable Khoisan origin, H. /tṣʔátṣʔá/ “test”: Xh. /tṣàtṣá/ “question sharply”; H. /tṣʔàō/ “fling”: Xh. /tṣàwúlà/ “sling”, Xhosa /tṣ/ often corresponds to a dental click in Hottentot and there are Xhosa dialectal variants with the /tṣ/ = /ṭ/ correspondence. This is apparently due to the fact that /tṣʔ/ and /ṭ/ were frequently dialectal substitutes in Hottentot also, and Xhosa borrowing took place at different times and different places. No P-N reflexes can be found with /tṣ/ (except with /tṣ/ in nC margins where it is the product of a morphophonemic change).

26. The palatals of box B appear in a number of words with Khosian cognates, usually with dental clicks (e.g. H. /ḥōmá-s/ “copse”: Xh. /i-týhólò/ “small thicket” — /-lo/ is the Hottentot diminutive suffix /-ro/), but a number of common P-B roots appear in Xhosa with phonemes of this order (e.g. /íli-týè/ “stone” < P-B *li-bwe). The possibility must be conceded therefore that phonemic splitting involving P-N palatals may have established this order (ct. Zulu /i(li)-čè/ “stone”) and Khoisan loans later helped to fill it out.

27. *Comparative note.* Khoisan influence therefore certainly added 21 (possibly 25) consonants to the stock of Xhosa phonemes during a contact of 3 to 5 centuries. Zulu was similarly influenced, deriving some 17 consonants out of its present total of 47 from Khoisan. Swazi was the least influenced and accepted only one order of clicks and a total of 5 Khoisan consonants.

28. *Acknowledgements.* Most examples have been culled from works of Bleek, Bourquin, Cole (whose unpublished $||\tilde{a}\tilde{u}||\rho\tilde{e}$ material is the most recent and reliable analysis of Bushman available), Doke, Kronlein, Meinhof. Examples cited reflect our own phonemicization. Phonologic patterns are similarly our interpretation.

Postscript. — There is a need to substantiate the unit-phoneme interpretation of the Check Consonants standing in series 3, 4, 8 in Xhosa as unit phonemes because a cluster interpretation of these units affects in a limited way the pattern of interference postulated here. A number of factors force the unit phoneme interpretation but only one need be cited: “Unit phoneme” or “cluster” are possible alternative solutions only as long as the two “segments” in question do not contrast in terms of “simultaneity” vs. “sequential occurrence”. In the nasals of series 8 in Xhosa, however, this contrast exists. A hypothetical minimal pair which is perfectly possible in terms of accepted distributional patterns is $[i|\widehat{\eta} a] : [i\eta||a]$. In the former the velar nasal is entirely simultaneous with the click; thus, in our analysis, it is the single phoneme $|\widehat{\eta}$. In the latter the velar nasal precedes and ends before the click is articulated; thus the sequence $/\widehat{\eta}||/$ which exemplifies typically the nC cluster — one of the two permitted cluster types of Xhosa.

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DISCUSSION

POLOMÉ:

Is the time assigned mainly on the basis of glottochronology to the first contact between the Nguni and Khoisan people (13th–15th c. A. D.) in keeping with the non-linguistic data we may have about the migration of the Nguni? Can earlier contact with click-languages like those still represented by Sansas and Hadza in Tanganyika not be assumed before the Nguni moved to the South?

LUNT:

Prof. Lanham’s glottochronological dating is interesting, but similar calculations on well documented languages spoken by peoples whose history is well known have proved to be unsatisfactory. Without repeating the many serious objections to lexicostatistical theory and practice, I should like only to call attention to one of the main conclusions reached by Fodor after a meticulous and conscientious analysis of the data of the Slavic languages: “The results of all investigations performed with the method of glottochronology are illusory and unreliable *even if they happen to agree with the correct results obtained by other means.*” (Italics mine, H.G.L.) *Studia Slavica*, 7 (Budapest, 1961), 331.

HAUGEN:

This extremely interesting and well-documented paper on language contact brings us such a startling invasion of the phonemic system of one language by another that we shall have to ponder it quite seriously. If we look at the phonemes borrowed, it is noteworthy how many of them are written in the text with complex symbols, e.g. / \widehat{ts} \widehat{dz} \widehat{tsh} $\widehat{!h}$ $\widehat{!n}$ $\widehat{!nh}$ / etc. The result is a consonant system of rather extraordinary complexity. Before contact the system had a more "normal" appearance. It is tempting to ask whether the complexity is not an artifact of the investigator's phonemic analysis. Since virtually all of the borrowed "phonemes" consist of features which also exist as simple phonemes, it would be easy to analyze them as clusters, merely by omitting the arch written above them. This would of course reduce somewhat the "stereotyped" nature of consonant clustering and complicate the phonotactics. But to me it seems probable that new clusters are added to a language rather more easily than new phonemes. In the case of the clicks there are only three new items of learning, with one new feature between them, viz. that of indrawn breath.

HARDMAN-DE-BAUTISTA:

I have come across a very similar situation in the languages of the Andes Mountains of Perú – Jaqaru, Aymará, and Quechua. My present hypothesis is that those dialects of Quechua which have aspirated and glottalized consonant series in the voiceless stops borrowed these ten consonants from the Jaqaru-Aymará languages at an early period, probably prior to the Inca Empire. (This hypothesis contradicts an earlier one that those dialects not having aspiration and glottalization had lost those features.) The borrowings are unit phonemes within the Quechua phonological structure. They are extensive within the vocabulary of Quechua, but they do not enter into any of the morphology (suffixes), and no more than one of the aspirated or glottalized consonants occurs in a single root. These two latter limitations are not, of course, operative in the Jaqaru and Aymará languages.

TRANSFERT DU SYSTÈME PHONOLOGIQUE DE BLAESHEIM SUR UNE AUTRE LANGUE, LE FRANÇAIS

MARTHE PHILIPP

Lorsqu'on veut étudier le comportement linguistique d'une communauté bilingue, il faut tenir compte de plusieurs facteurs importants. Sur le plan du système phonologique (2ème articulation) l'idiome appris en premier lieu peut "marquer" le sujet bilingue pour toute la vie et quand il apprend une seconde langue plus tard, il est obligé de se réadapter à d'autres habitudes articulatoires. Cette adaptation à un deuxième système de phonèmes est souvent incomplète; elle dépend de l'âge du sujet au moment où il commence à apprendre la seconde langue, de l'utilisation plus ou moins fréquente de cette langue, du milieu linguistique dans lequel le locuteur se trouve placé, enfin de son tempérament personnel le prédisposant ou non à manier à la fois deux systèmes phonologiques différents. Même lorsque les bilingues se servent très souvent de leur seconde langue, même s'ils n'utilisent plus que très peu leur première langue, ils peuvent conserver des particularités de leur système primaire en parlant la seconde langue. Selon les sujets, ces interférences sont plus ou moins nombreuses, plus ou moins frappantes: on dit alors qu'ils ont l'"accent", c'est-à-dire une prononciation caractéristique d'une province ou d'un pays déterminé.

Pour étudier ces phénomènes, nous avons choisi un cas extrême, afin de pouvoir observer le plus grand nombre d'interférences possible. Dans le cadre d'une étude approfondie du système phonologique d'un parler alsacien (Blaesheim, situé à 15 km de Strasbourg), nous avons été amené à observer également la prononciation française des bilingues de ce village dont la première langue est un dialecte germanique et la seconde le français, appris en classe depuis l'école maternelle. Pour ces locuteurs, le français est la langue écrite, la langue officielle. En réalité, ils utilisent très peu cette deuxième langue, le seul idiome parlé au village étant le dialecte alsacien. Ils ne se servent du français que lorsqu'ils rencontrent des personnes ne sachant pas le dialecte. Ainsi qu'on peut l'observer en étudiant leur prononciation française, ces bilingues, tout en ayant un vocabulaire suffisant pour manier le français avec une relative aisance, ont, au contraire, beaucoup de mal à s'habituer au système phonologique du français. A dessein nous avons choisi des sujets non intellectuels, absolument pas préoccupés par des problèmes linguistiques; pour eux, la langue est un instrument servant à la communication, sans plus.

La comparaison des systèmes phonologiques du parler de Blaesheim, de celui du

français et de celui du français tel qu'il est prononcé par les bilingues permet de constater que ces derniers conservent le système de leur première langue en parlant la seconde. Pourtant les deux systèmes n'ont pas du tout la même structure. Le système vocalique de Blaesheim est caractérisé par une corrélation de quantité, alors qu'en français la quantité n'est pas distinctive. Abstraction faite des oppositions de quantité, le parler de Blaesheim et le français ont le même nombre de timbres vocaliques :

Blaesheim				Français			
1.	i	ü	+	1.	i	ü	u
2.	ĩ		ɯ	2.	é	œ	ó
3.	ɛ	ö	o	3.	è	œ	ò
4.		ɛ	ö	â		à	á
5.		a					

Le bilingue substitue aux voyelles françaises les phonèmes du système de son parler local qui s'en rapprochent le plus. Dans l'ensemble, les voyelles alsaciennes semblent relâchées par rapport aux voyelles prononcées par un Français unilingue. Le degré no 2 du système de Blaesheim $i \text{ } \mathfrak{u}$ n'existe pas en français où $i \text{ } \ddot{u}$ ont une aperture analogue. Le bilingue de Blaesheim remplace u français par \mathfrak{u} ouvert de son propre parler; bien que le patoisant dispose d'une "case vide" au degré no 1, il est incapable d'ajouter u fermé au système de sa première langue: "jour blouse" sont articulés *šūr blūs*. Cette substitution ne gêne pas l'intercompréhension, car le système français ne comporte qu'un seul u . Pour les voyelles françaises $é \text{ } œ \text{ } ó$ (degré no 2) le patoisant utilise $ɛ \text{ } ö \text{ } o$ (degré no 3) du système de Blaesheim qui représentent des timbres plus ouverts que celles du système du français: "pré, berceuse, chose" sont prononcés *brɛ¹, bɛrsöš, šos*. De même $è \text{ } œ$ (degré no 3) français sont remplacés dans la prononciation du bilingue par $ɛ \text{ } ö$ de Blaesheim aux timbres un peu plus fermés que les phonèmes français. Le patoisant a en quelque sorte "rapproché" les deux degrés d'aperture du système vocalique du français, car dans le système de son parler les voyelles se répartissent sur cinq degrés d'aperture et non sur quatre comme en français. Il prononce "sème coeur" *sēm ghōr*. Il est assez surprenant que le bilingue ne se serve pas de \grave{a} pour \grave{o} ouvert français; au lieu d'utiliser l'opposition $o - \grave{a}$ du système de son parler qui rendrait de manière satisfaisante l'opposition $ó - \grave{o}$ du français, il confond les deux timbres de o et les remplace par o du système de Blaesheim qui, par rapport aux timbres français, est un o moyen, plus ouvert que $ó$ fermé français et plus fermé que \grave{o} ouvert français. Il prononce avec la même voyelle "horloge" *orloš* et "Vosges" *vōš*, "mort" *mōr*. Le bilingue pourrait se servir de \grave{a} pour rendre \acute{a} postérieur français; il n'en est rien. Jamais le patoisant ne se sert de \grave{a} quand il parle le français.

Au total, le bilingue n'utilise qu'une partie des phonèmes vocaliques de son système primaire en français; il n'en conserve pas moins son système à cinq degrés d'ouverture avec \mathfrak{u} :

¹ *b d g* sont des "douces sourdes", voir p. 395.

Blaesheim			Français du bilingue		
i	ü		i	ü	
i		u	+		u
ɛ	ö	o	ɛ	ö	o
ɛ	ö	â	ɛ	ö	+
	a			a	

Comme le système de Blaesheim est composé de voyelles orales seulement, les nasales françaises sont difficilement réalisables pour le bilingue. Dans sa prononciation, *õ* se rapproche de *u*, *ã* de *â*, *ẽ* de *ɛ*, *ã* de *ö*: “monte” *mũd*, “mĩnce” *mẽs*. En finale absolue, *ẽ* et *è* sont confondus: “lait” et “lin” *lẽ*.

Les interférences que l'on peut observer dans le français des bilingues ne concernent pas seulement l'inventaire des phonèmes des deux langues en contact, mais aussi le fonctionnement des deux systèmes phonologiques. Le bilingue transpose non seulement les phonèmes mais aussi les combinaisons de son système en français. De nombreuses combinaisons, théoriquement possibles pour le patoisant sont remplacées par d'autres qui lui sont plus familières. Quelques autres, en revanche, bien que jamais utilisées dans le parler, sont réalisées sans difficulté. Parmi les nombreuses interférences dues au fonctionnement différent des deux systèmes phonologiques, nous donnerons en exemple celles qui concernent la durée vocalique. A Blaesheim, on observe de nombreuses oppositions voyelle brève + consonne ~ voyelle longue + consonne. En français, la voyelle est longue devant consonne “allongeante”, surtout devant *r z ž v*, brève devant les autres consonnes dites “non-allongeantes”. Le rôle fonctionnel de la quantité est différent dans les deux idiomes. Un des traits les plus caractéristiques de l’“accent alsacien” est certainement l’allongement de la voyelle dans des positions où le Français unilingue prononce une brève. Devant les consonnes sonores *b d g* devant lesquelles la voyelle est brève en français le bilingue prononce une voyelle longue: “bague” *bāg*, “laidc” *lēd*, “robe” *rōb*. Or, dans le parler de Blaesheim, les occlusives sourdes (et non sonores comme en français) se combinent aussi bien avec les voyelles longues qu’avec les brèves; il ne s’agit donc pas d’une simple substitution de combinaisons; c’est une interférence d’un type plus complexe. Comme le bilingue confond les deux séries d’occlusives françaises en une seule (voir plus loin), il provoquerait de nombreuses homonymies en français: “vide” et “vite” seraient confondus dans sa prononciation. Au lieu de distinguer ces deux mots par la qualité de la consonne comme le Français unilingue, il les distingue par la durée de la voyelle: *vid* et *vit* français deviennent chez le bilingue *vid* et *vid*. L’opposition brève + consonne sonore ~ brève + consonne sourde est transformée par le bilingue en une opposition longue + consonne sourde ~ brève + consonne sourde. La distinction qui pour le Français unilingue repose sur l’opposition de sonorité des consonnes est transférée à la voyelle précédente; il s’agit d’un transfert de trait pertinent.² Le patoisant trans-

² Voir André Martinet, *Eléments de linguistique générale*, p. 212.

pose en français une opposition très vivante dans son parler local, c'est-à-dire une opposition dont le rendement fonctionnel est important. Cet allongement de la voyelle devant consonne sonore est étendu aux mots français formés de plusieurs syllabes. Au lieu d'accentuer la dernière syllabe du groupe rythmique français, le bilingue accentue fortement la première syllabe de chaque mot comme il en a l'habitude dans son parler, et la traite comme une syllabe tonique. En effet, il allonge la voyelle inaccentuée française comme s'il s'agissait d'une syllabe tonique. Il prononce: "tirer" *dhīrē*, "maison" *mēsū*, "pigeon" *bhīśū* devant les consonnes "allongeantes" du français, et "aider" *ēde* "aimer" *ēmē* devant les autres consonnes sonores en français. La voyelle nasale inaccentuée française est très régulièrement allongée lorsqu'elle est accentuée à tort par le patoisant: "monter" avec une brève en français est prononcé *mũdhe*, "français" *frāse*, "maintenant" *mēdnã* "enfant" *ãfã*.

Comme il le fait pour les voyelles, le bilingue remplace les consonnes françaises par celles de son parler qui s'en rapprochent le plus. Or, les deux systèmes consonantiques sont très différents l'un de l'autre. Le système du français comporte une série de consonnes sourdes et une série de consonnes sonores, alors que le système de Blaesheim ne connaît pas d'opposition de ce genre; la sonorité n'est jamais pertinente dans ce parler. A la place des sourdes françaises p t k, le bilingue utilise ou bien les douces sourdes alsaciennes b d g ou bien les combinaisons bh dh gh de son parler. Dans sa prononciation, l'opposition sourde ~ sonore du français conserve sa valeur distinctive comme opposition sourde aspirée ~ sourde non-aspirée. En effet, en position initiale p t k français sont prononcés bh dh gh et b d g sonores sont prononcés b d g, c'est-à-dire comme des douces sourdes: "un beau pot" *bø bhø*, "un bon pont" *bũ bhũ* "il donne" *døn*, "il tonne" *dhøn*, "côte" *ghød*, "guerre" *gēr*. En position intervocalique l'opposition est souvent interprétée de la même manière: "était" *ēdhe*, "aider" *ēde*, "magasin" *magasē*, "fréquenté" *frēghãdhe*. Pourtant, les combinaisons bh dh gh ne se présentent jamais en position intervocalique à Blaesheim; elles ne se réalisent que devant la voyelle tonique. En réalité, le patoisant décompose un mot comme "fréquenté" en trois syllabes accentuées, car dans chacune des trois syllabes il rencontre une voyelle faisant partie de son système de voyelles toniques: dans fré-quen-té chaque syllabe est traitée comme un lexème autonome, l'ensemble comme un mot composé du parler, soumis à une hiérarchie des accents toniques. En position finale, les deux séries d'occlusives françaises sont entièrement confondues dans la prononciation du bilingue; il prononce avec des douces sourdes non aspirées: "bague" *bāg*, "sec" *seg*, "pipe" *bhib*, "robe" *rōb*. Nous avons fait remarquer ci-dessus que le patoisant réussit à éviter les homonymies que pourrait entraîner la disparition d'une opposition aussi importante du système français en allongeant la voyelle devant la consonne sonore. Devant consonne, les occlusives sourdes et sonores sont également confondues "classe, glace" *glas*, "proche, broche" *brōš*, "cri, gris" *gri* deviennent des homonymes dans la prononciation du bilingue.

Dans le système du parler de Blaesheim, les spirantes sont moins nombreuses que dans celui du français:

français			Blaesheim		
f	s	š	f	s	š
v	z	ž	v	+	+

Pour les trois spirantes sourdes et v du français, le bilingue se sert des phonèmes correspondants de son parler dont l'articulation est moins tendue qu'en français. Bien qu'il y ait deux "cases vides" dans le système de Blaesheim, le bilingue ne réussit pas à les "remplir" quand il parle le français. Il prononce cette deuxième langue sans ajouter de spirante sonore à son système, en se servant uniquement des spirantes auxquelles il est habitué. Il confond, par conséquent š ž et s z en toutes positions: "chou" "joue" *šu* deviennent des homonymes. En position finale, le système des spirantes de Blaesheim est même réduit aux trois spirantes sourdes f s š, v ne se réalisant pas en finale absolue. En français, le bilingue distingue bien f v à l'initiale et à l'intervocalique, mais il confond ces deux phonèmes en finale: "Vive la France" *vīf*, "chauffe" *šof*, "chauve" *šōf*, "la poule couve" *ghūf*. Il semble qu'après voyelle s z, š ž, f v soient pratiquement en distribution complémentaire en français; les deux combinaisons voyelle longue + spirante sonore, voyelle brève + spirante sourde se distinguent à la fois par la durée de la voyelle et par la sonorité ou l'absence de sonorité de la consonne. L'opposition est marquée deux fois. Le bilingue fait l'économie d'un trait pertinent sur deux en confondant les deux consonnes, mais l'opposition conserve sa valeur: "ruse-russe" prononcés *rūz - rūs* par un Français unilingue sont prononcés *rūs - rūs* par le bilingue. L'opposition de sonorité, incompatible avec les habitudes articulatoires du parler, est éliminée. En définitive, le bilingue parle le français en utilisant les phonèmes consonantiques de son parler sans en ajouter aucun. Il n'a jamais l'occasion de se servir des affriquées et prononce sa deuxième langue en n'utilisant que 13 consonnes:

			(bf)	(ds)	(dš)		
b	d	g	f	s	š	(x)	h
m	n	(ŋ)	v/w			j/š	
	l						
	r						

h aspiré est d'ailleurs prononcé à tort, car le Français ne l'utilise pas.

L'intérêt de l'étude phonologique d'un "accent", des interférences dans le cas de deux langues en contact et en général de la prononciation des bilingues est multiple. Il est d'abord scientifique, car une telle étude met en relief la structure et les faits de distribution caractéristiques de chacun des deux systèmes par rapport à l'autre et elle est aussi intéressante pour la première que pour la seconde langue. Les observations que l'on peut faire en étudiant la prononciation d'un bilingue ont d'autre part un intérêt pédagogique. Elles devraient permettre d'élaborer une méthode d'enseignement de la prononciation et de l'accentuation qui tiendrait compte des caractéristiques

fondamentales de la première langue des sujets, non seulement de la qualité particulière des phonèmes, mais aussi des combinaisons de phonèmes, du fonctionnement du système phonologique primaire par rapport à celui qui doit être enseigné. Ce sont les "fautes" et les maladresses du bilingue qui nous indiquent quels sont les points sur lesquels le maître doit insister afin d'éviter à l'élève débutant une prononciation erronée qu'il est difficile de corriger plus tard. Seule la connaissance des deux structures, des oppositions phonologiques permettra de concevoir des exercices appropriés qui montreront à l'élève quels phonèmes, quelles combinaisons, quels mots ne doivent pas être confondus. L'étude phonologique de la "mauvaise" prononciation de certains bilingues a enfin un intérêt "thérapeutique". La connaissance approfondie des deux systèmes en contact et de leur fonctionnement respectif est indispensable si on veut déterminer les interférences caractérisant l'accent de ces locuteurs. La confrontation des systèmes phonologiques des deux langues permet d'expliquer l'origine et la nature de ces interférences et de trouver ensuite une méthode pour les corriger.

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ON THE USE OF DISTINCTIVE FEATURE ANALYSIS IN COMPARATIVE HISTORICAL LINGUISTICS

ALO RAUN

When reconstructing the sounds of a hypothetical proto-language, especially if our information comes from the comparatively recent data provided by the present day dialects, the phonemic status of the reconstructed phones remains a problem. Obviously our reconstructions offer considerable differences in time depth, some of them reaching deeper than others. Certain classes of sounds in related languages may display more variation than others. This would imply that the respective subsystem of the proto-language has changed more than other subsystems. Further, the certainty of our conclusions depends on documentation. For some sound correspondences there may be numerous examples, so that they can be considered as well established. For some other correspondences one may have only one or two examples, provided the etymologies are correct. The number of phones which we get in reconstruction depends on the procedure applied. A lesser number of reconstructed phones is achieved when operating with the phonemes of the underlying languages. Reconstructing phones from phones of the underlying languages would yield a considerably larger crop which may become a source of trouble. Traditional spellings provide us with often poor subphonemic data, while modern recordings, not phonemicized, offer a rich variety of phones. It is both simpler and surer to operate with underlying phonemes, if such can be established.

Since we are interested in the phonemic status of our reconstructed units, we may try out the recommended triad in the introductory textbooks of linguistics: 1. phonetic similarity, 2. distribution, and 3. pattern congruity. These terms are not defined or applied in a very rigorous way, although this could be done. When operating with reconstructions one apparently has to rely heavily on phonetic criteria. Distribution and pattern congruity could be definitely established only at the conclusion of the phonemicizing process, and a certain circularity seems unavoidable when dealing with these matters. Here the question may be asked: should the phonemicization of the reconstructed phones be forced at all? In order to be able to phonemicize, a certain simultaneousness of our reconstructions would be required, and it is just this that cannot be proved by our data. We are able, though, to establish systematic connections between the reconstructed phones, if we operate with distinctive features. These features can be defined either in acoustic or articulatory terms. It is best to define them in both ways, in order to get a more complete formulation. It is agreeable

to operate with binary distinctive features, because this way one gets neat contrasts, but if this is not the case, as in the case where there are three vowel heights, the necessary modifications have to be made. Distinctive features are universal, but this does not mean that one gets involved with general phonetics when dealing with distinctive features in a particular language. Universal features can be combined in numerous ways, and the use of these combinations varies from language to language. Certain combinations of features may prove to be quite widespread, while others are found very seldom. Because a proto-language is something individual, applying the distinctive feature analysis to the reconstructed phones of a proto-language is an operation in special, not in general phonetics. Operating with universal features provides the advantage that the results obtained in one family can be compared with those achieved in other language families.

I would like now to offer a couple of examples from Finno-Ugric languages since these have been the special subject of my investigation. It is characteristic of the Finno-Ugric language that the consonants of Proto-Finno-Ugric can be reconstructed with a much greater ease than its vowels. The disagreements concerning Proto-Finno-Ugric consonants can be considered as minor when compared with the differing opinions concerning vowels. One basic disagreement involves both consonants and vowels. It is perhaps best displayed by comparing the charts of Proto-Finno-Ugric vowels, as offered by W. Steinitz and E. Itkonen. These charts have been rearranged here for the sake of better comparability.

W. Steinitz:			E. Itkonen:		
i	ĩ	u	i	ĩ	ü u ũ
e	ě	õ	e	ē	o õ
ä	a	ɔ	ä	a	

The main difference between the approaches of the two scholars lies in the fact that the one operates with the contrast long vs. short, and the other with that of full vs. reduced. Although the total number of phones is eleven in both cases, there are only six identical elements. According to the traditional view, Proto-Finno-Ugric also had long vs. short consonants. Steinitz does not acknowledge this contrast either, and instead operates with the contrast stop vs. spirant. This controversy involves the interesting problem of the relationship of a prosodic to an inherent feature. In Estonian, for instance, a thorough rearrangement of length relations obviously was connected with the loss of certain segmental phonemes, more exactly, certain vowels. The reverse process can be seen in the diphthongization of vowels which is expected to occur just in stressed position. In this author's opinion, the strongly divergent views of E. Itkonen and W. Steinitz can be reduced to a common denominator by assuming that the point of departure was actually the contrast tense vs. lax. Such a contrast could have caused both reduction and/or loss on the lax side, and could have developed into the contrast long vs. short on the tense side, if, as it seems, this was important from the point of view of sharpening of the morphophonemic contrast. In consonants

the tense vs. lax contrast apparently has played an important part in certain Finno-Ugric languages. E. N. Setälä's hypothesis of an original paradigmatic length alternation has its basis in this kind of phenomena. Presumably, in some Finno-Ugric languages the inherent feature tense/lax was developed into the prosodic feature long/short, while in others nothing of the kind happened, and hardly any morphophonemic alternation is left. But let us return to our vowel charts. A compromise solution which would use the inventories of both Steinitz and Itkonen, would look as follows (t = tense, l = lax):

i _t	i _l		i	u _t	u _l
e _t	e _l	ö _l		o _t	o _l
	ä		a		ɔ

According to this formulation, in theory only two differing points would remain: *i* and *ɔ* of Steinitz. In practice, a redistribution of the inventories of both authors would be necessary. (This can be done only in a separate article.)

The part which the different distinctive features play in the history of a language, of course, varies with time. There are comparatively stable distinctions which one would be tempted to call basic, but changes may occur, even in the assumedly basic features. In practice, the history of reconstructed sound units and the history of features should complement each other. Phonemes would be established as different combinations of features. I would still like to give a couple of examples from Finno-Ugric on the changing part the distinctive features play in the history of a language family.

In Proto-Finno-Ugric the distinctive feature of sharpness or palatalization obviously played a very important part in connection with the dentals. The contrast sharp vs. non-sharp was much more important than the contrast compact vs. diffuse in the spirants *s* and *š*, which have widely fused in Finno-Ugric. And yet at the present time in Finnish palatalization does not play any part whatsoever, and in modern Hungarian its part is rather limited. In Mordvin one has to distinguish the remainders of the original Finno-Ugric palatalization which appear in the contrast *ś* vs. *s* before back vowels from the new wave of palatalization which appears in the neighborhood of front vowels.

Much more complicated appears to be the problem of retroflexion as a possible distinctive feature in Proto-Finno-Ugric. There are a considerable number of instances of the contrast palatalized vs. retroflex or cacuminal in affricates. Since there are only a few hints to the former existence of a third or plain affricate, the question is whether retroflexion in affricates is a distinctive or a redundant feature. In order to clear this point up, we have to look out for other ternary distinctions in dental consonants in various Finno-Ugric languages. Here the Ostyak language comes to mind with its distinction between a plain, a palatalized, and a retroflex *l*, which distinction quite widely has been considered as ancient, and the same distinction for *n* in Ostyak, though the retroflex *n* has not received general acknowledgment as ancient. Tradi-

tional Finno-Ugric linguistics also has operated with three kinds of dental spirant δ , two of them non-palatalized, and one palatalized. As a purely theoretical possibility, Steinitz sets up not only δ , δ' , and δ'' , but also t , t' , and t'' . Additional study of the entire problem complex is needed.

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INITIAL POINTS VERSUS INITIAL SEGMENTS OF LINGUISTIC TRAJECTORIES

YAKOV MALKIEL

It is a truism of the classic approach to linguistic reconstruction that the oldest forms on record are ideally suitable for comparative purposes. If we use the metaphor of diachronic trajectories, then it is ordinarily the initial points of such lines that actually matter in historical research. Typologically, one may legitimately contrast modern French with, say, contemporary Spanish or Italian; but in diachronic operations aimed at piecing together the parent language — in this instance, the late provincial varieties of spoken Latin — everybody prefers to have recourse to the oldest available deposits of these three and of other Romance languages. Indo-Europeanists and Semitologists evince a similar set of preferences, too familiar to require exemplification. Let us, then, concentrate on exceptions to this dominant trend of thought and organize them in the order of decreasing banality. Does it happen that initial points of linguistic trajectories fail to yield the best possible clues?

By way of preliminary remark, let us remind ourselves that, in speaking of “oldest” or “most archaic” records, we distinguish between anteriority along the line of absolute chronology and anteriority in terms of intrinsically less advanced evolutionary stage. The two labels may, but need not, coincide. The initial point of a line relatively unpromising, on account of a rapid rate of attrition and the consequent obliteration of valuable traces, may reveal less than do shreds of information extracted from the mid-segment of some other line symbolizing a markedly slower progress.

In the core of our argument, we discriminate between the evolutionary curve of a given language and the gradual refinement (or else crudescence) of its graphic representation. Thus, if the alphabet has been adopted from some other culture, the earliest records may be uncouth to the point of ambiguity, hence, with regard to important features, far less helpful than those of a subsequent period which, for the first time, show a neat correspondence between letter and sound. To the extent that this is true, mere chronological anteriority, not calibrated by the degree of dependability, tends to lose in importance.

A single example should suffice. At the embryonic stage of Romance vernacular literatures, the scribes had at their disposal an alphabet bequeathed by Latin and inapt for the notation of supervenient categories of sounds, such as palatal consonants and new diphthongs, eminently characteristic of early Romance speech. The result of this inadequacy was that the oldest records, expected to reflect the

earliest observable phase of Romance, in important respects were basically faulty. One detects two families of errors: the clumsy scribes either aimed at realism, crudely marking the forms they heard — so far as we can piece together the course of events — writing down, e.g., only one component of a diphthong or leaving palatalization altogether unmarked; or they resorted to Latinized spelling, an attitude which presupposes a measure of etymological deftness, and so suggested learned doublets where none existed in actual usage or before any, in fact, arose. Thus, the introductory lines of the *Reyes Magos*, a primitive play of the early 12th century probably from New Castile preserved in a unique MS, contain forms like, on the one hand, *quin* = “quien” (“who”), *timpo* = “tiempo” (“time”), *cudo* = “cuido” or “cuedo” (“I think”), *fure* = “fuere” (“may be”), *escarno* = “escarnio” (“mockery”), *strela* = “(e)strella” (“star”), and, on the other, *nocte* = “noche” (“night”) and *december* = “diciembre” (in startling rhyme with *fembra* “woman”). Only as we hit the layer of Alfonsine texts, at a remove of, presumably, a century and a quarter, can we trust the average copyist to keep apart monophthong and diphthong, *l* and *ll*, voiced *z* and voiceless *ç*. Similar remarks could be made on the *Oaths of Strasbourg*.

More noteworthy are situations involving language development pure and simple, without any regard for graphemics. As a rule, we symbolize, in our thoughts and our charts, such situations as progress, descent, and transmission by straight lines, with the implicit understanding that we toy with a calculated oversimplification, that curved or zigzagging lines would, strictly speaking, do greater justice to reality, on account of ever-present fluctuations like those that dialect geography has brought out in most dramatic fashion. In cutting out loops and corners, we argue, first, that minor departures from the straight lines are seldom, if ever, clearly discernible over major distances in time; and, second, that, in all likelihood, they balanced one another and thus leveled off.

Suppose now that, through an odd twist of circumstances, the curtain of firm historical tradition rises at the very moment when the oscillation is at its liveliest, as when the needle of the seismograph marks the sharpest deflection from the straight line connecting later stages — the same straight line which we incline to extrapolate also for the earlier, above all the immediately preceding stages, for the most part shielded from direct observation. Clearly, under such circumstances the examination of the initial points alone might lead to confusion. A generously carved-out initial segment, extending preferably over several centuries, is needed to help us discriminate between the principal line of advance and some temporary disturbance eventually overcome and conspicuous mainly because, through the caprice of our fragmentary records, the distortion it was causing coincided with the earliest tangible traces of a given development.

One concrete example, recently elucidated, may yield an illustration of this theoretically envisioned state of affairs. The Classical Latin imperfect, a tense, you recall, involving a fair degree of regularity, was characterized in all major conjugation classes by the segment *-ba-* attached to the root morpheme by the respective thematic

vowel, e.g. *laud-ā-ba-m* "I praised", *hab-ē-ba-m* "I had", *curr-ē-ba-m* "I ran", *fac-iē-ba-m* "I made", *aud-ī-ba-m* (beside *aud-iē-ba-m*) "I heard"; its six personal endings, except for the 1st sg., matched those of the pres. indic. and, with no exception, those of the pres. subj., and its accentual pattern, but for the absence of rhizotonic forms, resembled the schema of those tenses, insofar as in the 1st and the 2d pl. the stress hit a vowel other than that accented in the remainder of the paradigm.

Now there occurred, for reasons not yet fully clarified, an inferrable substitution of **-iam* for class. *-ēbam*, *-iēbam*, and *-ēbam* in provincial spoken Latin, so that one may reckon, in most regional varieties, with (say) **habia* for "I had", **facia* for "I made", and **audia* for "I heard", while full-bodied *-āba*, as in *laud-ā-ba-(m)*, remained intact. In the Luso-Hispanic subarea of this larger territory, there further took place a minor shift, independent, it would seem, of the loss of *-b-* since it likewise affected the paradigm of *laud-āba-m*: the accent of the 1st and the 2d pl. was withdrawn, a change making a single syllable, marked either by *a* or by *i*, the consistent carrier of the stress in all six forms: contrast OSp. *loáva* "I praised", *loávamos* "we praised" with Tusc. *lodá-va* or *-vo*, *lodavámo*. No further assumptions are required to justify historically the paradigms of the imperfect in Portuguese, Catalan, Provençal, and still other vernaculars.

The great exception to this apparent harmony is Old Spanish. I advisedly emphasize the qualifier "Old", because Classical and Modern Spanish, viewed in isolation, beautifully fit the description of the broad trend. Indeed, were it not for the abundance of unequivocal passages in medieval texts and for a few modern bits of corroborative dialectal evidence, we could never have reconstructed the medieval stage, simply because, counter to expectations, it fails to represent, in terms of actual products, any midpoint between late Antiquity and the premodern or modern stage. In other words, no straight evolutionary line may here be drawn, because, as microscopic inspection confirms, the early records which puzzled a whole generation of ingenious linguists simply reflect no such line, but a protracted temporary deviation from the main course of events.

Among the erratic Old Spanish forms with *-ié* instead of *-ía* that may be, on statistical grounds, declared dominant, five entrenched themselves in the paradigm of a verb typical of the *-er* and *-ir* classes; thus, from *aver* "to have" neatly edited 13th- or 14th-century texts allow us to slice off: *av-ía* "I had", *av-iés* "thou hadst", *a-vié* "he had" and, in the plural, correspondingly, *av-iémos*, *av-iédes*, *av-ién*. A fact incidentally discovered half a century ago but never, until 1959, capitalized upon is that the degree of predominance of aberrant *-ié* over predictable *-ía* was unequal for Forms 2-6: it reached its maximum, i.e., an absolute measure of prevalence, in the 1st and the 2d pl., while the 2d and 3d sg. plus the 3d pl. displayed only a relative measure of preëminence, in the neighborhood of 80%-90%. In the 1st sg., even at the crest of this movement, the *-ié* forms consistently represented a minority. Now it can be shown, first, that preterite and imperfect were syntactically closer to each other, hence, one gathers, more intimately associable, in Medieval than in

Modern Spanish; second, that in the ranks of the Old Spanish preterite and nowhere else, there spread, shortly before the dawn of vernacular literature, a plural paradigm in *-iemos*, *-iestes*, *-ieron* tending, for a while, to dislodge traditional *-imos*, *-istes*, *-ieron*; and third, that (as two American pioneers, Ford and Lang, had conjectured) the contact between the new, highly contagious *com-iemos* "we ate" [once] and *comíamos* "we were eating" could the more easily have produced a new imperfect: *comiémos* "we were eating" and have sparked the infiltration of *ie* into most of the remainder of the imperfect as the shift *ía* > *ié* was at that juncture sporadically observable even outside the verbal paradigm, cf. the well-known proper name *Didacu* > *Diago* > *Diego*.

The reason for the diphthong, *ié*'s failure to dislodge *-ía* from its last stronghold in the 1st sg. was the chance here afforded to speakers of a Romance language to differentiate between the 1st and the 3d sg. in an important sector of the verbal paradigm — a chance, independent sources confirm, definitely welcome to speakers, both as an escape from bothersome homonymy and as a token of compliance with the total conjugational pattern. The main reason why the 13th-century schema *yo avía*, *tú aviés*, *él avié*, etc. eventually collapsed was its utter atypicality as regards accentual distribution and the paradigmatic interchange of *ía* and *ié*, also its asymmetry with the parallel *-ava* schema. To these factors, add the disappearance of the very by-forms of the preterite whose analogy, in the first place, presumably launched the abortive development. This entire interlocking of causes and effects falls into place once we focus attention on the whole pertinent segment rather than on the easily misleading initial point of the trajectory.

Departures of such magnitude from the straight course are not frequently witnessed. The three specific complications in this instance which, understandably, baffled pioneer scholars were the sheer temporal span of the deviation — an estimated three centuries; the approximate coincidence of its initial phase with the beginning of records in the vernacular; plus the fact that after the mid-15th century the evolutionary line swung back almost perfectly into its original position, so far as that position can be adumbrated.

One afterthought: If the pressure of the pattern sufficed, after 1400, to wipe out the gains made by this internal contamination, why, we may ask, did this amount of pressure fail to block the interlude in the first place? Could it be that a rising glottodynamic movement has a greater impact and thus proves more difficult to ward off or to check than the same movement past its crest? The future may teach us to gauge such pressures with greater precision. Meanwhile, we shall do well to watch out for perplexing developments which are apt to yield to analysis once we grant that the initial points along the trajectories may suggest a misleading, uncharacteristic direction, in other words, that our operations, to bear fruit, require a generously placed cut-off point.

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DISCUSSION

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In answer to Professor Malkiel's question whether a "rising movement [may] have greater impact ... than the same movement past its crest" I would like to suggest that an innovation may seem to spread widely when it is filling a gap in a linguistic system. Subsequently, when the gap is filled, it cannot spread so readily and may therefore seem to have lesser impact.

POINTS DE REPÈRE DANS L'ÉVALUATION DE LA PARENTÉ SPÉCIALE DE DEUX LANGUES

ALBERT MANIET

La détermination de la parenté spéciale de deux langues à l'intérieur d'un groupe linguistique pose, comme on sait, des problèmes délicats de méthode. Un des cas les plus frappants dans ce domaine est celui de la relation du latin avec les dialectes italiens, qui depuis près d'un demi-siècle suscite périodiquement des prises de position irréductibles. Il m'apparaît que la méthode intuitive, qualitative, à elle seule, est impuissante en pareil cas, parce qu'elle ne peut fournir de critères assez objectifs pour écarter les implications préférentielles qui, en fait, orientent champions et adversaires de l'"italique commun". Ce cas de l'italique, bien qu'extrême, n'est pas isolé. Et même lorsqu'il s'agit de groupes unanimement reconnus, comme l'indo-iranien, le groupe hellénique, le groupe germanique etc., sans compter les langues du domaine non indo-européen, le grand défaut des pesées intuitives de données linguistiques est d'abord que le chercheur a tendance à ne considérer que certains faits frappants et laisse s'estomper l'ensemble dans une sorte de brouillard; ensuite que, même s'il voulait se baser sur cet ensemble, il serait incapable d'évaluer objectivement tous ces faits, compte tenu à la fois de leur quantité et de leur qualité. Il me semble donc hautement intéressant d'établir une échelle de rapports, à la fois plus précise, plus objective et plus simple que la formule habituelle: "tel dialecte (ou telle langue) est plus étroitement apparenté(e) avec tel(le) autre qu'avec un(e) troisième".

Le terme "précision" évoque assez naturellement un rapport quantitatif. Et en fait, il y a vingt-cinq ans, les professeurs Chrétien et Kroeber publièrent un article intitulé "Quantitative Classification of Indo-European Languages"¹, suivi peu après d'un complément incluant le hittite.² En 1950, le germaniste Ross reprit le problème d'un point de vue surtout méthodologique.³ En 1959, M. Ellegård proposa une technique analogue à celle de Chrétien-Kroeber, mais simplifiée et il l'appliqua aux données fournies par ses prédécesseurs.⁴ Cette méthode rencontra l'approbation de Kroeber,⁵ mais vient d'être critiquée à la séance de lundi dernier par M. Herdan, qui a ensuite exposé

¹ Cf. *Language*, 13 (1937), pp. 83-105.

² Cf. *ibid.*, 15 (1939), pp. 69-71.

³ "Philological Probability Problems", *Journ. of the Royal Statistical Society*, Ser. B, 12, 1 (1950), pp. 19-41.

⁴ "Statistical Measurement of Linguistic Relationship", *Language*, 35 (1959), pp. 131-156.

⁵ Cf. *ibid.*, 36 (1960), pp. 1-21.

ses vues sur le sujet. Mon propos est parallèle à celui de M. Herdan, mais je ne pense pas qu'il fasse double emploi avec lui.

Je voudrais I) montrer que, malgré ses résultats dans l'ensemble satisfaisants, c'est-à-dire généralement en concordance avec les classements obtenus par la méthode intuitive, la technique de M. Ellegård laisse à désirer sur plusieurs points; II) soumettre une méthode qui me paraît plus appropriée.

1.1. Les données de base de M. Ellegård ne sont pas suffisamment significatives. Elles consistent a) dans les traits communs aux deux langues (A et B) envisagées; b) dans les traits conservés par A et non par B; c) dans les traits conservés par B et non par A.

Or, l'indice le plus marquant d'une parenté spéciale est la communauté non pas des traits conservés, mais des innovations. Le nombre de ces innovations communes ne doit donc pas simplement s'ajouter à celui des conservations, ce qui est le cas dans la méthode de M. Ellegård. Il est clair que deux langues qui, sur un total de deux cents traits, en présentent en commun 180 originels et aucun original ont beaucoup moins de chances d'être étroitement apparentées que celles qui en présentent en commun 160 originels et 20 originaux. En réalité, le résultat obtenu par l'addition de ces traits ressortit au calcul du coefficient de ressemblance entre les langues envisagées, ce qui est certes intéressant, mais ne correspond pas nécessairement à un coefficient de parenté étroite à l'intérieur d'un groupe.

1.2. Les traits morphologiques et phonétiques (respectivement 30 et 44 dans les tableaux basés sur les données de Chrétien-Kroeber) sont mis sur le même pied. Or, les variations phonétiques, surtout à date ancienne, sont beaucoup plus difficiles à constater et à noter que les variations de morphèmes. Quant aux traits lexicaux (dans le tableau basé sur les données de Ross), ils sont à notre point de vue nettement moins significatifs que les données morphologiques; car, s'il est vrai que la différence qui les sépare est seulement quantitative, comme le veut G. Herdan,⁶ il reste que les mots s'empruntent beaucoup plus aisément que les éléments grammaticaux et il n'est pas toujours possible de déceler ces emprunts.⁷

1.3. M. Ellegård a opéré sur des groupes dont l'unité est discutée, p. ex., sur l'"italique commun". Précisément, qu'est-ce que l'"italique commun"?

1.4. Pour déterminer si la langue "mère" possédait déjà tel trait (p. ex., une racine), M. Ellegård se contente de la présence de ce trait dans au moins deux des langues "sœurs". Or, lorsqu'il s'agit de langues ayant eu des destinées communes après la période indo-européenne, le fait qu'elles contiennent un trait qui leur soit propre indique plutôt une innovation qu'une conservation. L'évaluation quantitative doit donc tenir compte davantage des caractères propres à chaque espèce de faits linguistiques. Je propose pour les langues indo-européennes la méthode suivante.

⁶ "Vocabulary Statistics and Phonology: A Parallel", *Language*, 37, 2 (1961), p. 249.

⁷ Cf. mon article "Les correspondances lexicales de l'osque et du latin, problème de méthode", dans l'ouvrage collectif *Études étrusco-latines* (= *Recueil de Travaux d'Histoire et de Philologie de l'Université de Louvain*, 4e série, fasc. 31) (Louvain, 1963), pp. 131-143.

II.1. PRINCIPE GÉNÉRAL. Noter dans une colonne *a* les conservations communes, dans une colonne *b* les innovations propres à A et non à B, dans une colonne *c* les innovations propres à B et non à A, dans une colonne *d* les innovations communes.

II.2. PRINCIPES PARTICULIERS.

A. D'ordre linguistique.

a) Prendre en considération d'abord les traits grammaticaux, parce que les plus typiques d'un système et les moins susceptibles d'emprunt. Les traits phonétiques et lexicaux ne viennent que corroborer le cas échéant les résultats obtenus. Les morphèmes sont considérés abstraction faite des transformations phonétiques éventuelles. Ainsi, à l'accusatif plur. des thèmes en *o/e*, osque *-ss*, ombrien *-f*, latin *-s*, sont comptés comme une conservation morphologique commune de l'IE *-ns*.

b) Tous les types de déclinaison et de conjugaison doivent être représentés à tous les cas, à toutes les personnes, à tous les temps et à tous les modes. De la sorte, il est tenu compte dans une certaine mesure de l'importance des traits. P. ex., une innovation qui atteint toute une classe (soit le subjonctif en *-ā-*) figurera dans le tableau plus souvent qu'une innovation propre à un cas (soit le génitif en *-ī*).

c) Je considérerai en principe comme ancien un trait qui se trouve au moins dans deux langues n'appartenant pas au même groupe géographique à date historique (à ce point de vue, j'admettrai jusqu'à preuve du contraire que l'indo-iranien, le baltique et le slave forment un groupe, les langues celtiques, italiques et germaniques un autre groupe, le baltique, le slave et le germanique un troisième, le grec et le latin un quatrième, le grec et l'arménien un cinquième; le hittite et le tokharien sont à part). Je ne tiendrai donc ici pour indo-européen commun qu'un trait figurant au moins en sanscrit ou en iranien et dans les langues italiques, celtiques ou germaniques et, d'autre part, un trait qui figure en slave ou en baltique et dans les langues italiques ou celtiques. Corrélativement, je tiendrai pour innovation un trait qui ne se trouve qu'à l'intérieur d'un des groupes géographiques précités.

d) Une innovation peut être plus ou moins caractéristique. S'il s'agit d'une analogie banale avec un autre élément du système, je lui attribuerai un point: c'est le cas, p. ex., en latin pour la finale *-ī* (< oi) du nominatif plur. des thèmes en *o/e*, analogique des pronoms démonstratifs, en osque de la finale *-eis* de ces mêmes thèmes au génitif sing., analogique des thèmes en *-i*. S'il s'agit de la voyelle thématique, comme l'état indo-européen était déjà sans doute confus, je noterai une divergence comme deux innovations séparées. S'il s'agit d'une innovation *sui generis*, je lui attribuerai cinq points (des statistiques ultérieures devront normalement aboutir à un chiffre moins arbitraire): ainsi pour les suffixes *-tt-* et *-f-* du prétérit osque.

e) Il faut que l'élément grammatical considéré comme une innovation ou une conservation communes aient pratiquement la même fonction dans les langues où il figure. Ainsi, je ne classe pas parmi les traits conservés l'élément *-ī* de latin *dominī*, irlandais ogamique *maqi*, gaulois *Segomari*, qui, à supposer qu'il représente à une époque reculée de l'indo-européen le même élément que le *-ī* du type sanscrit *samī karōti*, forme

dans les trois premières langues la classe du génitif des thèmes en o/e, tandis qu'il s'oppose à ce cas en sanscrit.

B. *D'ordre mathématique.*

Le coefficient (r) sera obtenu au moyen de la formule de Bernoulli légèrement modifiée:

$$r = \sqrt{\frac{a(d + 1) - bc}{(a + b)(a + c)(c + d)(b + d)}}$$

Les principales caractéristiques de l'emploi de cette formule par rapport à celui qu'en fait M. E. sont 1) l'introduction du facteur d = innovations communes; 2) la multiplication de a par d au lieu de la somme des facteurs communs représentée par l' a de M. E. ;3) l'addition d'une unité au nombre des innovations; elle me paraît nécessaire pour éviter que, si ce nombre est égal à zéro, il ne réduise à zéro par multiplication le nombre des conservations et fausse ainsi les proportions d'une façon le plus souvent très sensible. J'insiste sur le fait que je ne tiens pas absolument à cette formule de Bernoulli. Peut-être en existe-t-il une autre plus adéquate. Ce que je voudrais souligner, c'est la nécessité d'une formule qui réduise à une entité numérique les données complexes fournies par la recherche comparative.

II.3. L'application de la méthode que je viens d'exposer à des langues appartenant à un même groupe linguistique fournit donc un certain coefficient, p. ex., -0.16. Quelle est la signification de ce chiffre? Suffit-il pour le savoir de le comparer avec le coefficient obtenu en confrontant deux autres langues indo-européennes? Non, car la comparaison des deux coefficients montrera simplement qu'il y a ou non une différence entre les relations respectives des deux paires envisagées. Mais comment faut-il interpréter cette différence? La comparaison respective des deux langues en question, p. ex., le latin et l'osque, avec une langue témoin semblerait s'imposer. Elle aboutit en fait à montrer que les deux premières langues sont plus rapprochées entre elles que de n'importe quelle autre. Mais ce résultat n'est guère concluant. D'abord parce que les langues celtiques, qui seules seraient significatives en l'occurrence, sont attestées à une époque notablement plus récente que les langues italiques du 2ème siècle avant J.-C. et manifestent dans l'ensemble, malgré certains archaïsmes, un stade d'évolution tout différent. Ensuite, parce que les coefficients restent malgré tout relatifs. Ce qu'il faut, ce sont des *points de repère*, une base *concrète*, c'est-à-dire telle que les chiffres puissent être mis en rapport avec des données historiques et géographiques. Les langues romanes et l'indo-iranien se rapprochent de cet idéal. En appliquant à des échantillons de l'avestique et du sanscrit la méthode précitée, je constate un rapport de 0.65. Pour les langues romanes, pour lesquelles le latin vulgaire constitue, *mutatis mutandis*, le pendant de l'indo-européen, voici les résultats obtenus: italien-espagnol, $r = -0.48$; italien-français, $r = -0.44$; français-portugais, $r = -0.44$; français-espagnol, $r = -0.39$; italien-portugais, $r = -0.37$; espagnol-portugais, $r = 0.39$. Or, nous savons que les populations qui parlèrent une forme plus ancienne de l'indien et

de l'iranien constituèrent une véritable communauté, réunie sous le vocable de *ārya-* et, d'autre part, nous connaissons la longue communauté de destinée de l'Espagne et du Portugal et la géographie à elle seule suffirait à les séparer de la France et surtout de l'Italie, en même temps qu'elle sépare ces deux dernières, sans parler de la Roumanie.⁸ En fonction de ces cas, je propose *provisoirement* le critère suivant: il est raisonnable de considérer comme étroitement apparentées à l'intérieur d'un groupe deux langues dont le coefficient de relation est positif, comme non étroitement apparentées celles dont le coefficient est négatif. La croissance ou la décroissance du coefficient par rapport à zéro marque la plus ou moins forte quantité des facteurs qui ont rapproché ou différencié les deux langues au cours de leur histoire.

Il est bien évident que cette recherche d'un point de repère concret n'en est encore qu'à ses débuts et que le coefficient de relation n'aura de valeur vraiment scientifique et significative que lorsqu'on aura procédé au moyen *d'une même méthode* à l'évaluation quantitative des nombreuses langues et des plus nombreux dialectes dont on connaît l'histoire d'une manière satisfaisante. Plutôt qu'un résultat, c'est donc un point de départ que cette communication s'est proposé de soumettre à votre jugement.

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DISCUSSION

POLOMÉ:

Abstraction faite de l'attribution toute arbitraire de 5 points aux traits *sui generis* des langues envisagées, alors que leurs traits communs ne comptent que pour un point, le fait de compter p.e. le subjonctif latin en *-ā-* à toutes les personnes, tout en ne faisant compter qu'une fois le génitif singulier en *-ī* me paraît fausser leur valeur respective comme données statistiques, d'autant plus qu'aucun compte n'est tenu du rendement fonctionnel des formes envisagées. Il me paraît donc préférable de compter les morphèmes en leur attribuant des points en corrélation avec l'importance fonctionnelle qu'ils assument dans le système des langues examinées. Un autre point faible me paraît l'acceptation *préalable* de certains groupes en indo-européen, alors les données statistiques sont censées fournir les critères mêmes d'un regroupement valable des dialectes à l'intérieur de la communauté indo-européenne.

⁸ Les dialectes germaniques sont largement attestés, mais leur histoire ancienne est peu claire. On constate en tous cas une tradition poétique et scripturale commune aux Germains de l'Ouest. Ceux d'entre eux qui étaient destinés à parler l'"anglo-saxon" émigrèrent en Grande-Bretagne dès le 5ème siècle, laissant évoluer ensemble sur le continent Saxons, Frisons et Allemands. Les Scandinaves et les Gots étaient à part. Or, je constate un coefficient positif (0.30) pour le v. saxon et le v.h. allemand et des coefficients négatifs (de -0.025 à -0.36) respectivement pour le gotique et l'anglo-saxon, le v. saxon et l'anglo-saxon, le gotique et le v. h. allemand, le gotique et le saxon, le gotique et le v. norrois, le v. norrois et l'anglo-saxon, le v. norrois et le v. saxon, le v. norrois et le v.h. allemand, l'anglo-saxon et le v.h. allemand.

LA SEXTUPLE ARTICULATION DU LANGAGE

ERIC BUYSSSENS

Il n'y a pas longtemps M. Martinet a développé la thèse que le langage parlé se distingue de tout autre par sa double articulation : la phrase s'articule en signes, et les signifiants s'articulent en phonèmes. Je ne conteste nullement la validité de cette thèse, mais je pense qu'il y a plus de deux articulations.

Avant de les énumérer, il faut, pour éviter tout malentendu, que je vous donne ma définition de l'articulation ; j'entends par là le fait que des unités de rang inférieur sont combinées de n'importe quelle façon pour former des unités de rang supérieur. Cette organisation est économique ; avec un nombre donné d'unités inférieures il est possible de construire un nombre plus grand d'unités supérieures.

Je rappelle brièvement les trois articulations que j'ai ajoutées à celles de M. Martinet dans le résumé qui vous a été soumis :

1. les traits pertinents se combinent pour constituer les phonèmes ;
2. les signes, c'est-à-dire les unités sémantiques simples, se combinent pour former des unités complexes (composés, dérivés, syntagmes) ;
3. les unités complexes se combinent entre elles ou avec des unités simples pour former des propositions subordonnées.

Au moment où j'ai rédigé mon résumé, j'ai renoncé à parler d'une sixième articulation parce que je pressentais que cela entraînerait des objections et parce qu'il me paraissait suffisant de montrer qu'il y a plus de deux articulations. Mais le rapport de M. Benveniste, qui rejette l'idée d'un niveau supérieur à celui de la phrase, m'oblige à dire pourquoi j'admets un sixième niveau d'articulation. J'admets parfaitement que le rapport entre les phrases d'un discours n'est pas le même que le rapport entre les signes d'une phrase : mais à chaque niveau d'articulation il y a une différence : pour les phonèmes, la signification n'entre pas en jeu, et pour les traits distinctifs il n'y a pas de segmentation.

C'est aussi un fait que le nombre de phrases est illimité alors que le nombre de signes est limité ; mais le nombre des composés, dérivés et syntagmes est illimité.

À côté de ces petites différences il y a trois ressemblances fondamentales :

1. Pour définir le phonème il faut se référer au signifiant (unité supérieure), pour définir le signe il faut se référer à la phrase ou au membre de phrase. De même, pour définir la phrase il faut se référer au discours : comme moi, M. Benveniste définit la phrase comme une unité du discours.

2. On peut faire avec les phrases des oppositions comme avec les signes. Prenons la phrase *Je le sais*; si je remplace *sais* par *ignore*, j'obtiens une autre phrase: *Je l'ignore*. De même, si dans un discours je remplace la phrase *Je la sais* par *Je l'ignore*, j'obtiens un autre discours.

3. De même qu'il y a plusieurs types de signe, il y a plusieurs types d'unité du discours. Il y a d'abord le type auquel personnellement je réserve le nom de phrase; cette unité est constituée essentiellement par le verbe; cet élément peut être accompagné d'un sujet ou d'un ou plusieurs compléments; ce sont là les éléments accessoires. Ensuite il y a l'unité à laquelle je donne le nom de *rhème*: elle ne contient pas de verbe, elle contient un ou plusieurs des éléments accessoires. Enfin il y a l'interjection qui ne comporte ni verbe ni élément accessoire. Il y a donc divers types d'unité du discours.

Ces trois faits montrent que les unités du discours ont suffisamment de points communes avec les signes pour qu'on puisse parler d'un niveau supérieur à la phrase et pour qu'on dise que le discours comporte six degrés d'articulation.

DISCUSSION

MARTINET:

Les deux articulations réunies par la formule "double articulation du langage" sont fondamentales dans le sens que les autres que vient de suggérer M. Buysens n'ont pas le même caractère d'obligation. On peut, par exemple, facilement concevoir une langue qui ferait l'économie de l'articulation des phonèmes en traits pertinents.

PRIETO:

Dans la langue, les signifiants entités, à une face, s'articulent en unités plus petites et également à face unique, ce qui permet une économie de "substance" phonique: avec quelques deux domaines de phonèmes ou un nombre encore plus petit de traits pertinents la langue compose un nombre pratiquement infini de signifiants différents. D'autre part, les "sèmes",¹ entités à deux faces, s'articulent dans la langue en entités plus petites et également à deux faces; et cette articulation permet de réduire le nombre de rapports conventionnels entre signifiant et signifié sans diminuer le nombre de sèmes.

Ce sont là deux articulations de nature bien différente, ce qui justifie que l'on parle d'une "double articulation" de la langue. Par contre, il me semble que le fait qu'entre les signifiants et les traits pertinents il y ait ou non de unités intermédiaires (comme la phonème ou la syllabe), ou le fait qu'il j'ait ou non, entre le sème et la plus petite entité à deux faces des entités intermédiaires (comme le mot), ne nous permettent pas de parler de plus de deux articulations: il ne s'agit, respectivement, que de différents niveaux de l'articulation du signifiant ou de l'articulation du sème.

¹ Terme de M. Buysens pour désigner les entités sémiologique permettant pas elles seules la communication et n'étant pas analysables en entités plus petites et douées elles aussi de cette propriété.

A PROPOS DE LA THÉORIE DES OPPOSITIONS BINAIRES

KAREL HORÁLEK

La théorie des oppositions constitue une partie importante du structuralisme linguistique européen (y compris son rejeton de Harvard). Le structuralisme européen se rattache dans ce sens étroitement aux pensées de F. de Saussure. Il existe cependant une grande différence entre la manière dont, par exemple, l'Ecole de Prague et l'Ecole de Copenhague traitent les oppositions phonologiques. M. Hjelmslev reproche à l'Ecole de Prague de relier d'une façon inconsistante les idées saussuriennes avec celles de Baudouin de Courtenay. (Cette inconsistance, il la voit dans la façon dont l'Ecole de Prague conçoit les oppositions phonologiques.) Hjelmslev lui-même prétend que la structure phonologique de chaque langue est constituée seulement par les relations qui sont indépendantes de ce qui se réalise dans la substance phonique (ou bien graphique). La forme écrite de la langue diffère en effet fort nettement de la forme phonique, déjà par le fait qu'elle ne reproduit les différentes oppositions phoniques que d'une manière restreinte. Pour cette raison Hjelmslev, en décrivant le plan d'expression, ne tient pas compte des qualités phoniques (pour lui, il ne s'agit que d'établir une invariante des relations, de même, comme lorsqu'il analyse le plan du contenu). Pour Hjelmslev déjà, toute la phonologie semble présenter un écart de la conception purement linguistique des phénomènes de la langue, et cela vers la phonétique. C'est pourquoi il réfute également le structuralisme de Prague dans son ensemble, car la phonologie en constitue la partie importante.

Sur ce point, dans les derniers temps, on aboutit à une situation paradoxale dans le structuralisme linguistique. D'une part, l'influence de l'Ecole danoise va en croissant (principalement en Amérique), d'autre part, l'orientation phonétique en phonologie devient toujours plus forte. Une coopération étroite entre les phonologues et les phonéticiens commence à se déclarer par un raffermissement du point de vue phonétique, dans les travaux de l'Ecole de Harvard. Un trait spécifique du courant de Harvard, en phonologie, est la théorie des qualités distinctives combinée avec la théorie des oppositions binaires.

Ce sont principalement des idées de M. Jakobson qui caractérisent les travaux de l'Ecole de Harvard. Avec sa théorie des oppositions phonologiques, Jakobson se différencie nettement de la théorie de Trubetzkoy, comme elle est exposée principalement dans ses Grundzüge. Ce fut déjà vers 1935 qu'au sein de l'Ecole de Prague, on put remarquer deux tendances différentes dans la manière de concevoir les oppositions

linguistiques. L'une de ces tendances est représentée par Troubetzkoy et l'autre par Jakobson. A cette époque, Troubetzkoy quitte son opinion précédente d'après laquelle ne sont que deux sortes d'oppositions linguistiques, les corrélations et la disjonction, pour avancer l'idée que les oppositions présentent une différenciation plus riche et que les oppositions binaires ne consistent qu'un cas de plusieurs possibilités. Par contre, M. Jakobson persiste dans son opinion que les oppositions linguistiques ne sont que binaires dans leur essence. C'est dans cet esprit qu'il rédige sa conférence importante sur les oppositions des consonnes, datant de 1938. Dans ses travaux ultérieurs qui, pour la plupart, ont pris leur origine en collaboration avec les phonéticiens, on trouve, pour un certain temps, des idées reconciliantes, envers la théorie de Troubetzkoy, mais récemment le courant de Harvard reprend de nouveau l'orientation vers un binarisme radical.

Les éléments de la conception de compromis apparaissant dans quelques travaux commençant avec les *Preliminaries* doivent être expliqués, avant tout, par l'influence de Troubetzkoy. Les débuts de cette influence se manifestent déjà dans le traité de Jakobson sur les oppositions des consonnes, où il accentue la nécessité de faire valoir le point de vue acoustique en phonologie ainsi qu'en phonétique. Dans ce sens, le développement ultérieur de ces disciplines donna pleinement raison à Jakobson. En ce qui concerne la théorie des oppositions, Jakobson distingue en 1938 deux types d'oppositions binaires fondamentales. D'après lui, ce sont des analogies aux oppositions logiques, c'est-à-dire les oppositions contraires et contradictoires. La notion des oppositions contradictoires correspond essentiellement à la notion traditionnelle de corrélations, tandis que les oppositions contraires présentent le caractère des oppositions de contraste. On y fait valoir, de même que dans les oppositions graduelles de Troubetzkoy, le moment quantitatif. Pendant un certain temps, le groupe de Harvard opère directement avec la notion d'oppositions graduelles.

Récemment (comme je le viens de dire déjà), les phonologues de Harvard ont simplifié la théorie des oppositions binaires; pour la description complète des systèmes phonologiques, les oppositions de caractère corrélatif ou contradictoire leur suffisent. Un essai de justification théorique de cette attitude nouvelle est donné par M. Halle dans l'article intitulé „In Defense of the Number Two” (*Studies presented to Joshua Whatmough*, 1957). Cet article contient une polémique partielle contre le professeur Martinet qui exprima certains doutes sur la validité générale de la théorie binaire dans son livre *Economie des changements phonétiques* (1955), tout en pensant cependant plutôt aux langues jusqu'à présent non décrites par la méthode structurale qu'à celles dont la description des systèmes phonologiques nous possédons déjà. Dans les lignes qui suivent, je m'efforcerai de montrer que la stricte théorie binaire a ses limites, même s'il s'agit des langues qui ont été déjà explorées d'une manière satisfaisante. Je porterai mon attention particulièrement sur les langues slaves, et cela sur le russe en premier lieu.

Je considère comme indispensable de souligner d'avance que, même en supposant la validité générale des oppositions binaires, il est nécessaire de définir directement le

caractère de celles-ci, de les caractériser d'après leurs traits spécifiques et aussi de les classer, puisqu'elles ne sont pas toutes du même caractère. Naturellement, il ne s'agit pas seulement de différence de rang, par exemple d'après la fréquence des oppositions. Personne ne discutera, par exemple, les différences entre les consonnes sourdes et sonores. L'opposition entre les voyelles et les consonnes peut être également considérée comme binaire, mais il est clair qu'il s'agit là nettement d'une autre binarité que celle qui lie en couples les consonnes sonores et sourdes ou bien les voyelles courtes et longues. Les différences phonologiques entre les voyelles et les consonnes ne constituent pas de couples de corrélation, ce sont avant tout deux classes de phonèmes qui sont opposées l'une à l'autre. La situation est cependant compliquée par le fait que, dans maintes langues, certains phonèmes présentent une ambiguïté pour ce qui est de leur fonction et de leur caractère; par suite, ils peuvent avoir la validité des voyelles ainsi que des consonnes. Or, il s'agit donc plutôt d'une opposition ternaire que binaire.

Il existe également des sons qui se distinguent acoustiquement et des voyelles et des consonnes. Les phonologues harvardiens distinguent pour cette raison comme type individuel, par ex., le (j) russe, ils le comptent entre les "glides" tout en ne disant rien comment ce phonème s'intercale dans le réseau des relations binaires.

D'après moi, le principe de la binarité n'est pas tout à fait compatible avec la théorie des „triangles" phonologiques. L'établissement des affinités entre les triangles de voyelles et de consonnes est un trait incontestablement positif de la théorie des éléments distinctifs de M. Jakobson. L'affinité de certaines oppositions vocaliques et consonantiques est, à son tour, évidente: c'est là qu'il faut citer, par exemple, la corrélation de la nasalité. Beaucoup plus compliquée est la situation en ce qui concerne, par ex., l'opposition entre les phonèmes compacts et diffus. Peut-on dire, par exemple, du russe, que la voyelle *a* se distingue-t-elle des autres voyelles vraiment par la même qualité acoustique qui oppose les vélaires avec les palatales aux autres consonnes?

A l'intérieur des systèmes de consonnes, même certaines relations partielles présentent un caractère ternaire. Cela s'applique, par ex., aux consonnes affriquées; les (c), (č) russes sont en opposition à deux termes aux explosives alvéolaires et aux sifflantes. Ce fait découle déjà de leur caractère hybride. Les sonnantes peuvent être définies acoustiquement comme les sons hybrides envers les voyelles et les consonnes. C'est une catégorie transitoire. On peut caractériser la position des sonnantes dans les systèmes phonologiques directement par les modèles triangulaires. La classe des sonnantes russes diffère de celle des autres langues slaves. Les nasales russes, par ex., sont en relation de parenté étroite avec les consonnes orales (b, d) et leur amplification par la résonance de ton est facilement identifiable. De ce point de vue, le cas des liquides et du (j) russe est plus compliqué.

Pour la théorie binaire, les difficultés découlent également de certaines oppositions prosodiques. Cela s'applique aussi à l'accent qui, le plus fréquemment, est considéré comme une marque de corrélation dans le système des voyelles (opposition atonique).

D'après moi, l'accent du type russe n'est pas décrit d'une manière satisfaisante par l'opposition phonémique (au plan phonémique). Tout cela paraît sous un jour beaucoup plus compliqué, lorsque nous nous rendons compte que l'accent du type russe n'est pas seulement un élément phonémique, mais qu'il se rattache en même temps au mot. L'accent possède non seulement une fonction phonémique, il présente un pendant à la quantité vocalique, la fonction culminative en fait un élément constructif des mots de plusieurs syllabes. Le caractère particulier de l'accent soit nettement de la circonstance suivante: sa présence sur une syllabe d'un mot est toujours compensée par son absence sur une autre syllabe du même mot.

La position de la théorie binaire en phonologie est affaiblie par le fait que les applications diffèrent beaucoup entre elles, elles présentent quelquefois des formulations contradictoires.

En morphologie, l'application de la théorie binaire se heurte à des difficultés encore plus considérables. En syntaxe et en formation des mots, la situation est plus favorable, car c'est là que la conception binaire a déjà son ancienne tradition éprouvée. Le binarisme syntaxique, lui aussi, a cependant toujours ses problèmes définitivement encore non résolus. Il est d'ailleurs vrai que les deux relations syntaxiques fondamentales, la prédication et la détermination, forment nettement des couples de phrase, mais il s'agit, en même temps, de deux types différents d'oppositions binaires.

Je résume: les relations binaires constituent une base dans chaque système linguistique, mais on doit compter avec d'autres relations, en particulier avec les relations ternaires. Les relations binaires elles-mêmes sont différenciées dans leur structure.

Praha

SOME UNIVERSALS OF WORD ORDER

JOSEPH H. GREENBERG

Abstract

It has long been known that certain languages tend to put the modifier before the modified in differing constructions in a consistent way e.g. (Turkish, Japanese), while others favor the opposite order with the modified element before the modifier. Schmidt, in particular, emphasized the tendency of prepositions to go with nominative-genitive order and postpositions with the reverse. He likewise noted that the nominative-genitive order tends to appear in languages which have the order verb-nominal object while genitive-nominative order is correlated with the appearance of the noun object before the verb. By setting up a typology in which the order of the nominal subject in relation to the verb, which was ignored by Schmidt, is included, what appears to be merely a tendency in many cases leads to implicational universals without known exceptions. Since the nominal object practically always follows the nominal subject in the dominant pattern of main clause declarative sentences, in practice we reduce to three types VSO, SVO and SOV which may designate I, II and III respectively. Our other main typological criterion is the occurrence of postpositions as against prepositions. The non-existence of languages of Type I which are postpositional leads immediately to an implicational universal, namely, that if a language has dominant order VSO it always is prepositional. A consideration of noun-adjective order in relation to this typology also leads to interesting results. As is to be expected, NA order tends to go with I, and AN with its polar opposite III. It may be further noted, however, that whereas the number of AN/I languages is very small, the number of NA/III languages is considerable and NA likewise predominates over AN in languages of Type II. Therefore, the interpretation of a relatively weak statistical predominance of NA over AN as evidence for a universal tendency towards NA is greatly strengthened by considering it in the context of this typology, since almost all examples of AN are found to involve OV order likewise. A considerable number of other interpretations of universal scope are suggested by the use of this typology.

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DISCUSSION

WINTER:

Mr. Greenberg's conclusions are based on an inspection of some data from, and some statements about, thirty languages. If we assume that the total number of languages of the world is about 2,000, we have to take note of the disquieting fact that supposed universals have been established on the basis of a sample reflecting not more than 1.5% of the total. Mr. Greenberg's insistence that the languages used were truly representative of entire groups does not remove the difficulties: knowing, for instance, something about the degree of variety in word-order rules found in the

group of Indo-European languages, I wonder how any single one of them, or any small selection from them, could safely be considered representative of the entire group. Finally, not only the number of samples, but also the size of the samples is not such as to make Mr. Greenberg's findings persuasive. That a sample of only about one hundred sentences should be representative for purposes of syntactic observations has to be proved by a comparison with an entire series of samples of comparable length, taken from a variety of sources, before only generality can be claimed for the values obtained from the original sample, and only after the individual observations have been validated can any attempt be made to evaluate them in a universal context.

LAROCLETTE:

Il ne paraît extrêmement intéressant et instructif d'opérer, comme l'a fait M. Greenberg, sur des langues appartenant à des groupes très différents, un travail de comparaison portant sur la place respective des "modifiants" ou des "modifiés" dans la phrase.

Pour que ce travail aboutisse à des conclusions valables, il convient cependant que deux conditions essentielles soient remplies: 1) on ne peut comparer que des choses comparables; 2) on ne peut "forcer" la nature d'une langue en la classant dans un groupe donné parce que telle construction est dominante, alors que la construction opposée est également représentée.

L'état de choses qui existe dans les langues négro-africaines impose par exemple de tenir compte en particulier des observations suivantes.

1. Le nom sujet et le nom objet sont des formes libres dont la place, par rapport du verbe est moins fixe en générale que celle des pronoms sujet et objet. Des lors le comportement de ces derniers est au moins aussi intéressant à observer.

2. D'une manière générale, les possibilités d'inversion du nom et du pronom sujet ont été très mal étudiées (souvent elles n'ont même pas été examinées) dans les langues négro-africaines.

3. Le "génitif marqué", celui qui a une distribution interne, lorsqu'il coexiste avec le "génitif construit" se caractérise souvent par une plus grande mobilité. Il faut de toute façon les examiner séparément.

4. au "génitif construit", le pronom s'occupe par toujours la même place que le nom.

5. Lorsqu'un nom est juxtaposé à un autre nom, il peut constituer une apposition qui a la valeur d'un adjectif; cette apposition n'occupe toujours le même place que le génitif.

6. Il est nécessaire de s'entendre sur ce qu'on peut appeler "préposition" et "post-position"; il y a au moins six sortes de morphèmes qui peuvent précéder ou suivre un nom (ou une radical nominal): a) des ponctifs, b) des translatifs, qui changent la classe de distribution du nom, c) des anaphoriques, d) des locatifs, e) des démonstratifs, f) des classificateurs, g) des numérateurs.

Le même élément peut avoir plusieurs fonctions différentes; des éléments différents peuvent occuper des places différentes; dans certaines langues, certains morphèmes

sont doubles (en Lande par exemple, les mots suivis d'une postposition locative sont souvent précédés également d'une préposition locative).

7. Il convient aussi de s'entendre sur ce qu'on appelle un adjectif. Le numéral, le démonstratif, le possessif n'occupent pas nécessairement la même place que le qualificatif.

Faute de faire toutes les distinctions, on risque d'introduire de l'arbitraire dans l'établissement de "types" de langues.

DANEŠ:

Greenberg's lucid paper does not fully respect the great complexity and intricacy of word-order in natural languages. Now, he assumes that in every language a certain word-order *is dominant*. Yet we know that languages with so-called *free word-order* exist, i.e. languages in which the rules by which the word-order is governed do not belong to grammar (e.g. the Slavic languages). In those languages the arrangement of words in the utterance is to a great extent determined by other factors, such as context, situation, emphasis, and so called "topic—comment" organization of utterance.

My second remark pertains to such formulas as AN, NA, SVO. Under such formulas are hidden two essentially different types of word order, which must be distinguished: thus in the case of French word order NA or Latin SV, this order is simply fixed and the violation of this rule results in a sequence of words that has a very low degree of grammaticality (in fact it may be identified by the native speaker as the same sentence, but in a "broken" form).

On the other hand, e.g. in the case of the English word order SVO ("Mary hates John"), the violation of the rule necessarily leads to a new, different sentence or utterance ("John hates Mary").

In the first case we might speak about simply *ordering* rules, while the other rules are *functional* ones (the word order is a syntactic device for establishing systematic oppositions, e.g. S *versus* O). This difference I find a fundamental one even for establishing a set of differential criteria for linguistic typology.

THE STRUCTURE OF MEANING

ROBERT L. ALLEN

Perhaps the most natural way to conceive of "meaning" is as a dyadic relation between a sign and an object.¹ But such a conception makes no allowance for intermediary interpreters of the sign, for any two of whom the same sign may hold different "meanings" because of their own different past experiences.

Yet even if we grant, for the sake of argument, that a given sign may have the same "meaning" for all its interpreters, there is still the possibility of polysemy to be reckoned with:² if I utter the phoneme-sequence /bəl/, for example, you cannot tell whether I am referring to a spherical object or to a formal dance — or am indeed even ordering you to cry with a loud noise. In order to distinguish between the total range of potential "senses" of a linguistic form and the specific combination of semantic components denoted or connoted by the form in a specific situation, I will reserve the label "meaning" — or "meanings" — for the former, and will use the term "signification" for the latter. Thus a dictionary lists the *meanings* of linguistic forms, not their significations.

Even if a linguistic form has only one "meaning", this meaning is not necessarily its signification: a given form has signification only when it is used in a meaningful or "significant" situation.³ If I were to utter the expression "Safety pin" out of context, for example, I doubt that my utterance would really signify anything to you. It might suggest a certain kind of object, but the mere suggesting of an object can hardly be called a meaningful or significant utterance. If I were to say "Safety pin" and then stop, you might well ask me, "What do you mean?"

But what is it that makes a situation meaningful or significant? I suggest that significance always involves two essential factors, which I can perhaps best demonstrate by means of an example. I have here two rubber balls; I ask you to examine the *small* ball very carefully. . . . I beg your pardon — I held up the wrong balls. I will again hold up two rubber balls; again I ask you to examine the *small* ball

¹ Cf. the discussion in Rulon S. Wells, "Meaning and Use", *Word*, 10 (1954), 236-238.

² See in this connection the interesting discussion of polysemy by Uriel Weinreich in "On the Semantic Structure of Language", in Joseph Greenberg (ed.), *Universals of Language* (Cambridge, Mass., 1963), pp. 114-171.

³ Quine prefers the term *significant* to *meaningful* when applied to utterances. See Willard Van Orman Quine, *From a Logical Point of View*² (Cambridge, Mass., 1961), p. 11.

very carefully. . . . I suggest that you are now staring at the very ball which a moment ago you rejected as not being the one I meant by my expression "the small ball". In other words, "the small ball" of the present situation is different from "the small ball" of the first situation. Obviously, "smallness" is not an inherent attribute of this ball, even though I applied the term "small" to it. What, then, did I mean when I said "the small ball"?

It would appear that the signification of *small* in the expression "the small ball" was not a single referent or idea such as "smallness", but rather *a relation* which I perceived as holding between one ball and the other but which I expressed as if it were an attribute of one of the balls.

Such a relation bears a striking resemblance to the figure/ground relation which some psychologists consider basic to all perception. But in the kind of relationship which we are examining here, the ground need not appear as part of the immediate environment of the figure. It is highly improbable, for example, that any member of this audience attached any significance to the fact that on both the occasions when I asked you to examine "the small ball", the small ball was in my *left* hand. Probably on both occasions, the only part of the total context which any of you perceived as *relevant* to "the small ball" was the other ball — that is, the bigger ball.

Instead of the terms "figure" and "ground", therefore, I will use the more general terms "focus of attention" and "relevant context", which I will symbolize as *f* and *c*. Thus we may say that the signification of a meaningful linguistic form is the relation between a given focus and that part of its context which is perceived by the interpreter as relevant — or, more briefly, that the form signifies *f/c*.

I suggest that every signification entails these two essential factors: namely, a focus of attention, and a relevant context. Without one or the other there can be no signification, and therefore no meaningfulness. We might say that the recognition of "significance" or "meaningfulness" is basically the recognition of the relatedness of a focus to some part of its context.

The importance of the focus will become evident if once again I ask you to examine these two balls. This time, however, I will ask you to focus your attention on the *other* ball, not on the small ball. Now, although the actual *relation* between the two balls has not changed, the focus of attention has: the second ball is now *f*, and the first is *c*. Thus *f/c* now represents an entirely different signification from the signification it represented earlier, as can be seen from the fact that a different adjective is required to express this new signification: the ball you are now focusing on is "the *big* ball", not "the *small* ball".

Such a theory of significance has implications for more than just the use of language, but their discussion would make this paper too long. It must suffice to say that probably nothing has significance in and of itself; only when something is perceived as focus in relation to some relevant context does it become meaningful. (The specific relationship perceived as holding between a focus and its context will of course differ from situation to situation. It may be any one of several possible kinds of

relationship, such as identity, similarity, contrast, and the like — or possibly even combinations of these.)

The relevant part of the context of a given focus does not have to be present physically. If I were to ask you to describe this cup, for example, most of you would probably call it “a *big* cup”, where the adjective *big* would signify not a relation between two visible cups, but rather a relation between this cup and your past experience of cups. Since different interpreters will have different memories of the size of “ordinary” cups, the focus/context relation expressed by *big* with reference to this cup cannot have exactly the same signification for all those who see it. Instead of representing a given signification by f/c , therefore, it would perhaps be more accurate to represent it by $f/c_1, f/c_2, f/c_3, \dots$, where $c_1, c_2, c_3 \dots$, stands for the relevant context as perceived by different interpreters. Such a representation would allow for the truism that one and the same expression may mean different things to different people.⁴

But to turn more specifically to an examination of significant *utterances*, it appears that the linguistic unit most commonly used to express a focus/context relation is the sentence, which is basically a means for bringing some context (usually expressed as a so-called “predicate”) into a relevant or significant relationship with some focus (usually expressed as the “subject”). There is a germ of truth in the traditional grammarian’s claim that “a sentence expresses a complete thought”. But both the focus and the context need not be expressed in words. It is a common device in advertisements, for example, to show the focus by means of a picture (such as the picture of a tired-looking person) with only the context expressed verbally (as in the caption “Feeling tired?”). Such an advertisement would then probably treat the tired person (with whom the reader is supposed to identify) as a complex focus for the new context expressed in some such sentence as “Then you need Polly’s Prompt Pick-up Pills”.

Many of the transformations described by transformational grammarians seem to be primarily means for building up complex foci or complex contexts. A so-called “kernel sentence” such as *The cup is in my hand* expresses a simple focus/context relation; then by means of a transformation this focus together with its context can be made to serve as the focus for another context, as in *The cup in my hand is big*. This in turn can be transformed into *The big cup in my hand*, to serve as the focus for still another context.

I suggest that a linguistic form, be it a morpheme or construction, has a signi-

⁴ The fact that different interpreters may not perceive the same part of the context of a focus as significantly related to the focus raises several questions with regard to so-called synonymy. For an interpreter who does not know that Sir Walter Scott wrote *Waverley*, for example — or even for an interpreter who has read some of Scott’s novels but knows no more about *Waverley* than that it is the name of a book — the expression “The author of *Waverley*” cannot have the same signification that the name “Sir Walter Scott” has. One of the chief problems of the lexicographer is the selection of definitions suggesting context-focus relationships that will seem significant to the largest possible number of his readers.

fication only where it participates in the expression of some focus/context relationship, since only such relationships are significant. Thus the word *cup* by itself — or even as an item in a dictionary — has no real signification but only a potential meaning; but in a naming situation, as when an English teacher teaching foreign students holds up a cup and pronounces the word at the same time, the word *cup* does “signify”.

Most commonly, of course, a linguistic form takes on signification by virtue of its occurrence in some utterance. Bazell has pointed out that “neither the morpheme nor its place in the pattern . . . has meaning in their own right: their combination constitutes the *signifiant*”.⁵ Pike considers the correlation between a position (or “slot”) and a morpheme class one of the basic characteristics of the grammatical unit which he calls a “tagmeme”.⁶ Accepting his term, we may then call the correlation between a specific position and the specific morpheme occurring in that position a “tagma”. If now we let *s* represent the linguistic context or *setting* in which a given linguistic form, *l*, occurs on a specific occasion, we may paraphrase Bazell and say that usually neither *s* nor *l* has meaning in its own right, but rather that their combination — that is, the tagma *l/s* — has signification, or “signifies”. Thus, although the morpheme (or morphemes) pronounced /bəl/ may connote a large area of total meaning, the tagma manifested by the morpheme *ball* in the sentence *There is a ball in this box* has a very limited signification.⁷

For Pike, the “slots” in the kind of slot-class correlations which he calls tagmemes seem always to be positions in utterances. Now it is true, of course, that the position of the morpheme *ball* in the sentence *There is a ball in this box* limits the potential meaning of *ball* to the specific signification it has in this sentence. But a context other than that of syntactic position may accomplish the same result. If, for example, I hold up a ball in plain view of this audience and utter only the single morpheme “Ball”, it is probable that every member of the audience will take the morpheme as having the strictly limited signification “a spherical object”. Perhaps we may broaden the definition of a tagma so as to have it include contexts other than purely verbal contexts. The *s* in our formulation will then represent not merely Pike’s “slot” but rather the total context or *setting* in which a specific linguistic form occurs.

Within an utterance, each linguistic form has its own setting, on its own level. Thus, in the sentence *The ball in this box belongs to my son*, the prepositional phrase *in this box* is part of the relevant setting of the noun *ball* but not of the noun *son*. But it may happen that the relevant setting for a given linguistic form will lie outside

⁵ C. E. Bazell, “On the Problem of the Morpheme”, *Archivum Linguisticum*, 1 (1949), 1.

⁶ See, for example, Kenneth L. Pike, “On Tagmemes, Née Gramemes”, *International Journal of American Linguistics*, 24 (1958), 275-276.

⁷ It may be noted that when we confine our discussion of meaning to the significations of tagmas, we do not have to commit ourselves as to whether the two occurrences of *ball* in the sentences *There is a ball in this box* and *Those Jones boys always ball everything up* are occurrences of the same morpheme or of different morphemes.

the utterance in which that form occurs — perhaps in a preceding sentence, or even (as in the case of deictic signs) in the speech situation in which the form is uttered.

In general, a speaker uses different kinds of linguistic forms for the expression of different parts of focus/context structures. We may say, roughly, that an English speaker uses nouns for the expression of simple foci; verbs — or, more exactly, verbs with their complements — for the expression of contexts; such words as prepositions, and such endings as the possessive suffix, for the expression of relations between different elements within his utterance; modifiers like adjectives and some adverbs for the expression of relations between elements *in* his utterance and other elements (possibly unexpressed) which may be present in the given speech situation, or which the speaker may merely assume to be part of his audience's past experience.

Much has been made of the difference between “meaning” and “naming”.⁸ In trying to analyze this difference, it may help if we put ourselves in the position of someone trying to learn the “meaning” of an unknown word in a foreign language. If you will bear with me for a moment, I will try to teach you — by demonstration — one “meaning” of the Persian word /nazok/. First, I will hold up a sheet of [white] paper and say, “This is /nazok/.” Then I will hold up a white card and repeat, “This is /nazok/.” Then I will hold up a piece of white cloth, again saying, “This is /nazok/.” Next I will hold up this [thin] white book: “This is /nazok/.” Then I will hold up this [thin] blue book: again, “This is /nazok/.” And finally I will hold up a yellow card and a sheet of blue paper and a thin red book, saying for each, “This is /nazok/.” . . . By now you may have surmised that something which is /nazok/ is “thin”.

Throughout my demonstration the tagma manifested by /nazok/ in the sentence *This is /nazok/* remained constant. Thus you naturally assumed that the signification of that tagma also remained constant, even though the focus of attention differed each time I held up a different object. In other words, even though the focus kept changing, the relation holding between the focus and its relevant context did not change.

My guess is that the first time I said “This is /nazok/” — while holding up a sheet of paper — some members of this audience assumed that /nazok/ was the Persian word for “paper”. That is, *they took the paper itself as the relevant context for the paper on which they were focusing*. I suggest that this is exactly what occurs in the process of “naming”: naming is the special case of signification where that part of the context of the focus perceived as relevant is the focus itself. We can symbolize this by stating that, in naming, *l/s* signifies *f/f*.

As I held up other objects, however, it became evident that the word /nazok/ could not signify “paper” (that is, the relation of paper to itself), but must signify some other relation, a relation which remained constant for each different focus. Many members of this audience would probably say that they had tried to find the single attribute which all the objects I held up had in common. Thus, after being

⁸ See, for example, Quine, pp. 9, 21-22, 47-49.

shown a white card and a white piece of cloth and a white book, some of you probably assumed that /nazok/ meant "white". It is this kind of analysis which has led some philosophers to posit the existence of such abstract entities as attributes (in this case, for example, the attribute of whiteness). But to paraphrase Quine, one may admit that there is white paper and white cloth and a white book, "but deny, except as a popular and misleading manner of speaking, that they have anything in common".⁹ It was the *relation* of the color of each object to the object itself that remained constant, not the whiteness.¹⁰

It appears that in the relation *f/c*, the focus must be *unitary*, although its relevant context need not be. The focus of one's attention must always be some kind of unit or gestalt, but the context within which the focus is perceived may be either structured or unstructured, either one unit or more than one unit — or no unit at all.

When the relevant context for a given focus is itself a unit (or contains a unit), this unit in turn may become the focus for another signification. This "shift of focus" is exactly the process we went through when we concentrated first on the small ball, then on the big ball. Such expressions as *Caesar was stabbed by Brutus* and *the stabbing of Caesar by Brutus*, derived from *Brutus stabbed Caesar*, are further exemplifications of shift of focus. There is some evidence to support the hypothesis that people differ in their ability to shift foci, especially in signification-complexes larger than those corresponding to single morphemes. Rigidity in one's thinking may possibly be linked to an inability to shift foci easily. Certainly a person cannot claim to have examined an issue from his opponent's point of view simply because he has listened to the arguments advanced by his opponent: if he has maintained *his own focus* throughout, all of the arguments he has listened to have become merely additional parts of the context of his own focus. Not until he has actually shifted his focus and has, for a while at least, examined the context in its relation to *his opponent's focus*, can he say that he has really understood "the meaning" of what his opponent has said. Even the same context perceived in relation to different foci necessarily produces different significations.

To sum up, then: By means of focus/context relationships, commonly expressed in language as subject/predicate relationships — and by means of transformations applied to such relationships — we constantly build up larger and larger contexts around the foci of our significations; and for some of us, at least, these larger contexts in turn serve, by shift of focus, as complex foci for still larger signification-complexes. Of such is the structure of meaning.

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⁹ Quine, p. 10.

¹⁰ The potential for occurrence of certain tagmas in a person's language may influence his perception of significations: if we had a different word in English for the "whiteness" of paper as opposed to the "whiteness" of cloth, for example, it is possible that no native speaker of English would have assumed /nazok/ to mean "white".

STATISTICAL CONSIDERATIONS AND SEMANTIC CONTENT

LUIGI HEILMANN

The brief considerations set forth here are the gist of a more detailed study, in Italian¹; they are a tentative answer to the question put by the Organizing Committee of the Congress: "To what extent do statistical considerations define semantic content?"

It is a well known fact that the emergence of structural principles in modern linguistics opens new prospects to statistical methods, which point out the quantitative features implied in the functional yield of the units as well as the frequency of the oppositional patterns. Yet many authors still appear to distrust or overlook the application of statistics and mathematics to language problems.² But everyone concerned with methodological problems must acknowledge that recent formulations and experiments mean that the discussion cannot be delayed³.

Now, in a classification dividing sciences into natural – dealing with laws – and statistical ones – dealing with trends – linguistics may appear to be a typical statistical science⁴ in that it deals with communicative behavior, with events neither completely predictable nor completely unpredictable.⁵ And indeed, the statistical law of probability operates in language, as a system; the occurrence of units in speech and writing is not arbitrary, it is determined by the inherent characters of signs: function and its physical and psychical co-ordinates. In other words, a great similarity exists between the members of speech communities as regards the frequency of use of linguistic units. Therefore, a quantitative definition of linguistic events emerges beside the qualitative one. The latter concerns the diacritical function and the

¹ See *Quaderni dell'Istituto di Glottologia*, VII (Bologna, 1962), 1 f.

² Cf. I. Jordan's words in his Report on *L'état actuel de la linguistique romane et ses perspectives de développement*, Strasbourg Apr. 27, 1962 (IXth Int. Congress of Romance Linguistics and Philology): "Ils [les romanistes] négligent et, assez souvent, ils méprisent peut-être les courants modernes, tels le descriptivisme, le structuralisme, la linguistique mathématique... Ce qui surprend, d'une manière assez pénible, ce n'est pas la réserve à l'égard de ces nouvelles méthodes, mais le fait que leurs adversaires, qui sont très nombreux parmi les romanistes, n'interviennent pas dans les discussions théoriques qu'elles suscitent." See also G. Herdan, *Type-Token Mathematics. A Textbook of Mathematical Linguistics* ('s-Gravenhage, 1960), 32.

³ For details see e.g. P. Guiraud, *Bibliographie critique de la statistique linguistique* (Utrecht-Antwerp, 1954) and *Trends in European and American Linguistics 1930-1960*, ed. on the occasion of the IXth Int. Congress of Linguists, Utrecht-Antwerp, 21-57 (W. Plath, "Mathematical Linguistics").

⁴ Cf. P. Guiraud, *Problèmes et méthodes de la statistique linguistique* (Paris, 1960), 14-15.

⁵ Cf. G. A. Miller, *Language and Communication* (New-York, 1951), 8.

semantic one, the former concerns the statistical function and functional yields⁶.

It is not worth emphasizing here the importance of statistics in dealing with synchronic, diachronic, and stylistic problems, but I think one must not disregard the inherent limitations of this method. Firstly, the statistical study of the relative frequency of units in a language can be carried on whenever we have at our disposal copious data relating to normal utterances. In fact such records are still lacking for many languages and many epochs.⁷ Secondly – to quote Prof. Ullmann – “in language, quantity can never eclipse quality”.⁸ Our research does not end by determining mathematical schemes, it aims at entirely interpreting linguistic reality. “Not every scientific treatment”, R. Wells very rightly said, “is mathematical. And neither is every mathematical treatment scientific; for it is possible to mis-apply mathematics, and to mis-interpret its results”.⁹ In this connection it is worth while to recall that the mathematical approach to the evaluation of the rate of change in language has sometimes led glottochronology to conclusions that clash with well ascertained historical facts.¹⁰ Now, we cannot leave out of consideration the question put by Prof. Hönigswald: “How unstatistical is linguistics?” It implies, unavoidably, that “the field of language requires its own statistical methods”, which, in their turn, must aim at “the reconciliation of differences in quality and differences in quantity”.¹¹

A momentous improvement in working out the principles of an independent language statistics appears to be Herdan’s theory equating Saussure’s dichotomy of language into “langue” and “parole” to that between the concept of the statistical population and of random samples from it.¹² In the frame of the principles of such a quantitative linguistics the concept of the frequency of occurrence of linguistic forms has been universally recognized as a basic aspect of language.¹³ As a matter of fact, the investigations carried out till now stress the interdependence between frequency and phonic structure, phonic shape, age of the units, etymological class, number of meanings, etc. Traditional linguistics, for its part, had ascertained, long before, many facts linked with frequency as, for instance, its increasing under the pressure of social needs, the wear and tear of language units, and their analogical changes.¹⁴ Last, everyone must now take for granted that statistical features are actual attributes of style.¹⁵

⁶ See P. Guiraud, *Stat. Ling.*, Ch. II, 14 f.; G. Herdan, *Language as Choice and Chance* (Groningen, 1956), 67.

⁷ See L. Bloomfield, *Language* (London, 1957) 277.

⁸ Cf. S. Ullmann, *The Principles of Semantics* (Glasgow-Oxford, 1959), 2nd ed., 294; see also p. 307.

⁹ R. Wells, “A Mathematical Approach to Meaning”, *CFS*, XV (1957), 117-136.

¹⁰ See E. Coseriu, “Critique de la glottochronologie appliquée aux langues romanes”, paper read at the group meeting of Apr. 24, 1962 in Strasbourg (IXth Int. Congress of Romance Ling. and Phil.).

¹¹ Cf. G. Herdan, *Type-Token*, 70; J. Whatmough, “Statistics and Semantics”, *Debrunner-Festschrift*, 445-46.

¹² G. Herdan, *Type-Token*, 34, 230.

¹³ G. Herdan, *Loc. cit.*, 21; see also p. 39 and P. Guiraud, *Stat. Ling.*, Ch. II, 25 f.

¹⁴ Cf. L. Bloomfield, *Language*, 275 f., 404-410.

¹⁵ In this field the statistical method was vigorously developed by Guiraud’s and Herdan’s works. See also W. Plath, *Trends*, 26 f., and Ch. Muller, “Les ‘Pronoms de dialogue’: interprétation stylistique

But the leading principles of quantitative, mathematical linguistics and the methods arising from it – basic, as they are, for information theory and for many synchronic and diachronic problems – are they also convenient to the analysis of semantic content? It goes without saying that a genuine quantitative approach to language must rely upon structural principles, but it is a well known fact that many structuralists are not interested in semantic problems and feel reluctance to handle them; indeed, some even state that semantics is unsystematic and therefore inaccessible to structural methods.¹⁶ Now, in fact, a structural description can be applied more easily to the clear-cut problems of phonemics and morphology than to the loosely organized field of semantic relations, where we are in danger of sliding from the linguistic method to the logical or psychological ones; but how can a structuralist paradoxically grant that a section of language, indeed *the* most important section for the purpose of communication, is unsystematic and not structured? Immanent linguistics calls – on the contrary – for determining the functional relations connecting units in the synchronous system.

The functional conception of meaning is set forth in an exemplary way by Prof. Ullmann¹⁷ stating that between acoustic shape (*name*) and mental content (*sense*) an evocatory reciprocal relation (*meaning*), takes place, which is the main object of semantics. Meaning, as such a relation binding *name* and *sense* to each other, is conventional as well as motivated (in such semantic types as onomatopaeias, compounds, derivatives, metaphors, metonymies, popular etymologies). Prof. Ullmann's statements have their foundation and complement in the theory of the synchronous associative fields which has lately strengthened ideas about "semantic systems".¹⁸

If then the single elements of language are interpreted as components of higher units from which they derive their significance, that is if the unit takes on different significance in different constellations, the question arises to what extent any verbal unit is determined by the other verbal units surrounding it.¹⁹ The answer appears to be easy enough for those types of units that Prof. Hjelmslev names *classes fermées*, very difficult for those types which are comprisable within a theoretically unlimited *classe ouverte*.²⁰ As Prof. Hjelmslev says "Une description structurale ne pourra s'effectuer qu'à condition de pouvoir réduire les classes ouvertes à des classes fermées"²¹; which we can evidently undertake only resorting to commutation.²² But

d'une statistique de mots grammaticaux en français", paper read at the group meeting of Apr. 27, 1962 in Strasbourg (IXth Int. Congress of Romance Ling. and Phil.).

¹⁶ See S. Ullmann, *Principles*, 317 f., and L. Hjelmslev, "Pour une sémantique structurale", *TCLC*, XII (1959), 97, 101, 111; Th. E. Hope, "L'interprétation des mots d'emprunt et la structure lexicale", paper read at the group meeting of Apr. 25, 1962 in Strasbourg (IXth Int. Congress of Romance Ling. and Phil.).

¹⁷ See especially *Principles* and *Précis de sémantique française* (Bern, 1952).

¹⁸ See the essential of this theory in S. Ullmann's *Principles*, Ch. II, 152-170.

¹⁹ Cf. G. A. Miller, *Language*, 81.

²⁰ Cf. L. Hjelmslev, *TCLC*, XII (1959), 110.

²¹ L. Hjelmslev, *ib.*

²² L. Hjelmslev, *ib.*, 102.

commutation means, in his turn, to have recourse to value, that is to a formal differentiative fact unrelated to actual material utterances. The intercourse between value and meaning is not immediately intuitive; as was already pointed out by Saussure:

La valeur, prise dans son aspect conceptuel, est sans doute un élément de la signification, et il est très difficile de savoir comment celle-ci s'en distingue tout en étant sous sa dépendance... Faisant partie d'un système, [le mot] est revêtu, non seulement d'une signification, mais aussi et surtout d'une valeur, et c'est tout autre chose... Quand on dit que [les valeurs] correspondent à des concepts, on sous-entend que ceux-ci sont purement différentiels, définis non pas positivement par leur contenu, mais négativement par leurs rapports avec les autres termes du système. Leur plus exacte caractéristique est d'être ce que les autres ne sont pas."²³

If we cling to Saussure's statements, we must own that value does not yet express anything specifically semantic: "ayant un caractère purement différentiel, oppositif et négatif, la valeur n'a encore rien de sémantique"²⁴; value is only "l'élément qui sert à définir l'agencement paradigmatique des corrélations"²⁵. Our attention must, therefore, be focussed on meaning. Saussure's theory of sign as a whole combining two sides develops profitably into Prof. Ullmann's definition of meaning as a reciprocal relation between name (= *signifiant*) and sense (= *signifié*).²⁶ In this way, the sense assumes an intermediate function between name and referent and partakes – at the same time – of the nature of language and of the non-linguistic. Therefore, the study of sense – as far as the relations between things and mental representations are concerned – is beyond the domain of linguistics.²⁷ Linguistics does, on the contrary, include the study of the basic feature of functional semantics, the binary relation obtaining, in the opposite direction, between name and sense.

This relation, potential and – I dare say – ambiguous on the level of lexical units (that is the units of *la langue*) endowed with an essential or central meaning, comes at actualization on the level of words (that is the units of *la parole*) provided with an applied or contextual meaning and the feeling-tones of style.²⁸ Now, what is conveyed in communication is the name, the expression; it acts as a stimulus, but in order that it can act as such and assume social significance, it is necessary for the members of the communicative event to be previously provided with the same system of engrams, that is the same *a priori* structure of senses,²⁹ in many respects similar to, but not identical with the structure of names, as conceptual structure is distinct from linguistic form.³⁰ It is a clear-cut fact that the evocative operation in speakers and hearers is

²³ Fr. de Saussure, *Cours de linguistique générale*, 5th ed. (Paris, 1955), pp. 158-162.

²⁴ L. Hjelmslev, *TCLC*, XII (1959), 106.

²⁵ L. Hjelmslev, *ib.*

²⁶ See *Principles*, 43-106.

²⁷ *ib.*, 163.

²⁸ A. H. Gardiner, *The Theory of Speech and Language* (Oxford, 1932), 44, 50; G. A. Miller, *Language*, 89.

²⁹ G. Herdan, *Type-Token*, 210.

³⁰ See Z. S. Harris, "Distributional Structure", *Word*, X (1954), 152; see also G. Herdan, *Type-Token*, 209.

essentially an individual creative operation, acting on similar structures and qualified, each time, by different physio-psychical agencies.

At this point it is worth recalling that information theory as well as all statistical calculations concern names, that is the statistical structure of formal representations, not the semantic content of codes and messages. The independence of semantic content and information was expressly stated by many statisticians of language; they calculate the quantity of information of a sign (proportional to the negative logarithm of his probability), not the information content.³¹ In other words, the statistical information theory is interested in systems of empty units. This does not mean that his data are no use to elucidate content problems. I will only remember, for an example, as number and frequency are powerful factors of changes of meaning.³² But do they bring facts to define and to elucidate meanings?

For a semanticist the relations between information and content are very important, and frequency is not conceivable as directly determined by phonetic form. For him frequency is determined by psychological needs; it is a variable feature of concept, and this feature is translatable into a quantitative feature of form inasmuch as conceptualization cannot do without signs, in other words, as thought cannot do without language.³³ The paramount importance of concept frequency appears in all those instances where frequency comes to clash with the cost of signs, bringing about shortenings, substitutions or such modifications referred to as popular etymology.

Mathematical linguistics can, therefore, quantify the formal features of language and it gives excellent results in methods that leave meanings out of consideration, or in the valuation of functional yields.³⁴ Must we, on the contrary, infer that quantitative linguistics can by no means help the semanticist intent on defining meanings? Let us go back, once again, to Prof. Ullmann's terminological definitions, to his fourfold classifications of *lexical morphology*, *lexical semantics*, *syntactic morphology*, *syntactic semantics*.³⁵ It is clear that the first and the third sections, dealing with morphology, cannot leave out of consideration the statistical data and the mathematical quantifications; not so the second and the fourth sections dealing with semantics. Yet I would not go so far as to say that these cannot get any advantage out of mathematical treatments. I am not referring to formal analyses of linguistic statistics as they were given forth till now, but I mean just that mathematical treatment of meaning known as the method of *factor-analysis*.³⁶

This method is a means of measuring some fundamental components of meaning through the relations between a basic concept and bipolar adjectives; so it can test

³¹ Cf. W. Plath, *Trends*, 30-31, 27; see also P. Guiraud, "Langage et communication. Le substrat informationnel de la sémantisation", *BSL*, 1954, 119, 121, 125.

³² Cf. L. Bloomfield, *Language*, 409-10, 404 f., 275 f.; W. Plath, *loc. cit.*, 29.

³³ See P. Guiraud, "Lang. et Comm.", 127-128; G. Herdan, *Type-Token*, 230; see also L. Rosiello, "La semantica: note terminologiche ed epistemologiche", *AGI*, XLVII (1962), 32-53.

³⁴ Cf. G. Herdan, *Type-Token*, 330.

³⁵ Cf. S. Ullmann, *Principles*, 33-34.

³⁶ See R. Wells, *CFS*, XV (1957), 135-136, and L. Rosiello, *loc. cit.*

experimentally the dimensional structure based on binary comparisons and the place in a system. Factor-analysis supplies a standard determining in what extent two concepts are similar and in what extent they differ; in other words, factor-analysis is a statistical method by which we can detect the fundamental factors explaining the interrelations of a population of empirical variables and estimate the psychological considerations which are involved in determining meaning as a basis of semantic choice. Factor-analysis can be a method in reducing to *classes fermées* the lexical units of the *classe ouverte*, and gives an objective foundation to disclose a dimensional structure in vocabulary. But it has in itself its bounds and cannot therefore be acknowledged as a method quite suitable to an exhaustive definition of semantic content in general. As a matter of fact, factor-analysis relies upon a choice of bipolar opposite types whose semantic content is preliminarily accepted and, in this way, not mathematically tested.

To sum up, an integration between quantitative and qualitative methods is desirable in semantics also, but it is clear that language features that are inherently connected with semantic content cannot be completely represented in formal mathematical schemes.³⁷

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DISCUSSION

HERDAN:

Professor Heilmann's paper is an interesting contribution to the vexed problem of statistics vs. semantics, which is very little understood. What statistics are here often blamed for, viz. that they disregard the grammatical connection and the finer shades of meaning of words is, rightly understood, the very virtue of statistics. Statistics can be defined as the mathematical science which enables us to draw conclusions from incomplete evidence. It follows that if we are in a position to take everything possible into account in all its detail, there would be no further need for statistics.

The abstraction from meaning which is implied in this makes mathematical linguistics appear to be in full accord with L. Bloomfield's requirement for linguistics as a science, viz. that the emphasis on meaning must be severely curbed.

Mathematical linguistics can be defined as the application of mathematics to such aspects of language as are, wholly or partly, independent from meaning. And it is only to such language aspects that mathematics can be applied. Such features are: the sounds of language, which de Saussure had already recognised as being independent of meaning; word frequency, whose nature as a chance or random variable has been established beyond reasonable doubt; and – to a much lesser extent – word order within the sentence. Turning in particular to the second variable, the discussion

³⁷ See N. D. Andreyev, "Models as a Tool in the Development of Linguistic Theory", *Word*, XVIII (1962), 197.

on word frequency and meaning mostly suffers from the lack of a clear distinction between the frequency of particular words and word groups with a particular frequency. That the frequency of use of a particular word will depend largely upon its meaning, and with it its usefulness in the universe of discourse, goes without saying. However, in a group of vocabulary items, all of which have the same occurrence frequency, the frequency is clearly independent of meaning, since all the different vocabulary items in the often very large frequency group have, by supposition, the same frequency. Thus, although individual meaning and frequency are connected, the general structure of frequency is such as to be largely independent of meaning. It may be said to represent the structure of meaning in general. If a literary text is divided into a number of equal parts, and the vocabulary connectivity in those parts is ascertained, or if a number of equal samples from a group of texts is so compared, vocabulary connectivity has been found to be very much in accordance with a random distribution, the so-called Random Partitioning Function, which I believe is what has been sought under the term of the general structure of meaning.

POTTER:

We should be reluctant to put any aspect of meaning, however untidy, outside the province of linguistics and then say that it is no longer our concern. This is too easy. Indeed, it is unprofitable to delimit any science unduly. We may profitably regard semiotics, that comprehensive science of signs, as reaching far beyond the bounds of semantics, which is included within it. But semantics, the science of meaning, has its three levels of semantic value, lexical meaning and contextual sense corresponding broadly to the three levels of analysis – phonemic, morphological and syntactic. All of these lie wholly within the domain of linguistics proper although all or some may be temporarily excluded for the purpose of detailed examination or analysis.

THE HEREDITARY AND ENVIRONMENTAL COMPONENTS OF LANGUAGE

F. BROSNAHAN

That language is of human rather than of divine origin is a proposition which has not been seriously contested since the eighteenth century. Nevertheless, some of the corollaries of this proposition do not seem to have been fully realised, and the purpose of this paper is to present, at a high level of generality, a survey of the theory that language, as a property of the human species, is the result of human heredity and human environment, and of nothing else.

LANGUAGE AND THE SPECIES

Language has evolved clearly, and I take it that there is no dispute about this, on the basis of abilities available to man. These are such abilities as to produce and perceive sound within certain ranges of frequency and intensity, to experience, within the limitations of his sensory apparatus, his internal and external world and to segment and organise this experience, to associate segments of experience with specific stretches of sound and so form symbols, to manipulate these symbols in such a way as to build up more complex symbolizations, and so on. It may well be that the full development of some or perhaps all of these abilities depends on the use of language; it may equally well be that these abilities are more strikingly developed in modern man than in his earliest ancestors; but it does not seem possible to imagine the development of language save on the basis of at least rudimentary forms of such abilities.

These abilities of human beings are based on and derive from the structure and functioning of a complex manifold of nerves and sensory apparatus, of organs and muscles — a manifold which for convenience I shall term the “language apparatus”. This language apparatus, in its turn, is the product of heredity and environment.

The nature of the human language apparatus is laid down by the heredity of the species; and develops in the general environment in which that species lives. The multifarious organs which go to make up this apparatus, their general form and structure, their innervation and their interconnexions are all determined — potentially — in the heredity of the species. Further, the nature of their operation and the limits of their capacities are also, in broad outline, potentially determined by heredity:

that the ear is sensitive to wave-like variations in the molecular compression of the air (within an average frequency range of about $32\frac{7}{8}$ cycles to nearly 20,000), that the tongue has a characteristic range of movement possibilities, and so on, are so determined.

Equally, of course, an environment is necessary in which these hereditary potentials can develop and mature. Such environment will comprise both a physical and a socio-cultural part, and included in the latter will be language itself.

We have as yet little evidence of the effect that features of the physical environment — e.g. diet or climate — or of the socio-cultural environment excluding language — e.g. type of culture, nature of the family group, etc. — have on the development of the language apparatus. The functioning of the language apparatus, and probably in some small way its form, are, however, greatly influenced by the language of the environment — the articulatory habits developed with it, the modes of symbolization practised in it, etc. The role of this part of the environment in the growth and development of the language apparatus has, however, been widely suspected by linguists, while that of the inherited factors has not; and for this reason I concentrate here on the latter.

It follows from the above that appearance and the maintenance of language in the human species is dependent on an apparatus whose main form and functioning is the product of the development of the hereditary endowment of the species in the environment of that species. In biological terms language is phylogenetically a property of the human species and, like all properties of organic species, the product of heredity and environment. Without the hereditary potential or without the environment which enables that potential to develop, there would be and have been no language.

LANGUAGE AND THE MATING GROUP

The human species is however distributed widely over the earth's surface. From the view point of heredity, it is divided up into mating groups, communities of individuals who mate more frequently among themselves than they mate with individuals from other mating groups and thus have common ancestors and common descendants. Such a mating group may be regarded as bearing in the chromosomes of its members a pool of genes. From this pool, genes are distributed, in different combinations, to the individuals of each generation. The gene pool of a mating group is subject to change, under the processes of selection, natural and otherwise; and since these processes are never the same in total for any two mating groups, each mating group has its own characteristic pool of genes. No two mating groups therefore have the same inheritance.

Likewise, very obviously, no two mating groups have the same total environment.

Now language is one of the major factors in the establishment and perpetuation of a mating group — save in the most exceptional cases, marriages of speakers of

different languages make up only a negligibly small percentage of the total in any area. Thus, in by far the widest range of the human situation, the boundaries of the language community and of the mating group approximate to one another.

Taking for discussion the theoretically simplest case, that in which the mating group is also a community speaking a unidialectal language, and assuming, first that there has been an indefinitely long period of continuity between that group and its language, and secondly, that this group has lived in complete isolation from other human groups and languages, we are then forced to lay the responsibility for developing its language solely on that mating group. Nothing else is possible. That language can have been maintained and developed only through the existence and the operation of the language apparatuses of the members of that group. A language has, of course, no existence independent of the speakers, no power to live, to develop, or to change — it is a system of communication derived from and manifesting itself in a series of physiological activities of the language apparatuses of its speakers.

Now consider the differences in the development of that language from other languages, the differences which have led to synchronic features specific to that language. In the case under discussion, such developmental differences and hence the synchronic features must be the product of the language apparatuses of the speakers, of structures whose form and functioning are determined by the heredity and the environment of those speakers.

Phratringenetically, then, to use a term which I have coined — from *phratría*, a clan or tribe — to refer to development in the mating group, the specific synchronic features of the language of our mating group may be regarded as having one component which derives, through the language apparatuses, from the heredity of that group, and one which derives, through those same apparatuses, from the environment of that group.

The features of the language of this isolated mating group which are not specific to that language will also, under the same assumptions as before, be the result of the interaction of the heredity and the environment of the group. Their independent occurrence in the language of other groups, up to all groups in the species, are most likely to be, if we argue from evidence in other fields of organic evolution, the result of the interaction of similar heredities and similar environments. The other possibilities — similar heredities in different environments, or different heredities in similar or even different environments — are impossible to exclude, but are statistically much less likely.

In the more usual case in practice, where the mating group is not isolated, some features of its language will almost certainly be, in some respect, the result of contact. Such features, however, in their source language, will have been developed from the interaction of the heredity and the environment of that language's speakers. The borrowing of these features and their subsequent adaptation to the receiving language will reflect the heredity and the environment of the group speaking this

language. In the last analysis, therefore, borrowed features are, with regard to hereditary and environmental components, hardly distinguishable from features internally developed.

LANGUAGE AND THE INDIVIDUAL

Each individual of our theoretical isolated mating group draws from the gene pool of the group a complement of genes which is, in its totality, unique, and endows him with a unique potential with regard to his language ability. He grows up in a physical and cultural, and in a language environment which is likewise in its totality unique. From the interaction of these two factors, his heredity and his environment, he develops an idiolect which is also as a whole unique. The specific features of this idiolect have thus their hereditary and environmental components. On the other hand, the features which are common to¹ this and to other idiolects are, in similar fashion to the common features of languages, most likely statistically to be the result of the interaction of similar heredities and similar environments.

GENERAL IMPLICATIONS

From these considerations, it follows firstly, that those features which are, as far as we know, common to all languages, those features, in other words, which we regard as characteristic of human language, can only be, if we exclude the statistically improbable cases touched on above, the result of the interaction of features of heredity and features of the environment which are common to men everywhere. Secondly, that those features which are derived internally from and characteristic of the language of a particular group must be the result of the interaction of features of heredity and of environment which are specific to that group. And thirdly, that those features which are specific to the speech of any individual are the result of features of heredity and of environment which are specific to that individual.

Thus, to exemplify, the use of the medium of sound in language derives in part from man's inheritance from his prehuman ancestors, his inheritance of a vocal apparatus with certain potentialities of sound production and of an auditory apparatus with certain ranges of sensitivity, and in part from his gregarious living in an air-filled environment. Likewise, the symbolic nature of the units of language derives in part from man's inheritance of a brain and nervous system which has certain characteristic modes of functioning, and in part from his inhabiting an environment to features of which his nervous system has evolved to react.

In a language, the features which are specific are usually wholes or sets: the set

¹ Or similar to: the distinction here seeming to be primarily of fineness of analysis or precision of description.

of phonemes or of allophones, the structure of the phonological system, the grammatical system of forms and categories, the semantic system, and so on. But in fine analysis, each of the units of such sets is in itself specific. Thus, /t/ in English is different in its systemic and paradigmatic relations, and in the number and nature of its allophones, from /t/ in French, Hausa or Chinese. Similarly the meaning associated with the form /frijdəm/ differs from that associated with the form /frahajt/ in German or /uhuru/ in Swahili, it has its own set of relations with other members of its *bedeutungsfeld* — /libəti/ and /lajsins/ for example — and so on.

These and all other specific features of the English language must derive from the reaction of the genetic inheritance of the English-speaking group in or to the total environment of that group — a reaction which has predominantly cumulative, but also modificatory and loss-causing effects. The picture is complicated: the genetic inheritance of the community is subject to constant change and the products of this inheritance and its environment become continually part of the environment to which the inheritance reacts.

In some cases there may be discernible traits of the environment which seem to have played a role in the reaction of the English-speaking people to their language — as, for example, the cultural and historical traditions of England may have influenced the development of the present-day meaning of the word /frijdəm/. But on the genetic side the traits of the heredity of the English people that have so far been identified — the frequencies and distributions of a few genes and groups of genes — not yet connectable with any of the specific features of the English language.

In similar fashion, the individual features of an idiolect have a genetic component. The individual, it is true, cannot, like the mating group, be considered in isolation from other individuals, since he can only learn his language from the other members of his group. But the design and the maturation of his language apparatus is laid down by his heredity, and both the process and the product of his learning will be, through this factor, unique and characteristic of him alone. Hence, the rate and the variations in the rate at which the individual learns his language, the specific sequence of stages through which he passes in the mastery of the articulatory and the auditory patterns of his language, and the nature and result of the finished product are all partly determined by his inheritance. His individual voice quality, his pitch and stress range, his speech defects or giftedness, his word grouping and pausation, in sum his whole handling of his own language (or, of course, of his languages) is the result of the reaction of his hereditary endowment in the totality of his environment.

The overall role of the individual's inherited capacity or endowment for language learning has long been recognized, but it is only recently that the strikingly individual nature of an idiolect has come to be realised, and a start made with the relating of individual articulations and defects of speech to inherited variations of the orofacial complex. It is probable that progress and understanding in this field will be rapid, as case histories are built up by the orthodontists and speech therapists.

According to the theory outlined here, each and every feature of language, of

any specific language, or of an idiolect is the result of the interaction of the heredity of man, of men, or of an individual, with the appropriate environment. Each feature thus has a genetic and an environmental component.

These two components may of course be of widely differing importance in the development of different features. For example, it would seem likely that the role of genetic factors may be of more importance in the determination of the specific set of allophones used in a language, or of the specific set of word-initial phoneme clusters than in the determination of which semantic fields are well developed, and which are not, in the vocabulary of that language. For the latter — I am thinking especially of such classic cases as the Eskimo coverage of “snow” and the Arabic coverage of “camel” — obviously reflects closely the nature of the speakers’ environment and is largely dependent on it. On the other hand, the former, that is, the set of allophones or of word-initial clusters, in spite of a considerable amount of investigation, has not been shown to be correlated in any way with any aspect of the nature of that environment, but might well reflect functional articulatory preferences of the group of speakers concerned, such preferences being linked in a causal chain through mean characteristics of the language apparatus to the nature of the gene pool of the group.

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DISCUSSION

GALTON:

I should like to suggest that there are factors of two different orders responsible for linguistic change, i.e. in addition to the hereditary and environmental factors mentioned by the speaker there are also structural factors operating from within the linguistic system, coming into a complicated interplay with those from the outside.

PHASIA

On the Phylogensis of Speech: a Bioanthropological Contribution

EARL W. COUNT

I

Years ago, the Linguistic Society of Paris banished from its agenda all sterile speculations on the origin of speech.¹ The problem remains none the less a cardinal obligation of the science of man; for the capacity to process his life-space – his Merkwelt/Wirkwelt – via symbol – hereinafter, *Symbolopoesis* – is the only clear distinction between man and the other Primates – hereinafter, *Alloprimates*. It is a neurophysiological problem quite as much as a psychological one. Moreover, symbolopoesis externalizes also via graphic arts, music, and gesture quite as genuinely as via grammarized utterance – which latter exercise we shall term *Phasia*.

Some late-Pliocene – early-Pleistocene Primates, in the course of their simultaneous social and biological evolution, passed from a brain incapable of phasia to one capable of it. Can we say anything substantial about this?

We shall never possess from the ground any fossil proto-glosses. Yet, like Darwin too, we have as source materials the extant animals. On the other hand, unlike the situation of comparative anatomy, none of the alloprimates present homologues, however humble, of human speech; and moreover, linguistically speaking there are no “primitive” languages among any groups of *Homo sapiens*. Still, let us see.

Kindly permit me an apologia. This inquiry into the origins of phasia is a node in a larger setting. It seems that the vertebrates, like the termites, the ants, etc., possess a life mode that is determined by their characteristic central nervous system (to say nothing of the rest of their bodily organization); and although there is evolution in both nervous system and life-mode, “plus ça change plus c’est la même chose” – even as to culturized man. This life-mode I have elsewhere described as the vertebrate biogram. So in the topic at hand a vertebrate problem becomes an anthropological problem, and we are summoned to account for human phasia out of alloprimate anlagen or antecedents.

This impels me to supply a corrective to the perspective of some of my fellow-anthropologists; in that they note the cultural achievement of man and the non-

¹ The research for this paper has been supported by a research grant from the National Institute of Mental Health.

achievement of apes; beside this, they compare the respective sizes of ape and human brains, note that man's is about treble or quadruple that of apes – and finally observe that the human increase was an event of the Pleistocene; in so brief a span there must have occurred (we are told) an unaccountable “explosion of brains”, which – post hoc ergo propter hoc – takes care of the explosion of culture.

Now, in our century surely we have gotten over the fallacy that differential cultural achievement is a measure of the difference in brain power of a civilized human and a primitive human: we appreciate the exponential compounding which intercommunication and reciprocal stimulation makes possible. Yet it does not seem to occur to us that by a like token cultural discrepancy is not a way to measure the differential brain powers of ape and man.

Still, we cannot dodge the fact that the human brain size is several times that of the nearest ape. But the biological difference between ape and human brain is very slight indeed. The weighty fact is, that hemispherical tissue is very simple as compared with that of the thalamus and of the brain stem in general; the thalamus is vastly the older structure, and it remains from fish to man a fundamental organizer of the animal's life-mode, which the hemispheres are far from being. Now were the ape hemispheres to add but two mitotic cell divisions to the very long train of these during its embryogenesis, its brain would have human size. Does this seem too much to happen in a million years? Add to this that from our primitive insectivore ancestors to our monkey relatives, both hemispheres and thalamus have become much more complicated than from ape to man, and the development has taken an incomparably greater time as well as “evolutionary effort” to accomplish; and it would not surprise me if, were the matter ever investigated, the respective psychological powers should show a commensurate discrepancy. So I discern no “explosion of brains” that produced man and his culture. I do presume socio-psychological interstimulations among the proto-humans which apes do not have, by virtue of those biologically very slight *Weiterbildungen* in the human brain which we are about to consider.

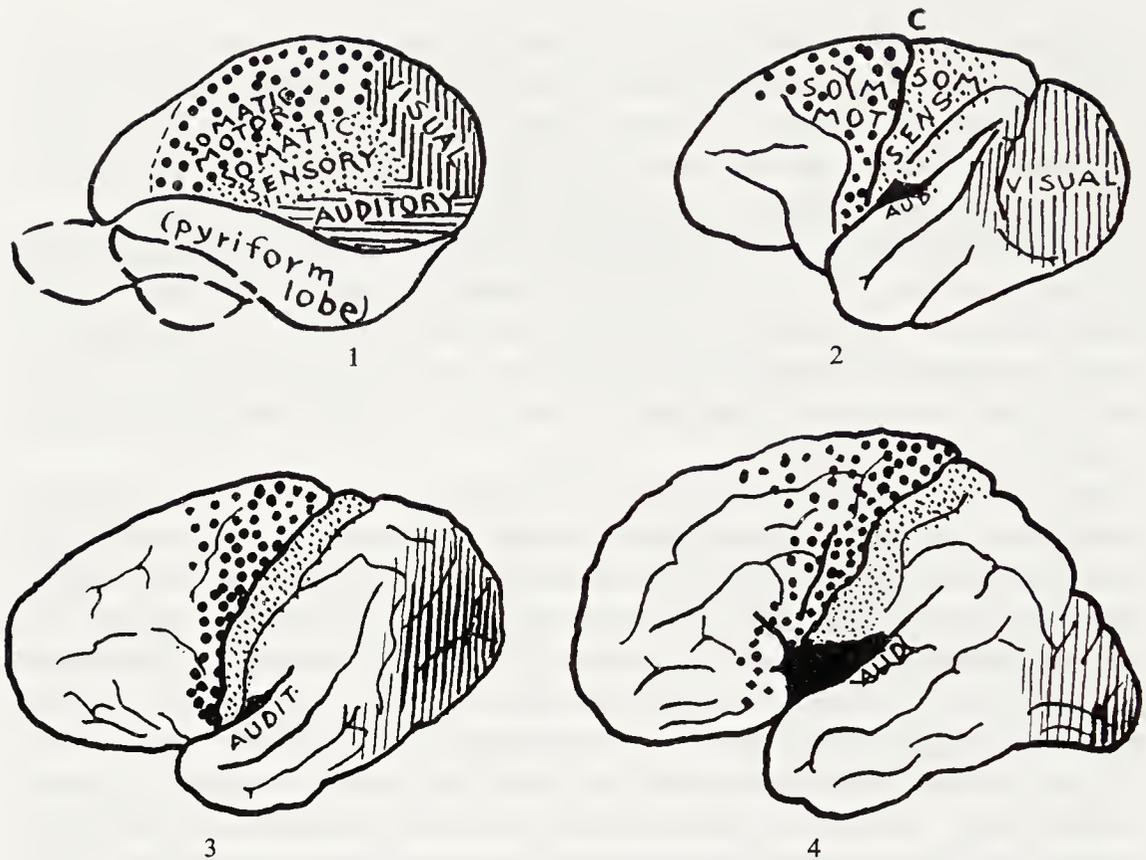
II

Neurophysiology no longer assigns to the function of speech a particular spot in the cortex and its sub-cortex. To be sure, there are regional foci of specialized activity, and several of them are especially sensitive to the disturbances that appear in the aphasias; but we speak with the entire brain as truly as we perform any other act with the entire brain; obviously, however, the “temples” differ per act.

Eventually we shall focus upon one aspect of phasia; but first we require some perspective. Let us look at figures 1-7.

This temporo-parietal region includes Wernicke's area – the most essential of all for normal phasia – and Déjerine's area of alexia-agraphia.

Primitive mammals possess 3 areas of cortex for primary sensory perception: the



Figures 1-4. To illustrate the progressive expansion of the "associational" cortex, from primitive mammal to man. The expansions crowd apart the primitive ("primary") regions of the visual, auditory, and "somatic" projections. The zones contiguous to the respective "primary" regions have a kind of transitional functioning.

Fig. 1. Monotreme. (The "pyriform lobe" and adnate structures, in the higher mammals, are blanketed by the exuberant spilling-over of the matter of the cerebral hemispheres.) Fig. 2. Cercopithecus. Fig. 3. Orangutan. Fig. 4. Man.

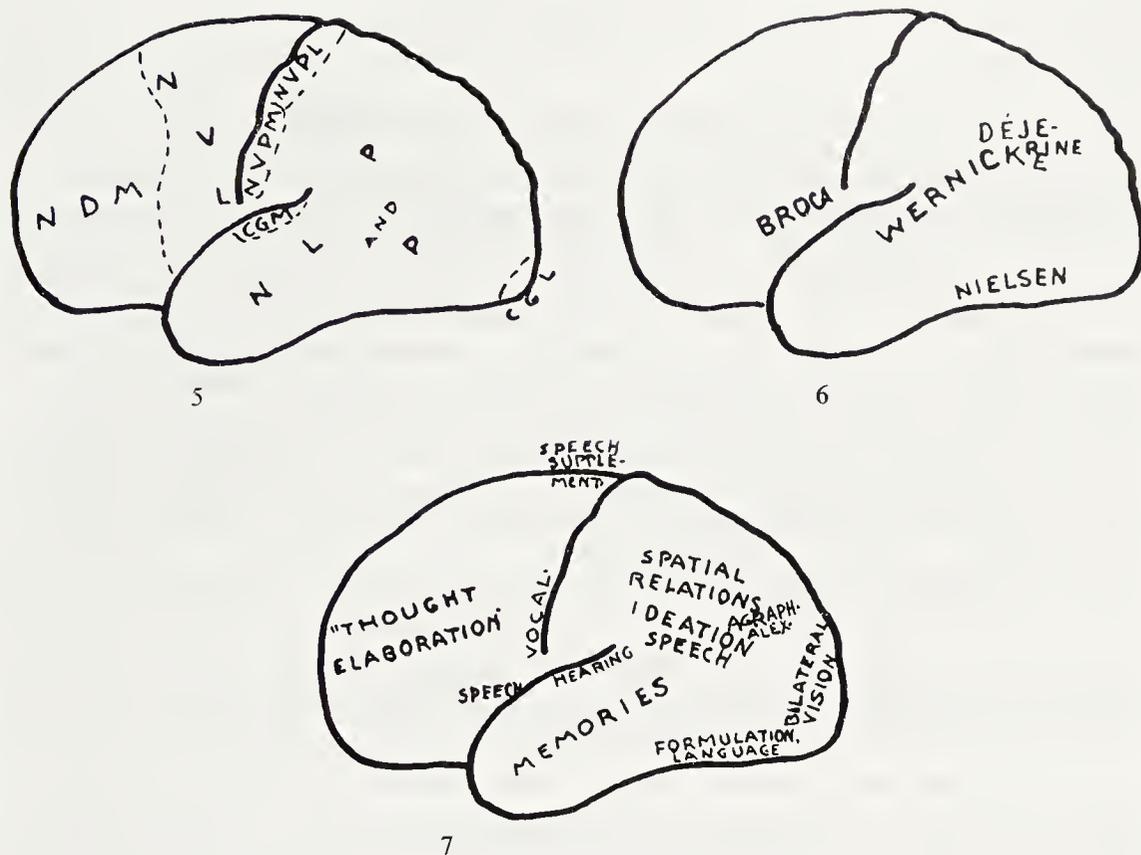
Black patch above the auditory cortex: the insula, partly exposed. An attempt is made to indicate, in the visual cortex of man and of ape, the primary cortex plus the marginal, transitional visuo-associative zone. The relatively small amount of primary visual cortex in man is apparent only: the expansion of the "associative" cortex crowds the primary cortex to the medial surface of the hemisphere.

The drawings are composite, from various sources.

Rolandic tactile-gustatory-etc., the supratemporal auditory, the occipital visual.

Sensations from the body's periphery arrive in the thalamus, there to be processed and relayed synaptically to the appropriate cortex.

During phylogenesis, the cortex between these domains expands disproportionately; here the primary perceptions are worked over into what Nielsen terms "engrams for recognition". Apparently, this obtains among other mammals as well as in man. The process does not stop there, even in other mammals, for as wider fields are drawn into this conversation, still "higher" elaborations occur. At the human level, one of these elaborations is phasia – we might say, a kind of distillate among other products of the same region which are essential to the individual's life-adjustment.



Figures 5-7. Left cerebral hemisphere, human.

Fig. 5. The approximate subdivisions of the cortex that traffic with the thalamic nuclei. (The data are from the monkey, applied inferentially to man).

NDM: Dorsomedial nucleus. NLP and P: Posterolateral and Pulvinar nuclei.

NVL: Ventrolateral nucleus. NVDM: Medial posteroventral nucleus. NVDL: Lateral posteroventral nucleus. CGM: Nucleus of the medial geniculate body. CGL: Nucleus of the lateral geniculate body.

NVL, NVDM, NVDL, CGM, CGL are "extrinsic"; i.e., they effect synapses with tracts arriving from elsewhere in the brain stem.

NDM, NLP, and P are "intrinsic"; i.e., they do not so synapse. They indicate that the "associative" cortex involved in speech and other ideational functionings is a matter of traffic between the thalamus (centrally placed and in the brain stem) and the cerebral hemispheres. The "extrinsic" cortex appears to have been developed in the most primitive mammals.

Fig. 6. Aphasic arrests of different kinds occur in disturbances of the areas indicated. The indication is approximate and not exhaustive.

Fig. 7. Some functional indications. Note particularly that spatial relations and speech-ideations are effected in approximately overlapping or identical regions.

We see in figures 5-7² that this cortex – significantly – does not traffic with the exterior via synapses; it traffics intrinsically with the thalamus; which means that the mechanisms of phasia must involve relations between both these structures.

² Adapted from various illustrations in Wilder Penfield and H. Jasper, *Epilepsy and the Functional Anatomy of the Human Brain* (Boston, 1954); W. Penfield and Theodore Rasmussen, *The Cerebral Cortex of Man* (New York, 1955); W. Penfield and Lamar Roberts, *Speech and Brain Mechanisms* (Princeton, 1959).

III

But to remain with the cortex, particularly the temporo-parietal.

The role of the cortex in the world of touch is mainly discriminative ... a question of the appreciation of a change of state ... objects are localized and compared ... judgments are made as to physical properties and identification is effected. ... We furthermore witness the operation of a temporal factor, whereby one stimulus, by succeeding another, forms a meaningful pattern. ... The peculiar role of the parietal (lobes) in the building-up of the postural scheme of the body leads to an important association with corporeal awareness, imagery and memory. Hence the appearance of unusual disorders of the body-image with parietal disease. Spatial manipulations and ideas are of themselves essentially bound up with parietal integrity, and in so far as they entail tactile and visual perception, they may be concerned with their three-dimensional qualities ... Language disorder occupies a comparable role in the problem of parietal function. (Critchley, *The Parietal Lobes*, 410-412, passim.)

The evidence behind Critchley's summary derives from the bizarre disturbances of spatial and phasic organization due to brain lesions and epilepsies. There is corroborative evidence from alloprimate and other mammalian experiments. For instance – parieto-temporal ablations in macaques result in strikingly similar behavior deficits of spatial organization. There are graphic and drawing tests of brain-injured humans, normal and subnormal children – and by now, drawings by normal chimpanzees, orangs, and even an adult gorilla and some capuchin monkeys.³ Taken together, they are very suggestive as to the development of the Primate's symbolopoetic treatment of space; but we must hasten on.

IV

But all this clearly is not yet phasia.

For in phasia, there is an escape of symbolopoesis from its tether to the space-organizing which is the source of its being. To the human child, to be sure, the word for an object seems to be learned first as an attribute of the object; but this is possible because (1) the child is exploring and appropriating the already established world of things and language of the adults; and (2) his brain is already *Homo sapiens*, running through its already-programmed ontogenesis. As an event of phylogenesis, on the other hand, the problem is to account for the free synthesis of two mutual irrelevances – words and such and things and such; in which eventually and somehow the word becomes the stand-in for something having no logical relation to it.

I think the key lies in the functional lop-sidedness – the anisometry – of the two hemispheres; which has led the clinicians to speak of one – usually the left – as “dominant” and the other as “subordinate” – although they seem increasingly unhappy with these terms.

³ See particularly Desmond Morris, *The Biology of Art* (1962).

As far as evidence goes, the hemispheres have no significant difference of size, histological architectonics, metabolism; yet they certainly behave in part differently in dysfunction. And we add:

(1) Evidence accumulates that in most humans, whether right- or left-handed, the “dominant” hemisphere is the left. The trait is genetic, and diagnostic of the human species. But the degree of the “dominance”, and perhaps the pattern, varies idiosyncratically.

(2) Both hemispheres participate in building the speech function – but they do not behave as mutual mirror-images.

(3) Laterality – anisometry – is a broad biological topic, and it is not to be dismissed as though it were but a geometric imperfection of organism. Various animals exploit it to advantage.

(4) Neurological anisometry occurs among the alloprimates, although not as markedly as in man. (There even is experimental evidence that the macaque hemispheres may not learn the same task with the same facility. Cf. Trevarthen, *Science*, 136, 1962, 288f.)

(5) The human anisometry becomes increasingly detectable only as the child develops (under duress of injury, it can be inverted) – the while he is learning to organize his space-world and to speak.

I am suggesting that it is this very anisometry, and the fact that the two hemispheres do not act as mirror-images, yet both are involved, that has permitted the engrams of space-experience to loosen their ties and so become matrix for symbol.

I had arrived at this surmise last spring – and only after that did I discover a report by Gooddy and Reinhold published the previous fall in *Brain*, 84, 2, 231-242, entitled “Congenital Dyslexia and Asymmetry of Cerebral Function”.

These clinicians have long been studying the organization of space by humans – they call it “orientation”. And in this report their subjects were otherwise intelligent and adjusted children with no brain pathologies, who were having peculiar difficulties in attaching printed and written words to the things they stood for, and who proved on clinical testing to be poorly capable of orientation. Gooddy and Reinhold assert the following (p. 240):

We wish to put forward the theory that *asymmetry* of right and left hemisphere function is normally established as the child grows up, and that this asymmetry of function is closely related to the performance of reading and writing. We believe that children with congenital dyslexia fail to establish asymmetry of function in the cerebral hemispheres. . . . The cerebral defect may be related to a too close similarity of function, i.e., a lack of asymmetrical function of the two hemispheres. . . . In congenital dyslexia, there may exist, among other abnormalities, a defect in the organization of spatial perception.

So I submit the following mechanisml account of how two equally developed hemispheres which nevertheless behave with some configurative or templet difference may together contribute to the build-up of phasia: please look back at figures 5-7.

Both hemispheres clearly receive the same experiences, however non-identically they may handle them. Via their corpus callosum they intercommunicate and must therefore interstimulate and condition reciprocally. Each has its own traffic nevertheless with the centrally-placed thalamus. There is no reason to believe that the thalamus projects to them preferentially. But the hemispheres report back non-identically, and the thalamus can complete synthesis only out of what it receives – be that material from the respective hemisphere normal or defective from lesion.

SUMMARY

1. The roots of phasia in phylogenesis must be sought as far back as the origin and differentiation of the mammalian hemispheres.

2. Differential expansions of cortex are involved in the promotion of conceptualization, of engrams of recognition, among mammals.

3. Rudimentary symbolopoesis is achieved not later than some alloprimate level.

4. The cortical regions involved in organizing the animal's space-world continue this function in man, but there is a *Weiterbildung* reflecting the further quantitative expansion. On its ideational side, phasia owes much of its existence to this development.

5. At the human level, this *Weiterbildung* externalizes via several modalities, utterance being one; in which verbalization is a morphosynthesis between mutual irrelevances: concepts, and vocalizations which develop patterned articulations. Meaning translated via irrelevance is an essential of symbolopoea.

6. A neurophysiological requisite for the ideation aspect of phasia is marked anisometry of the two hemispheres.

And finally:

7. In utterance origination-and-reception, the act of assembling and externalizing an utterance and the act of receiving and referring it archivally to the fields which also generate ideations, occur in two separate individuals; and the anatomic paths of output and input are not at all identical. Yet neither mechanism makes utility-sense without reference to the other; and both evolved simultaneously in one and the same organism. I have termed this a *geminal* evolution. Although such evolution has wide incidence, it has received very little consideration by scientists.

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DISCUSSION

KELEMEN:

This excellent presentation of the phylogenetic influence of central nervous factors could be rounded out by inclusion of the role of peripheral elements, in the special case of language of the laryngeal apparatus. Feedback mechanisms control not merely the standard performance but they work as well in evolution. Environmental factors can bear upon the vocal organ resulting in changes not necessarily of phonetic significance, but still capable to arouse, from their part, an echo in central structural development.

Along this line animal vocalization offers instances, going back, e.g., to the move from aquatic to terrestrial or even to arboreal life. Experience regarding behavior in its connection with evolution is rapidly piling up but with the tendency to study animal languages without due regard to the peripheral organ. It is hoped that study of animal voice and language will not produce a "linguistics without larynx".

CLASSIFICATION ET TYPOLOGIE LINGUISTIQUE: LA FAMILLE TUPÍ-GUARANÍ

BERNARD POTTIER

0. La vogue actuelle de la typologie s'explique par le courant formaliste et mathématique qui caractérise la linguistique moderne dans certains pays. Il semble opportun de mettre en garde contre cette apparente science exacte.

1. LA TYPOLOGIE FORMELLE

1.1. Critères phoniques

1.1.1. Un inventaire formel de phonèmes fera apparaître qu'en Sirionó le *p* est absent. Plus important que ce fait quantitatif est le fait qualitatif et spécialement fonctionnel: cette absence entraîne-t-elle des homophonies gênantes pour la langue?

Comparer: Guaraní *py* "pied" / *y* "eau"
 Sirionó *i* dans les deux cas.

Une distribution morphologique apparaît alors:¹

<i>e-i</i> = "son pied"	<i>e-r-i</i> = "eau"
	<i>i</i> = "eau"
<i>ira-i</i> = "pied d'arbre"	<i>e-r-esa r-i</i> = "larme"
	("eau d'oeil")

1.1.2. Une comparaison dans le domaine des sifflantes donne:
Guaraní *s/š*, Guayakí, Mbyá *č*.

L'important est de montrer que cette différence en nombre et en nature n'affecte pas les possibilités distinctives:

Guaraní	<i>a-ha-se</i>	<i>še-ru-hape</i>
Mbyá	<i>a-a-če</i>	<i>če-ru-apy</i> ²
	("je veux aller chez mon père")	

Tous les exemples allégués montrent une distribution différente des morphèmes en question.

¹ A. Schermair, *Vocabulario Sirionó-castellano* (Innsbruck, 1958), p. 108 et 325.

² Enregistrements de l'auteur.

Le Mbyá et le Guaraní connaissent deux séries pronominales (type *a* actif et *še/če* descriptif). Le Guayakí ignore cette distinction, et ne possède que *čo*.

Le Mbyá et le Guaraní expriment le pronom devant chaque groupe prédicatif:

Guaraní *i-porã ñande retã*
 “lui-beau notre-pays”
še-resa o-heša
 “mon oeil il-voit”
 Mbyá *i-karu jaea vae, i-kyra*
 “lui-manger-beaucoup-qui, lui-grossir”

Le Guayakí procède par simple juxtaposition:

Pyteragi-ro čo-imbi mubú
 “Dos-Poilu ma-lèvre perfora”
 (“C’est Dos-Poilu qui a perforé ma lèvre”)⁷

2.2.2. On peut rechercher par quels moyens un énoncé est actualisé.

2.2.2.1. Cette actualisation peut:

(a) rendre l’énoncé viable (= indépendant et suffisant):

Guayakí *čo-rapa* = “mon arc”
čo-rapa-RO = “c’est mon arc”

(b) mise en relief fonctionnelle.

Guayakí *kuja nako japo* = “la femme fait des paniers”
kuja-RO nako japo = “c’est la femme qui fait des paniers”

(c) mise en relief temporelle.

Guayakí *membo-ro chyvaete u-TY*
 “serpent homme mûr manger”

(“L’homme mûr mange [réellement, d’ordinaire] du serpent [mis en relief]”)

2.2.2.2. Chaque dialecte a ses particularités à cet égard. En Guaraní, *-ína* fonctionne souvent ainsi:

ñande po ta h-ína = “notre main probablement ici-devant” (“c’est probablement notre main [que je vois sur ce dessin]”)

a-karu a-ína = “je-manger je-à-présent”.

2.2.3. Lorsqu’une catégorie d’expérience est grammaticalisée en langue, elle emprunte ses formes à celles qui existent déjà.⁸ On doit donc étudier, dans l’ordre d’importance:

- (i) “présence / absence” d’une catégorie;
- (ii) s’il y a présence, quelles formes l’expriment.

Ainsi convient-il de rechercher si les dialectes connaissent la catégorie “en fonction /

⁷ Phrases tirées de l’enquête de M. de Colleville et L. Cadogan, réalisée en 1961 grâce à une mission CNRS.

⁸ Ex.: l’article roman, catégorie nouvelle, emprunte ses formes à *unus, ille, ipse* . . .

hors-fonction" (sémantiquement parlant, cf. ci-avant, 1.2.3), et on pourra ensuite comparer les formes: Guaraní *kwe*, *ngwe*, Guayakí *bwe*, Sirionó *ke*, etc. . .

3. PORTÉE DES ÉTUDES TYPOLOGIQUES

On peut faire autant de classements typologiques que l'on choisit de critères de base. Il est évident que ces classements ne coïncident pas. La conclusion logique est donc qu'on ne peut pas *classer* des langues selon un ou plusieurs critères typologiques. Sur la base d'un même critère, les résultats sont d'ailleurs déjà divergents selon les auteurs.⁹

Vouloir affubler de chiffres précis des hiérarchies toutes relatives, c'est donner une apparence scientifique à un travail dont les bases sont incertaines.

Il convient donc d'utiliser les rapprochements typologiques comme des *contributions* à une meilleure connaissance des structures linguistiques. Lorsque les indices typologiques formels et fonctionnels concordent, et si les données historico-géographiques ne s'y opposent pas, on peut alors tenter des classements réellement linguistiques.¹⁰ L'inventaire comparé (résultat habituel des rapprochements typologiques) est instructif, mais n'est pas une fin en soi. Il peut servir, et c'est déjà beaucoup, à un meilleur nuancement des classements génétiques, les seuls au fond qui reposent sur une "réalité" linguistique.

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⁹ Cf. A. D. Rodrigues, "Classification of Tupi-Guarani", *IJAL*, 24 (1958), 231-234, où le mbyá et le guayakí sont très éloignés (a.1 et c), et L. Manrique, "Algunos problemas de parentesco en Sudamérica", *XXXV Congreso Internacional de Americanistas, Mexico 1962* [communication], où les relations entre les deux dialectes sont mises en relief. Les deux études sont basées sur la méthode de la lexico-statistique.

¹⁰ On oublie trop souvent le but de notre discipline. *Classer* les langues du monde d'après le nombre des voyelles est un jeu gratuit, sans portée linguistique du point de vue comparatif. L'intérêt est de montrer les *limites connues* de l'exploitation du champ vocalique humain. Toute méthode n'est pas bonne pour tous objets.

THE CURRENT STATUS OF LINGUISTIC STUDIES
ON THE CLASSIFICATION
OF THE MANCHU-TUNGUS GROUP

CORNELIUS J. CROWLEY

Abstract

The classification of the Manchu-Tungus linguistic group has troubled linguists for close to a century. It is the purpose of this paper to show the relationship between Manchu and Tungus and to discuss and appraise current linguistic thinking on the problem. At first, linguists felt that these tongues should be kept apart. P. P. Schmidt (*Ėtnografija Dal'nego Vostoka*, Vladivostok, 1915, 30) considered the relationship exaggerated and Louis Gray (*Foundations of Language*, New York, 1939, 370) tended to regard the languages as separate. Happily, recent work has shown that all the Tungus languages have many affinities with Manchu and anthropologists have classified the Manchu as a Tungus people.

Between 1860 and 1930 very few studies appeared, but from 1930 on scholarly activity increased. Since 1952 some twenty articles and books have come out in German alone, many in Russian, but almost nothing in English. The most recent contributors are V. I. Cincius, *Sravnitel'naja fonetika tunguso-mančurskix jazykov* (Leningrad, 1949), and J. Benzing, *Die tungusischen Sprachen, Versuch einer vergleichenden Grammatik* (Wiesbaden, 1955). These authors reconsider the question of classification and set up the Southern or Manchu Group and the Northern or Tungus Group. They find a close relationship between Tungus and Manchu and single out for detailed comparison *Tungus* proper or *Evenki* and *Manchu*, in both of which they find common features, such as vowel-harmony, absence of initial *-r* and the presence of stems with vocalic endings and the anterior palatal nasal *-n*.

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PHONEMIC VELARIZATION IN LITERARY ARABIC

RAJA T. NASR

INTRODUCTION

Reality is. Truth is what is said about it.

A phonemic analysis is a linguistic description of a phonological reality. While the reality is the same at any given time, the descriptions of it may vary considerably. Velarization in Contemporary Literary Arabic verifies the hypothesis that in any given dialect at any given time there is only one phonemic *system* but maybe several phonemic *analyses*.

VELARIZATION DEFINED

The *velum* is the back part of the palate.

A *velarized* sound is one that is produced with the tongue flattened and grooved from the mid back — i.e., at a point contiguous to the velum.

Velarization, then, is a feature of sound modification effected by the flattening and grooving of the tongue at the velar point of articulation.

THREE PHONEMIC ANALYSES OF VELARIZATION

1. *Velarized Consonants* — /T D S Đ L R /¹

If these six velarized consonants are analyzed as separate and independent phonemes, they would reduce the vowel phonemes (though not the vocoids) in number. The vocoids contiguous to velarized consonants would tend to become more backed. For example:

- a. [ii] becomes [II]
/ʔassiinu / [ʔassiinu] “the s” – /ʔaSSiinu / [ʔASSIInU] “China”
- b. [i] becomes [I]
/sir/ [sir] “walk” – /SiR/ [SIR] “become”
- c. [aa] becomes [AA]
/saaħa/ [saaħa] “he toured” – /SaaħA/ [SAAħA] “he shouted”

¹ Throughout this paper, capital letters will be used to indicate velarization.

- d. [a] becomes [A]
/sayfun/ [sayfun] “sword” – /Sayfun/ [SAyfUn] “summer”
- e. [uu] becomes [UU]
/suur/ [suur] “fence” – /SuuR/ [SUUR] “Tyre”
- f. [u] becomes [U]
/yusabbih/ [yusabbih] “he praises” – /yuSabbih/ [yUSAbbIh] “he says
good morning”

2. *Velarized Vowels* — / I II A AA U UU /

If these six velarized vowels are analyzed as separate and independent phonemes, they would reduce the consonant phonemes (though not the contoids) in number. The contoids contiguous to velarized vowels would tend to become more backed. For example:

- a. [t] becomes [T]
/?albattu/ [ʔalbattu] “the decision” – /ʔAlbAttU/ [ʔALbATTU] “the swans”
- b. [d] becomes [D]
/mada/ [mada] “extent” – /mAdA/ [mADA] “he passed”
- c. [s] becomes [S]
/massa/ [massa] “he said good evening” – /mAssA/ [mASSA] “he sucked”
- d. [d̥] becomes [Ð]
/maðalla/ [maðalla] “insult” – /mAd̥AllA/ [mAD̥ALLA] “parachute”
- e. [l] becomes [L]
/malla/ [malla] “he got bored” – /ʔAllAAh/ [ʔALLAAh] “God”
- f. [r] becomes [R]
/birr/ [birr] “righteousness” – /bArr/ [bARR] “land”

3. *Supra-Segmental Phoneme Of Velarization* — /··/

If velarization is analyzed as an independent supra-segmental (secondary) phoneme, both the consonant and vowel phonemes would be reduced in number, though the total allophonic variants would remain intact. For example:

- [t d s d̥ l r i ii a aa u uu] become [T D S Ð L R I II A AA U UU]
- a. /ʔassiinu/ [ʔassiinu] “the s” – /··ʔassiinu/ [ʔASSIInU] “China”
- b. /birr/ [birr] “righteousness” – /··barr/ [bARR] “land”
- c. /saaha/ [saaha] “he toured” – /··saaha/ [SAAhA] “he shouted”
- d. /maðalla/ [maðalla] “insult” – /··maðalla/ [mAD̥ALLA] “parachute”
- e. /tallatun/ [tallatun] “a hill” – /··tallatun/ [TALLATUn] “a peep”
- f. /salla/ [salla] “he entertained” – /··salla/ [SALLA] “he prayed”

CONCLUSION

Theoretically, all three phonemic analyses of velarization in Literary Arabic are equally descriptive by virtue of the fact that they are mutually convertible. The choice of one analysis over the others, however, would depend upon its:

1. *Convenience*. Of the three analyses, the first two (velarized consonants and velarized vowels) are typographically more convenient to use than the third (secondary phoneme), and visually easier to detect.

2. *General Acceptability*. The first analysis (velarized consonants) is potentially more acceptable especially by native speakers primarily because of their familiarity with some velarized consonants in the Arabic Alphabet.²

3. *Aesthetic Value*. Criteria for the assessment of aesthetic value must first be determined. However, general observers may select the third analysis (supra-segmental phoneme of velarization) as having the greatest aesthetic value, primarily for its consistent external harmony and simplicity.

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² All the non-velarized phonemes considered in this paper (t d s ḍ l r i ii a aa u uu) have separate symbols in the alphabet, except /i a u/ which are designated by small vowel characters rather than letters. Of the velarized counter-parts of these phonemes, only /T D S Ḍ/ appear in the alphabet as separate letters.

STANDARD CANADIAN ENGLISH: PRONUNCIATION

DONALD E. HAMILTON

The findings presented here were obtained in a survey of pronunciation habits among educated Canadians in all the provinces of Canada. An attempt to establish a standard in Canadian pronunciation was made mandatory by the preparation of a recently published Canadian dictionary,¹ and the desire to avoid being arbitrary about it necessitated some kind of national survey. Although work had been done by linguists in certain areas of Canada² there had been no uniform sampling all across the country. Officially, the Department of Education in each province sets the standard of pronunciation and spelling by approving a dictionary for use in its schools. Since nine of the provinces approved the Oxford dictionary, and only one chose Webster, British usage is definitely the official standard. This standard was so obviously out of line with linguistic facts that it became another reason for conducting the survey.

The actual sampling was done by means of questionnaires which were sent out in batches to intermediaries at the major university in each province. The intermediaries distributed the questionnaires to members of the staff and student body who were born and had lived most of their lives in the province in which they were residing. Aside from the restrictions as to education and residence the informants were picked at random, but a good cross section was obtained in each province. They ranged in age from seventeen to fifty with those in the seventeen to twenty-five age group predominating. Out of the three hundred questionnaires sent out, two hundred and thirty were satisfactorily completed and returned by mail.

The questionnaire was fairly short and easy to complete. Since previous investigations had shown that Canadian pronunciation had been influenced by both American and British usage,³ the questionnaire aimed mainly at testing the pronunciation of those classes of words which are pronounced differently by educated speakers in the United States and England. However, provision was made for turning up unique features of Canadian speech. While the use of a written questionnaire employing

¹ McClelland and Stewart, *The Canadian Dictionary* (Toronto, 1962).

² See Walter Avis (Ontario), R. J. Gregg (Vancouver), D. E. Hamilton (Montreal) and H. B. Allen (Prairie Provinces) in recent issues of *The Journal of the Canadian Linguistic Association*.

³ W. S. Avis, "Speech Differences Along the Ontario-United States Border", *J. Canadian Ling. Ass.*, 2 (1956), 43.

rhyme words and key words is not the best means of testing pronunciation, tape recordings of some of the informants indicate that it is reasonably accurate for recording broad speech patterns.

The major conclusion that can be drawn from this survey is that there is a standard pronunciation among educated speakers in Canada and that in defining the standard one may say that although Canadian usage divides between British and American practice, the influence of the latter is increasing quite rapidly. Having said this, one must admit that each province has its peculiarities of speech, and that the four Maritime provinces form a region which is not keeping up with the pace or pattern of linguistic change in the rest of the country.

A good example of a changing pattern in Canadian speech occurs in word like *suit*, *new*, *tune*, where British practice favours a diphthong [ju] in contrast to the American [u]. The survey showed that Canadian usage parallels British in *new*, *tune*, *student*, *dues*, and *duke*, but at least one third of the informants favoured the American pronunciation of these words. In the case of *suit*, *elude*, and *enthusiasm* the American pronunciation is already dominant. Throughout the whole series it was evident that the younger the informant, the greater the tendency to avoid the use of the diphthong, whereas some older informants used it consistently. In each item of this series there was at least one Maritime province in which the results were at variance with the general pattern.

Much the same pattern emerged in the series of bi-syllabic words ending in *-ile* where British practice uses the diphthong [aj] and American an [ə], [ɪ] or syllabic [ɹ]. Indicative of the impact of American culture upon Canada is the fact that the only word in this series in which most informants followed American practice was *missile*.

Canadian usage closely parallels American with respect to the use of [æ] in words like *ask*, *class*, *path* and the change to [a] in *calm* and *palm*. The pronunciation of these last two words as [kæm] and [pæm] which was recorded in the Prairie provinces by Harold B. Allen⁴ occurs sporadically all across the country and was offered by forty percent of the informants in the Maritime provinces of Nova Scotia and Newfoundland.

Most Canadians, especially those in the Maritimes, follow American practice in pronouncing *hoof* as [hʊf], but except in the Maritimes it is rarely that *root* and *roof* are pronounced as [rʊt] and [rʊf].

Among the single words where Canadian usage parallels American one can cite the preference for

[ˈreɪdɪ,eɪtr] over [ˈrædɪ,eɪtr], [ˈskædʒu!] over [ˈʃædʒu!],
 [veɪz] over [vaz], [təˈmeɪtəw] over [təˈmatəw],
 [ˈɪjðɹ] over [ˈajðɹ], [ˈliɪzɹ] over [ˈlɛzɹ].

Mention should also be made of the increasing occurrence, especially among younger

⁴ H. B. Allen, "Canadian-American Speech Differences Along the Middle Border", *J. Canadian Ling. Ass.*, 5 (1959), 21.

informants, of the American pronunciation [ˈkæki] for *khaki* in contrast to the standard Canadian [ˈkaɪki].

Canadians and Americans are also in agreement on the pronunciation of the large group of polysyllabic words ending in *-ary*, *-ory*, and *-ery* where British usage either reduces the second last vowel to [ə] or eliminates it altogether. Canadians maintain a secondary stress on the penultimate syllable so that *-ary* and *-ery* are usually pronounced [-ɛɪ] as in *secretary* and *-ory* is [-ɔɪ] as in *laboratory*.

There are, of course, words in which standard Canadian does deviate from American practice and such differences are usually attributable to British influence. The Canadian preference for [bɪjɪn] over [bɪn], [brɪˈlɔŋ] over [bəˈlɔŋ] and [ˈæntɪ-] over [ˈæntaj-] are good examples. While most Americans favour a strongly aspirated [hw] at the beginning of words like *whistle* and *what*, most Canadians reduce this to [w] so that *which* and *witch* are pronounced identically.

However, the best example of a general tendency to divert from American practice occurs in a large number of words in which Canadians maintain a stress which has disappeared in American speech. The result is that Canadians continue to differentiate between vowels which have been reduced to [ə] or [ɪ] in the United States. The following examples will make this clear:

Canadian	American
[dajˈrɛkt]	[dɪˈrɛkt]
[dajˈvɪt]	[dɪˈvɪt]
[owˈfɪʃəl]	[əˈfɪʃəl]
[prɔwˈdʌkʃən]	[prəˈdʌkʃən]
[prɔwˈgrɛsɪv]	[prəˈgrɛsɪv]

Admittedly, [dajˈrɛkt] and [dajˈvɪt] have been listed as American variants, but the others have not. British dictionaries record [prɔwˈgrɛsɪv] as standard and [prɔwˈdʌkʃən] as a variant, but do not list [owˈfɪʃəl]. Thus, while it seems clear that this tendency in Canadian speech may be attributed to British influence, it appears that Canadians are extending it.

Having touched on what appear to be the emerging patterns in Canadian pronunciation one feels bound to attempt to account for them.

That the English spoken by educated Canadians is reaching a standard may, perhaps, be accounted for by the increasing mobility of the Canadian population and by the establishment of a national broadcasting system. That this standard seems, in many ways, to be moving parallel to educated American usage is symptomatic of the fact that since World War II, the United States has replaced Great Britain as the major influence in every phase of Canadian life except politics. Television has, of course, done much to increase the linguistic impact of this influence. In addition, since World War II the make-up of the Canadian population has changed drastically as a result of large-scale immigration from continental Europe. The number of those of British descent is declining as a proportion of the whole population, and they are

giving way to immigrants whose children are quick to pick up the American variety of English. The fact that relatively few immigrants have settled in the Maritimes has left the area more homogeneously British and accounts, in part, for its resistance to American influence.

Since the factors producing these changes are not likely to disappear, it seems probable that the day is not far distant when the speech of educated Canadians and Americans will fall together in some general North American standard. Insofar as pronunciation is concerned, a good beginning has already been made.

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LES CONSONNES POSTNASALISÉES EN NOUVELLE CALÉDONIE

ANDRÉ G. HAUDRICOURT

Dans son enquête de 1938 publiée en 1944 (*Les langues austromélasiennes*) M. Leenhard signale le son *tn* dans deux langues de Nouvelle Calédonie, le Pwapwã et le Nemi. J'ai pu vérifier le fait en 1959-61 dans deux dialectes Nemi.

La langue Nemi, parlée à l'ouest de Hiéngène, a des mots du type $C^1vC^2vC^3$. En C^1 le système des occlusives est le suivant:

t	th	c	ch	k	kh	p	ph	p ^w	ph ^w	+ h, x, hy, y hw, w̃, ÿ, ʔ
ⁿ d	tn	^{ñ} j	cñ	^{ñ} g	k ^{ñ}	^m b	pm	^m b ^w	pm ^w	

En C^3 il est le suivant:

t	c	k	p
n	^{ñ}	ŋ	m

En C^2 dans le dialecte du sud il admet les mêmes occlusives qu'en C^1 . tandis que dans le dialecte du nord les deux occlusives sourdes se neutralisent dans la spirante sonore correspondante:

t	c	k	p	p ^w					
th	ch	kh	ph	ph ^w					
} r		} y		} γ		} β		} β ^w	

Les deminasales sonores, prénasalisées sont stables: ^{ñ}d, ^{ñ}j, ^{ñ}g, ^mb, mais les deminasales sourdes, postnasalisées, se neutralisent avec les deux autres nasales, de sorte que pour ce dialecte on trouve en C^2 :

r	y	γ	β	β ^w
^{ñ} d	^{ñ} j	^{ñ} g	^m b	^m b ^w
n	^{ñ}	ŋ	m	m ^w

Ces consonnes ont trois sources:

1) Réduction d'une syllable par perte de la voyelle: "père" *tama > tna-; "mère" *tina > tne-.

2) Infixe nasale des verbes: "faire" *pm^waʔi*. (Ni l'infixe, ni le suffixe apparaissent dans les langues de Voh-Koné: *v^wa*, ou de Touho-Wagap: *p^wa*.)

3) Onomatopée dans les noms d'oiseaux: *kɲao* "heron", *kniik* "poule sultane", ou des mots imitatifs: *éñihe* "éternuer".

Ces consonnes ont existé dans les langues voisines, où elles subissent deux traitements:

1) occlusive aspirée + voyelle nasale, dans la langue de Hyéngghène, où "faire" = *p^whãʔi* > *f^wãʔi*.

2) nasale forte (aspirée), dans les langues de Voh-Koné:

	Nemi	Hyéngghène	Voh-Koné
"haricot"	<i>pmu</i>	<i>fũ</i>	<i>hmu</i>
"gendre/beaupère"	<i>pm^waa-</i>	<i>f^wã</i>	<i>hm^wa-</i>

Ce dernier exemple indique que des consonnes postnasalisées ont existé également dans la famille Thai en Asie, puisque dans la langue La-Kia récemment découverte, les voyelles nasales coexistent avec les finales -n, -m, -ŋ (*Zhongguo yuwen*, 1962, No. 2, pp. 141-148).

Nous avons:

	Thai	Tong	La-Kia
"porc"	<i>hmu</i>	<i>hñu</i>	<i>khũ</i>
"chien"	<i>hma</i>	<i>hñ^wa</i>	<i>kh^wã</i>

ce qui suppose une initiale *kñ^w/pm^w* pour ces mots d'ailleurs d'origine onomatopéique (chien = qui fait *ñ^wa*, porc = qui fait *ñu*).

C. N. R. S., Paris

HOW DID THE INDICATOR OF THE OBJECTIVE CASE DEVELOP IN JAPANESE?

ATSUKO OYAMA

I. PROBLEM

In modern Japanese the objective case is clearly indicated by the particle *wo* following the object.¹ In the materials of ancient Japanese, however, we can find many examples where *wo* is absent:

Form A: Object + *wo* + Verb (Casus definitus)

Form B: Object + Verb (Casus indefinitus)

This is a problem not only in Japanese but also in many other Altaic language, for instance, Mongolian, Korean, and Nanai. Böhtlingk termed them "Casus definitus" and "Casus indefinitus".²

In the oldest materials written in the seventh and the eighth centuries, the grammatical system was not well established and the indicators of cases were quite ambiguous. For instance, the word order was just the same in both objective and subjective, so we can hardly discriminate subject and object.

- Noun + Verb
- a. hito ku-u Subjective: Man . . . eat . . . (something)
(man) (eat)
- b. tori ku-u. { 1. . . . Objective: (Someone) . . . eat . . . chicken
2. . . . Subjective: Chicken . . . eat . . . (something)
(chicken) (eat)

As it is quite unusual that a man is eaten by someone except man-eaters or wild beasts, we conjecture that a. is the subjective case; b. is, however, interpreted in two ways as the above.

To avoid such difficulties, the indicators of cases gradually developed. In modern Japanese, consequently, we have indicators between the nouns and the verbs.

- Noun + + Verb
- a'. hito + *ga* + ku-u. . . . *ga* is the indicator of the subject.
{ tori + *wo* + ku-u. . . . *wo* is the indicator of the object.
b'. { tori + *ga* + ku-u. . . . *ga* is the indicator of the subject.

¹ In ordinary speech the particle *wo* is often omitted, but people still have the concept that the speech is informal.

² Böhtlingk, *Über die Sprache der Jakuten*.

How did such a primitive language gradually obtain its logical and grammatical system in accordance with human mental development?

II. MATERIALS

Fortunately we have abundant materials from the tenth and eleventh centuries. *The Tale of Genji*, written in the early eleventh century, affords a great amount of data, especially in the dialogues, and here we can note the actual utterances of people in those days. Secondly, we have earlier works in the tenth century, from which we can add to the data taken from *the Tale of Genji*.

III. METHODS

All examples of the objective case were noted. In the cases of ambiguous context, I omitted the examples, and after the conclusions were obtained, reexamined the omitted examples to see if they would change the conclusions. Actually in *the Tale of Genji*, the counter-evidence was not significant.

IV. DATA

My theory is that the indicator of the objective case developed in accordance with human mental development, and leads to the following hypothesis:

HYPOTHESIS 1: The frequency of Form A(*wo*) would be greater in logical writing.

1. *Frequency in four categories*: This is the complete survey on how the frequency of two forms, A (*wo*) and B (absence of *wo*), varies in accordance with four categories. The objective criteria of the classification in the four categories are: a poem must be limited to the traditional thirty-one syllables; the dialogues and monologues are always followed by the indication of quotation “*to yu* (say)” and “*to omou* (think)” successively. Exceptions are quite rare in *the Tale of Genji*.³

TABLE I
Frequency of Form A (wo) and B (absence of wo)

	Description	Dialogue	Monologue	Poem	Total(i)
Form A	4666(56%)	2150(61%)	753(73%)	406(80%)	7975
Form B	3598(44%)	1354(39%)	280(27%)	103(20%)	5335
Total(j)	8264	3504	1033	509	13310

Application of the X² Test to the comparison of frequency yields the following:

$$X_0^2 = N \left(\sum \frac{f_{ij}^2}{T_i T_j} - 1 \right) = 713.416 \quad df = 3 \quad X_{0.01}^2 = 11.345$$

Therefore, it is significant beyond the 0.01 level, and the difference between the four groups was significantly greater than chance.

³ Atsuko Oyama, “The Objective Criteria of the Quotations in *the Tale of Genji*”, *Transactions of the Japanese Linguistic and Literary Society*, XXX (March, 1953).

Now it attracts our attention that the % of Form A (*wo*) is the greatest in the *Poem* (see Table I), the category which we can never regard as *logical* expression.⁴ The same facts are observed not only in *the Tale of Genji* but also in many other earlier works coincidentally (Table II). Consequently hypothesis I is disproved by this result, and we come to new hypothesis.

HYPOTHESIS II: The particle *wo* in the Form A was *not* a *logical* indicator of the objective case in the eleventh century.

TABLE II
The % of wo in works of the tenth and the eleventh centuries

	Description	Dialogue	Poem
The Tosa Nikki*	52%	38.5%	58%
The Tale of Taketori*	65%	61%	64%
The Tale of Ise*	61%	40	52%
The Tale of Yamato*	60%	38.8%	67%
The Tale of Utsubo**	44%	59%	77%
The Tale of Genji	56%	61%	80%

* Osamu MATSUO, "The Particle *wo* in the the Objective Case", *Japanese Language and Literature*, XV, 10 (October 1938).

** Reiko HIROI, "*Wo* in the Tale of Utsubo", *Japanese Literature*, IX (July 1957).

2. *Frequency in Dialogues*: The next observation was made on the frequency in dialogues, which we can regard as the actual utterance of people in those days. The data were analyzed from the aspects of the speakers' sex, age, caste, profession, and religion; and the speakers' relationship to each other (see Table III).

Now it attracts our attention that in these data the % of Form A and B are not always constant: there must be some unknown factors due to which the percentage varies. What are the unknown factors?

We can notice that in Table IV, when the speaker is older or of a higher class than the companion he is talking with, the % of Form B (55% and 54%) is greater than the Form A. On the contrary, when the speaker is of a lower class or younger, the % of A becomes greater (63% and 66%). In other words, when the speaker is in a formal situation, the % of Form A (*wo*) increases; in an informal situation, the % of Form B is (absence of *wo*) increases. Therefore, we can point out Factor A from this. *Factor A* the factor which is concerned with "formal" vs. "informal", "emphasis" vs. "brevity".

We can explain several data by this factor. For instance:

⁴ Not only in poems, but also in the parts in which the death, funeral, departure, love-affair and so forth are described, the % of *wo* increases significantly.

TABLE III
Speakers' Sex, Age, Caste, Profession and Religion

	Sex				Age			
	Male		Female		Male		Female	
	Noble	Middle	Inside	Outside	Young	old	Young	Old
Form A	563 ^{54%}	230 ^{63%}	375 ^{61%}	229 ^{72%}	298 ^{52%}	159 ^{59%}	343 ^{64%}	261 ^{65%}
Form B	480 ^{46%}	137 ^{37%}	242 ^{30%}	90 ^{28%}	275 ^{48%}	109 ^{41%}	191 ^{36%}	143 ^{35%}
Total	1043	367	617	319	573	268	534	404

	Caste, Profession, and Religion			
	The Tale of Genji			The Tale of Utsubo
	Emperor and Empress	Confucianist & Buddhist	Buddhist (nun)	Confucianist & Buddhist
Form A	95 ^{68%}	138 ^{68%}	116 ^{69%}	138 ^{79%}
Form B	45 ^{32%}	66 ^{32%}	53 ^{31%}	37 ^{21%}
Total	140	204	169	175

TABLE IV
Speakers' Relationship to Each Other

	Sex		Age		Caste	
	Male Male	Male Female	Older Younger	Younger Older	Higher Lower	Lower Higher
Form A	139 ^{51%}	424 ^{55%}	109 ^{45%}	180 ^{63%}	155 ^{46%}	90 ^{66%}
Form B	131 ^{49%}	349 ^{45%}	131 ^{55%}	106 ^{37%}	179 ^{54%}	46 ^{34%}
Total	270	773	240	286	334	136

These data are also significant enough by application of X² Test beyond the 0.01 level.

a. In poetry (Table I), the % of the particle *wo* is significantly greater than in other categories. Not only in poems, but also in the parts in which the death, funeral, or departure of lovers and so forth are described, the frequency of *wo* goes up significantly. Apparently they have something in common: they are all emotional expressions and need *emphasis*.

b. In Emperors' and Empresses' speech, too, the frequency of *wo* goes up significantly for keeping their dignity (Table III).

c. On the contrary, the absence of *wo* shows higher frequency in informal utterances in exciting dialogues, (the same as in the modern Japanese), in the parts of extraordinary events happening, and the enumeration of events and things . . . which we can explain by the theory of economy.

3. *Frequency in the Chinese Translation and the Dialogues of Confucianists and Buddhists*: We still have some difficulty, however, in explaining the data of Table III (the dialogue of Confucianists and Buddhists) by Factor A only. There must be other factor here. Now we can notice that these Confucianists and Buddhists were engaged in Chinese translation. In earlier works which were influenced by Chinese translation, the frequency of *wo* is constantly high (Table V).

TABLE V
The % of wo: in Comparison with Chinese Translation

	Ordinary Japanese	Works Influenced by Chinese Translation				
	The Tale of Genji	The Tale of Utsubo			The Tale of Taketori	
	Others	Toshikage	Tadakoso	Kasugamode		
Form A	56%	44%	69%	56%	58%	65%
Form B	44%	56%	31%	44%	42%	35%

In the *Tale of Utsubo*, the three chapters, Toshikage, Tadakoso, and Kasugamode describe Buddhists' stories, and form a contrast with other 17 chapters.

Consequently we can point out Factor B in this case. *Factor B* is the factor which is concerned with the influence of Chinese translation.

V. CONCLUSION

From deductions based on the above observations, I would explain the development of the indicator of the objective case, the particle *wo*, as follows: Originally, *wo* was one of the basic vowel exclamations of primitive people. In the seventh and eighth centuries we find many examples of *wo* used as an interjection. In these examples, *wo* followed not only nouns but also several other parts of speech, namely, verbs, auxiliary verbs, and other particles. In the "noun-*wo*-Verb" sequence, *wo* would seem to have been the indicator of the object, but it was originally an interjection which followed the noun and *emphasized* it. This is the reason why it occurs more frequently in poems and other emotional writings than in ordinary prose. . . . (Factor A).

In the seventh century a great many Chinese classics and Buddhist scriptures, written in Chinese, were introduced into Japan. When Japanese people were confronted with a difficult passage to translate from Chinese into Japanese, they made use of this particle *wo* as the definite indicator of the objective case to make their translations intelligible. In this use, the concept which supported the indicator was apparently *logical* and *grammatical*, a use which has been inherited by the present Japanese. This factor can explain why the frequency of *wo* increases significantly in the dialogues or stories of Confucianists and Buddhists. . . . (Factor B).

LA RELATION GÉNITIVE DANS QUELQUES LANGUES MANDÉ

GABRIEL MANESSY

Nous désignons ici par relation génitive le rapport établi entre deux noms ou pronoms dont l'un, le déterminant, réduit l'extension du contenu de l'autre, le déterminé, et en précise la compréhension. Cette définition très large inclut le rapport de qualification qui ne possède pas, dans les langues mandé, de mode d'expression grammaticale particulier.

La forme générale du syntagme génitif est partout la même à l'intérieur du groupe considéré: le déterminant précède le déterminé; mais le rapport qu'il exprime peut y présenter plusieurs aspects. Dans certaines langues, la relation génitive n'est normalement indiquée que par le procédé dont use l'idiome en question pour signaler que deux mots sont unis par un lien grammatical: seule est affirmée l'existence d'un rapport de détermination entre les deux termes; si la nature de ce rapport doit être spécifiée, il faut substituer au déterminant simple un syntagme complexe. Dans d'autres langues, cette distinction est transposée dans le domaine de la grammaire et et se mue en une opposition entre deux types de rapports, rapport médiat, formellement marqué, et rapport non médiat, sans caractéristique morphologique qui lui soit propre. Enfin, selon les termes employés par D. Westermann et M. A. Bryan (*The Languages of West Africa*, 1952, p. 45): "It seems to be characteristic of some Mande languages that in the Genitive construction or with Possessives a grammatical distinction is made between (a) parts of the body, (b) names of relatives, (c) other Nouns."

Notre propos est d'illustrer ces diverses éventualités par l'étude du kpelle, du loma, du mende et du bandi, et de montrer qu'elles constituent moins des choix divergents que les étapes d'un même processus évolutif.

KPELLE ET LOMA

Kpelle.¹

Lorsque, en kpelle, la nature du rapport qui unit le déterminant au déterminé n'a pas

¹ Fr. Hintze, "Zum konsonantischen Anlautwechsel in einigen westafrikanischen Sprachen", *Zeitschr. für Phonetik*, 1948, 164-182, 322-335. – W. E. Welmers, "New Light on the Consonant Change in Kpelle", *Zeitschr. für Phonetik*, 1950, 105-119. – P. Lassort, "Grammaire guerzée", *MIFAN*, 20 (Dakar, IFAN, 1952). – J. Casthelain, "La langue guerzée", *ibid.* – D. Westermann and H. J. Melzian, *The Kpelle Language in Liberia* (Berlin, 1930).

à être spécifiée, les deux noms sont immédiatement juxtaposés, le lien grammatical étant marqué par cette proximité même. Le sens du syntagme résulte de la confrontation des contenus sémantiques :

Welm.	<i>kwéli kɔɔ</i>	une peau de léopard
	<i>kwéli kpòndò</i>	un piège à léopard
	<i>gbòno</i>	son piège (le piège du léopard, c.-à-d. propre à la capturer)
	<i>téé γàlòη</i>	un oeuf de poule
	<i>ηάλóη</i>	son oeuf (l'oeuf pondu par la poule)

(Dans *gbòno* et *ηάλóη*, le pronom de la 3.p.sg. est réalisé comme un ton bas accompagné d'une modification de la consonne initiale.)

W.M. *gáloη gelei* la case à palabre (*kélè*) du chef (*káloη*)

(Les deux termes sont à la forme définie, marquée dans l'un et l'autre par la modification de la consonne initiale et dans le second, en outre, par le suffixe *-i* que les mots en *-η* ne reçoivent pas.)

Il faut traiter à part le cas de substantifs qui, au lieu d'avoir une référence "objective", c'est-à-dire de désigner telle ou telle entité dont l'identité est indépendante de celle du locuteur: être, objet, notion abstraite, n'ont de sens que par rapport à une personne: je, tu ou il. Il en est ainsi de "mère" par exemple; une mère est nécessairement et par définition mère de quelqu'un: moi, toi ou autrui, et n'est mère que par rapport à cette personne, alors qu'une chèvre est chèvre en soi. Ces substantifs sont en quelque sorte "fléchis" pour la personne, en ce qu'ils sont obligatoirement déterminés par un pronom personnel possessif:

Welm.	<i>í náη</i>	ton père
	<i>ηάη</i>	son père
	<i>í lee</i>	ta mère
	<i>ηee</i>	sa mère

Si le déterminant est de la 3ème personne, son identité est éventuellement énoncée avant le syntagme d'annexion:

Welm. *gáloη ηee* le chef, sa mère

Lass. *Pierre da Hua da Kwiloηdye di le* Pierre, Hua et Kwiloηdye, leur mère

Appartiennent à cette catégorie, d'après le P. Lassort:

<i>naη</i>	père	<i>baláη</i>	ami
<i>le</i>	mère	<i>yoyo</i>	ennemi
<i>γóla</i>	belle-mère	<i>tγγγ</i>	homonyme
<i>hile</i>	mâle, au sens de mari.		

Il faut, semble-t-il, y ajouter:

wə partie, part (part, share), d'après W.M.

La construction particulière de ces noms s'explique par leur sens, mais le rapport qui unit le pronom personnel au substantif est de même sorte que celui qui avait été précédemment étudié: dans *náŋ* "son père" et dans *ŋálóŋ* "son oeuf", la nature du lien qui unit les deux termes est impliquée dans le contenu sémantique du déterminé.

Tel n'est pas toujours le cas. La langue dispose alors d'un moyen de préciser la valeur du rapport évoqué. Il consiste à former une locution complexe dont le premier membre comporte l'un des mots:

<i>wɔ</i>		partie, part (cf. ci-dessus)
<i>pɔ</i>	(Welm. <i>pɔɔ</i>)	trace, au sens de empreinte,
<i>ye</i>		main, au sens de mainmise,

déterminé par le nom du "possesseur"; ce syntagme détermine à son tour le nom de l'objet possédé; le R. P. Casthelain, parlant des dialectes de Guinée, fournit les exemples suivants:

<i>Pepe po tay (ta.i)</i>	le village de Pepe
<i>Pepe ye pèlè</i>	la (une?) maison de Pepe;

wɔ, ainsi qu'il a été indiqué, est "fléchi" pour la personne:

(Casth.) *Pepe n wo bowa* le couteau (Welm. *boa*) de Pepe,

n wo se contractant très fréquemment en *nö* (lire *ŋə*):

Pepe nö bowa

Ainsi s'opposent:

<i>Pepe hüo</i>	la chair de Pepe
<i>Pepe nö hüo</i>	la viande possédée par Pepe

Ce procédé est en voie de grammaticalisation; le contenu sémantique de *wɔ*, *pɔ*, *ye* est très vague; dans une certaine mesure, *pɔ* et *ye* sont interchangeable; en particulier, *ye* est employé, et non *pɔ*, lorsque le nom de lieu est à la forme définie avec un sens locatif:

(W.M.?) *ku ye gweli* dans notre cour.

On saisit là l'amorce d'une évolution qui pourrait aboutir à la création d'une particule dont la seule fonction serait d'indiquer le caractère "médiat" de la relation génitive. Mais le kpelle semble s'être orienté dans une direction différente: au syntagme "pronom personnel + *wɔ*" peut être substitué un "pronom spécial" qui, selon le P. Lassort, lui est "tout à fait équivalent":

<i>wɔ heye</i>	ou	<i>ŋá heye</i>	mon habit
<i>e wɔ heye</i>		<i>ye heye</i>	ton habit
<i>wɔ heye</i>		<i>ŋə heye</i>	son habit
<i>ku wɔ heye</i>		<i>kuo heye</i>	notre habit
<i>ka wɔ heye</i>		<i>ka heye</i>	votre habit
<i>di wɔ heye</i>		<i>die heye</i>	leur habit

Ce “pronom spécial”, dont W.M. donne d’ailleurs une série partiellement différente, résulte très probablement d’une contraction, mais il est apparemment devenu inanalysable, de telle sorte que le “rapport médiat” en tant que tel, sans précision sur sa nature: origine, usage, possession, dispose désormais d’une marque (emploi du pronom spécial) qui l’oppose au rapport non médiat (emploi du pronom simple). Un grand nombre de noms, en raison de leur sens, sont habituellement en relation médiante avec leur déterminant. C’est là un fait de lexique, qui n’autorise pas à instituer en kpelle, comme le fait W. E. Welmers, une classe de noms de possessions aliénables opposée à celle des possessions inaliénables; le cas du piège à léopard, ou celui de la chair de Pepe, montrent que les transferts de l’une à l’autre ne sont nullement impossibles.

*Loma.*²

Un examen rapide nous incite à penser qu’une explication analogue pourrait rendre compte des faits loma rapportés par W. Sadler. Le rapport de détermination générale est exprimé par la juxtaposition des deux termes à laquelle s’ajoutent les effets d’une alternance consonantique frappant l’initiale du second. Cette alternance est très différente en son principe de celle du kpelle et relève du sandhi plutôt que de la morphologie.

(Sad.)	<i>dóowo vólo</i>	jour (<i>folo</i>) de repos
	<i>bóoa laya</i>	gaine (<i>taya</i>) à couteau
	<i>zunui yee</i>	main (<i>zee</i>) de l’homme
(Welm.)	<i>é βéléli</i>	ta maison

Sadler institue en outre une classe de noms de “possessions inaliénables” toujours précédés, dans le rapport d’annexion, d’un pronom personnel de forme identique, sauf à la 1^{ère} et à la 3^e p. du singulier, à celle du pronom sujet:

sg.1. ᵛ (sujet <i>ge</i>)	pl.1. exclusif <i>gé</i>
	inclusif <i>dé</i>
2. <i>e</i>	2. <i>wo</i>
3. ᵛ (sujet <i>é</i>)	3. <i>té</i>

Il semble bien que ces substantifs soient ceux dont le contenu sémantique implique une référence personnelle:

<i>wo lee</i>	votre mère (<i>dée</i>)
<i>e yéea</i>	ton père (<i>keea</i>)
<i>númai. tiè té yéea</i>	les gens, leur père.

Devant d’autres noms, dits de possessions aliénables, le pronom personnel est vocalisé en *a* comme il l’est, selon W.E. Welmers (art. cit., p. 109), en kpelle oriental:

² W. Sadler, *Untangled Loma. A course of study of the Lɔma Language* (Board of Foreign Missions, 1951). – W.E. Welmers, art. cit.

sg.1. <i>na</i>	pl.1. exclusif <i>gá</i>
	inclusif <i>dá</i>
2. <i>ya</i>	2. <i>wa</i>
3. <i>ná</i>	3. <i>tá</i>
ex. <i>na dabái</i> ³	ma soupe
<i>gá leeβéi</i> ⁴	notre table (<i>téeβei</i>)

En réalité, l'opposition paraît être, comme en kpelle, entre une relation dont la nature est impliquée dans le contenu sémantique des deux termes en présence, et une relation dont le caractère contingent doit être souligné. On peut d'ailleurs, dans un contexte suffisamment clair, se dispenser de cette précision : Sadler évoque le cas où le pronom possessif en *a* est "sous-entendu", c'est à dire, selon nous, omis :

<i>folomo na sáa vái</i>	la mort (<i>sáa váa</i>) de Folomo
ou <i>folomo zaa vái</i>	même sens

MENDE ET BANDI

*Mende.*⁵

La distinction entre syntagme non marqué et syntagme marqué est beaucoup plus rigoureuse en mende qu'elle ne l'est en kpelle, du moins dans les dialectes de Guinée, et il semble qu'elle y traduise bien une opposition entre deux types de rapports, immédiat et médiat, plutôt que la simple spécification d'un rapport général de détermination.

Deux noms, ou un pronom et un nom en rapport immédiat d'annexion sont seulement juxtaposés, chacun conservant son propre schème tonique, et le lien qui les unit n'étant signalé que par la modification de la consonne initiale du second terme, modification caractéristique du syntagme en tant que tel (elle affecte tout aussi bien le verbe intransitif précédé de son sujet) :

<i>númú gbàkíi</i>	le bras (<i>kpàkí</i>) de quelqu'un (<i>númú</i>)
<i>nyàhá gbówéi</i>	la cheville (<i>kpówó</i>) de femme (<i>nyàhá</i>)

Ce rapport est le seul qui puisse s'établir entre un pronom possessif et un nom désignant une partie du corps, une relation spatiale, un parent consanguin, ou le possesseur (*ti*) de quelque chose :

<i>nyá wómbíi</i>	mon genou (<i>ngómbi</i>)
<i>nyá búí</i>	mon dessous (<i>bú</i>) : la partie inférieure de mon individu.
<i>nyá bú</i>	au-dessous de moi.

³ L'alternance consonantique ne se produit pas après les pronoms de la 1.p.sg. (ʎ, *na*) et de la 3.p.sg. (ʎ, *ná*) non plus qu'après les substantifs à suffixe de défini sg. *-gi* et suffixe d'indéfini pl. *-ga* (probablement primitivement terminés par *-g*; cp. bandi; voir Hintze, art. cit., p. 170).

⁴ *β* dans la transcription de Welmers note une spirante labio-dentale sans frottement; Sadler l'écrit *v*.

⁵ K. H. Crosby, *An Introduction to the Study of Mende* (Cambridge, 1944). – Fr. Hintze, art. cit.

Il s'instaure de même obligatoirement entre le nom le et morphème *-i* de défini (de ton haut, sauf après un groupe de mots ˆˆ ("cl. B 1"), tels que *kpàkí*, où il est de ton bas) et entre le nom et les divers morphèmes de pluriel (*-ɲgà* pl. indéfini, *-sìà* pl. défini, *-nì* pl. collectif):

<i>fóló</i>	jour	<i>fóléí</i>	le jour
<i>sàwà</i>	loi	<i>sàwèí</i>	la loi
<i>ndèwè</i>	frère	<i>ndèwèɲgà</i>	des frères
		<i>ndèwèísìà</i>	les frères
<i>ndòpó</i>	enfant	<i>ndòpónì</i>	enfants

Le rapport médiat est au contraire caractérisé par une modification du schème tonique du second terme, celui-ci étant nécessairement de ton bas (ˆˆ , ou ˆ s'il est monosyllabique), sauf après certains noms ("cl. A") qui, lorsqu'ils sont employés sous forme radicale (c'est-à-dire au singulier indéfini) et à l'initiale d'un groupe de mots, imposent au second terme du rapport d'annexion un schème ˆˆ , ou ˆ s'il est monosyllabique. Bien entendu, l'alternance consonantique se produit dans ce cas comme dans le précédent:

<i>númú wélè</i>	une maison (<i>pélé</i>) qui est à quelqu'un (<i>númú</i> : cl. A)
<i>númú gbákìí</i>	la branche de quelqu'un
<i>nyàhá gbòwèí</i>	les anneaux de cheville d'une femme (<i>nyàhá</i> : cl. B).

Employés comme postpositions locatives, les noms de relation spatiale sont en rapport médiat avec leur déterminant:

<i>tà hú</i>	dans une ville (<i>tà</i> : cl. A)
<i>tèí hù⁶</i>	dans la ville

Le pronom possessif de la 3e personne peut être inséré entre le premier terme et le second terme d'un rapport médiat:

<i>màhèí ɲgì wélèí</i>	le chef (<i>màhá</i>) ⁷ , sa (<i>ɲgì</i> : cl. A) maison
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Il l'est obligatoirement si le premier terme est pluriel:

<i>màháɲgà tì wélèí</i>	des chefs, leur maison
<i>màhèísìà tì wélèí</i>	les chefs, leur maison

Comme en kpelle, le rapport médiat s'établit entre le pronom et le substantif qui le suit (ici *wélèí*), ce syntagme étant apposé au premier nom; *ɲgì* et *tì* sont en effet considérés comme étant à l'initiale de locution: s'il n'en était pas ainsi, le schème tonique de **wélè(i)* serait du type régulier ˆˆ (ˆ). En revanche, on ne peut assimiler l'usage

⁶ *tà* n'est plus ici à la forme radicale; d'où le schème ˆ de *hù*.

⁷ Certains noms ˆˆ de la classe A sont ˆˆ devant *-í*.

mende à celui du kpelle. Dans cette dernière langue, ou bien le pronom, sous sa forme simple, est préposé à un nom qui, de par son sens, exige une référence personnelle, ou bien il est, sous sa forme spéciale, la marque même du rapport médiat. En mende, la modification du schème tonique du second terme suffit à marquer le caractère médiat de la relation génitive. Il est possible que les morphèmes de pluriel *ngà* et *sià* conservent un sens lexical suffisamment net pour qu'il puisse y avoir hésitation dans l'analyse d'une expression telle que :

- **màhèl sià – wèlèl* la maison de la pluralité de chefs
 **màhèl – sià wèlèl* la maison faite pour plusieurs, appartenant au chef

La présence de *tì* aurait pour fonction de lever l'équivoque. Une hypothèse semblable, ou la simple analogie, rendrait compte de l'emploi de *ngi* au singulier.

Par rapport à ces deux types de relations, les noms du mende se répartissent en deux catégories :

- a) ceux qui peuvent figurer en second terme d'un rapport médiat aussi bien que d'un rapport immédiat. Ce sont les noms de parties du corps, de relations spatiales, de parenté dont il a été précédemment question à propos du rapport immédiat ;
 b) ceux qui ne se rencontrent jamais qu'en rapport médiat avec leur déterminant.

Cette distinction est encore fondée sur le sens des mots en question ; mais elle réduit considérablement le champ de la relation immédiate et lui confère un contenu positif qu'elle est loin de présenter en kpelle ; alors que dans cette dernière langue, elle constitue la forme générale du rapport d'annexion, elle en est ici un cas particulier. On voit aisément comment, applicable seulement à un petit nombre de noms, elle pourrait en devenir la caractéristique principale et servir à délimiter une classe de "possessions inaliénables".

Bandi.⁸

Il semble bien que ce pas ait été franchi en bandi. Nous n'avons sur cette langue que peu d'informations. Il apparaît cependant, d'après les brèves indications fournies par Fr. Hintze et par G. Innes, que l'extension du rapport immédiat y est très réduite. Il ne concerne plus que les noms désignant des êtres ou objets définis par référence à Ego : certains membres de la parenté consanguine, en particulier de la parenté "à respect" :

<i>keye</i>	père, oncle paternel	<i>ndia</i>	frère aîné, soeur aînée
<i>ɲfe</i>	mère, tante maternelle	<i>ndeye</i>	jeune frère ou soeur
<i>keya</i>	oncle utérin	<i>kwela</i>	grand père
<i>tena</i>	tante paternelle	<i>mama</i>	grand mère,

l'ami : *ndiamo*, le camarade : *mbala*, et la propriété : *nda*. Ces trois derniers possèdent une forme "définie" (en *-i* ou *-ngi*), les autres n'en ont pas. La marque du rapport

⁸ G. Innes, "A Note on Consonant Mutation in Bandi", *Sierra-Leone Studies*, new series, n° 14 (dec. 1960). – Fr. Hintze, art. cit.

immédiat est la “suture ouverte” entre les deux mots, qui interdit la lénition de l’initiale consonantique du second :

(Innes) *ní kɛɣɛ* mon père

Dans tous les autres cas, la lénition se produit chaque fois qu’elle est possible, c’est-à-dire après les pronoms personnels à finale vocalique, après la forme définie (suffixée en *-i* ou *-ɣgi*) de tous les noms, et après la forme indéfinie des noms à radical terminé par une voyelle, à l’exclusion de ceux qui comportaient jadis un *-ɣg* final amui :

(Hintze)	<i>ní velei</i>	ma maison
(Innes)	<i>sale velei</i>	la “maison de médecine” (medicine house) – <i>salei</i> : la médecine
	<i>masa pelei</i>	la maison de chef
	<i>masangi velei</i>	la maison du chef

Il faut mentionner en outre le cas de certains noms de parties du corps devant lesquels d’après Innes, on emploie, au lieu des pronoms “possessifs” 1 sg. *ní* et 3 sg. *ngi*, des variantes toniques de ces pronoms : $\acute{\text{~}}$ et $\grave{\text{~}}$; dans ce cas, l’initiale consonantique n’est pas modifiée :

<i>i wungi</i>	[$\acute{\text{~}}$]	ta tête
<i>ngungi</i>	[$\acute{\text{~}}$]	ma tête
<i>ngungi</i>	[$\grave{\text{~}}$]	sa tête

Mais ces variantes toniques sont employées aussi comme complément d’objet du verbe, également sans lénition :

<i>masangi loli</i>		appelle le chef
<i>toli</i>	[$\acute{\text{~}}$]	appelle-le
<i>toli</i>	[$\acute{\text{~}}$]	appelle-moi

L’occurrence des formes toniques du pronom personnel ne peut donc être considérée comme signalant une modalité particulière du rapport d’annexion.

Nous ne savons rien des modifications éventuelles du schème accentuel des noms en rapport d’annexion ; mais les procédés d’expression des modalités de ce rapport sont analogues à ceux dont use le mende : les deux termes en relation immédiate demeurent nettement distincts, chacun conservant en mende son schème tonique, la lénition de l’initiale consonantique du second terme, qui est en bandi la marque de toute liaison syntaxique étroite, étant supprimée dans cette dernière langue. Réciproquement, deux noms en relation médiate forment, du point de vue du ton, un groupe unique en mende, le schème tonique du second étant conditionné par la classe tonique du premier ; dans les mêmes conditions, les deux termes sont unis en bandi par un phénomène de sandhi externe. La principale différence entre les deux langues réside dans le petit nombre des noms susceptibles d’entrer, en bandi, dans une relation de détermination immédiate, et dans le fait que ces noms sont apparemment exclus de tout autre type de détermination.

Le petit nombre des langues examinées limite étroitement la portée des conclusions qu'on est en droit de tirer de cette étude. Il nous semble cependant important, du point de vue de la théorie linguistique, qu'un phénomène aussi remarquable que l'existence, en certaines langues, d'une classe de substantifs désignant des "possessions inaliénables" se révèle susceptible d'interprétation proprement linguistique, alors qu'on a trop tendance, à notre avis, à le traiter comme un fait de mentalité, comme une "curiosité" relevant de la psychologie des peuples. Il a été possible d'établir une continuité entre la simple faculté, offerte par certains parlars, de préciser par des moyens lexicaux le contenu d'un rapport de détermination, et l'institution de deux classes de noms dont le comportement est imposé au sujet parlant par la structure même de sa langue. Les deux termes extrêmes de cette évolution nous ont été fournis par le kpelle septentrional et le bandi, les chaînons intermédiaires par les parlars kpelle du Libéria et le mende, auxquels il faudrait ajouter le susu, le mandingue et probablement bien d'autres langues: on y voit se former une marque propre à la relation médiate, et, surtout en mende, le domaine de la relation immédiate se restreindre à un nombre toujours plus limité de substantifs. Plutôt que l'émergence d'une structure mentale propre aux populations de langues mandé, nous avons observé un processus de grammaticalisation, la sclérose d'un procédé lexical analogue à celui qui, en un tout autre domaine, a permis la formation du futur dans les langues romanes. Accessoirement, nous avons constaté que le traitement particulier accordé en appelle à certains noms de parenté, mais aussi au nom de l'ami, de l'ennemi, de l'homonyme, etc. s'expliquait bien par la nature même de leur signifié qui les range dans cette catégorie des "indicateurs se référant à la présente instance de discours" qu'a décrite M. Benveniste ("La nature des pronoms", *For Roman Jakobson*, p. 34-37). Il serait intéressant d'examiner si d'autres langues négro-africaines ont également affecté un mode d'expression grammaticale à cette distinction logique.

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DISCUSSION

ROSÉN:

Ce qui est le plus frappant parmi les faits exposés ici est que les moyens d'expression morphologiques et syntagmatiques relevés dans ces langues africaines sont à peu près parfaitement identiques avec ceux qui s'observent dans les autres langues dans lesquelles l'opposition des possessions "aliénable" et "inaliénable" a été établie et étudiée. (Voir *Lingua*, 8, 1959, 275 ss., et la littérature citée, Havers, Lévy-Bruhl, Uhlenbeck, Benveniste, Baader, Rosén.) Etant donné que les phénomènes étudiés par M. Manessy à l'égard de quelques langues africaines ne sont point 'caractéristiques' à ces langues, on serait intéressé à savoir, quelles sont les limites exactes de la catégorie "possession inaliénable" dans ces langues, afin de pouvoir faire la comparaison catégoriale avec les autres systèmes linguistiques qui la connaissent.

NON-OMISSIBLE DETERMINERS IN SLAVIC LANGUAGES

MILKA IVIĆ

1. A determiner (D) of a given grammatical unit (X) is to be called omissible if the phrase $D + X$ may be transformed into $\emptyset + X$, without changing the grammatical value of X. For instance, the Serbocroatian Accusative form *devojk* in *vidim lepu devojk* "I see the beautiful girl" remains in the same grammatical function of direct object even if the phrase *lepu devojk* becomes transformed into $\emptyset + X$: *vidim devojk* "I see the girl."

Thus, the determiner is of the OMISSIBLE type if:

$$D + X \rightarrow \emptyset + X = X$$

2. NON-OMISSIBLE determiners are qualifiers and quantifiers which cannot be omitted without specific grammatical consequences. Either (instance 1) the transformation of $D + X$ into $\emptyset + X$ cannot take place for it would lead to nonsense or (instance 2) the transformation of $D + X$ into $\emptyset + X$ would yield a new grammatical unit (Y).

Instance 1: The Czech example with genitivus qualitatis *dívka černých vlasů* "the girl with black hair" cannot undergo the transformation test, the phrase **dívka vlasů* (= $\emptyset + X$) being ungrammatical.

Instance 2: The Serbocroatian phrase *celo proleće – čekao je celo proleće* "he was waiting the whole spring" may be transformed into $\emptyset + X$, but that would mean the transformation of the temporal quantifier into the direct object: *čekao je proleće* "he waited for spring".

3. In instances concerning the use of the omissible determiner the relationship of D with X may be described as a simple conjunction of two grammatical units in the speech string ($D + X = D + X$). As far as the phenomenon of non-omissible determiners is concerned, the conjunction of the two grammatical units D and X is used as a specific morphological device for the identification of a third grammatical unit (Z): $D + X = Z$.

Thus:

Phrases with non-omissible determiners function as minimal grammatical units and that fact should be correctly observed and described in Slavic grammars.

The grammatical category of non-omissible determiners is to be found, of course, in non-Slavic languages too. But the Slavic languages are characterized by a comparatively wide use of non-omissible determiners in various situations.

4. It is noteworthy that in many instances in which grammars of Slavic languages mention the archaic free use of cases, we are concerned with a misunderstanding based on the fact that the existence of the grammatical category of non-omissible determiners has not been recognized. Examples quoted in this respect concern usually the case-form without prepositions which belongs nevertheless to the bound type being obligatorily used with a determiner. We should remember, for instance, the archaic "free" (= without prepositions) use of Instrumental forms denoting quality in examples like Old Russian: *bě že Mstislavъ debelъ tělomъ, čermenъ licemъ, velikyma očima* "Mstislav was thickset and ruddy-faced, with big eyes".¹ The phrase *M. velikyma očima* could not be transformed into **M. očima*. The same rule of transformation is valid even today, as may be shown by examples from certain Serbocroatian dialects in which the old case-category of this type is still in use.²

The Genitive denoting quality has occurred exclusively with non-omissible determiners in Slavic languages, from the very beginning of the historical period till now.³ (Serbocroatian: *devojka crnih očiju* "the girl with black eyes" → **devojka očiju*). The Genitive denoting possession appeared from the very beginning side by side with possessive adjectives. As is well known, in Old Slavic the repartition of their use was established following the principle: the Genitive form appeared only if the determiner was present with the noun, the adjective if the noun was used alone, without determiners (so *synъ boga živago* "the son of living God" as opposed to *synъ božii* "the son of God").⁴ The same principle is more or less respected in some modern Slavic languages (especially in Serbocroatian).⁵ The genitivus temporis, used without prepositions, requires also the presence of a non-omissible determiner (Serbocroatian: *udala se prošle godine* "she got married last year", but never **udala se godine*). The same is valid for the Accusative denoting time or quantity (if used without prepositions): Serbocroatian *spavao je ono veče* "he slept that evening" but never **spavao je veče*; *radio je celu nedelju* "he worked the whole week" but never **radio je nedelju*. And so on.⁶

¹ For more examples see A. A. Potebnja, *Iz zapisok po ruskoj grammatike*, I-II (Xar'kov, 1888), 147.

² See M. Ivić, *Značenja srpskohrvatskog instrumentala i njihov razvoj* (Beograd, 1954), p. 216-221.

³ There are only a few examples of exceptions to this to be found in Polish; see A. Mirowicz, *O grupach syntaktycznych z przydawką* (Toruń, 1949), 77.

⁴ See N. S. Trubetzkoy, "O pritižatelj'nyx prilagatel'nyx (possessiva) starocerkovnoslavjanskogo jazyka", *Zbornik u čast A. Belića* (Beograd, 1937), 16.

⁵ The question whether a bound Genitive form is used as qualifier or to denote possession can be resolved by transformation test. In both cases the phrase with the Genitive form becomes transformed by means of a grammatical device which helps to convey the meaning "being in possession of". If the substantive with the Genitive mark becomes recognized as the possessor, we have to do with genitivus possessivus. If it is not the substantive in the Genitive form but the head of the phrase which is recognized as the possessor by the transformation test we have to do with genitivus qualitatis. Cf. the transformation of the following Serbocroatian phrases: (I) *crna kosa moje sestre* "my sister's black hair" → *moja sestra ima crnu kosu* (= genitivus possessivus) / (II) *moja sestra crne kose* "my sister with black hair" → *moja sestra ima crnu kosu* (= genitivus qualitatis).

⁶ For more examples (from different Slavic languages) see my article: "Srpskohrvatski padežni oblici obavezno praćeni odredbom kao pomoćnim morfološkim znakom", *Godišnjak Filozofskog fakulteta u Novom Sadu*, IV (1959), 151-163.

5. The grammatical relevance of the conjunction labelled $D + X$ has to be observed also within the scope of verbal categories.

The Serbocroatian phrase *dodje ponekad* "he comes sometimes" with the PERFECTIVE present tense *dodje* (where *dodje* = X, and *ponekad* = D) cannot undergo the transformation into $*\emptyset + X$ for it would lead to grammatical nonsense (*dodje* being not recognized as a minimal unit). On the contrary, the phrase *dolazi ponekad* "he comes sometimes", with the IMPERFECTIVE present tense form, may undergo the transformation test which would yield a change of the grammatical meaning (following the pattern: $D + X \rightarrow \emptyset + X = Y$): *dolazi ponekad* "he comes sometimes" \approx *dolazi* "he is coming". As may be shown by transformation tests, in the majority of South Slavic languages the present tense forms of perfective verbs do not occur except in phrases. Thus, for such languages, we must correct the well-known statement (usually quoted in Slavic grammars) that every Slavic finite verbal form automatically may function as a sentence. We have to add: but as to present tense forms of perfective verbs, they constitute a sentence *only if* occurring in phrases (for otherwise they cannot be taken as minimal units, either on the morphological, or on the syntactic level).

Just one more example as illustration (this does not exhaust the actual list of instances when Slavic verbs are used with non-omissible determiners): The Serbocroatian past gerund forms of PERFECTIVE verbs may be used free (= without determiners) but the past gerund forms of IMPERFECTIVE verbs may occur only in phrases, being obligatorily bound to the presence of a quantifier (*čekavši /impf. verb/ dva sata, otišao je kući* "having waited for two hours, he went home", $*čekavši, otišao je kući$).⁷

6. All the different examples already mentioned belong either to the type $D + X \rightarrow * \emptyset + X$, or to the type $D + X \rightarrow \emptyset + X = Y$. It would be a contribution to the structural characteristics of the language under investigation to state all instances in which an opposition between bound and free forms may or may not occur. Even more, elaboration of any similar kind of grammatical phenomena, which has not hitherto been correctly observed, would certainly provide new information about the intersection of the morphological and syntactic levels.

7. The intersection of the grammatical and lexical levels would also be seen under a new light by pointing out correctly the grammatical phenomenon concerning the obligatory use of a determiner. As a matter of fact, it is not only the presence of the determiner which may be a relevant grammatical condition. It is even the choice of the lexeme used as non-omissible determiner which may have to be grammatically determined. In the example *čekao je celo proleće* the Accusative denotes the quantity of time. But if instead of the quantifier *celo* we put another adjective, like *sunčano*

⁷ More examples are quoted in my article "The Grammatical Category of Non-omissible Determiners", *Lingua*, XI (1962), 199-204.

“sunny” or *lepo* “beautiful”, the object-meaning of the free case-form remains unchanged: *čekao je proleće*, or *sunčano proleće*, or *lepo proleće* = Acc. of object ∞ *čekao je celo proleće* = Acc. of temporal quantifying.

The grammatical criteria by which a similar sort of lexical choice happens to be determined may be changed in the course of time (in Serbocroatian, for instance, one century ago it was possible to use examples like *desilo se nemačkog rata* “it took place during the war with the Germans”, now only: . . . *ovog rata* “. . . during this war”, . . . *prošlog rata* “. . . during the last war” and the like, but never * . . . *nemačkog rata*). Slavic diachronic grammar should throw more light on such phenomena.

8. General linguistic theory may be interested in the fact that it is only the marginal use of a flexional form which is usually marked by the presence of a non-omissible determiner. We can mention here the use of Slavic case-forms as an illustration. There are three basic morphological possibilities to indicate case meanings in Slavic languages. They are: the free case-form, the case-form accompanied by a preposition, the case-form obligatorily accompanied by a determiner.⁸ The case-forms which by no means can have adjectives or adverbs as their substitutes must be used free. Adjectives or adverbs may be used as substitutes only in some instances but not invariably if the case-form is bound to the presence of a preposition. As to the case-forms requiring a non-omissible determiner, they may not only undergo in some instances the same kind of substitution, but they even must, without exception, belong to the same distributional class as qualifiers and quantifiers.

Novi Sad

DISCUSSION

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The pioneering endeavor to complement the conceptual pair admissibility/non-admissibility by the pair omissibility/non-omissibility is of substantial value for the inquiry into the scale explicitness/ellipsis and asks both for a clear discrimination between the grammatical and lexical pattern and for an exhaustive study of their interplay.

⁸ See M. Ivić, “On the Structural Characteristics of the Serbocroatian Case System”, *International Journal of Slavic Linguistics and Poetics*, IV (1961), 38-47.

LEXICOSTATISTICS HAS NOT YET ATTAINED THE STATUS OF A SCIENCE

EPHRAIM CROSS

In 1950 Morris Swadesh approached me with a request that I test a certain suggested measurement system by applying it to the Romance Language subfamily of the Indo-European languages. The technique included the selection of a relatively small standard and current vocabulary composed of so-called "basic" or "universal" terms, as concrete as obtainable within the framework of natural languages. The determination of what is "basic" or "universal" is, pending much further scientific accumulation of data to establish the definitive fact, a purely empirical foundation compounded of a not too subjective, but none the less relatively limited knowledge of human language, present and past.

In anticipation of this exposition it seems essential to note that in the realm of the Romance languages the lexeme "to be" is basic and universal because it is found in the parent speech and has persisted in all of the daughter languages for a period of altogether two thousand years.

However, the usefulness or significance of the inclusion of such a vocabulary element might be questionable for general Indo-European, would be confusing for a late stage of Indo-European and would certainly be inappropriate for Proto-Indo-European in any comparison with Semitic or Finno-Ugric and quite untenable for American-Indian and indigenous African languages, whether for internal or external comparisons.

The suggested method of comparison and calculation was a very ingenious one. It derived, as so much of our current linguistic terminology and methodology, from the sciences of physics, chemistry, biology, and mathematics. I omit engineering, which is likewise drawn upon. Specifically, it was the structuring of an analogue to the use under parallel conditions of carbon 14 in dating archeological and geological finds.

After using several longer trial lists with variant results I finally constituted a vocabulary of 241 words:

above – over (adv.), alive, all, animal, arm, ashes, aunt (pat.), ax, back (n. – of man), bad, bark (of tree), before – in front (adv.), belly, below – under, bend (trans. – twig), berry, big, bird, bite, bitter, black, blood, blow (v. – with mouth), blue (like sky), bone, branch (of tree), break (trans. – stick), breathe, brother (older, of man), burn (intrans.), call out – shout, (I) can, carry (in arms), child (not infant), claw, close – shut, cloud, cold (weather), cry – weep,

dark, daughter, day, dead, dig, dog, drink, dry, dull (blade), dust, ear, earth (soil), eat, egg, eight, eleven, empty (container), eye, fall (e.g. tree), far, fat (of animal), father (of man), fear (v.), feather (plume), feel (with hands), find, fire, fish, five, flat, flesh, flow, flower, fly (v.), fog (mist), foot, four, front, full, gather up (acorns), give, go (afoot), good, grandfather (paternal), grandson, grass, green, guts (entrails), hair, half, hammer, hand, hard (not soft), he, head, hear, heart, heavy, hide – conceal, hill, hit – strike (with hand), hold (in hand), hot (heat – weather), how much?, hurt – pain (v. intrans.), husband, I, ice, inside (adv.), kill, knee, knife, know (facts), lake, leaf, lefthand (at or to left), leg (of man), lie (down), lift – raise, light (n.), light (of weight), lip, little – small (adj.), little (adv.), long, lose, louse, love – like (a person – not necessarily sexually), man (human), man (male), middle, moon, more (adv. quant.), mother, mouth, move (v. intr.), much, name, near (adv.), neck, new, night, nine, nose, not, oil, old, one, open, other, outside (adv.), part (n.), play, pull – draw, put – place, quick, rain, red, right (at, to the), river, road – path, root (e.g. tree), rub, run, salt, sand, scratch, sea, see, seven, shade, sharp (e.g. blade), shine, short, sister (elder), sit, six, skin, sky, sleep, slow, smell (v. trans.), smoke, snow, soft (to touch), son, speak – talk, spit, squeeze, stand, star, stone, straight, stretch, strong, suck, sun, swallow, sweat, sweet, swim, tail, take (in hands), tear (v.), tear (from weeping), teat, ten, that, thick, thin, this, three, throw, thunder, tongue, tooth, tree, turn (v. intrans.), twelve, twist, two, uncle (paternal), vomit, wash (objects), water, we, weak (not strong), wet, what?, where?, white (like milk), who?, wife, wind, wing, wish – want, woman, wood, worm, year, you (int. sing.), young.

By means of this vocabulary I examined and correlated the corresponding lexica of Latin, French, Italian, Romanian, Catalan, Spanish, Portuguese, Rhaeto-Romance (specifically, Upper and Lower Engadinian), and Sardinian (specifically, Southern and Northern Logudorese). My approach was entirely free of bias, with an open-minded, enthusiastic wait-and-see attitude and, as can be ascertained, without any intention of forcing answers.

I could have limited my list to one hundred items and thus facilitated the entailed mathematical calculations. This facility I rejected, despite the unfavorable laboratory conditions under which a teacher and researcher in my university is constrained to operate. During the twelve years of this investigation there were available no computers, nor human assistants to furnish relief to the drudgery of preliminary and elementary chores that have been a drag on the aspirations, exertions, and accomplishments of energetic teachers. I admit that not only for Romance, but no doubt for more general modern Indo-European application, several items of my list could well be excluded.

I gave the project the unambitious title of “Correspondences and Linguistic Proximity” and presented the results to an annual meeting of the Linguistic Society of America (1950). My report was limited to various correspondences and the conclusions as to chronological and geographic proximity to be deduced therefrom. This information could at all points be checked against our historically established record of the time and area distribution of the Latin and Romance Languages.

Present at the publication of my findings, E. H. Sturtevant, while not being in a position to challenge the accuracy of my interrelational percentages, nevertheless, voicing the hostile reaction of vested interests of that day, remarked forcibly that it was

“better to stick to the old historic and comparative method”. The remark was irrelevant and Sturtevant was mistaken on a very critical point. There was no offer to displace or reject the historic and comparative method. In fact, the historic and comparative method was strictly adhered to and scrupulously used in the assignment of cognates to the correspondence ratings. The proposed test utilized the results of approved linguistic labors that met all the scientific requirements of this discipline. The verification of a generally known fact may be neither useless nor unscientific. At the stage mentioned neither the experiment nor the experimenter had advanced to the level of glottochronology and no claims were made on that ambit of the technique.

As to the Latin base, I used the vocabulary of the Classical period. This presents no distortion of the historic picture because if *domus* is the word for “house” in the standard educated speech or was once the current term it is not found in Romance and this circumstance constitutes one of the insensitivities of this procedure, as will be further detailed. In other words, it could be that at the time of the implantation of Latin in the several subregions of the Roman domain the current word was “*casa*”, or very shortly became so. This observation is purely by way of anticipation and illustration, since *house* is not one of the lexemes incorporated in the selected vocabulary. Any other similarly conditioned example could be used. Our experiment, in short, deals not with presumed, assumed, or deduced, but with known, attested languages.

Our testing apparatus, if it performs its immediate function, will measure correspondences and mutual proximities.

In a period of about 2000 years ending in 1950 of the Christian era, with the longest span not more than eleven percent in excess of this figure (Sardinia and Spain, Gallia Narbonensis, Gallia Transalpina) and the shortest span less than six percent subtracted from the period mentioned (Rhaetia and Dacia) the retention of the original Latin vocabulary, expressed in percentage, was shown to be as follows: Italian, 72.61; Rhaeto-Romance (i.e. Engadinian), 70.12; Spanish, 67.23; Portuguese, 66.8; Sardinian, 63.48; French 62.24; Catalan, 60.16; Romanian, 57.26. Certain pairs interrelate as follows, in descending order of percentage of correspondence:

Spanish-Portuguese, 88.79; Italian-Sardinian, 78.83; Catalan-Spanish, 78; Italian-Rhaeto-Romance, 77.59; French-Italian and Italian-Spanish, 76.76; Italian-Catalan, 72.61; French-Catalan, 71.78; French-Rhaeto-Romance, 71.37; Spanish-Sardinian, 70.12, French-Spanish, 67.22. In present-day view, Italian-Sardinian seems a closer-knit pair than Italian-Rhaeto-Romance, but not by a wide difference. It is to be noted that Spanish-Italian and French-Italian are closer-knit pairs than French-Spanish. The Catalan-Italian pair is fairly far removed from its nearest superior. Of course, the Italian-Romanian correspondence stands with the lowest of all the Romance pairs of which Romanian is one of the constituents, namely, from 51.45 to 59.33.

The historically attested chronological order of the extension of Latin speech is: Italy, Sardinia, Spain, Gallia Narbonensis [Provençal], Gallia Transalpina [French],

Rhaetia, Dacia [Romanian]. That is to say, Dacian Latin was physically detached from Italy 1857 years ago; Gallic Latin left the homeland 2000 years ago; the Latin of Gallia Narbonensis (Southern Gaul) dates from 2070-2150 years ago; Hispanic Latin, 2150 years ago; Sardinian Latin, 2190 years ago.

From this series of percentage figures we disengage the following observations: Italian is closest to Latin, while Romanian has diverged the most. The pair showing the closest internal units is the Spanish-Portuguese, with very minute variation, but not absolutely identical vocabularies. Their situs represents the area that has maintained itself closest to the Italian-Rhaeto-Romance subregion. Lastly, Sardinian, French, and Catalan group themselves together in the theater that shows the greatest divergence from the Latin of Italy and Rhaetia in the extension of the mother speech into Sardinia, Gallia Narbonensis, and Gallia Transalpina. I omit temporarily the enclaved Dacia. This representation appears in all respects, so far, the historic and linguistic fact.

Without proceeding to any further step we note that there is plentiful dichotomy here. Hispania was the first territory outside of Italy to be Latinized, except possibly Sardinia. Of Sardinia it should be noted that although the Romans got it in 238 B.C., as far as we know, there had been no Indo-European language there before Latin. Latin may not have been implanted immediately. There was probably great resistance not only to linguistic surrender but to Romanization in general. I think my development of the place of Sardinian furnishes a truer photograph of history than the revelation of a high-ranking pair offered by Rea and Kroeber, whose work I treat below. None the less, Sardinian and Italian have a north-south line of geographic propinquity. Further, the conditions of the spread and evolution of Latin in Spain appear clearly different from the circumstances attendant upon the introduction of the language into Sardinia.

In all of our reckonings we should bear in mind that the introduction of a language into new or alien territory does not always signify the inception of its divergent course. Spanish Latin and Sardinian Latin seem to have begun synchronously, but to-day Sardinian is much closer to Italian than to Spanish. I therefore have firm ground for disagreeing decidedly with Kroeber's fixing of the "divergence" dates as the dates of the extension of the language outside of its original situs. My findings make my disagreement unassailable.

It was not until 150 years later than Spain that Gaul north of the Narbonensis sector was Latinized. Yet the Spanish-Portuguese complex has retained a greater proportion of the Latin word fund than French. Lastly, the time span from the implantation of Latin in Dacia to present-day Romanian is nearly a third of a century shorter than the passage from Latin to Spanish. The findings report the actual present situation of the languages. This is a fortiori striking when it is considered that in 120 A.D. the basic vocabularies of the Latin of Spain and the Latin of Italy were presumably no more divergent than the basic vocabularies of contemporary British English and American English or, to adduce a probably more precise parallel, between the

basic vocabularies of present-day Mexican Spanish and the Spanish of Castile. Obviously, our device is sensitive to some kind of propinquity, whether of time, place, or culture, but it does not distinguish one from the other. On this instrument the effect is the same. This does not immediately render it unscientific. Therefore, the divergence of Romanian can be due to severance from the general body of Latin speech to the accompaniment of an impact of non-Latin speech against the entire periphery. The French divarication may be due, in part, to substratum intrusion. Cultural conditions may have kept Spanish Latin close to the Latin of Italy. The retention rate seems to depend upon some kind of proximity and cultural borrowings constitute one variety of proximity, such as in English the presence of large numbers of Latin and Greek terms without the physical contact of living speeches or even oral speeches. Finally, our test, applied to pidgin, would detect the correspondences in two separate vocabularies, the source and the resultant mixture, but it would not necessarily establish a time period of development, nor a genetic relationship of the speeches.

Kroeber, in discussing Romance history and glottochronology on the basis of a note by J. A. Rea, is wrong in maintaining that the figures afforded by Rea's test are in conformity with known intra-Romance history (*Language* 34.455 [1958]). Kroeber appears to force the answer, even against Rea's incorrect equating in the creation of such pairs as Catalan-Portuguese and Spanish-French. Kroeber refers to "Spanish and Portuguese whose historic centers of development were closely adjacent in Castile and Galicia and whose separate linguistic identities verge on the merely dialectic". Where do Rea's obtained figures demonstrate this? We do not know what vocabulary Rea used, but his percentages can clearly be characterized as ridiculous in the light of known fact.

Kroeber has worked wonders for eight Romance languages with percentage ratings that are quite awry. Kroeber's heart is right, but Rea's figures and Kroeber's structure erected thereon are wrong. This does not prove that the methodology is unscientific. It does show that there has been an error in the manipulation, for Kroeber's inconsequential but correct conclusion aptly applies to the figures obtained by me. He concluded from these untenable findings: "In short, what the statistics result in is a neat, coherent, internally consistent classification of Romance in excellent accord with the geographical distribution of the languages tested."

A vocabulary which I applied in August, 1950, showed Italian and Romanian as correct extremes in the development of Latin. However, it equated French, Spanish, and Portuguese and this certainly is not precise enough.

Contrast this early result of mine with Rea's retention rates. Any finding that does not give equal weight to Spanish and Portuguese and which, further, places Portuguese ahead of Spanish and French, especially when Spanish and French are given equal weight, is clearly wrong.

Obviously, the vocabulary selected constitutes an important wheel in the machinery and the exact determination of cognates is likewise vital. It may be that it is wrong, as

I have done, to equate Italian *cuore*, French *coeur* with the Spanish-Portuguese reflex *corazón-coração*, since the latter has modified the original form by the addition of a suffix – a frequent phenomenon in Romance – and this could scrupulously be considered a different word. Perhaps such an item should be evaluated at a figure less than unity (one half?) On the other hand, Romanian *inimă* is definitely a different word and our relationships would be blurred if we confused the two departures from the Latin original.

Plainly, we have not yet perfected a scientific machine, either as to the selection of a vocabulary, the length of the selected vocabulary, and the determination of our cognates.

With reference to my observations on proximity and my corollary suggestion that we have evidence of the correlation between geographic location, linguistic center and conservatism in basic vocabulary, Swadesh (personal letter, 18 Dec. 1952) treats my findings in the matter of Italian, French and Romanian in the following language; “You may be on the threshold of a discovery, and an important one. For, if we know that the central dialect of a group has the slowest rate of change, then it will be possible to determine from vocabulary statistics which of a series of contemporary languages represents the core of the old speech area, even when recent migrations may have changed things around.”

It is not the several absolute percentages, but the interrelationships that count and have significance. Therefore, some importance seems to attach to the selection of a valid test vocabulary. Such a vocabulary has apparently not been agreed upon.

We advance now to the Teutonic subfamily. We examine and compare Gothic, English, Dutch, German, Danish, and Swedish. I regret that I have not yet completed Norwegian. For Gothic, due to incomplete historic records, I was restricted to a vocabulary of 159 items. For the others I retained my count of 241.

The results of the test show that the correspondences with Gothic, in percentages, are: Dutch, 77.98; German, 76.73; Danish, 74.85; Swedish, 71.69; English, 67.29. The percentage correspondences of pairs are: Dutch-German, 88.79; Danish-Swedish, 87.55; German-Danish, 73.85; Dutch-Danish, 72.61; German-Swedish, 70.12; Dutch-Swedish, 69.70; English-Dutch, 64.31; English-German and English-Swedish, 60.58 for each pair; English-Danish, 60.16. English and Dutch are closer to each other than are English and German.

I find fascinating figures for Gothic-Modern West Frisian and for English-Modern West Frisian. These I am withholding for further check. It has generally been held that Frisian and English are nearer to each other than Dutch is to English. Of course, we should make allowance for the reduced vocabulary into which we were forced to frame our sample of Gothic. There is also a shorter time depth for possible changes in Gothic and its congeners in comparison with the Romance group. Finally, it is clear that Gothic is closer to the other Teutonic languages than Italian is to its Romance sisters and that the Teutonic speeches have parted asunder much less than Romance, even in the light of 1600 years between Gothic and the present-day lan-

guages studied. It thus seems clear that Gothic is at least as close to a resemblance of the mother speech of its group as Classical Latin to is Romance, even if we exclude Romanian from consideration.

It is also evident that English has lost more of the original word stock than have the others. Since Dutch has retained the largest portion, with German a close second, the Dutch-German pair are closer to one another than are English and Dutch. Interestingly, English and Danish are furthest apart by virtue of the circumstance of the vocabulary losses of English as against the relative conservation of the older status by Danish.

In December, 1952, I presented to the Linguistic Society of America the results of a comparison of Romance, Teutonic, and Slavic under the title of "Chronometric and Telemetric Determination of the Relationship of Latin, Gothic, Old Church Slavic, and Their Present Affiliates". I was able to find for a vocabulary of Old Church Slavic a total of 190 words. The other Slavic languages which I treat are Russian, Polish, Bulgarian, and Ukrainian. With reference to correspondence with Old Church Slavic the standing of the languages is Russian, 79.473 percent; Polish, 77.894; Bulgarian, 75.789; Ukrainian, 74.21. Pairs of languages rank as follows: Russian-Polish, 76.348; Russian-Ukrainian, 73.44; Russian-Bulgarian, 73.029; Polish-Bulgarian, 69.709.

The proximity of Russian and Old Church Slavic in vocabulary has previously been suggested by some scholars. Geographically, Russian and Ukrainian belong to the eastern group of Slavic, while Polish stands to the west of them. However, unless these figures are successfully challenged, Russian seems closer to Polish than to Ukrainian. All three are mutually contiguous. Our photograph, although to my regret it omits Serbian, Croatian, Slovenian, and Czech, reflects the tight proximity in vocabularies of the Slavic languages and thus their slight divergence from each other. This is in contrast to Romance and Teutonic. Here, however, our oldest language dates from about 850 A.D., so that our time depth is only slightly over one thousand years, that is, 1100 years.

I essayed a similar comparative examination of Akkadian, Biblical Hebrew, Classical Arabic, and Biblical Aramaic. My correlation figures are incomplete, but on the basis of 82 forms for each language I found a Hebrew-Arabic correspondence of 64.63 per cent. With 157 items I made a tentative calculation of the interrelationship of Akkadian and Hebrew. This produced a percentage of 56.05. With only a 47-word vocabulary I found the correspondence percentage of Hebrew-Aramaic to be 89.38.

Calculations based on the statistics obtained in the previously detailed manner have been offered as a chronometric device for determining time depths or let us say the length of time that separates two stages of a single language and also the point of time at which two or more speech forms diverge from each other to become so many separate languages.

We thus could fix terminal points in the form of dates for the beginning and end

stages of individual languages, living and dead, and the rise of new languages out of older stages of speech, as well as the divergence points of sister languages. I omit the somewhat overenthusiastic and excessive claims as to genetic relationships (Kroeber and Chrétien) and "convincing taxonomy" (Greenberg). We have enough difficulty with the question of time depths, as my analysis shows.

To test the degree of dependability, if not the scientific accuracy of this machinery I engaged in the study I have here indicated. In addition, I made examinations that brought into the purview of this system other languages and correspondences.

These languages include Ancient Classical Greek and Modern spoken Greek, Old Irish, Modern Irish, and Albanian. As a basis for the undertaking I cite these figures of correspondence: Latin-Gothic, 40.25 percent; English-French, 28.63; English-Russian, 20.746; Gothic-Old Church Slavic, 35.172; Latin-Greek, 34.44; Latin-Old Church Slavic, 32.105; Classical Greek-Modern Greek, 55.6; French-Russian (tentative) 22.406; Latin-Old Irish, 30.60; Old Irish-Gothic, 35.766; Old Irish-Modern Irish, 69.945. I planned Albanian-English, but did not complete this pair.

A preliminary view of these figures, which are not to be regarded as totally dissociated from my figures for Latin-Romance and for the Teutonic subfamily, show that there is a kernel of truth buried here, but whether we have the groundwork for a science called "glottochronology" is open to very serious question and doubt.

To begin our discussion, it is appropriate to review the presumption that there is a universal vocabulary retention rate per language for each millenium elapsed. With a plus-or-minus variation we might still have an acceptable scientific formula. None the less, the saving grace of the institution of a slack in our measurement does not necessarily make our scheme a science.

If I am permitted to translate the recorded percentages into terms of the theorized formula they mean, in terms of time span since "divergence": English-Russian, 7875 years; French-Russian (tentative), 6625; English-French, 5750; Latin-Old Irish, 5500; Latin-Old Church Slavic, 5000; Latin-Greek, 4750; Gothic-Old Church Slavic, 4630; Old Irish-Gothic, 4500; Latin-Gothic, 3750; Classical Greek-Modern Greek, 2625 (this is actually, historically, 2400 years), Old Irish-Modern Irish, 1620 (this is actually, historically, 1100 years).

In the first place, many of our numerical values are not absolute. Rather, they are relative and may even be subjective values. As relative values they have worth and some scientific significance. Secondly, I find that the application of the formula enunciated is too complicated with other elements of obstruction to produce anything but frequent distortions.

Using the Latinian languages as our control group we find that a chart of the actual historic distribution in time makes a pretty crazy graph. However, the structural result possesses the virtue of truth, even though the truth is crushed by the weight of a force pushing it into a shape that it does not seem to possess naturally. The graph which results from the proponents' hypothesis is correct in the relative position, for example, of rather remote pairs of languages. The English-Russian time depth does

reveal itself as greater than the Gothic-Old Church Slavic. So, that of French-Russian is pronouncedly greater than Latin-Old Church Slavic. English and French are clearly seen to be further removed from each other than Gothic is from Latin. The utmost it means is that the further backward we move in time the more similar are the linguistic features of related languages.

I am not a mathematician. As I contemplate the contributions of mathematics to the physical sciences I have no inclination to burn the textbooks of that language nor to imprison those who specialize in numbers. I do venture to ask whether statistics cannot lie. It can be affirmed that all truth is not confined to mathematics and there are some truths that obviously are not reducible to mathematical formulae. Statistics can not only be misused, but it can also mislead, even when powered with good intentions.

Without the aid of a corps of assistants and without computing machines I have myself made calculations through the years. I have carefully interpolated the percentages which lie between the whole numbers of the powers of the hypothesized eighty-percent retention rate per millenium on an extensive graph. It has been shown that with all its mathematical armor, the machinery, even when operating in a period of 1500 years, strikes complicating impediments. At 2000 years and beyond, it meets a counterforce of frustrating effect. Phonological observation has shown that ultimately any single phoneme can shift into any other, and if we at some point of time do not find a broad enough sampling we might be prevented from matching cognates.

In accordance with the justice I am meting out to the proposal I will insist that, given the absolute accuracy of the formula, although 17,000 years are a vast period to linguists, the finding of four words in common out of my 241 could indicate a remote and ultimate relationship, or an originally close enough contact to result in borrowing. In this connection, it so happens that in my English test vocabulary there are thirteen words of non-English origin. The placement of three is unclear. This amounts to at least five percent of my vocabulary. Eleven are French and two are Latin. Two, not counted, are unclear. The transformation took place less than 1000 years ago. I would not care to universalize on this occurrence. It is unnecessary to belabor the possibility of interferences by reborrowings and even the resumption of older forms, formerly discarded for a period.

Linguistic scientists do not need to be adherents of the philosophy of numerology and the religion of digitology may lead us astray into the paths of unrighteousness. By their intensive and extensive borrowings from the disciplines of physics, chemistry, biology, and engineering, linguists have paid ample tribute to their colleagues' efforts to uncover, unravel, and codify the ways of nature. But linguistic researchers should not be satisfied with the mere germ of truth, nor a formulation that has wide gaps and the facile quality of elasticity.

Lexicostatistics is diverting, but not very useful and it is even misleading when employed to set up a taxonomy of language. I experimented with it immediately

after the publication of an article by Kroeber and Chretien in *Language* (13.83-103 [1937], "Quantitative Classification of Indo-European Languages"). After a painful experience I found the alluring method very much of a failure.

I regret these results and the disappointment of my hopes and the hopes of other researchers.

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PROBLÈMES DE LEXICOSTATISTIQUE SUD-SÉMITIQUE

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La discussion sur la validité théorique de la glottochronologie se développe sur des lignes diverses dont deux me paraissent essentielles: d'une part la recevabilité du modèle mathématique qu'elle propose, d'autre part la légitimité sur le plan de la linguistique générale, des postulats sur lesquels elle se fonde. Sur ce dernier point, s'il est possible à tout linguiste de se faire une opinion, il peut être utile, je crois, avant de la formuler, d'essayer de la fonder sur une expérience directe. Car à opposer des principes à des principes et des postulats à des postulats, on risque de ne guère avancer. C'est là le côté parfois un peu décevant de certains des échanges de vues récents dans *Current Anthropology*, d'avril 1962.

Mais naturellement, dans ces échanges de vue, ce n'est pas le texte inducteur lui-même, c'est-à-dire l'article de M. M. Knut Bergsland et Hans Vogt qui est critiquable, bien au contraire. C'est à mon avis avec pleine et entière raison que ces auteurs ont incité ceux qui ont été conduits à tenter quelque essai de glottochronologie à en faire une analyse approfondie, analyse dont ils ont eux-mêmes donné un modèle éclatant.

Voici à propos d'une expérience d'ailleurs partielle pour l'instant, menée accidentellement en marge d'une autre recherche, quelques observations sommaires et provisoires.

Le tableau des langues sémitiques est dans l'ensemble relativement clair. La disposition en grandes masses si l'on veut, apparaît assez nettement, même si dans le détail, il ne manque pas de problèmes encore non résolus.

En simplifiant un peu, en négligeant aussi des aspects certes non négligeables en eux-mêmes de la situation, mais qui sont au fond de peu de conséquence pour une première approche, on peut distinguer dans les langues sémitiques, en particulier une sous-unité assez bien délimitée, celles des langues méridionales, essentiellement: arabe et sudarabique-éthiopien. C'est cette sous-unité qui, en raison de notre documentation, du fait en particulier qu'elle comporte des dialectes encore vivants, se prête peut-être le plus facilement à l'expérience en nous proposant un cas de contrôle assez précis: celui de l'éthiopien, et un cas de divergence datée également avec une certaine précision, celui de l'arabe.

Mais d'abord quelle liste diagnostique employer?

Celles qui ont été mises en oeuvre par Morris Swadesh, et qui ont servi pour de nombreux travaux sur des langues pourtant très diverses (indo-européennes, amérindiennes,

nes, langues d'Extrême-Orient, etc.), ne se révèlent pas toujours d'un usage très simple pour les langues sémitiques. On est souvent hésitant devant les équivalents à donner à certains termes – et ceci, même, et peut-être surtout, dans les dialectes vivants.

En fait on est rapidement amené à constater par exemple, qu'une notion comme celle de l'opposition d'un démonstratif rapproché à un démonstratif éloigné (*this – that*), est secondaire dans l'état où on la saisit dans les langues sémitiques, et qu'elle n'est pas réalisée partout, en tout cas pas de façon strictement parallèle. De même, l'utilisation des noms de couleurs (il y en a cinq dans la liste des cent termes de 1955) est très délicate. Il est difficile de voir quelle est réellement la structuration du domaine sémantique des couleurs, et à quoi correspondent également les noms dont nous disposons. Ce qui est pour nous "vert" ou "rouge" en particulier, entre, selon la langue, dans des parties du champ qui peuvent comprendre pour nous des couleurs autrement nommées. On peut même, à considérer les choses de près, se demander dans quelle mesure il est possible de nommer chacune des cinq couleurs dans tous les dialectes sémitiques.

Il y a aussi des noms de qualité comme "grand", "petit", "rond", etc., qui n'ont pas la même extension dans tous les dialectes, et qui posent des problèmes parfois embarrassants.

Naturellement de tels problèmes ne sont pas propres au sémitique, et la liste des termes supplémentaires fournie par Swadesh lui-même, est là pour aider à résoudre ceux qui se posent.

Mais il a semblé, puisque l'essai n'était tenté qu'à des fins de vérification, qu'on pouvait commencer par vérifier en quelque sorte la liste elle-même, c'est-à-dire par tester de façon indépendante son degré d'universalité. En particulier, il a semblé utile de partir, non pas de façon apriorique sur des bases purement conceptuelles, mais de façon concrète et empirique, du vocabulaire de base sémitique lui-même. En fait, il avait été établi pour le travail auquel il a été fait allusion précédemment, un fichier complet, par racines, de tout le vocabulaire sémitique. L'idée consistait donc à voir, au moyen de ce fichier, les termes qui apparaissent à travers le temps et l'espace, comme doués de la plus grande stabilité. Si un terme représenté, selon des correspondances phonologiques normales et avec une même signification bien déterminée, dans la majorité des groupements, l'est en même temps aussi bien dans les plus anciennement attestés: akkadien, ougaritique (c'est-à-dire, en fait, au moins deux millénaires avant l'ère chrétienne), que dans les dialectes modernes, il paraît pouvoir être pris en considération.

L'essai n'a pas permis, après diverses éliminations pour des causes très variées, de retenir plus de 116 termes, donc, et c'est là un premier fait digne d'être relevé, une liste approximativement de la même dimension que celle de 1955. Elle comprend d'ailleurs 66 termes communs avec cette dernière liste et 20 termes de la liste supplémentaire. Il a tout de même fallu y inclure 30 termes nouveaux, et cela aussi est peut-être à remarquer.

Parmi ces derniers 30 termes, certains se réfèrent à des faits socio-culturels que Swa-

desh a éliminé soigneusement à des fins d'universalité. Des termes de parenté comme "mère" et, à un moindre degré, "frère", apparaissent comme fondamentaux et généraux, de même que des noms d'objets comme "corde" ou "puits", ou d'activités sociales comme "enterrer", etc., jouissent d'une remarquable stabilité dans les langues sémitiques.

Mais ce qui mérite davantage peut-être d'être souligné, ce sont les termes qu'il a été nécessaire d'éliminer. Par leur nature même ils fournissent un nouvel exemple de l'impossibilité de déclarer stable a priori ou même relativement générale, quelque terme que ce soit.

Ainsi ce sont des quantitatifs comme *many*, des descriptifs comme *big, long, small*, des noms d'animaux, ou de parties d'animaux ou de plantes, comme *fish, feather, leaf*, des noms de parties du corps comme *nose, neck*, etc., et même le simple outil de la négation comme *not*, qui apparaissent fluides, mal individualisés, peu utilisables.

Voici la liste obtenue :

A. Termes se trouvant également dans la liste-test (1955) de Swadesh:

je	oiseau	graisse	ongle	entendre	eau
tu	chien	oeuf	pied	savoir	pluie
nous	pou	corne	genou	dormir	pietre
qui?	arbre	queue	main	mourir	feu
quoi?	semence	cheveux	ventre	tuer	nuit
tout	racine	tête	sein	venir	plein
un	écorce	oreille	coeur	assis (être)	nouveau
deux	peau	oeil	foie	debout (être)	bon
femme	chair	bouche	boire	soleil	froid
homme (<i>vir</i>)	sang	dent	manger	lune	chaud
personne	os	langue	mordre	étoile	nom

B. Termes se trouvant dans la liste supplémentaire :

quand?	aile	mère	lointain	gauche	corde
ciel	lèvre	père	proche	souffler	année
jour	intestins	sel	droite	vivant	têter
humide	lait				

C. Termes nouveaux :

gorge	enterrer	ouvrir	ivre	de (<i>ex</i>)	après
larme	pleurer	se vêtir	puits	pour	ou
doigt	malade	sortir	amer	jusqu'à	et
frère	rire	interroger	ombre	sur	rester
enfanter	prendre	rêver	maison	sous	éclair

Les termes suivants de la liste de Swadesh ont été éliminés :

this	small	see	say	burn	white
that	fish	swim	sand	path	black
not	leaf	fly	earth	mountain	round
many	feather	walk	cloud	red	dry
big	nose	lie	smoke	green	
long	neck	give	ash	yellow	

C'est donc cette liste qui a servi, concurremment à la liste même de Swadesh, de base à l'essai.

Les résultats permettent de faire quelques constatations :

1. Sur le plan du degré de parenté. La question est de savoir si l'examen des divers dialectes méridionaux au moyen de la liste diagnostique, conduit à une organisation acceptable, conforme à celle que l'étude classique a permis de proposer.

La liste a été essayée d'abord pour toutes les langues sémitiques d'Éthiopie pour lesquelles nous disposons de vocabulaires suffisamment étendus, à savoir : le guèze, le tigré, le tigrigna, l'amharique, le harari, le gouragué-čaha, l'argobba, le gafat. Malheureusement les comparaisons des langues deux à deux n'ont pas toujours été possibles pour ce qui concerne l'argobba en particulier. Pour les autres, elles ont conduit à des résultats attendus, donc positifs du point de vue de la vérification glottochronologique, sur les points suivants :

a) Niveau élevé et proximité des pourcentages : ils varient de 76 % pour l'amharique-argobba, obtenu malheureusement avec une liste incomplète, en tous cas au moins de 73 % de termes communs au tigré et au tigrigna, à 51,5 % pour le tigré-čaha, qui est d'ailleurs l'un des très rares exemples à descendre au-dessous de 60 %. Il s'agit donc, si on s'en tient aux critères rappelés par Sarah Gudschinsky dans *Word* (X, 1954, p. 326) d'une famille à cohésion relativement forte, ce qui confirme bien ce que nous savions déjà par l'étude des systèmes phonologiques et morphologiques.

b) Rapport étroit qui lie deux à deux l'amharique et l'argobba d'une part, le tigré et le tigrigna (73 %) de l'autre. Ces chiffres peuvent être considérés comme une indication également positive. En particulier, il ressort bien que le tigré et le tigrigna sont beaucoup plus proches l'un de l'autre qu'ils ne le sont de toutes les autres langues des régions plus méridionales.

c) Autre confirmation : le guèze, éthiopien ancien, apparaît très proche du tigré (83 % de termes communs) et du tigrigna (81 %), alors qu'il n'en partage que 65 % avec l'amharique et 53 % avec le gafat par exemple. Cela laisse la place à l'hypothèse classique d'un groupe éthiopien septentrional.

d) Le fait que, le groupement amharique-argobba mis à part, les pourcentages de termes communs aux diverses langues méridionales ne soient pas plus élevés que ceux des termes communs à chacune de ces langues avec les langues du Nord, est peut-être inattendu. Il est assez étonnant en particulier que l'amharique et l'argobba apparaissent plus proches du tigré et du tigrigna septentrionaux que des autres langues méridionales. On ne peut cependant y voir une indication franchement négative. En effet, bien que traditionnellement on considère les langues éthiopiennes comme constituées par deux groupes : un septentrional et un méridional, rien n'impose absolument, de façon véritablement contraignante, la réalité d'un groupe du Sud. On ne peut rejeter a priori l'hypothèse de l'existence, en face d'un groupe cohérent des dialectes septentrionaux, de plusieurs ensembles méridionaux. Mais que l'un de ces ensembles qui contiendrait l'amharique et l'argobba, soit plus proche des langues

du nord que du harari ou du čaha méridionaux, jusqu'à présent ni l'analyse phonologique ni la comparaison des systèmes grammaticaux n'avaient conduit à l'affirmer, et il semble bien qu'il y ait là une certaine difficulté.

Mais en tout état de cause, le problème le plus grave pour cette expérience n'est pas interne au domaine éthiopien. Il concerne l'organisation même de l'ensemble sud-sémitique.

Classiquement, et sur la base d'évidences de toutes sortes, l'éthiopien est considéré comme une projection historique du sudarabique sur le continent africain. La plupart de ses dialectes ne seraient donc que le produit de différenciations secondaires, et d'ailleurs récentes (au cours du Ier millénaire avant l'ère chrétienne) sur le terrain même. Or, comparée aux listes équivalentes de deux langues éthiopiennes, une liste soqotri (sudarabique moderne) se révèle partager 46% de termes communs avec le tigré, mais seulement 34,5% avec l'amharique. La différence est considérable et apparemment inexplicable dans le cadre des hypothèses actuellement prévalentes.

Chose plus inattendue encore: les rapports entre ce même dialecte sudarabique, le soqotri, et l'arabe n'apparaissent pas plus lointains du point de vue lexicostatistique. Ils sont même beaucoup moins lointains, si c'est aux 34,5% de termes communs avec le tigré qu'on oppose les 44% qu'il partage avec le dialecte arabe du Caire ou les 39,5% avec celui de San'a du Yemen.

Du point de vue lexicostatistique il a donc été impossible, dans les limites de cette expérience, de retrouver l'organisation classique qui pose une unité sudarabique-éthiopienne en face de l'arabe. On peut naturellement invoquer, comme facteur de trouble, l'ambiance arabe dans laquelle a évolué le sudarabique depuis 12 ou 13 siècles. Cependant un examen du détail des termes, et c'est très frappant, n'a pas permis de déceler une telle influence en ce qui concerne les termes constituant la liste diagnostique.

Donc sur ce premier plan du degré de parenté, essai positif dans l'ensemble pour ce qui concerne les langues éthiopiennes (avec réserves pour les positions respectives de l'amharique et des langues du Nord), négatif pour ce qui concerne l'organisation générale du sud-sémitique.

2) Du point de vue des datations proprement dites. Le sémitique méridional offre également avec les langues éthiopiennes un cas de contrôle assez intéressant. Un état relativement ancien d'un dialecte nous est connu par le guèze qui fut la langue du royaume d'Axoum, et qui ne survit aujourd'hui que dans des usages liturgiques (et, très partiellement, semble-t-il, littéraires) reflétant en gros un état de langue figé à situer sans doute dans les environs du VIIIe siècle. Or, sur l'aire même où le guèze fut vivant, existent aujourd'hui les deux langues septentrionales qui en sont les descendantes: tigré au Nord, tigrigna au Sud. Lorsqu'on compare du tigré ou du tigrigna parlés aujourd'hui à du guèze, c'est donc à quelque 12 siècles qu'il faut estimer la profondeur de temps dont on remonte dans l'évolution d'une même langue.

Pour ces 1200 ans, le rapport entre le guèze et le tigré est, avec la liste spécifique déterminée empiriquement, de 83%. Elle est de 81% pour le guèze-tigrigna. Soit, com-

me taux de rétention (r): 85,5% si on part de la première computation, 84% si on part de la seconde.

L'expérience a été faite parallèlement au moyen des listes Swadesh de 1955. Les résultats ont été de 71 et 69%, correspondant respectivement à des taux de 75 et 73,5%.

Il apparaît donc là quelque chose qu'on ne sait trop comment interpréter, qui est en même temps, selon l'angle où on veut le considérer, comme une confirmation ou comme une infirmation. Infirmation dans ce sens que la liste de Swadesh conduit à un taux relativement bas, et qui, sur le plan chronologique, aboutit à des évaluations difficiles à admettre et à organiser. Confirmation dans ce sens que la liste spécifique suggère tout de même un taux de rétention très proche de celui qui est admis par Morris Swadesh. Mais naturellement, avec ce taux, les indications chronologiques ne peuvent que refléter les imprécisions et les chevauchements qui ont déjà été signalées dans le tableau.

Le deuxième essai de vérification sur le plan chronologique a été tenté sur l'arabe et ses dialectes. On a pris en considération cinq dialectes très éloignés, disséminés à travers tout le domaine arabe, à savoir: le san'ani, le cairote, le maltais, le tunisois et le hassane de Mauritanie. Sauf pour le san'ani, pour lequel on s'est basé sur le glossaire de Ettore Rossi, les autres dialectes ont fait l'objet d'enquêtes directes.

Ce qu'il nous a semblé possible d'essayer, en l'absence de documents vraiment anciens sur chacun de ces dialectes, c'est de prendre en considération les dates de séparation qui nous sont connues en gros, pour en tirer une suggestion sur le taux de rétention utilisable.

La considération du maltais en premier lieu peut être intéressante. On a là un terminus possible. Malte est conquise par les Arabes en 870. Elle est arabisée en deux siècles, puis se sépare définitivement du reste du domaine arabe à l'extrême fin du XIe siècle (1090). On peut admettre qu'à partir de cette époque, absorbé en quelque sorte par l'Europe chrétienne, soumis à une influence exclusivement romane, le maltais a évolué en toute indépendance des autres dialectes arabes. Par ailleurs, il y a des raisons de penser que ce maltais est originellement très proche du dialecte de Tunis. Or la comparaison des vocabulaires de base, au moyen de la liste diagnostique, fait ressortir 80% de termes communs. Ce qui signifierait, si on prenait pour point de divergence la date même de la conquête chrétienne, un taux élevé de 88%. Il faut souligner cependant que placer la divergence à la fin du XIe siècle, c'est se donner un point de départ vraisemblablement trop rapproché, et qu'à tout prendre nous devons être assez près ici des 86% de Swadesh.

L'autre possibilité de vérification est donnée par l'ensemble des dialectes considérés, dont on peut situer le point de divergence, d'une façon générale, vers le IXe siècle, au moment de la dissolution de l'empire abbasside. Or la comparaison des listes deux à deux permet de constater que, mis à part le rapport particulièrement étroit maltais-tunisois, les pourcentages sont relativement proches l'un de l'autre, s'ordonnant entre les limites de 69 à 75%, ce qui induit également à trouver vraisemblable le taux moyen de 86%, si on veut bien faire partir la divergence d'un millénaire en moyenne.

Naturellement il n'y a pas là, et il ne pouvait pas y avoir, d'indications précises. Les dates de séparation admises sont dans une large mesure hypothétiques. Mais elles le sont toujours, puisque dans tous les cas, parler de point de divergence c'est se référer aux schémas de l'arbre généalogique auquel, je crois, on ne reconnaît ici qu'une valeur heuristique.

Mais dans la mesure où il s'agissait, en se plaçant sur le terrain même de la glottochronologie, de vérifier concrètement dans un groupe de langues bien définies, la recevabilité de certaines de ses propositions, il semble ici précisément, pour ce qui concerne les dialectes arabes, que rien n'y contraint de rejeter pour le taux de rétention, la moyenne de 86% par millénaire proposée par Swadesh.

Et ceci nous conduit à quelques conclusions toutes provisoires d'ailleurs, des observations plutôt que des conclusions.

La première, c'est donc que, pour autant qu'on ait pu le vérifier pour deux ensembles, les remplacements des termes dans une liste diagnostique de base, se sont opérés au cours du millénaire considéré selon un taux variable suivant les langues, mais qui ne s'éloigne guère, semble-t-il, du taux de 86%.

La deuxième remarque que suppose d'ailleurs la première, est que, malgré les difficultés signalées, il semble bien ressortir de l'examen des langues sémitiques, qu'il est possible de trouver un vocabulaire de base qui reflète grossièrement le degré de parenté des langues entre elles, c'est-à-dire de présenter une image chiffrable des rapports que l'analyse des autres structures permet de dégager. Mais les difficultés signalées dans les rapports entre certaines langues ne permettent pas d'affirmer que pour le sémitique méridional tout au moins cette image soit d'une grande précision. Ce qui paraît avec clarté au contraire, et qui constitue une réserve à l'égard de l'hypothèse glottochronologique, c'est que la liste diagnostique n'est pas universelle, qu'elle n'est pas indépendante des langues et des familles de langues considérées, et qu'elle devrait être établie dans chaque cas de façon spécifique. Cela entraîne la conséquence que la constance relative du taux de rétention à travers l'espace n'apparaît maintenue que grâce à la variabilité de la liste diagnostique.

Mais en outre pour ce qui concerne cette constance du taux, il est nécessaire de souligner également qu'elle ne s'est manifestée de façon relativement nette que pour le millénaire considéré. Dans les calculs basés sur ce taux, on a eu pour les langues dont on peut estimer la divergence à une période comprise entre un et deux millénaires, des résultats fluides, vraisemblables dans l'ensemble, mais peu précis, avec des chevauchements difficiles à expliquer, et qui au moins dans un cas, celui des rapports de l'amharique avec les langues du Nord, ne semblent pas refléter rigoureusement les réalités qu'on croit apercevoir.

Et quant au seul exemple où il s'agit d'une période de plus de deux millénaires, celui des rapports du sudarabique avec l'arabe d'une part, avec l'éthiopien de l'autre, on ne voit pas comment interpréter sainement les évaluations obtenues.

LE FAIT DICTIONNAIRE

MARCEL COHEN

C'est un fait linguistique et un fait sociologique. L'histoire de cet instrument intellectuel, notre serviteur et notre tyran, devrait être contée. La question est celle-ci : à quel moment, dans quelles civilisations a-t-on éprouvé le besoin d'avoir sous la main un répertoire complet des mots de la langue? La réponse, pour les langues de l'Europe occidentale est : dix-septième siècle. Avant de parler de cet événement, de ses caractères et de ses développements, déplaçons-nous dans l'espace et le temps.

Les premières civilisations à écriture que nous connaissons sont celles de l'Égypte et de la Mésopotamie, avec des documents depuis —3500 environ.

En Mésopotamie on connaît au 2^{ème} millénaire de courts documents bilingues, akkadien et sumérien, fournissant plus des paradigmes de verbes que des vocabulaires ; on a quelques fragments d'autres vocabulaires bilingues ou trilingues : la multiplicité des langues était évidemment une question importante pour les empires de cette région. On a aussi quelques formulaires fournissant du vocabulaire juridique.

Parmi les papyrus égyptiens on a retrouvé, datant de —1750 environ, une liste d'environ 300 mots, rangés, par matières. Un autre document, datant d'environ —1000, qui a dû être répandu puisque nous en avons plusieurs exemplaires, contient plus de 600 mots, aussi répartis par matières. Le besoin de répertoires était donc satisfait, pour des scribes au moins, dans une certaine mesure.

Les plus anciens usagers de l'écriture sont ensuite les Chinois. On a des documents depuis —1350 environ (très postérieurement à l'invention présumée de l'écriture), mais ce n'est que vers +100 qu'on trouve des premières listes de caractères, qui sont en même temps des listes de mots, au nombre modeste de 9353. C'est seulement au 6^{ème} siècle qu'apparaît un dictionnaire, encore incomplet, de 24000 signes. Le sous-continent indien a été le domaine d'une grande activité littéraire depuis —1000 sans doute ; il est resté celui d'un intense enseignement oral. Les textes religieux ont dû être mis par écrit vers —500, dès lors scrutés par les philologues. C'est seulement en +400 environ qu'on rencontre un grand dictionnaire destiné à fournir le vocabulaire du sanskrit classique à des lettrés assez nombreux de langues maternelles diverses : ce n'est pas un ouvrage à feuilleter, mais à apprendre par cœur, en 1500 stances versifiées, contenant tout le nécessaire classé par matières.

Restant encore en Orient, je donne ici une trop brève mention à la lexicographie arabe, qui s'est développée en pays arabisé par la conquête : après un ouvrage pré-

curseur au 8ème siècle, c'est du 11ème siècle que datent des dictionnaires monolingues de grand intérêt.

Revenons à l'Occident et d'abord aux anciens, grecs et latins. Ils n'avaient pas de dictionnaires ni pour s'instruire, ni pour s'aider à réaliser leurs œuvres, ni les grands tragiques, ni Platon, ni Démosthène, ni Cicéron, ni Virgile. En particulier quand toute l'élite latine a appris le grec, c'était sans dictionnaires (on ne cite que quelques glossaires juridiques bilingues).

Pourtant dans cette foisonnante activité intellectuelle d'Athènes, Alexandrie, Rome et autres centres, la place de la lexicographie n'était pas vide. Seulement ce n'était pas sous la forme moderne: il s'agissait dans l'ensemble de gloses pour des explications de textes ou des listes de mots remarquables par leur rareté ou leur caractère étranger. Il y a eu aussi des grandes études de vocabulaire par matières. D'autre part les grands ouvrages de sciences particulières ou de plusieurs sciences à la fois, s'ils n'avaient pas d'index, étaient des réservoirs de termes variés. Notons dès ce moment qu'on ne peut pas dissocier l'histoire des dictionnaires de celle des encyclopédies; il faut penser à leurs conjonctions comme à leurs oppositions. Du côté grec on peut citer, vers + 100 le lexique homérique d'Apollonios le Sophiste, avec un certain nombre d'apax; au 1er ou 2ème siècle l'ouvrage d'Harpokration, sur les expressions particulières des dix grands orateurs; Hesukhios d'Alexandrie vers 400 avec sa précieuse collection de termes étrangers. Noter que le terme *lexikon* est du 5ème siècle.

Les Latins employaient le terme grec de *glossographie*. Du savant Marcus Terentius Varro (±116-27) on a une importante part de son ouvrage *De lingua latina*. C'est en grande partie un dictionnaire des choses et des noms propres, mis par catégories, avec une préoccupation d'étymologie. Le livre V qui contient diverses choses sous la rubrique générale de l'espace fournit environ 800 mots, en général des substantifs. D'un lexicographe du 1er siècle, Verrius Flaccus, nous avons des parties du *de significatione verborum* abrégées par Festus (2ème ou 3ème siècle) et Paul Diacre au 8ème siècle: c'est une liste assez abondante, rangée alphabétiquement, de mots plus ou moins curieux, en partie comme archaïsmes. (Les verbes sont cités à l'infinitif, à moins qu'ils ne soient cités avec un contexte les donnant sous une autre forme.)

Ajoutons ici, pour faire la jonction avec la suite, le souvent cité Isidore, évêque de Séville, mort en 636, qui cataloguait les raretés, non les mots courants.

Dans l'époque suivante où le latin n'est plus parlé comme langue maternelle, mais est la langue des universités, il a été enseigné pendant des siècles, à peu près sans lexiques, puis avec de modestes lexiques latins, sans traduction. Jusque vers 1050 on ne trouve que quelques gloses explicatives. Au milieu du 11ème siècle, un premier lexique dû à Papia ou Papius répondait si bien à un besoin qu'après avoir été maintes fois copié il a été finalement imprimé en 1476. (Noter que les verbes sont enregistrés, comme encore dans nos dictionnaires du latin, à la 1ère personne du présent de l'indicatif.) Dans l'ouvrage qui vient ensuite, d'un Italien appelé en Latin Ugutio ou Hugutio, mort en 1212, les verbes sont aussi à l'indicatif, mais ils sont à l'infinitif dans les explications (comme en français dans un dictionnaire latin français). C'est à la fin du 13ème

siècle que se situe un ouvrage d'un autre Italien intitulé *Catholicon* ou *Summa* ou *Prosodia* dont une des parties est le premier lexique latin abondant et rigoureusement alphabétique – qui a eu l'honneur de l'impression en 1460.

Le latin perdant décidément du terrain, le 14^{ème} siècle a vu paraître les premiers petits lexiques latins-français. Le principal, que certains appellent "adaptation française du *catholicon* latin" comporte un enseignement grammatical, les verbes latins étant donnés avec l'indicatif aux deux premières personnes, puis au parfait et à l'infinitif et au supin, les substantifs ayant nominatif et génitif. Cet ouvrage aussi a été imprimé à la fin du 15^{ème} siècle.

Le temps de l'épanouissement de la grande érudition laïque de la Renaissance et de l'essor de l'imprimerie a été aussi celui de l'éclosion véritable des dictionnaires.

Deux aspects :

1. Dictionnaires d'une seule langue : les deux langues classiques ont reçu les répertoires complets, reflets de leurs littératures, qu'elles n'avaient pas eues étant parlées : le savant et imprimeur Robert Estienne a publié en 1532 le *Thesaurus linguae latinae* (les verbes y sont d'abord à la 1^{ère} personne du présent de l'indicatif, ensuite à la même personne du parfait, ensuite viennent le supin et l'infinitif. Les explications, en latin elles mêmes, sont à l'infinitif). Henri Estienne, fils du précédent, a publié le *Θησαυρὸς τῆς ἑλληνικῆς γλώσσης Thesaurus graecae linguae* en 1572. (Les verbes sont rangés à la 1^{ère} personne de l'indicatif présent, la traduction en latin est donnée à la même personne.)

2. Dictionnaires en plus d'une langue. Le savant italien Calepino a inauguré dès 1502 les dictionnaires étendus, mais sommaires dans la rédaction, de plusieurs langues en parallèle ; on a eu ainsi à la fois : latin, grec, français, italien, espagnol. Les éditions poursuivies par des continuateurs jusqu'au 17^{ème} siècle ont atteint onze langues. Le titre était le terme nouveau *dictionarium*.

Fait sans doute plus important, marquant une nouvelle orientation des études, Robert Estienne a publié dans la même année 1539 un *Dictionarium latino-gallicum* et un *Dictionnaire français-latin*. C'est l'apparition, du même coup, du terme nouveau (*dictionnaire*) en français. Dans le premier, les verbes latins sont donnés comme dans le *Thesaurus*, la traduction française étant à l'infinitif ; dans le second les verbes français sont à l'infinitif et traduits par l'infinitif latin.

Dès sa première édition, ce dictionnaire français-latin était beaucoup plus qu'un glossaire sommaire. Il donnait une certaine idée des ressources du français, de plus en plus écrit à cette époque pour toutes matières et substitué au latin en matière judiciaire par l'ordonnance de Villers-Colterets de la même année 1539.

La suite de l'histoire est un peu compliquée ; Robert Estienne étant mort en 1559, une nouvelle édition du dictionnaire a paru avec le même titre, sous son nom, avec mention de Jean Nicot comme collaborateur partiel. Le texte français est considérablement développé, la partie latine réduite. En 1606, l'ouvrage a reparu avec la signature de Jean Nicot et le titre de *Thrésor de la langue française tant ancienne que moderne*, sans mention du latin, qui continue à figurer.

Donc, le premier dictionnaire français n'est pas le *Thésor* de 1606, mais le Robert Estienne de 1573; les deux avec le latin, ce qu'on oublie généralement pour le *Thésor*.

Quelle a été la diffusion de l'un et de l'autre ouvrage; dans quelle mesure Corneille, Melle de Scudéry et leurs contemporains s'en sont-ils servis?

En dehors de France, il faut noter que l'Espagne a reçu son dictionnaire monolingue en 1611 par initiative privée: Covarrubias *Tesoro de lengua Castellana o española*, alors que l'académie espagnole a traîné plus d'un siècle pour éditer son dictionnaire en 1726-1739. L'Italie prenant de l'avance, le dictionnaire longuement mûri de l'Accademia della Crusca est venu au jour en 1612, et devait avoir de nombreuses rééditions.

L'Angleterre, assez active au 16^{ème} et au début du 17^{ème} siècle pour les dictionnaires de plus d'une langue, n'avait pas encore son 'trésor' au 17^{ème} siècle (Dictionnaire de Nathan Bailey en 1721).

Pour suivre l'histoire du dictionnaire français il va nous falloir sauter à 1680. Néanmoins il faut mentionner les discussions sur les mots, dont la littérature et les *Remarques* de Vaugelas et autres nous fournissent l'écho, mentionnons aussi les *Curiositez françoises* d'Antoine Oudin (1640) ainsi que le livre à titre ambitieux *Dictionnaire servant de bibliothèque universelle* de Paul Boyer (1649) qui est surtout un dictionnaire de rimes, et remarquons d'autre part que la librairie ne restait pas inactive, fournissant au public divers dictionnaires bilingues, dont certains importants pour le français, comme l'*Inventaire des deux langues*, du Père Monet (1635).

C'est donc en plein temps du classicisme qu'a paru enfin un premier gros dictionnaire du français sans latin, sous la signature de Pierre-César Richelet, qu'on sait avoir consulté divers collabotateurs. Dictionnaire de langue, en un seul gros volume, non encyclopédique. En 1690 est venu l'ouvrage d'Antoine Furetière *Dictionnaire universel, contenant généralement tous les mots françois tant vieulx que modernes et les Termes de toutes les Sciences et les Arts*, en deux grands volumes: c'est donc le premier dictionnaire encyclopédique français. En 1694, après diverses mésaventures, a paru le *Dictionnaire de l'Académie françoise* en deux volumes, seulement dictionnaire de langue puriste excluant les mots bas ou techniques, rangés, d'une manière innovante et heureuse par familles, avec rappels aux endroits voulus pour l'ordre alphabétique. Pour des raisons dont il faudrait faire l'histoire, l'Académie est revenue à l'ordre alphabétique servile dans les éditions postérieures. La même année paraissait sous les initiales MDC, couvrant Thomas Corneille, un *Dictionnaire des Arts et des Sciences* aussi en deux volumes, qui recueillait un certain nombre des termes écartés du dictionnaire signé de l'Académie entière, réédité du 18^{ème} siècle.

Le *Dictionnaire universel historique et géographique* du même auteur ne devait paraître qu'en 1708, ce qui donnait toute satisfaction à l'esprit encyclopédique du côté académique, dans le temps où paraissait d'autre part le *Dictionnaire historique et critique* de Pierre Bayle (6 volumes de 1695 à 1697), annonciateur de la grande Encyclopédie.

On peut donc supposer qu'à la fin du 17^{ème} siècle tout Français cultivé était armé d'un dictionnaire.

Il serait intéressant de suivre dans le détail les publications au cours du 18^{ème} siècle.

Du côté dictionnaire de langue, rééditions successives de génération en génération, du dictionnaire de l'Académie (avec profonde réforme orthographique en 1740). Du côté encyclopédique l'œuvre de Furetière fournissait la base du *Dictionnaire universel français et latin*, entreprise d'inspiration jésuite, avec la réintroduction du latin à Trévoux, dans la principauté de Dombes, d'où le nom de dictionnaire de Trévoux, généralement donné à l'ouvrage: première édition en 1704; il devait y en avoir une en 1771 et l'ouvrage a été signalé comme encore en usage en 1842.

Entre temps la grande Encyclopédie de d'Alembert et Diderot (écho d'une entreprise analogue en Angleterre) avait commencé à paraître en 1751. Un côté de la publication était spécialement important pour la suite: l'abondance des planches soigneusement exécutées.

Au point de vue qui nous occupe, qui est essentiellement la place du dictionnaire dans notre vie intellectuelle, il est nécessaire de donner au moins un aperçu rapide des publications du 19^{ème} et du 20^{ème} siècles. Tout d'abord les éditions continuées du dictionnaire de l'Académie (dernière en 1931-1935). Il faut y ajouter le gros complément extra-académique dont une première édition a paru en 1842.

Boiste, *Dictionnaire Universel*, 1801, éditions renouvelées jusque 1857 au moins.
Encyclopédie des gens du monde, 1833-1845, 22 volumes.

Encyclopédie du XIX^{ème} siècle, 1836-1859.

N. Landais, *Dictionnaire général et grammatical*, 1839-1843.

Louis Bescherelle, *Dictionnaire national encyclopédique*, 1845-1846 [encore en usage de nos jours].

Encyclopédie moderne (L. Renier), 1846-1851.

Dictionnaire de la conversation et de la lecture, 1851-1858 (16 volumes, et 5 supplémentaires).

Poitevin, *Dictionnaire de la langue française*, 1851

Emile Littré, *Dictionnaire de la langue française*, de 1859 à 1872, avec supplément en 1878 (Grand recueil d'exemples des 17^{ème} et 18^{ème} siècles, et aussi des siècles précédents).

Pierre Larousse, *Grand dictionnaire universel du XIX^e siècle*, en 17 volumes sans illustrations, 1866-1876.

Grande encyclopédie dirigée par Marcellius Berthelot, 1886-1902.

Larive et Fleury, *Dictionnaire français encyclopédique*, 1887-1889 (illustré).

Petit Larive et Fleury, 1901.

Dictionnaire général de la langue française (Hatzfeld, Darmesteter, Thomas), 1890-1895.

Nouveau Larousse illustré, 1889.

Petit Larousse illustré, 1908, bizarrement divisé en une partie *langue*, qui comprend les sciences en général, et une partie *lettres, sciences et arts*, qui donne l'histoire (avec l'histoire de la littérature) et la géographie. A eu et a encore divers concurrents négligés ici.

Dictionnaire Robert à partir de 1953, nouveau dictionnaire d'exemples, prolongeant le Littré doit être terminé en 1963.

Un *Trésor de la langue française*, avec des crédits officiels, est maintenant en vue d'une réalisation.

Le dictionnaire, œuvre humaine peu à peu élaborée, a pris des caractères déterminés, où des routines se sont plus ou moins indurées. Il est devenu une puissance plus ou moins redoutable qui parfois peut nuire à l'intelligence au lieu de la servir.

Il convient de voir en quoi un dictionnaire tyran plus ou moins mal fait peut avoir des inconvénients graves pour l'enseignement, et rechercher les moyens à employer pour y échapper. A cet effet il faut d'abord bien en connaître les rouages.

Le dictionnaire impose le mot, alors que nous parlons par phrases. Il fournit les verbes à l'infinitif, c'est à dire sous un aspect abstrait, qui se présente dans l'usage beaucoup moins souvent que l'ensemble des formes conjuguées. De même le substantif est présenté à la forme supposée fondamentale, au singulier, sans accompagnement d'un article ou équivalent. Cet exercice constant d'abstraction, contraire à l'usage courant, conforme à un des aspects de l'analyse réfléchie de la langue, est-il bon ou mauvais tant pour les enfants que pour les adultes? La question est à poser. Il y a une résistance pédagogique du côté de ceux qui présentent pour l'enseignement des listes de mots dans de petites phrases, ou au moins accompagnés d'articles et pourvus de flexions, autant que possible avec des images. Il y a des dictionnaires en images. Il y a aussi des répertoires de concepts.

Ceci pose la question de l'ordre alphabétique. C'est assurément un appareil pratique pour les recherches, à condition de commencer par apprendre l'ordre d'une trentaine de caractères qu'on s'exerce à tracer. Mais il vaut la peine qu'on y réfléchisse. Cet ordre existait déjà vers -1500, comme l'ont montré les trouvailles sur le site d'Ougarit, sans qu'on en connaisse les causes. Résidaient-elles dans des formes de caractères? Il est sûr en tout cas que l'ordre traditionnel ne répond à aucun classement phonétique, comme celui qu'ont opéré les grammairiens de l'Inde. Il en résulte que la perpétuation de cet ordre est une invitation permanente à ne pas prendre conscience des articulations et de leurs équilibres. Le remède ne pourrait venir que d'un profond bouleversement.

L'ordre alphabétique passé à l'état de seconde nature mécanisée a des conséquences qui atteignent le comique. C'est ainsi que les dictionnaires français arrivent à de grandes bizarreries quand il s'agit de *saint*, composante de noms propres. Dans un dictionnaire encyclopédique sans séparations on trouve: Saintes (nom de ville), Saint Esprit sainteté, Saint-Evremont; dans un dictionnaire qui isole l'histoire et la géographie: Saintes, Saint-esprit (ordre du), Saint-Estèphe, Sainte-Suzanne, Saint-Etienne. Le remède ici serait facile, pour un dictionnaire intelligent: mettre à part les *Saint*, les *Sainte*, les *Saints*, les *Saintes*, isoler les noms de lieux et les noms de personne. Autre histoire: l'habitude s'est introduite de cataloguer les noms de peuples au pluriel, de sorte qu'il faut chercher *Huns* après *hune*, *Hunéric*, *hunier*, *Huningue*, *hunnique*: il semble qu'il serait aisé de rompre cette routine et de mettre *Hun* à sa place. Il est plus grave de voir, surtout dans les dictionnaires français, démembrer les familles de mots ou au

moins en cas de groupement les disposer dans des ordres ridicules; ainsi on trouve *plombage* et *plombé* entre *plomb* et *plomber*, *pleuvoir* et *pluie* à quatre colonnes de distance: c'est à dire qu'on ne peut pas se servir du dictionnaire pour enseigner la composition du vocabulaire. Il est assez simple de regrouper les familles avec le mot principal, verbe ou nom en tête et de mettre un rappel dans l'ordre alphabétique pour les mots qui n'y sont pas rangés.

L'habitude s'est installée de faire suivre chaque mot d'une définition, et il y a une certaine émulation pour les définitions élégantes par leur concision. Très souvent elles n'expliquent pas de quoi il s'agit. Ainsi *apposer* "appliquer, mettre". Ce n'est qu'ensuite qu'on introduit un exemple, si on en met un. Ici nous avons à citer un grand méfait de ce style dictionnaire visant essentiellement à la compression. C'est la néfaste invention des *tronçons*, ou bouts de phrase incomplets. Ainsi, comme soi-disant exemple pour *apposer*, on trouvera *apposer une affiche*.

Un dictionnaire sain devrait présenter d'abord un exemple, dans une phrase complète, ensuite une explication, aussi sous forme non tronquée, même si elle est brève. Ainsi: "Le garde-champêtre a apposé une affiche sur le panneau devant la mairie" (Il l'a appliquée en la collant).

Le style dictionnaire se révèle aussi dans l'art économique de faire entrer le plus de notions possibles dans la même phrase, ceci particulièrement dans les articles historiques: *Campanella* Astrologue, il passa la plus grande partie de sa vie en prison. *Balzac* [Romancier français] écrivit aussi des contes et des pièces de théâtre. Il mourut accablé de travail et de dettes.

Instruments indispensables de la vie moderne, les dictionnaires ne doivent pas s'ankyloser dans de vieux errements; il ne faut pas que ceux qui naissent soient inhibés par l'imitation superstitieuse de leurs prédécesseurs.

Il sera toujours difficile de faire un bon dictionnaire non seulement bien conçu mais soigné dans tous ses détails comme le commande le respect du public qui accorde sa confiance à ce qui lui est offert. Une condition nécessaire pour la réussite est que, ce qui n'est presque jamais le cas, les lexicographes disposent du temps et des ressources indispensables pour leur belle tâche.

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TRANSLATION WITHOUT MACHINE

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If the human organism is viewed as a machine, then all translation is machine translation, though obviously that will be so only in a trivial sense. In the present paper I propose to consider those aspects of translation which are not likely to be taken care of in the near future by non-human machines. Most of these factors, to be sure, have already been considered by speakers at this Congress and writers on the translation problem in recent years or even in the distant past. The point of my paper is to present them with a subjective estimate of their remoteness from or propinquity to machine translation.

The process of translation may be viewed as a space of many dimensions in each of which a translation is good, indifferent, or bad. Since many of the factors will conflict, the total result should be a somewhat complicated function of them. It is indeed premature even to speak of functions, since these dimensions are still pre-systematic, undefined notions, rather than measured quantities. For my present purposes, I shall only consider the various dimensions separately, without attempting to set up any overall evaluation of translation in general or of actual translations in particular. I shall consider in turn 1. Physical makeup of the text; 2. Size of unit to be translated; 3. Style; 4. Grammatical Structure; 5. Subject Matter and cultural categories; and 6. Pragmatics of translation.

1. PHYSICAL MAKE-UP OF THE TEXT

Most translations have to do with written into written text between different languages. However, it may be useful to take a broader perspective by considering other physical forms of texts. In the work of interpreters, the "text" is in the form of live speech. As is well known, the work of written translation and that of the so-called simultaneous translation at the United Nations call for very different kinds of skills and belong to quite different parts of that organization. For good, theoretical reasons, modern linguistics have taken spoken sounds as the proper study of language, but for practical and equally good reasons, machine translation has so far chiefly concerned itself with visual messages for both the original language and the target language, and thus brings back to a more respectable status the expression "written

language" which we linguists have been looking down upon. However, now that so much work is being done on automatic speech recognition at the acoustic input end as well as on speech synthesis at the acoustic output end, a three stage machine translation from speech to speech will not be many years farther off than graph to graph machine translation. As a rough guess, I would venture to suggest the stages of progress somewhat as follows: (1) before 1965, translation without machine for both writing and speech; (2) translation with machine for writing will begin some time between 1965 and 1970, but still without machine for speech; (3) translation for both speech and writing will begin some time between 1970 and 1975. It may be noted in passing that at stage (3), it may not be necessary to go through the ordinary orthographies of the languages involved, which have to be coded anyway when going through the machine, but may be by-passed through phonemes or their coded equivalents. This will be the case especially with a language in which the writing system is in larger units than phonemes.

2. SIZE OF UNIT TO BE TRANSLATED

The piece to be translated may be of the size of a book, a play, an article, a lecture, a poem, a letter, or a speech, each as a more or less complete piece of discourse, for which there is usually a best translation in a target language. But even here one may have to go beyond the text (cf. 6 below) to decide upon a translation; in fact, otherwise there would be no philology. A unit of the size of a sentence may admit of more than one possible translation and it may need either the linguistic or the situational context to determine in what way the original is to be understood and translated. Such determination, if needed, will still to a large extent be a non-machine part of the translation under present conditions.

When we come down to the sizes of phrases, words, and morphemes, then the absence of one-to-one correspondence between languages becomes even more of a problem, as can be verified by opening any bilingual dictionary. Much of present day research in machine translation, as you are aware, consists in compiling and coding for machine operation units of these sizes, especially at the word level,¹ and in finding automatic ways of decisions on multiple choice by scanning over as little context as possible and as much context as necessary.

Finally, when we come to the size of phonemes, then translation between different languages is reduced to a vacuous case with a correlation of almost zero. In other words, any phoneme in one language translates into any phoneme in another language and the conditions of correspondence will have to be determined by factors from the morpheme level up. The same can be said of distinctive features, if we go beyond

¹ Morris Swadesh puts it more broadly thus: "the unit of translation ... corresponds to the linguist's structural unit", in his article "On the Unit of Translation", *Anthropological Linguistics* 2.2.40 of 39-42 (February 1960).

phonemes to distinctive features. I said that the correlation was "almost zero" because when one translates poetry and song, or wit and humor, even phonetic comparability becomes relevant.

Suprasegmental morphemes (not counting phonemic tones), however, are more translatable, like other morphemes. They are more amenable to machine treatment in that some of them approach cases of language universals, such as rising or higher pitch for suspense and falling or lower pitch for conclusion;² on the other hand, they are less amenable to machine translation or even non-machine translation, since they usually are not written at all in conventional orthography and are thus literally lost sight of and neglected, even though they may form an essential part of the message.

3. STYLE

Comparability of style between the original and the target language is of course an important desideratum. If possible, one wants to translate prose into prose, poetry into poetry, archaic into archaic diction, colloquial into colloquial, and slang into slang.³ On the whole, since machine translation is now fully occupied with problems of multiple choice in lexical units and with rendering of syntactical and morphological interchange, one will have to leave to non-machine translation to take care of most of the problems of comparability of style.

However, one aspect of style, that of the frequency of occurrence of items, seems to permit quantitative treatment and thus partial machine treatment. Everyone is familiar with the special effect produced when a phrase or sentence in one language is rendered word for word into another. Assuming that matters of grammar and vocabulary have been taken care of, the disparity of frequency of occurrence of the correspondence will make the effect either fresh and interesting, or dull and flat, or strange and bizarre, or even unintelligible. I do not say that rendering items into those of comparable frequency of occurrence will necessarily result in a good translation, but its total disregard will tend to contribute to disparity of style. It may, therefore, be useful for dictionary entries, for translation purposes, to contain not only meaning and function, and perhaps style (as some dictionaries already do: *Slang*, *Arch.*, etc.), but also the frequency of occurrence.⁴

Related to this factor of frequency is the size of units considered in the preceding discussion, especially at the level of the phoneme and the syllable. Roughly speaking, the variety of kinds of units is a decreasing function of the number of units (i.e. size) needed to carry a given amount of information. For example, with a small inventory

² See Dwight L. Bolinger, "Intonation as a Universal", p. 833 of this volume.

³ On problems of style see for example J. P. Postgate, *Translation and Translations* (London, 1922); Ronald Knox, *Trials of a Translator* (New York, 1949).

⁴ The only dictionary I know of which has that is C. H. Fenn's *The Five Thousand Dictionary*, 5th ed. (Peking, 1940), and Amer. ed. (Cambridge, 1942), in which entries are graded approximately by frequency, for teaching purposes, from A to K.

of phonemes and syllables in Japanese, it takes more syllables to make a morpheme than in languages with a larger inventory. But since length and syllabicity are factors of style, especially in poetry, any disparity in this respect between two languages will complicate the translation between them. That is the reason why European translators of classical Chinese poetry have had to use two or three times as many syllables as the original in order to get in all the original message, and that was also why I had a much easier time of it when I tried to follow the same meters and rhymes in translating Lewis Carroll, because the syllabicity in modern colloquial Chinese is more nearly comparable to that of English.

4. GRAMMATICAL STRUCTURE

The grammatical structure of the languages in translation has been the concern of workers on machine translation even to a greater degree than the problem of vocabulary. The treatment of obligatory items and categories have occupied the attention of all translators,⁵ human or other. One often has to choose between overtranslation, as when an inflection is translated as a full word⁶ and undertranslation, as when the machine is instructed to "throw out article" or "suppress plural". In common practice one tends to run the risk of overtranslation in order not to lose anything in the original message, though comparability in style will put a limit on that.

One important question with regard to structure is at what level one should set up the equivalences. "Most frequently," as Jakobson says, "translation from one language into another substitutes messages in one language not for separate code-units but for entire messages in some other language."⁷ But most of machine translation at the present stage has to be concerned with starting with constructions of a certain type and ending with a similar or a different but regular type in the other language, such as postposed modifying clauses into preposed modifying clauses in a target language which does not allow postposed modifiers. But certain cases of non-correspondence, or at least complicated patterns of correspondence will, at the present stage of the science, have to be left to non-machine translation. I have in mind such cases where one language has one form of structure, say S-V-O, and the other language has a similar structure, for certain instances, but a different structure for other instances, conditioned by non-structural but lexical factors.

A common distinction is often made between literal or word-for-word translation and idiomatic or free translation. But there are more than just two degrees on the scale of literalness and idiomaticity. If we go below the level of the word, there can also be

⁵ For example, Robert E. Longacre, "Items in Context: Their Bearing on Translation Theory", *Language*, 34 (1958), 482-491.

⁶ As Roman Jakobson has observed, the meaning of grammatical categories may be expressed by lexical means, if necessary. See *On Translation*, ed. by R. A. Brower (Cambridge, Mass., 1959), 235.

⁷ *On Translation*, ed. Brower, 233.

morpheme-by-morpheme translation, while if one tries to translate proverb by proverb, there is often no corresponding internal structure at all. Voegelin developed a technique of multiple-stage translation in connection with programming electronic computers.⁸ Related to this is Hockett's treatment of intermediate stages of immediate constituents, which one might call IIC.⁹ These approaches are useful steps in clarifying non-machine translation and bringing it closer to machine translation. Hockett's idea is related to, though not identical with, that of N. D. Andreyev's idea of intermediary language (IL) in his paper "Linguistic Aspects of Translation" (p. 625 of this volume). The idea of the IL is more ambitious and more intriguing than that of the IIC, but it is something for the future, while Hockett's IIC can be used any time now.

A specially important type of structure is at the level of word formation, either by way of derivation or by way of compounding. In this area the task of translators of scientific and journalistic subjects is fairly easy, because, irrespectively of whether complex words are translated by their morpheme components, as for example between Latin and German, or translated as wholes regardless, the units of translation in science and politics are for the most part international. In some sense, one might say, all modern life is of one culture and therefore does not run into difficulties one runs into for periods and areas remote from that of the target language, where there is much room for argument for various approaches. Typical cases of this sort are translations of era names of the Chinese dynasties. The common practice is to transliterate them, which is to make them comparable as to syllabicity, but is definitely under-translating, since transliteration is a zero-degree translation, of words which to the users of the original language do have definite overtones in their constituent morphemes, if not tangible denotata. On the other hand, if one completely translates the components into full-sounding words, then not only will the syllabicity be increased three or four times, but much more is said than what a native reader or hearer understands of those words. In problems of this sort even a literary person is faced with a dilemma, let alone machines.

5. SUBJECT MATTER AND CULTURAL CATEGORIES

Under structure we have noted the ease with which scientific and journalistic terms can be equated between languages, regardless of their internal structure. That is because science and current affairs belong on the whole to one contemporary culture. When dealing with diverse cultures, then the difficulties become serious even for non-machine translation. We have just noted the dilemma translators face in translating the dynastic era names. Another interesting case of cultural divergence is that of terms

⁸ Charles F. Voegelin, "Multiple Stage Translation", *IJAL*, 20 (1954), 271-280.

⁹ Charles F. Hockett, "Translation Via Immediate Constituents", *IJAL*, 20, 313-315.

of address, including kinship terms. What is an everyday short word occurring with high frequency and usable in direct address in one language may have to be equated to a long descriptive phrase, which will make a bad translation from the point of view of most of the other dimensions. You certainly can't very well greet a person with: "Good morning, my female-cousin-on-father's-side-younger-than-myself!"

It would seem that numerals and quantitative notions should be easy to manage by both machine and non-machine, but this so only in the sense that all numbers and quantities can be put in a common code from which it can be translated into the target language. On the other hand, cultural patterns enter into all practical use of numbers and quantities, thus making them as complicated as other disparate cultural items. Some languages have no "dozen" except as a foreign borrowing. The conception of "teenage" is a pure accident of languages which start a special pattern from thirteen on. Units of length, time, money, especially coinage denominations, etc. are also largely cultural. They not only influence the translation of words, they even influence the sizes and prices of things.

Proper names would seem to need no translation and some coding of the phonemics (or the graphemics) of the target language would seem to suffice and is readily translated (or transliterated) mechanically. But even here, especially in the case of names of persons, the translation is sometimes as much of a problem as in the case of terms of address, as one can see by examining the proper names in any bilingual dictionary. National and international committees have been set up to regularize the translation and/or transliteration of proper names and their task is never done.

Another category of cultural material to consider is music. So far as music itself constitutes a presentative rather than representative art, and is not language in the ordinary sense, it would seem that music would not need, nor be capable of translation any more than dance or architecture. But even here there is the same tendency for a person of one cultural background to "read" the pattern of another culture into his own, just as in the case of inexperienced translator from one language into another, or the case of the learner of a foreign language who substitutes for the phonemes of the foreign language with a non-congruent set of phonemes from his own language. A striking example of cross-cultural "translation" of music was from my own experience. Once I heard a piece of Javanese music as consisting of notes *do, re, mi, sol, la*, slightly out of tune to be sure. But I was informed afterwards that it was actually in an equal-tempered pentatonic scale!

6. PRAGMATICS OF TRANSLATION

Finally the pragmatics of translation, or the circumstance of use of the language, is the least amenable to treatment by machine, since here we are considering translation in so far as it is influenced by the situational context. A machine, if enlarged

without limit, could be envisaged to take care of all linguistic context. But to take situational context into account would require that the language had to be lived to be translated, in other words, the question would become: "What would you say under the circumstances?" To a limited extent, one could lexicalize certain typical situations and list the cases under idioms. If the circumstance is that of meeting on the street, the "How goes it?" in German would translate into "Where are you going?" in Chinese. To a praise or compliment, a speaker in one language may say something like "Thank you" but speakers of the target language do not say "Thank you" to a praise but say "No, not at all". One language may record parts of dialogues on stage-direction fashion by saying "laughter" or "sigh", while under the same circumstances a writer in another language may use actual interjections like "Ha ha!" or "Heigh-ho!" for which the original language lacks commonly accepted written forms.¹⁰

All these problems seem to lead us back again to the matter of literal vs. idiomatic translation. But if I have done anything to justify my going over these already well-known problems of translation, it is to show that there are not only many degrees of literalness and idiomaticity, but also many dimensions in which various degrees of literalness and idiomaticity can be ranged, and, while the initial degrees and the more elementary dimensions can be and are already being handled by machine, much that is interesting and important will remain for some time for translation without machine. Between man and machine, he will have to continue to do as much as he must, though he would like to do as little as he can.

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¹⁰ For further examples see discussion by Einar Haugen after the paper by N. D. Andreyev, "Linguistic Aspects of Translation", p. 625 of this volume.

PATTERNS OF GRAMMATICAL DEVELOPMENT IN CHILD LANGUAGE

WICK R. MILLER

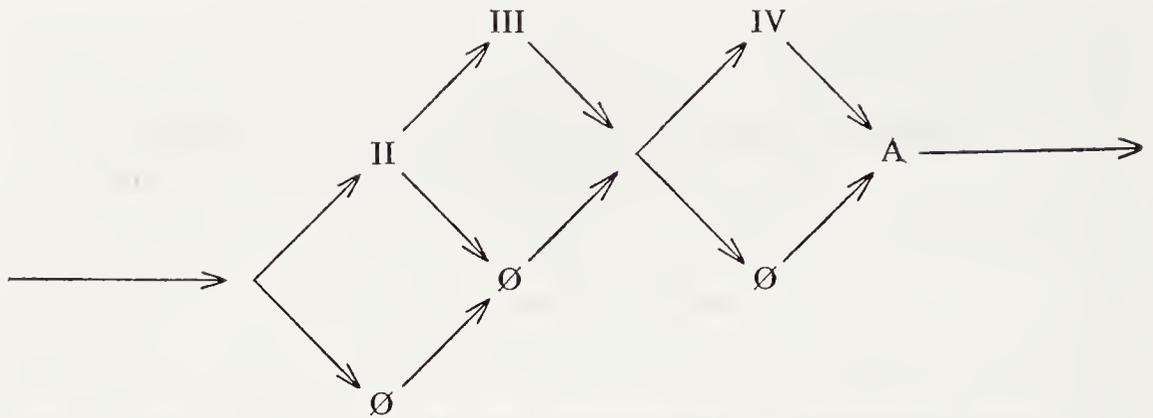
In the earliest phase of language development, the child cannot be said to have a grammatical system, or grammatical rules for forming sentences, because words are not combined into sentences.¹ All utterances consist of only one word. Primitive sentences of two or more words appear when the child is about a year and a half to two years old. These sentences are composed primarily of content or lexical words, words that are usually stressed in adult speech. Function words and affixes are largely lacking, resulting in what Brown and Fraser have termed a "telegraphic" style of speech.²

The child's early sentences are not composed of a random juxtaposition of words. The sentences are patterned. The purpose of this paper is to indicate the nature of the patterns, and to discuss their implications in regard to learning the grammar of the adult, or model language. To this end, we will examine the language patterns of two children, Donnie and Lisa. I have selected these children because they represent the two extremes of our sample. Donnie had the most consistent and most easily discovered patterns, whereas Lisa had the least consistent and least easily discovered patterns.

Donnie's corpus consisted of 174 different multiword sentences, collected when he was two years and two months old. The sentences were composed of two kinds of items, a small group of high frequency items which we will designate *operators*, and a large group of low frequency items which we will designate *remainders*. There were six operator classes, designated classes I through VI, that were differentiated by position of occurrence; and there was one remainder class, designated class A. A minimum sentence consisted of a single class A word. That is, class A words, but not operators, could be used in one word sentences. The most common operators are listed in Fig. 1, along with a finite state diagram that will account for 72 of the 174 different sentences. This diagram represents the most common and basic patterns of Donnie's speech. Fig. 2 lists all the operators and a finite state diagram that will account for 111 of the 174 different sentences. The validity of the second diagram is less certain than that of the first. It should be noted that the items listed as operators did not necessarily have the

¹ The data for this paper is taken from a study on child language being conducted by Susan M. Ervin and the author. The work is supported by a grant from the Department of Health, Education, and Welfare (M-3813) to the Center for Human Learning, University of California, Berkeley.

² Roger Brown and Colin Fraser, "The acquisition of syntax", *Child Development* (in press).



Operator Classes
(numbers indicate frequency)

II here (20), there (22),
where (20)

III 's (from "is") (52)

IV a (29), the (23)

Remainder (Class A)

airplane, baby, ball, bead, bird,
blanket, block, box, bubbles,
bunny, bus, car, choo-choo, cooky,
duck, fan, flower, hotdog, keg,
kitty, po (a nonsense word), powder,
sandwich, shoes, spoon, sweater,
toast, train, truck, water

Sample sentences:

Here truck.

Here's truck.

A truck.

Here a truck.

Here's a truck.

There bead.

There's truck.

A bead.

Here the truck.

Here's a water.

Where truck.

Where's car.

The block.

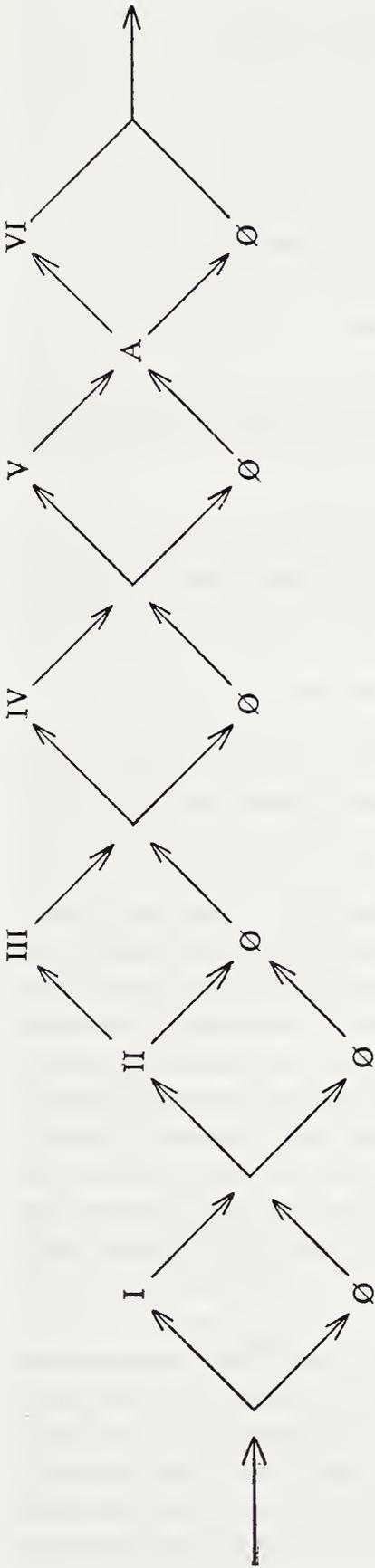
Where the bead.

Where's a truck.

Fig. 1. Limited Finite State Diagram of Donnie's Language (age 2:2)

same grammatical or semantic function in Donnie's speech and the model language. The plural suffix, for example, did not indicate plurality, but instead seemed to be used, along with the articles "the" and "a", to identify class A words. The operators "here" and "there" appeared to have the same meaning, and may have been allomorphs of one morpheme.

Lisa's linguistic material was collected between the time she was two years, two months, and two years, four months old. Her sentence patterns were rather obscure, and I have not attempted to state them. But it is clear that her sentences were patterned; they were not composed by a random juxtaposition of words. And it also seems fairly certain that if her patterns were known, it would be necessary to express them by phrase structure rules rather than by a finite state diagram. That Lisa's sentences were not composed of a random sequence of words can be seen in Fig. 3, in which the sequences in two-word sentences are given. All the operators have been lumped together under class I. Longer sentences indicated that the operators should have been divided into a number of classes, but the exact composition of the classes was not certain. The division of the remainders was based on the word classes of the model.



Remainder (Class A)

(same as on Fig. 1, plus:
bow-wow, doggy, ring)

Operator Classes
(numbers indicate frequency)

- I oh (8), see (4)
- II here (25), there (25), where (29),
that (3), thi-s (5)
- III 's (from "is") (56), go (6)
- IV a (36), the (38)
- V big (8)
- VI -s (from plural) (8)

's (class III) was usually [s], sometimes [ʃ]. -s (class VI) was usually [s], sometimes [ʃ]. The retroflex [ʃ] was the normal substitute for adult /s/ and /z/. It is assumed that "this" is composed of two elements, [di-] (class II) and [-s] (class III).

Sample sentences (in addition to those of Fig. 1):

- Oh, there's the bead. Where go the bead. Here's a big truck.
- See that the block. Here the bead-s. Here big truck.

Fig. 2. Extended Finite State Diagram of Donnie's Language (age 2:2)

First Word	Second Word			Totals
	I	A	B	
I	2	53	1	56
A	—	16	4	20
B	1	8	—	9
Totals	3	77	5	85

Class I (Operators):

a, all-gone, bye-bye, more, no, one, other, that, this, there, two.

Class A (Remainders derived from nouns and adjectives):

airplane, apple, baby, ball, book, boy, broken, bunny, bus, car, chair, cow, cup, dog(gy), doll, dressed, duck, egg, eye, feet, fried, Joe, Johnny, girl, goat, green, hair-pin, hat, hole, horse, hungry, Kennedy, kitty, Mama, new, Papa, piece, plate, play-doh, po (a nonsense word), pony, puzzle, shoe, spoon, teaspoon, teddy-bear, toast, toy, truck, Wick, worm.

Class B (Remainders derived from verbs):

doing, go, make, play, read, saw (cutting wood), see, take, touch, want.

Thirteen sentences that contained one of the following words were not included: *all, all-finished, all-through, away, here, home, in, me, my, off, please, the, what*. Eight sentences with a name, indicating the person addressed, were not included. Total number of sentences (based on the number of different sentences): 106.

Fig. 3. Class Sequences in Two-Word Sentences, Lisa (age 2:2 to 2:4)

Class A included items derived from model nouns and adjectives, and class B included items derived from model verbs. The division was at least partially confirmed by internal evidence; class B words did not occur in certain environments where class A words occurred.

There were a number of differences between Donnie's and Lisa's language systems. But there were also a number of similar features, features found in the language systems of all the children in our study. All the children had high frequency vocabulary items, or operators, and low frequency vocabulary items, or remainders. The operators tended to occur in fixed positions, and tended to define the meaning of the sentence as a whole. Most important of all, the operators appeared to be instrumental in the development of classes. In Donnie's speech, operators defined class A words, words that were derived from nouns, usually count nouns. In Lisa's speech, operators marked class A words, words that were derived from nouns and adjectives, and the absence of operators marked class B words, words derived from verbs. In some children's speech, "want" was used as an operator to mark verbs in sentences such as "want play", "want read", "want see", etc.

We would like to know if the patterns found in the young children's speech were productive. It is possible that they had memorized adult sentences, and sometimes abbreviated them to produce the telegraphic sentences so characteristic of children's speech. But it is difficult to find adult analogues for such sentences as "here's a water" in Donnie's speech, or "all-gone puzzle" in Lisa's speech, sentences that were quite regular for these children. Some sentences were undoubtedly memorized sequences,

but it is impossible to determine how numerous they were, using only observation techniques with little control over input. But if we assume that most of the sentences were memorizations of adult analogues, which I think is unlikely, it is obvious that the memorizations were selective, since most of the sentences were patterned.

In an effort to introduce some controls over input, nonsense words were presented to Donnie. The words were given semantic content, and one each presented in a frame for count nouns, adjectives, and verbs. The nonsense noun "po", for example, was presented in the sentences "this is a po", and "that's a po". Donnie did not use the nonsense adjective or verb. But he did use the nonsense noun in the sentence "here's a po". Thus, he used it in one of his own sentence patterns rather than imitating the frame in which he heard the word. This is a clear indication that at least one of the sentence patterns was productive, and did not represent a memorized sequence.

So far it has been demonstrated that the young child's sentences are not random, but that they are patterned, and the patterns are productive. Can we also say that the child has a grammatical system? The question can be stated in more concrete terms for Donnie, namely: Is the diagram on Fig. 2 a grammar of Donnie's language? The answer to the question depends to some extent on what a grammar is conceived to be. We will assume that a grammar is a system that predicts, or generates, all and only grammatical sentences. Sentences that do not conform to the grammatical rules, then, must either reflect the inadequacies of the grammar, or else must be mistakes, ungrammatical slips of the tongue. In normal language situations, there are a variety of ways in which a grammatical rule can be checked. We can make up sentences and ask an informant if they are acceptable. Children over three sometimes correct their mistakes, and in this fashion they indicate that they have an internalized system or grammar by which they are able to identify their mistakes or ungrammatical sentences. But two year old children do not correct themselves, and the usual techniques for checking grammaticality cannot be applied.

We cannot expect to make a complete linguistic analysis that is based primarily on text material, since it is impossible to discover all the grammatical rules and their limitations without checking the rules in some way with an informant. Thus we can be sure that if Donnie had a grammatical system, the diagram in Fig. 2 cannot be a wholly accurate representation of his grammar. This point is illustrated by the sentence "where's a big choo-choo car". The sentence contains a sequence of two class A words, and therefore cannot be generated by the diagram since only one class A word is allowed. There are enough sentences that contain sequences of two class A words to indicate that this was a productive pattern, but there are not enough sentences to indicate what, if any, limitations there were for such sequences. The sentence "car-s big", and other unpatterned sentences like it, offer greater difficulty. Such sentences can be treated in one of three ways: (1) The diagram can be changed and made more general so as to include these sentences. (2) The child can be considered to have a grammatical system, and the aberrant sentences would then represent mistakes,

ungrammatical deviations from the child's grammatical system. (3) The child can be considered to have a system to be described in probabilistic terms, and the aberrant sentences would represent patterns that have a low probability. The first alternative can be rejected because the changes would make the grammar too general to have much predictive value, that is they would predict a large number of unlikely sentences. It is impossible to choose between the second and third alternative until appropriate techniques are devised for checking grammaticality with small children.

I have indicated that the development of operators appears to be instrumental in the development of classes. It is assumed that the child learns that certain frames identify certain word classes, and that the words that compose the frames become operators for that child. Thus Donnie may have learned that the same words are found in the frames "that's a ----" and "here's a ----". When he heard the nonsense word "po" in the sentence "that's a po", he knew that he could use the word in "here's a po". A second, and perhaps more important factor in the development of classes has to do with the semantic consistency of word classes. Roger Brown has demonstrated that among four and five year old children word classes are semantically more consistent than among adults.³ An examination of Donnie's class A words shows that all the words refer to objects. Thus when Donnie learned that the nonsense word "po" referred to a small plastic object, he may have simply put that word in the word class that referred to objects.

In learning a language, the child must learn two systems, the phonological and the grammatical, the systems concerned with the channel and the message. In addition, the child must learn the relation between the two systems, that is he must learn the morphophonemics of the language so that he may give a phonological representation of the grammatical system. Jakobson has proposed a theory which accounts for the development of the phonological system by successive binary divisions. The system is learned by the imitation of words in which the phonological sequences occur. The phonological sequences are given, they are not constructed. Child language studies are not sufficiently advanced to make it worthwhile to attempt to construct a theory for the development of the grammatical system, but we can specify some of the features that such a theory will have to include and will have to account for. In grammar, the child must learn to create new sequences. The child must learn rules rather than sequences, and therefore imitation necessarily plays a smaller role. And before the child can use the rules, he must learn to place his vocabulary items into classes. The development of classes is undoubtedly one of the first and most important steps in learning the grammatical system.

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³ Roger Brown, "Linguistic determinism and the part of speech", *Journal of Abnormal and Social Psychology*, 55 (1957), 1-5.

DISCUSSION

FISCHER:

This paper brings up two important and related questions:

- 1) At what point can a young child be said to have the beginnings of a grammar?
- 2) How can the child's grammar be investigated in the very early stage when the child will never tell the investigator whether an utterance is grammatical or ungrammatical, right or wrong?

With regard to the first question, the author suggests that a child evinces grammar when he starts combining two or more words in a single utterance. But what about intonation differences in the pronunciation of a single word before then? If these are meaningful, are they grammatical? Children understand more than they say themselves. If through behavior they can be shown to understand a two or three word utterance of an adult, then some grammar generating this utterance may be said to be "in the child's possession" or at least close to it. Study of adult speech directed at the child and the child's comprehension of this would be of great value.

With regard to the means of investigating the earliest stages of a child's grammar, investigation certainly would become more efficient after the child can correct his grammatical mistakes, even if only occasionally, but I believe that investigation is quite possible before this. Certainly there are modern grammars of extinct languages which have never been checked with informants. What is necessary if the investigator cannot check with an informant is a much larger corpus in order to achieve a given degree of accuracy. For a synchronic analysis of an early stage of a child's stage, it might be desirable to obtain and interpret a corpus as close as possible to the entire linguistic output of the child for several consecutive days. With enough recording equipment and the full cooperation of dedicated parents this might be achieved.

MOSES BEN EZRA'S "GRAVES":
THE LINGUISTIC ANALYSIS OF A SHORT POEM

GENE M. SCHRAMM

Abstract

If poetry in general is to be considered an art-form with language as its medium, it is more than reasonable to expect that the interrelation of expression to content in poetry is a matter of greater importance than it is for an ordinary utterance, in that its arbitrariness is somehow consciously or semi-consciously tempered to produce an elegant thought elegantly stated. This paper considers the relevance of linguistic analysis to literary criticism via the text of a short poem by Moses Ben Ezra (Spain, late eleventh and early twelfth centuries). Textual variants are also discussed in so far as they affect the phonological and morphological patterns of the poem.

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LINGUISTIC FACTORS IN THE EVOLUTION OF THE ALPHABET

JOHN V. WALSH

Abstract

Since an alphabet, in any technical sense, is a limited group of linear characters corresponding by functional convenience to the basic phonemic areas in any given speech-pattern, it follows that to grasp essential features in the origin, the transmission or the adaptation of any specific alphabet involves fundamentally linguistic rather than primarily epigraphic factors. While this is obvious and has long been recognized, it remains equally true that the linguistic factors in alphabetic discussion have frequently been obscured and subordinated by historians of philological rather than linguistic bent. Concentration therefore on phonemic and dialectic phenomena where these are ascertainable might help to clarify some long standing alphabetic-historical problems, e.g. the mutual bearing of Egyptian and Canaanite in the early phases of alphabetic evolution, the peculiar puzzles involved in the early Hellenic epichoric alphabets, and modern attempts to establish so called phonetic or more consistent notations. The entire question therefore is relevant to what we may call the possible obsolescence of the alphabet as a linguistic instrument, as well as to the purely historical issue of its rise. It is the concern of this paper to underscore some linguistic elements in each of these three areas, in illustration alike of the achievements, the inadequacies and the possibilities of any alphabet as a linguistic, as distinct from a literary instrument.

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DISCUSSION

W. LEE:

While agreeing with J. V. Walsh that linguistic factors are, obviously, involved in the understanding of the functions of an alphabet, I would query whether an alphabet is always to be viewed as a "group of linear characters corresponding ... to the basic phonemic areas in any given speech-pattern". Exactly what functions the alphabet performs seems to vary from language to language, but certainly in English the "linear characters" do not correspond only to "basic phonemic areas": one has only to think of the *-ed* ending in words such as *washed*, *loved* and *added*. Do the "linear characters" *ed* (one cannot take them separately) here "correspond" to /t/, /d/, and /id/ (or /əd/), or do they not rather (and more usefully for the *silent* reader) have a morpheme-indicating rather than a phoneme-indicating value? Taking an example of a somewhat different character, what about the "linear characters" *wh*, when initial in the sentence? It would be interesting to know how often this *wh* does *not* visually signal the onset

of one type of interrogation, apart of course (for readers *aloud*) from corresponding to a /w/. These are but elementary examples, and, although several more might be quoted, this aspect of language has not yet been properly explored.

The assumption that it is the sole business of an alphabet to represent phonemes underlies most schemes for spelling reform. It would seem desirable for other functions of alphabets, as they have *come to* be used in particular languages, to be more thoroughly studied: there would then be less danger of future spelling reforms stripping away certain useful functions that an alphabet sometimes (and in English, perhaps, fairly often) performs at present. Such a study might also cast light on historical developments.

STRUCTURAL VARIATION IN LANGUAGE

ANDRÉ MARTINET

1. VARIATION THROUGH TIME AND SPACE

Words designating processes also designate, as a rule, situations resulting from those processes. "Variation" is no exception, and the reader confronted with the above title may well wonder whether it refers to the variation any linguistic structure may undergo through time or the by now well established facts that structural differences do not necessarily prevent mutual understanding and that languages may be said to vary through geographic or social space. It is likely that those whose main interest lies in diachronic research will understand "variation" as pointing to a process, whereas for those who concentrate on synchronic description, the term may suggest dialectal and even idiolectal differences.

Offhand, variation through time and variation through space seem to raise totally different problems for any one who is not satisfied with pure and simple observation and listing: on the one hand why do languages change; on the other hand how do people manage to understand one another in spite of differences in their handling of the language. Yet there is between the two domains a fairly obvious connection: it would seem that any language at any moment is in a process of change so that the startling differences which come to sight when comparing two fairly different stages of the same language are nothing but the accumulation of successive modifications and adjustments. Now, the co-existing members of a given community are not all affected at the same time by the factors that determine linguistic variation, which implies that some transient factors will affect only certain geographic sections, social strata, or age groups of the community: changes will take place both in the process of transmission of language to new generations and through the spread of new linguistic habits, which means that differences will exist between the speech of different generations and that at a certain point in time some sections of the community will have been affected by the spread of a new habit, whereas other sections will not yet have been reached. In other words linguistic change, i.e., variation in time is hardly conceivable without variation in space from one section of the community to another. This implies that any attempt to account for linguistic change will have to reckon with variation in space as one of the dimensions in which it takes place: when proceeding at a certain speed, linguistic change does not affect mutual understanding or, if we put it differently, the necessity of mutual under-

standing regulates the process of linguistic change. This should by no means be understood as implying that mutual understanding is always necessarily a retarding or restraining factor: language changes under the pressure of changing communicative needs in permanent conflict with least effort on the one hand, tradition on the other hand. Linguistic variation as a process can be fully understood only through a synchronic consideration of the dynamics of language.

Structural variety through space is of course a result of structural variety through time. But, once established, it raises a number of specific problems such as how people with different linguistic structures manage to communicate. This leads to the positing of diasystems singling out such features of various dialects or idiolects as are common to all members of the community at large. A further problem is raised by the fate of those features which are no part of diasystems, one which should be solved within the frame of a chapter devoted to linguistic convergence.

2. VARIATION IN STRUCTURE AND STRUCTURALLY DETERMINED VARIATION

If "Variation" has been found ambiguous, how much more so "structural". Structural variation will undoubtedly be understood by many as implying variation *in* the structure whether variation is interpreted as a process or as a situation resulting from a process. In such a case, a study of structural variation would be descriptive, descriptive of what takes place in the course of the process, or descriptive of the difference between what was and what is, between what is here and what is there. But "structural variation" may also be construed as variation *through* structure or, more precisely, structurally determined variation. It is clear, of course, that any language (and whatever is called its structure) is exposed to changes determined by impacts from outside: no one will doubt that man's changing needs in general will affect his communicative needs which, in turn, will condition linguistic structure. The impacts from outside may consist in the pressure exerted on each other by two languages "in contact". The linguist will feel competent to deal with the latter, but he may be excused if, in his capacity as a linguist, he declines the invitation to investigate sociological conditioning. If however it appears that the traces left in the language by some external agent determine further modifications which may, in their turn, result in a wholesale reorganization of some aspects of the language, all this taking place long after the initial outside agent has ceased to act, it is obviously the duty of the linguist to observe these successive events and try to discover how they have determined one another. If the consequences of an initial impact can thus develop through years, centuries, and millennia, conflicting and coming to terms with other chains of consequences, it must be that the various items which constitute a language, whether they be conceived as habits or mental entities, are closely dependent on one another as to their forms and functions, although their interdependence is not such as to let an impact somewhere have immediate repercussions on all parts of the whole. There is too much redundance in language to allow for an immediate

wholesale adaptation of it to the new needs of its users. Besides, new needs usually conflict with old needs, and linguistic conservatism often runs parallel with resistance to new developments in some sections of the community. As a result of this slow spread, linguists will rarely be confronted with chains of events they can trace back to some initial non linguistic impact otherwise than through bold hypotheses. As a rule they must be satisfied if they can explain a particular change as a normal development of a chain of linguistic events and identify it as one of the factors of some other particular change.

There is thus a place for an explicative study of structural variation beside a purely descriptive one. But the preceding discussion is highly metaphorical in its wording (“impact,” “chain,” “conflict”) and likely to make sense only for those who have practiced this kind of explicative study and can appreciate how much of it is pure observation and how much fruitful hypothesis. A further objection that could be raised stems from an implied practical identification of “language” with “language structure”, one that would probably be widely questioned.

3. STRUCTURE, SYNTAGMATIC AND PARADIGMATIC

If most contemporary linguists are agreed that there is some justification in mentioning structure in connection with language, agreement ceases as soon as the problem arises of where linguistic structure is to be found. With some scholars, what is meant by linguistic structure is nothing but a pattern imagined by the linguist in order to account for the working of linguistic communication; in other words, there should be no such thing as structure *in* language. With most others, structure is to be looked for in language itself, our patterns and diagrams being, as it were, two-dimensional translations of existing relationships. But here again, there is no agreement as to what “structure” is meant to cover. For those who are on their guard against the snares of mentalism, linguistic structure is equivalent to distributional restrictions: linguistic units, distinctive as well as significant, are normally excluded from some collocations. The ones that appear in a given collocation form a class, and the structure of the language is the network of relationships among the various classes. Whatever relationships may exist among the units belonging to a given class are not considered a part of the structure of the language. These can only be evaluated in terms of substance, phonic substance in the case of distinctive units, semantic substance in the case of significant units, and it is often felt that if phonic substance can be the subject of scientific investigation, this does not apply to semantic substance. Those who want above all to give linguistics the status of an exact science prefer limiting its field to what can be dealt with in terms of discrete units and groups of such. Whatever relationship may exist among the members of the same class, apart from the fact that they “contrast” with each other, is accordingly considered non-linguistic or, at best, “metalinguistic” and no part of the proper structure of language.

Other scholars, who also found their analysis on distributional restrictions or, what

amounts to the same, on combinatory latitudes, are less adverse to investigating structure among units belonging to the same class. For them, the relationships among units that belong to the same class (or paradigm) are no less important than those among the members of various classes appearing in the same utterance. The former they call "oppositions", an opposition being the relationship between two units that exclude each other at a certain point in the utterance or, in other words, that oppose each other's presence there. The latter they dub "contrast", i.e., the relationship between two actually co-existing units; pure distributionalists, for whom there is no need to keep paradigmatic and syntagmatic phenomena apart, do not distinguish between contrast and opposition, and actually use "contrast" where others say "opposition".

In the eyes of those who extend linguistic structure to the relationships within classes, the disadvantage of having to operate with less clearly defined quantities seems definitely outweighed by the clearer understanding of the nature of language afforded by the study of paradigmatic reality. They argue that, in the course of the analytic procedure, no one can identify two different segments of utterance as one same unit without some reference to at least some aspect of its substance, be it phonic or semantic. Now, it is sounder not to forget at a later stage what has been operated with before, the more so if it may help us in our task as linguists.

4. CHOICE

Units that stand in opposition or, what amounts to the same, that belong to the same class, are of course the ones among which the speaker will have to choose at each point in order to make his message what he wants it to be. Linguists, in many quarters, have been conditioned to react most unfavorably to such a statement: they will stop their ears or, at best, argue that the choices of the speaker cannot be used as a valid criterion for analyzing utterances because we could only know what he has chosen through asking him, a procedure which is scientifically acceptable only in the frame of a statistical treatment. Yet the statement that an utterance contains just as many units as there are different choices is not derived from some vague philosophical speculation; it is simply an explicit statement of the foundation and value of the method of commutation, a method which, under various labels, is practiced by linguists of all structural persuasions: looking for minimal pairs would not make sense if it did not aim at determining minimal distinctive choices on the part of the language user. Defining linguistic units in terms of choice does not, in itself, imply the adoption of a given method of analysis. But using the commutation procedure implies an overt or covert conviction that every unit corresponds to a new choice.

5. SUBSTANCE, PHONIC AND SEMANTIC

Few linguists will deny that the number of units among which the speaker may choose in each class may be considered a constitutive feature of the linguistic structure. Those who would be tempted to demur if they happen to think first of huge lexical classes

where the addition of a handful of new items does not seem to matter much, will readily agree that the appearance of a new phoneme or the confusion of two previous ones does change the phonological structure of the language. On the plane of significant units, no one would seriously argue that the elimination of the distinction between singular and plural, definite and indefinite, past and non-past would not affect the structure of the language. Even if one were tempted to decree that linguistic structure proper is based exclusively upon the relations among classes and is not affected by those within each class, it is clear that the disappearance of a distinction between past and non-past, in a language where the tense pattern is restricted to these two units, would amount to the elimination of a structural class. In other words, the maintenance of a class depends on the preservation of the oppositions between the individual members of that class.

The preservation or elimination of oppositions in the course of time is largely determined by the substantial nature of the differences between the units involved: a phonological unit performed as [t] is not likely ever to get confused with another unit of the same class performed as [ʒ]; but a confusion of a /t/ unit with a /d/ unit, as illustrated by the identification of *latter* and *ladder* in some American usages, is of frequent occurrence. Therefore a structuralist cannot be indifferent to the nature of the substantial features that keep distinct the units of the same class. If, as is frequently the case, each one of several units of a class forms a pair with some other unit of the class, so that the difference between the two members is the same in all the pairs, it is presumable that a change affecting one of the pairs will also affect the other pairs: when, in a class (e.g., that of implosive syllable finals in many languages) grouping the following pairs

p	t	k
b	d	g

the distinctive value of the /p/ ~ /b/ opposition disappears because the use of [p] or [b] is made to depend on the context (neutralization), it is to be expected that the /t/ ~ /d/ and /k/ ~ /g/ oppositions will follow suit. The nature of the difference between the two members of the pairs may, in some cases, undergo a change in some of the pairs involved, but not in all of them, as when Germanic word initial pattern

p	t	k
b	d	g

turns out as

pf	ts	
	t	k
b		g

in standard German (with a new /p/ in loans and a new /d/ from a former fricative). Even those who would be tempted to argue that the structure proper can only be said to be affected when the number of units in the class has changed must agree that what may happen in this respect is not likely to be the same when starting from one pattern or from another. In other words, any reorganization of the substantial relations among

the units of a class will condition the further development of the class, including the possibilities of numerical recession or expansion.

In the field of significant units, the problem, at this point, is less one of preservation and elimination of distinctions which it is the duty of phonemes to secure, than one of how the semantic extension of each unit is checked by its "neighbors", i.e., the units of the same class that are semantically akin. This is particularly significant when a new unit has found its way into the class. This increase in the number of units may in itself have far less importance for the language as a means of communication than the implications the presence of the newcomer will have for the internal organization of a large section of the class if not for the class as a whole. It should, of course, be perfectly clear that the receptivity of a class for new units is largely determined by the nature of its internal organization: if a specific designation for females is a wide-spread feature of the class, that class (or, more exactly here, the substantival classes) will be very receptive for new designations for females ("filling of gaps").

The study of structural variation through time, whatever restrictions are attached to the term "structural", cannot be carried out unless substantial differences, both phonic and semantic, existing among the units of the same classes are taken into consideration. Some sort of structure is to be found among the units of each class. For clarity's sake, this type of structure should be distinguished from the structure resulting from the relationships existing among the various classes. The former should be called "paradigmatic structure", the latter "syntagmatic structure". Both, being characteristic of language, should be nothing but two aspects of linguistic structure.

6. OCCASIONAL AND PERMANENT PHONIC VARIATION

As for changes in the phonic substance, a fundamental distinction must be made between occasional variation and permanent variation. Occasional variation is conditioned by a set of particular circumstances: a cold or a sore throat may entail occasional variations in the articulation of speech sounds; permanent variation results from a stable conditioning; that conditioning may be non-linguistic if, as seems to be the case in contemporary French, the spread of education, together with various other factors, determines an expansion of a didactic initial stress. It may be linguistic but external when determined by language contact. It is linguistic and internal when determined by the action of one speech segment on a neighboring segment in the utterance or by the pressure exerted on one member of a class by its paradigmatic neighbors. Occasional variation will not affect the structure: intoxication is widely supposed to lead to a blurring of a distinction between /s/ and /š/; but intoxication is exceptional enough, at least in so far as it affects the articulation of sibilants, not permanently to affect the distinctive latitudes of the language. Permanent variation with external conditioning, both non-linguistic, will affect the structure, as when one of two languages in contact furnishes a new phoneme to the other. In such a case, the circumstance of the loan may be

determined by some aspect of the receiving language, as when the adopted phoneme fills a previous "gap". But sociological factors will remain determinant. Internal variation, resulting from either syntagmatic or paradigmatic pressure should be considered structural whether or not it entails a change in the number of distinctive units because what counts is less the immediate result than the internal causation.

7. OUTWARD VARIATION AND FUNDAMENTAL STRUCTURE

When dealing with significant units the problem is made more complex because variation may affect either their outward form or the number of the units of the class and their mutual significant relations in the utterance and within the class.

As regards the form of these units, it is quite essential to distinguish between the type of variation which results from the regular evolution of the distinctive pattern and other types: when Modern English *stones* /stounz/ is found to correspond to Old English *stānas* /stānas/, the variation cannot be said to raise any problem which has not been taken care of in the treatment of the structural variation of distinctive units within their various classes. As people used to say, /stounz/ is regularly developed from /stānas/ in accordance with the "sound laws" of the language.

But formal variation may result from the analogical replacement, in a given context, of the traditional form of a certain unit by another form previously restricted to some other contexts: Modern English *love* is, segment for segment, the exact correspondent of Old English *lufu*; but *love's* is not that of the Old English genitive *lufe* because *-e*, the mark of the genitive function, was at some point replaced by a competing form involving an *-s*. All this is well-known and traditionally dealt with under the rubric "analogical changes". But, in the frame of a structural hierarchy, it certainly deserves to be reconsidered. For any one, infant or adult, who learns a language, it is far from immaterial whether it presents two or more different forms for the expression of the genitive, or whether, as the result of some analogical leveling, there remains only one way of expressing that case relation. This largely accounts for the insistence, in language description, traditional and "structural" alike, on what is often called allomorphic variation. It is worth noticing that, once we start from an exhaustive analysis of utterances into minimal significant units (variously called morphemes or monemes) and do not posit, on the plane of general linguistics, any unit between these and the sentence, the only meaning we can attach to the word "morphology" is that of "study of the synchronic variation of the form of significant units" or, in other words, a listing of the "allomorphs" of each "morpheme" with directions regarding their distribution; in that sense, the use of the traditional word "accidence" would be perfectly justified, since it could be conceived as the presentation of the accidents to which the formal face of significant units are exposed. Yet, whatever the practical importance of morphology thus defined, it must be stressed that the various accidents it describes represent only fairly marginal aspects of the real structure of language.

This statement will, to many, sound like a paradox. Typological attempts in linguis-

tics have mostly concentrated on morphological characteristics implicitly believed to be truly representative and even descriptive of language structure: the traditional triptych, with isolation, agglutination, and inflexion as its three catch-words, was founded upon the structure of what was believed to correspond, in different languages, to the word of classical Indo-European languages. But since words are mostly clusters of “morphemes” permanently glued together in such a way as not to permit the insertion of new morphemes between them, former typologists implicitly operated with, as their only criterion, the degree of formal separability of minimal significant units in the utterance. It is clear of course that, sooner or later, inseparability implies (1) the appearance of combinatory variants (“allomorphs”) in different contexts, as when the Indo-European mark of the accusative singular **-m* appears in Greek as *-n* after vowels, as *-a* after consonants; (2) the amalgamation of formerly successive “morphs” that results in making impossible any immediately and universally acceptable analysis into successive elements, as when Latin *ciuis* is analyzable either as *ciui-s*, with *ciue-* as the allomorph of *ciui-* in *ciue-m*, or as *ciu-is*, with *-is* as the “allomorph” of the nominative singular; (3) the developing of concord, i.e., discontinuous “allomorphs”, as when Latin presents *fortis ciuis* with *...-s ...-s*, a repetitious and discontinuous mark of the nominative function.

Restrictions as to the possibility of inserting some “morpheme” somewhere in the utterance would, at first blush, seem to affect the structure of the language. But what really counts, in syntax, is *not* the possibility or impossibility of inserting a new segment *at a given point* in the utterance, but that of using a certain unit that will affect the message in a given way, irrespective of whether this unit is inserted in the message here or there, whether it manifests itself as an easily isolable segment, such as an English preposition, as a discontinuity, as in the case of concord, as the positive or the negative feature of some amalgam like the preposition in French *au* /o/ or the past in English *he cut*. We find here one of the main differences between distinctive units and significant units: distinctive units are distinctive in themselves through their opposition to other units and at a definite point; *lap*, *pal*, and *alp* are all composed of the same phonemes /l/, /æ/, and /p/, but the order in which they appear is relevant and the three words are perfectly distinct. In the case of significant units, order may or may not be relevant so that the relevancy of order has to be explicitly mentioned wherever it exists as just one of the tools available for the communication of experience. Generally speaking, the way the speech segments that are the manifestation of significant units combine in the process of linguistic communication is far less decisive for the structure of the language than the number of units in a given class and the substantial interrelations of these units: the formal differences between the German pair *in das Zimmer* – *in dem Zimmer* and the English equivalent *into the room* – *in the room* which loom large for whoever is fascinated by the set of formal accidents which grammars present under the form of German declensions are structurally insignificant if contrasted with the corresponding pattern of French which, with its undifferentiated *dans la chambre*, leaves to the lexicon the burden of marking the distinction.

Whether structural variation, on the plane of significant elements, is understood as “variation undergone by the structure” or “internally conditioned variation”, it must be clear that what has to take precedence is the kind of structure which is revealed by the specific syntagmatic and paradigmatic patterns every language uses for the analysis of experience required by linear, vocal communication. By “syntagmatic patterns” I do not mean the bare order of succession of the different units in the utterance, but the possible co-existence of units belonging to different classes whatever their respective positions in the spoken chain: the structural variation which consists in developing a distinction between an active voice and a passive voice, i.e., two different ways of orienting the predicate in relation to the participants of the action, is far more fundamental than the choice and grouping of the distinctive elements necessary for the expression of that distinction.

It is not meant hereby that formal variation of all sorts will not play a role in the shaping of those fundamental patterns: it is easy to imagine how the formal means used for the expression of some fundamental distinction may be determinant for the fate of that distinction. No aspect of language is indifferent as soon as evolution is at stake. But it remains essential to keep neatly distinct the different planes that the analytic effort of the last decades has dissociated for a better understanding of linguistic processes.

Sorbonne

DISCUSSION

PILCH:

The close connection between structural variation on the synchronic and on the diachronic level as stressed in the present report may be elucidated from a slightly different angle. Synchronic statements very often imply diachronic ones. This is brought home in a striking manner by the currently fashionable metaphor “transformational history” which sometimes suggests not only derivational, but real history. For instance, we say synchronically that Middle English disyllabics have (in their first syllables) either a long vowel + short consonant or a short vowel + long consonant. Analyzing the inflexional paradigms of monosyllabics with a short vowel + short consonant we state that in the disyllabic inflected forms either the vowel or the consonant (of the stem) is long (e.g. *graf*: *grāves*, *god*: *goddes*). Such a statement would probably be accepted as a synchronic (non-historical) description at least by a substantial body of linguists. However, this statement requires only slight re-wording (“lengthened” for “long”) in order to be turned into the description of a historical development of Early Middle English.

SHEVELOV:

The view that phonemic changes are brought about by external factors and only the following re-groupings which are triggered by the disturbance of balance may be

accounted for by linguistic regularities as such – this view can be applied only, in the best case, to sub-systems of a language. If, for instance, one takes a language with the classical five-vowel system and observes a change in this system one may be inclined to say that the system of vowels in the given language contained no germs of imminent mutation and, consequently, one has to seek the reasons for the change outside the language in question. In point of fact, however, the system of vowels is but one of the sub-systems of a more general system which is the language as a whole. And while a sub-system may be well balanced within itself, there is no known language in which all sub-systems are in balance within themselves and/or in the relation to the other sub-systems. In this sense it might be said that linguistic change caused internally never begins because it never ceases. It is inherent in language.

Remaining in the analysis within particular sub-systems (a procedure which was by necessity typical of the years immediately after the rise of phonemics, but has too often characterized modern research as well), in many cases forces the linguist to renounce any analysis whatsoever of causality in phonetic changes. For instance, in Slavic such a radical change as loss of *jers* in many positions and, in the most Slavic languages, as a phoneme, has found so far no explanation. Slavic, before this change, was supposed to be a language of open syllables, weak stress, and no reduction of vowels or syllables. With the loss of *jers* all these features changed, e.g. in Eastern Slavic, to their opposite. This was quite a drastic switch, and yet apparently there were no internal causes for it; at least they could not be discovered as long as the researcher remained in his analysis within the framework of the system of vowels. In reality, however, the loss of *jers* was prepared by the foregoing changes in other sub-systems of the language. It is impossible and unnecessary to go here into details, but one might refer to an extreme overgrowth of the consonants, as initiated by the changes of *j*-clusters and the first palatalization of velars, and to the general decay of the old *Ablaut* relations. The first fact made in many cases vocalic distinctions redundant, the second weakened the morphophonemic motivations for the use of *ǔ* and *ĩ* in many morphemes.

The insistence on internal factors as the most influential in phonemic developments does not imply that the speakers realize what is the make-up of the language's sub-systems, where the balance is lacking, and consciously strive for the restoration of the balance. This may occur only rarely. Usually the restoration of balance is carried out by the simple fact that, in speech, variations easily move into holes, thus creating a possibility for materialization of speech sounds absent in the phonemic system of the language. If, then, a variation of these becomes phonemic, one faces a change in the structure of the language, a change which logically was motivated by the interplay of internal factors within the language, but psychologically (from the point of view of a speaker) was but fixation of a till then random variation. This applies, naturally, to losses of old phonemes as well as to acquisitions of the new ones.

BONFANTE:

Despite my admiration for Prof. Martinet, I must point out one question today on

which I certainly cannot follow him: when he asserts that all linguistic changes can be explained from structure itself, without any "extra-linguistic" influence (to use his vocabulary). I absolutely cannot understand this assertion. It is quite obvious that, once a system has reached a balanced equilibrium (such as a vocalism *a e i o u* as contrasted to a lame, unbalanced system like *a i o u*), there is no reason whatever why such a system should ever change, unless under an "extra-linguistic" influence (such as mixture with a foreign language, dialect mixture, or, for an idealist like myself, "free creation"). From inside of the system no change can come, once the point of perfect harmony is reached. And, according to structural linguists, it *must* be reached some day, inevitably, since all systems tend to such a harmony. Such harmony is in fact frequently reached (e.g. *a e i o u* in Latin), but then it always fades away and again gives way to some "unbalanced" system. As for Prof. Shevelov's insistence on the fact – a quite indisputable one – that one part of the structure of language can influence and modify another part of it, he is merely shifting the problem from right to left, replacing an *x* by a *y*, without solving it. Granted that a change, say, in morphology, may cause a change in syntax, where did the morphological innovation come from? At the beginning of this chain reaction, no matter how long it may be, we must go outside of the system, we must necessarily find an "extra-linguistic" impulse. Inside of the system no explanation can be found.

GALTON:

Prof. Georgiev has referred in the debate to the fact that in Bulgarian and Macedonian, conjugation is richly developed as against the practically non-existent inflection of nouns, whereas in the other Slavic languages the reverse is true. However, we are not dealing here with effects within one system as conjugation and inflection do not form part of the same sub-system of a language. It appears rather to be the inability of the human mind to develop fully both a rich system of inflection and conjugation at the same time, which I would not consider a structural factor. As for the Germanic sound shift, I postulate that not only the "original move", as suggested today, but all its three stages are the result of an impact from outside the phonemic system. I have tried (*Journal of English and Germanic Philology*, 1954) to show a single underlying cause, namely a weakening of muscular intensity.

HAHN:

Professor Martinet contrasts the German *in das Zimmer* vs. *in dem Zimmer*, and the English *into the room* vs. *in the room*, with the undifferentiated French *dans la chambre* for both ideas. But the languages are not completely consistent in this respect. Latin and German express the distinction involved by a change in case (accusative vs. ablative or dative) and English by a change of vocabulary (*into* vs. *in*), whereas French makes no change at all. But when we consider the question which would elicit the answers involved, we find that Latin and German still make a distinction, this time of vocabulary, *quo* vs. *ubi* and *wohin* vs. *wo*; but now English resem-

bles French in making no change at all, using *where* in either instance just as French uses *où*.

WEINREICH:

One of the perennial hopes of structural linguistics has been to demonstrate that there is a correlation between the structural depth and the historical stability of linguistic phenomena. But every theoretical construction in this domain, no matter how bold, has foundered on the rocks of empirical evidence. Even if "structural" is opposed, as a whole, with "non-structural" (in the domain of sound: the phonemic with the subphonemic), the facts speak against the theory of privileged conservativeness in matters of structure. The results of linguistic geography, where – in contrast to historical reconstructions – the substructural and structural facts are equally accessible to observation, is particularly eloquent. Isoglosses of major systemic import are found to run a course no less capricious than those of subphonemic detail; and structural innovation is no less free from areal diffusion than other kinds of innovation. Conversely, such subphonemic facts as the uvular vs. apical pronunciation of *r* have shown remarkable stability over large areas and epochs. Now that advances in phonological theory are making possible much finer differentiations, within the structural domain of sound itself, between higher and lower levels of structure (ordering of phonological rules), I think that the correlation between structural depth and historical stability will be weakened even further. I would conclude that there is little prospect for using historical considerations for justifying descriptions of language. The perennial hope of linguists on this point has little chance of realization.

EXPANSION VS. REDUCTION GRAMMAR: SEPARATE, RECIPROCAL, OR WHAT?

LOUIS G. HELLER

What I shall attempt to do in this paper is to explore the relationship between a few grammatical models as well as a few different approaches. In passing, I shall use some terms in a way that I know will distress some scholars who prefer a different terminology. I shall define my terms however. Then if you prefer other words — fine. The important thing is the communication, not the specific sign used to convey it.

Last December at the National Convention sponsored by the Linguistic Circle of New York, I suggested the term *macroreplacive* to cover the three structural types of morphological relationships enumerated by Eugene Nida in his *Morphology*² (p. 69). These are the *additive*, the *subtractive*, and the *replacive* (the *microreplacive* in my own frame of reference). The additive is the replacement of zero by an overt morpheme ($M \leftarrow \emptyset$); the subtractive is the replacement of an overt morpheme by zero ($\emptyset \rightarrow M$), and the microreplacive is the replacement of one overt form by another ($M_2 \leftarrow M_1$). Basically this formulation is the pattern of generative grammar — restricted to the morphological level. Note, in particular, that zero is the rewrite target of the subtractive. By definition, the zero of the subtractive is distinctive (or else it would not be a subtractive morpheme).

Now let us consider four types of grammar.

Type I of the pattern given by Chomsky (*S.S.*, 26, 27, etc.) is what I prefer to call here an expansion type grammar. I am well aware that Gleason and perhaps others

Model I – Chomsky's [Σ, F] model

Sentence \rightarrow NP + VP

NP \rightarrow T + N

VP \rightarrow Verb + NP

etc.

Sentence

NP + VP

T + N + VP

. . .

The man hit the ball

would rather use the term *reduction* in place of *expansion*. I can also see how the same thing could be called a type of *replacement* grammar. With the addition of rules that can look back to earlier stages of derivation, as well as with one or two other kinds of rule one might even call it a transformational grammar. The reason I use the term expansion is that a recursive rule of the type that might produce the sentence, *I saw the man who saw the man who saw the man who...* and so forth, can also produce still longer utterances. This type starts from a finite set of initial strings and expands to a complex set of terminal strings.

Model II gives what I call a *concrete morphemic maximal reduction* type. Its initial string which may be finite or — in theory at least — infinite in length is composed of real units that can actually occur in the language. This initial string may be reduced

Model II

$$\begin{array}{c}
 \text{a the this very extremely big fat ... man ... was...} \\
 \text{oblig. } \left. \begin{array}{l} \{ (\neq) \text{ a (the), a (this), ... a (...)} \text{ man} \} \\ \updownarrow \\ \{ \text{(a) the} \} \dots \end{array} \right\} \\
 \emptyset \leftarrow \left. \begin{array}{l} \text{oblig.} \\ \text{opt.} \end{array} \right\}
 \end{array}$$

to manageable size by use of the zero replacive (i.e., the subtractive morpheme). No matter how long the string may be, the time required to make a zero form is still zero. As you see from the sample rule, the conditions of zero replacement must be clearly specified so that the output or terminal string fits the minimal requirements for a real utterance. Some uses of zero would be obligatory while others would be optional.

In his report, professor Chomsky cites Dwight Bolinger's article in *Language* (1961, 381), where Bolinger envisions a grammar whose "constructions are not produced one from another or from a stock of abstract components, but filed side by side" and the speakers do not "produce" constructions, but rather "reach for them from a pre-established inventory". Chomsky goes on to say that "it is difficult to comment on the proposal in this form because of the vagueness of the notions 'construction' and 'filed'." Although, as presented here, Model II is reduced by a replacive process — a notion Bolinger rejects — in practice zero replacement comes to nothing more than rejecting under specified conditions part of the total inventory. Perhaps this Model II is similar to what Bolinger had in mind. I really don't know.

One might also characterize the reduction of Type II as slot-class zero. Certain difficulties — perhaps not insurmountable — then arise regarding the specification of the conditions under which zero may occur. One would also have to consider the anomaly, pointed out by William Haas, of contrasting the presence of nothing with the absence of something.

Model III may be called a *maximal abstract reduction* type. The initial string or strings are composed of symbols convertible to real units but which do not themselves

which starts with a fuller form and then reduces it. I must confess that I myself find it difficult to even visualize an infinite model, let alone grant its existence. If, however, the infinite or maximal model is generated by a finite list of rules, replacement zero can reduce this model simultaneously with the expansion. Then one need never in actual fact keep an infinite model in one's head.

This model would provide another theoretical distinction between our angry wife's silence and the silence, say of a chair or a table. To push the example to an absurd conclusion, were the husband to address the furniture with precisely the same words he used to his wife, "Darling, please speak to me", the silence of the chair would not denote its continued anger. Why not? The commonsense answer, of course, is that it is inanimate and cannot think, but, to put it into linguistic terms, because the chair is incapable of generating the model framework in which replacive zero is meaningful, that is, where it contrasts with some overt form.

If the time necessary for the production of zero is zero, then for Model II, the maximal concrete morphological type, the reduction by zero is *parole* and the initial string is *langue*.

Models I and III have *parole* only as their terminal strings, if they have any.

Model IV has the expansion as *langue*, while the reduction — again only if instantaneous — is *parole*.

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SIGNES ET SYMBOLES DANS LES GRANDEURS LES MOINS COMPLEXES DU PLAN DU CONTENU

LEIV FLYDAL

Les phénomènes dont je vais parler concernent deux problèmes essentiels :

- 1) celui de l'interférence des SYMBOLES avec les SIGNES en grammaire, et
- 2) celui des changements linguistiques en général, dans le plan du contenu.

Les sources de mes idées sont notamment Eugenio Coseriu et, davantage encore, la glossématique.

Je me baserai sur des exemples concrets, empruntés en premier lieu au français. Il s'agit d'un certain nombre de morphèmes flexifs à double sens, qui embarassent tant les linguistes que les grammairiens – si on veut bien me permettre de faire cette distinction.

Ceux qui tiennent à ne s'occuper que des signes linguistiques et de leurs éléments constitutifs, éludent la difficulté que présente la description de ces phénomènes, en considérant la double fonction de ces signes comme de simples faits d'usage ou d'emploi, et, par conséquent, comme non pertinents en ce qui concerne le système de la langue.

Ceux, par contre, qui s'estiment dans l'obligation de mentionner et de décrire même les faits qu'ils ne savent guère comment classer, voient le plus souvent dans ces flexifs lorsque ceux-ci ont des contenus difficiles à expliquer, des "variantes stylistiques" (Gougenheim, *Système grammatical de la langue française*), mais des variantes d'autres signes que ceux dont ils représenteraient, suivant les linguistes, les emplois.

D'autres encore (Stolz-Schmalz, *Lateinische Grammatik*, paragraphe 75) qualifient de "Verschiebung" ou "Entwertung" des flexifs, ou même de "Austausch" de leurs contenus les faits en question.

Les notions plus précises et plus techniques de "variantes sémantiques libres et combinatoires" ne semblent guère pouvoir s'appliquer, comme on le verra, à des cas dans lesquels on se trouve en présence non pas d'un, mais de deux contenus coexistants.

Je me proposerai, dans ce qui suit, d'expliquer ces grandeurs comme des signes sur lesquels se sont greffés des symboles, conformément à cette *triple articulation* fondamentale du langage, qui comporte: les grandeurs de l'expression, les grandeurs du contenu, et, dans les deux plans, les symboles, éléments caractéristiques du langage poétique. (Voir le numéro 1.)

1. Rapports entre signe et symboles:

		SIGNE					
		Expression		Contenu			
SYMBOLE	Ex-pression	Substance phonique, graphique, . . . [maître à danser]	Forme	Forme	Substance sémantique "professeur de danse"	Ex-pression	
	Con-tenu	"Onomatopées"		"compas d'épaisseur" "Métaphores"		Con-tenu	

Ex. *maître à danser* comme signe: "professeur de danse".
comme (signe +) symbole: "compas d'épaisseur".

J'entendrai par SIGNES des grandeurs dont le contenu comporte, en plus d'une substance, aussi une FORME linguistique, qui, elle, est en principe analysable en éléments, ou: figures du contenu, et par les SYMBOLES MOTIVES dont il est ici question, des grandeurs dont le contenu ne comporte aucune forme linguistique qui se laisse analyser en ces mêmes éléments, mais repose sur quelque identité sémantique partielle avec la substance sémantique du signe. Dans le symbole, le contenu du signe, contenu premier ou sens propre, ne sert que de face expressive à un contenu second, le sens figuré ou métaphorique.

Les critères dont nous nous servirons pour distinguer entre ces deux contenus, ne seront cependant pas des appréciations de sens subjectives, mais les deux épreuves "intersubjectives" et vérifiables de la commutation et de la substitution.

Le trait qu'ont en commun toutes les paires de phrases qu'on trouvera dans les colonnes de gauche de nos exemples, c'est qu'elles nous présentent des cas dans lesquels il y a entre les deux flexifs qui sont comparés, une commutation parfaite, et facile à constater expérimentalement.

Dans les colonnes de droite on retrouvera les mêmes paires de flexifs, mais ici dans des contextes dans lesquels une épreuve expérimentale donnera un résultat différent: les uns diront que, évidemment, les deux formes sont commutables; *avouera* et *avoue*, par exemple, ne signifient tout de même pas la même chose; les autres, par contre, soutiendront qu'ici la première des deux grandeurs dont il s'agit, n'a pas son vrai sens, pas sa vraie fonction, et qu'entre *avouera* et *avoue* il y a dans ce cas essentiellement un rapport non pas de commutation (c.-à-d. d'in-interchangeabilité), mais de substitution (c.-à-d. d'interchangeabilité), la différence de sens qui les sépare étant sans importance appréciable.

Au lieu de ces réponses par oui ou par non, il va falloir répondre, me semble-t-il, que dans toutes les phrases qu'on trouvera pour chaque numéro à droite et en haut, on a affaire à deux contenus différents, dont l'un est absolument in-interchangeable et

2. Quand tu <i>va-s</i> te marier, . . . Quand tu <i>vien-s</i> te marier, . . . (Commutation entre signes)	Quand tu <i>VA-s aller</i> te marier, . . . Quand (bientôt) tu <i>i-R-as</i> te marier, . . . (Substitution de la métaphore <i>va-</i> + infinitif au signe <i>-r-</i> de la futurition)
<i>Flexifs verbaux</i>	
3. Je vous avou <i>erai</i> (demain) que . . . Je vous avou <i>e</i> (auj.) que . . . (signes commutables)	Je vous avou <i>erai</i> que je ne parle votre langue qu'avec difficulté = Je vous avou <i>e</i> (volontiers) que . . . (2 contenus de <i>-rai</i> : avec celui du signe il y a commutation, avec celui du symbole il n'y a pas comm. avec le présent)
4. Le présent historique.	
5. Imparfait pour présent: Je ven <i>ais</i> (hier) pour vous dire . . . Je vi <i>ens</i> pour vous dire . . . (signes commutables)	Je ven <i>ais</i> pour vous dire que je vais partir = Je vi <i>ens</i> pour vous dire que je vais partir (comme signe: commutable; comme symbole: non commutable avec le présent)
6. Présent pour futur par substitution (métaphore): Aussitôt terminé, je <i>pars</i> !	
7. Présent pour futur par extension (syncrétisme): <i>Si</i> , demain, je <i>pars</i> , . . .	

l'autre facilement interchangeable avec la grandeur correspondante de la phrase suivante, avec laquelle elle fait paire.

Comme les rapports d'identité sémantique partielle qui relie un sens propre à un sens figuré, ou, plus précisément, les substances sémantiques du signe et de sa métaphore, sont souvent moins nettement discernables dès qu'on descend au-dessous de la dimension du mot, je vais me servir, en guise d'illustration, d'une périphrase courante dans différentes langues, pour expliquer le procédé que j'appellerai la symbolisation du contenu ou, plus brièvement, mais moins bien, la métaphorisation. (Voir le numéro 2.)

Ce qui, dans toutes les colonnes de gauche est commutable, ce sont deux signes. Dans le cas du numéro 2, ces signes sont des mots, plus précisément les radicaux d'un paradigme qui compte un assez grand nombre de verbes.

Dans la colonne de droite, le contexte formé par l'infinitif *aller* fait sortir le mot *vas*, en le défectivisant, du paradigme mentionné. La preuve nous est fournie par le fait que le rôle paradigmatique de *vas* ne peut plus ici être rempli par le mot *viens*. Avec le contenu second qu'il a acquis grâce à ce contexte, *vas* entre dès maintenant, comme membre affilié, dans le paradigme auquel appartient le flexif *-r-* de la futurition. La preuve, c'est qu'avec ce contenu second ou métaphorique, il y a entre ces deux grandeurs un rapport d'interchangeabilité presque parfait. La substitution parfaite, au sens strict de ce terme, est possible uniquement entre grandeurs du même ordre, comme par exemple entre deux signes, et non entre grandeurs d'ordre différent, comme le signe (ici: *-r-*) et la métaphore (ici exprimée par *va-s* suivi de l'infinitif), étant donné

<i>Flexifs verbaux</i>	
Mode:	
8. Indicatif pour subjonctif: S'il <i>avait</i> bien travaillé, . . . = Quand il <i>avait</i> bien travaillé, . . . (signe commutable avec <i>eût</i>)	S'il <i>avait</i> bien travaillé, . . . = S'il <i>eût</i> bien travaillé, . . . (<i>avait</i> comme symbole, non commut. avec <i>eût</i>)
9. Indicatif pour conditionnel: S'il <i>travaillait</i> , . . . = Quand il <i>travaillait</i> , . . . (signe commutable avec le condit.)	S'il <i>travaillait</i> , . . . = S'il <i>travaillerait</i> , . . . (employé en style pop.) (imparfait comme symbole, non commutable avec condit.)
10. Imparfait ind. pour condit. passé: Parmi eux j' <i>étais</i> perdu. Parmi eux j' <i>aurais été</i> perdu. (<i>étais</i> commutable avec <i>aurais été</i>)	Sans lui, j' <i>étais</i> perdu. = Sans lui, j' <i>aurais été</i> perdu. (<i>étais</i> comme symbole, substituable au condit.)
Aspect:	
11. Thème verbal ponctuel pour thème non-ponctuel: il <i>mourut</i> il <i>agonisa</i> (<i>mour-</i> commutable avec <i>agonis-</i>) Cf. "Imperfectum de conatu".	il <i>mourait</i> = il <i>agonisait</i> (<i>mour-</i> symbole pour <i>agonis-</i>)
Nombre (cf. 15):	
12. Pluriel pour singulier par courtoisie.	
Personne:	
13. 3ième pour 2ième personne par courtoisie. Personne définie pour personne non définie (Ita ne facias. – <i>vous</i> dat. pour <i>on</i> au nom.) lère personne pluriel pour 2ième personne plur. ou sing. (<i>Allons !</i> pour <i>Va !</i>)	

que celle-ci, participant déjà à la nature du signe, présuppose non pas *un* contenu, mais la coexistence de deux contenus.

L'épreuve de la commutation nous sert donc à constater un rapport entre signes, l'épreuve de la substitution un rapport entre le contenu d'un symbole et celui d'un autre signe que celui sur lequel s'est greffé ce symbole.

La coexistence de deux contenus conventionalisés, celui du signe et celui de la métaphore que lui impose le contexte, a été reconnue non seulement pour les signes de grandes dimensions, comme le roman et la fable, par exemple, mais aussi entre autres pour les éléments des mots composés. (Ex. Le dernier élément de *Fledermaus* – mais non pas celui de *Waldmaus* – est, dans l'état de langue actuel, un tel signe-symbole.) En ce qui concerne les flexifs, cette coexistence est également reconnue pour certains emplois dans lesquels les deux fonctions et, par conséquent, les deux contenus se laissent constater d'une façon particulièrement nette et incontestable. (Moritz Regula, dans sa *Grammaire française explicative*, et sporadiquement encore d'autres auteurs, ont même vu qu'il s'agit d'emplois métaphoriques.) C'est le cas notamment de certains des flexifs verbaux de notre liste, par exemple ceux des numéros 3,

Flexifs verbaux:

Diathèse:

Actif pour passif:

- | | |
|--|---|
| 14. The salesman <i>sells</i>
The slave <i>is sold</i>
(Flexif de diathèse commutable
avec <i>is</i> + participe passé) | This article <i>sells</i>
=This article <i>is sold</i>
(Flexif de diathèse en emploi symbolique,
non commut. avec <i>is</i> + participe passé) |
|--|---|

Flexifs nominaux:

Nombre (cf. 12; souvent manifesté dans l'expression des verbes):

15. Pluriel pour singulier par courtoisie (
- vous*
- pour
- tu*
-)

Genre:

- 16a. Masculin pour féminin par substitution: Ah! Suzon, vous êtes une bath copine,
- mon petit*
- .

- 16b. Masculin pour féminin par extension:

un <i>docteur</i> une <i>doctoresse</i> (belge) (flexif masculin en rapport d'exclusion avec le féminin)	une femme <i>docteur</i> (français) =(une <i>doctoresse</i>) (flexif masculin en rapport de participation avec le fém.)
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Article:

17. Emploi dit "générique":

<i>The dog is coming</i> <i>A dog is coming</i> (<i>The</i> commutable avec l'art. indéfini)	<i>The dogs are useful animals</i> = <i>Dogs are useful animals</i> (<i>The</i> non comm. avec l'art. indéf. zéro)
---	---

Comparaison:

- 18a. Métaphorisation libre du flexif:

Comparatif absolu (et Superlatif absolu) <i>längere Zeit</i> (als . . .) <i>lange Zeit</i> comparatif commut. avec positif)	<i>längere Zeit</i> = <i>lange Zeit</i> (comparatif en contexte de positif)
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- 18b. "Métaphorisation structurelle" du thème adjectival extensif, provoquée par le flexif:

Es ist ein bisschen <i>WÄRMER</i> als gestern (da es ja auch sehr <i>warm</i> war) (Thème adjectival extensif ne subissant aucune altération de son sens propre)	Es ist ein bisschen <i>wärmer</i> als gestern (da es ja ausserordentlich <i>kalt</i> war) (Thème syncrétisant les contenus des termes extensif - <i>warm</i> - et intensif - <i>kalt</i>)
---	---

(Cf. la comparaison du terme intensif:

Es ist *kälter* als gestern
(da es ja auch sehr *kalt* war).

4 et 5: le présent historique, le futur dit de politesse, et l'imparfait pour le présent.

Parmi les emplois à propos desquels cette coexistence de deux contenus est le plus souvent signalée et commentée, il faut mentionner encore ceux cités aux numéros 10, 12 et 13: l'imparfait indicatif pour le conditionnel passé, la 3^{ème} pour la 2^{ème} personne par courtoisie, et les pluriels de politesse.

Mais il m'a fallu ranger, comme on le verra, dans cette même catégorie des "flexifs

à double sens”, encore d’autres phénomènes – et peut-être moins incontestablement symboliques, ceux-là – parce qu’ils rentraient d’eux-mêmes dans cette catégorie lorsqu’on les soumettait aux deux épreuves de la commutation et de la substitution. Il s’agit notamment des numéros 8 et 9, 14, 17 et 18a: le comparatif et le superlatif absolus, l’“article générique”, la diathèse irrégulière, et l’imparfait pour indiquer l’irréel dans le présent.

La technique toute simple et imparfaite – il faut bien le reconnaître – que j’ai adoptée, révèle bien, dans ce dernier cas, par exemple, une duplicité de sens, mais incapable de prouver la coexistence simultanée, dans l’esprit de l’homme, des deux contenus en question, elle doit se borner à constater la possibilité de cette coexistence, l’existence des conditions nécessaires à la perception de deux contenus simultanés ou alternants, suivant la nature du contexte.

Ces conditions sont données, précisons-le, d’une part, comme dans les colonnes de gauche de notre feuille, par la fonction de signe des grandeurs en question, et à ce titre par les relations très solidement établies qui existent entre les signes dans leur qualité de membres d’un système paradigmatique.

D’autre part, pour la perception d’un autre contenu, d’autres conditions, plus ou moins impérieuses celles-là, et en rivalité avec les premières, sont fournies par le contexte.

Celui-ci de quelle nature est-il dans les cas qui nous occupent? Il va sans dire qu’il est essentiellement d’un caractère social, mais extralinguistique dans le cas des “formes de politesse”, comme on les appelle parfois (v. nos. 3, 5, 13, 12 et 15), où c’est tantôt la personne et le nombre qui sont employés métaphoriquement et tantôt les flexifs temporels, et ceux-ci ou bien pour faire reculer, fictivement, le moment présent dans l’avenir ou dans le passé ou bien pour faire ressembler le passé ou l’avenir au moment présent en mettant dans un cadre contextuel fait pour les flexifs du futur ou du prétérit, le flexif du présent. C’est cette dernière chose qui se produit dans les cas du présent historique et du présent employé comme ce qu’on a appelé une “variante stylistique du futur” (numéro 6) (cf. Gougenheim, *loc. cit.*).

Cette substitution par laquelle le présent prend la place du futur, est, on le sait, particulièrement fréquente dans des langues qui, comme le français, possèdent ces deux formes temporelles.

Il faut remarquer l’identité du résultat que produisent d’une part cette métaphorisation librement choisie par le sujet parlant, et d’autre part le syncrétisme du futur et du présent qu’un contexte d’une autre nature, purement linguistique, à savoir le système de fonctionnement de la langue, impose au sujet parlant, sous la forme du présent, dans les propositions introduites par le *si* conditionnel. (Voir l’exemple numéro 7.)

Ce syncrétisme, qu’on pourrait en effet considérer comme une “variante combinatoire” du futur (cf. au numéro 3, la prétendue “variante stylistique” du présent), est une expression du fait que par rapport au futur, terme intensif, le présent est le terme extensif dans le système sémantique partiel que constituent ces deux formes.

Le fait qu'un terme est sémantiquement extensif veut dire que ce terme est susceptible de changer, sous certaines conditions, de contenu: par une extension du contenu qui lui est propre, il arrive à embrasser aussi un contenu second, qu'il aura alors en commun avec un autre signe. Il s'opère donc dans un tel signe extensif – ici, dans le flexif du présent – un changement de sens à l'intérieur même du signe, et – élargissons la perspective – à l'intérieur même de l'état de langue en question. Un signe qui constitue le terme extensif dans une opposition, est donc un signe doté d'une certaine changeabilité de par sa nature même; autrement dit, *l'extensivité constitutionnelle d'un signe représente un principe de changement inhérent à sa structure*. (Cf. Coseriu, *Sincronía, diacronía y historia*, p. 154 et ailleurs.) Et, répétons-le, cette espèce particulière de changement est, comme on le voit, un fait de fonctionnement, qui, loin de rester extérieur au système de langue en question, en constitue, au contraire, une partie intégrante.

Contrairement à la métaphore proprement dite, qui est le résultat d'une substitution, ce transfert de contenu d'une case sémantique dans un autre à l'intérieur d'un tel signe est une espèce de *μετάφορα* par extension, consentie par la structure de signe et imposée, dans des cas déterminés, par le contexte. Il paraît peu vraisemblable qu'il n'y ait aucun lien, aucun rapport, entre d'un côté ce transfert de sens par extension, cette espèce de "métaphorisation" structurellement conditionnée et systématiquement imposée, et, de l'autre côté, la métaphorisation proprement dite et librement choisie de ce même flexif, c'est-à-dire, la métaphorisation par substitution. Logiquement la substitution métaphorique n'est qu'une des variétés de l'extension de sens. Et les deux opérations conduisent bien, on l'a déjà dit, au même résultat concret: à un présent tenant lieu de futur.

Au numéro 16 les flexifs nominaux nous offrent pour ces deux espèces de transferts de sens des exemples analogues à ceux dont nous venons de parler: d'abord, sous a), le flexif du masculin introduit par substitution dans un cadre contextuel qui demanderait le féminin, et ensuite, sous b), les deux solutions choisies et adoptées respectivement en France et en Belgique (Hanse, *Dict. d. diff. gram.*, Bruxelles, 1949, p. 255) pour résoudre un problème que soumettait aux sujets parlants un nouveau contexte social. L'avènement des femmes à la dignité de docteur a conditionné 1) en Belgique, le remplissage, par le moyen d'un emprunt (*-oressse*), de la case sémantique que laissait vide un suffixe dont on répugnait à étendre le domaine, et 2) en France, le remplissage de la même case par l'expédient qui consistait à rendre ce même suffixe extensif.

La communauté linguistique française semble n'accepter qu'à demi ce néologisme (*une femme docteur*), parce qu'on tient, comme en Belgique, à n'associer le suffixe *-eur* qu'avec le genre masculin. Mais il ne doit peut-être guère nous étonner que le contenu premier puisse ne pas disparaître, mais subsister à côté du contenu élargi dans un syncrétisme ou une métaphore (?) d'une création si récente qu'il ou elle n'a pas encore eu le temps d'être adopté par des cercles bien larges de la société. La conclusion semble devoir être qu'il y a entre la métaphore et le syncrétisme des oscillations. Ce qui est confirmé par le fait qu'il arrive que même des syncrétismes bien solides et bien con-

sacrés par l'usage soient dissous par le besoin d'explicitement la notion qui est étouffée par ce syncrétisme. On rencontre même, par exemple, le futur simple après le *si* conditionnel chez quelques écrivains audacieux (Hugo, par exemple, d'après Brunot, *La Pensée et la Langue*).

Par ce qui a été dit plus haut sur la coexistence, dans la métaphore, de deux contenus de nature différente, s'explique déjà pourquoi sont restés hésitants et infructueux les efforts de tous ceux qui ont cherché à déterminer dans les termes d'une alternative, si le comparatif et le superlatif dans leurs emplois absolus gardent ou non leur contenu premier. (Voir le numéro 18a.)

On trouve aussi une solution au problème que posent ces emplois, si l'on reconnaît que, dans le domaine sémantique, toute fonction est solidaire d'un certain contenu. Fonction double implique donc contenu double. Dès que l'on sort le flexif du comparatif du contexte qui lui est particulier (celui dans lequel il est lié à *quam* ou à l'ablatif en latin) pour le transposer dans celui d'un positif, on obtient donc forcément que le contenu du flexif se double d'une fonction et par là d'un contenu supplémentaires.

Quant au rapport non pas historique, mais génétique entre ces deux contenus, il est évident que, dans les langues qui le possèdent, comme l'allemand et le latin, le contenu second du comparatif absolu n'a d'existence que grâce au comparatif relatif. Celui-ci engendre parfois, mais pas bien souvent, celui-là, et cela par le jeu normal de la métaphorisation. La création ne se fait pas en sens inverse: on ne laisse pas entrer dans le paradigme de la comparaison des signes qui n'ont pas les formes qu'il faut pour y avoir accès.

Aussi les comparatifs comme les superlatifs absolus, les trouve-t-on surtout dans les textes artistiquement élaborés ou dans des formules qui prennent vite un caractère conventionnel ou locutionnel.

Si le contenu du comparatif et du superlatif absolus a été tant discuté et commenté, c'est, je crois, aussi en partie parce qu'on n'a pas toujours avec suffisamment de netteté distingué entre le contenu des flexifs et celui des radicaux. Il est d'autant plus important de faire cette distinction qu'il existe aussi, à côté de la métaphorisation libre et proprement dite du flexif, une "métaphorisation structurelle" par laquelle ce n'est pas le flexif qui est affecté, mais un thème adjectival extensif. (Voir le numéro 18b.) Cette extension du contenu du radical s'opère automatiquement, par la seule influence du flexif. C'est donc, autrement dit, celui-ci qui agit comme contexte, et c'est le radical d'un adjectif qui, dans une paire d'opposition, constitue le terme extensif, qui a son domaine sémantique étendu.

D'une façon analogue, ce n'est pas le flexif, mais le radical des verbes à aspect ponctuel (comme *mourir*, voir le numéro 11) qui subit une extension de sens lorsque ce radical est mis dans le cadre contextuel constitué par le flexif de l'aspect non-ponctuel que représente l'imparfait.

Pour dire en quoi ces observations concernent les systèmes grammaticaux et leur description, résumons notre pensée:

Vu que les systèmes ne se manifestent qu'à travers quelque substance – substance du contenu ou substance de l'expression –, et que ces substances sont dotées, elles aussi, d'un certain pouvoir d'“information”, les flexifs, dont se composent les systèmes grammaticaux, ont souvent, comme d'autres signes, deux contenus de nature différente, dont c'est tantôt l'un, tantôt l'autre qui prévaut dans le texte. Etant donné la haute fréquence d'emploi des signes grammaticaux, leurs contenus symboliques sont particulièrement enclins à se conventionaliser et à devenir par là, à l'intérieur des systèmes que forment ces signes, des pièces parfois très importantes, constituant quelquefois des prolongements du système des signes et quelquefois aussi des systèmes partiels qui, avec le temps, sont préférés à ceux avec lesquels ils font double emploi.

Cette opération d'enrichissement des inventaires grammaticaux qu'est la métaphorisation, est comme l'emprunt et comme l'économie des moyens mis en œuvre, de tous les temps et de tous les climats.

*Norges Handelshøyskole
Bergen*

DISCUSSION

KURYLOWICZ:

Tout en félicitant M. Flydal de son importante communication, je voudrais poser ici une question de terminologie. Ce qui a été entendu ici par *signe* et *symbole* a été en effet appelé par moi depuis plus d'un quart de siècle *fonction primaire* et *fonction secondaire* du signe. La fonction primaire du signe est sa valeur, donnée par les oppositions pertinentes à l'intérieur du système. Les fonctions secondaires proviennent de l'action du contexte exercée sur la fonction primaire. Déjà Bühler dans sa *Sprachtheorie* (1934) a distingué entre *systembedingt* et *feldbedingt (kontextbedingt)*. Tout comme en phonologie on parle d'alternances de phonèmes (chose différente de variantes phonétiques!), de même en morphologie on peut parler d'une alternance de signes conditionnée par le contexte. Je préférerais donc parler ici de fonctions sémantiques hiérarchisées d'un seul signe que d'employer le terme *symbole* nécessaire autre part et prêtant à des malentendus.

Pour illustrer mes propres vues sur ce sujet je ne citerai qu'un seul exemple emprunté à la communication de M. Flydal. Tous ces exemples peuvent être réduits à une espèce de schéma triangulaire :



Entre A et A' il y a un rapport de fonction primaire à fonction secondaire mais entre

B et A' il y a un rapport de synonymie. Le modèle triangulaire symbolisant le syncrétisme figure dans un article sur le sens du mot publié en russe dans les *Voprosy Jazykoznanija* en 1955.

Noter qu'à l'étape A' il peut y avoir des variantes facultatives (stylistiques), p.ex. *s'il travaillait il gagnerait*, mais aussi) dans le langage populaire, *s'il travaillerait, il gagnerait*.

FLYDAL:

J'ai l'impression que M. Kuryłowicz n'appellerait pas "variante combinatoire" les cas dans lesquels, comme dans "maître à danser", on se trouve en présence de deux contenus simultanément coexistants dont celui qui prévaut n'est pas analysable en éléments (figures) de contenu.

UNE CATÉGORIE VERBALE: LE FUTUR DU PASSÉ

JOAQUIM MATTOSO CAMARA, JR.

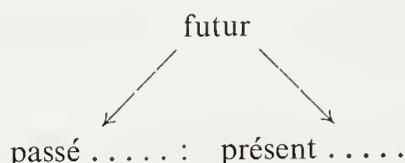
La grammaire comparative des langues romanes a souvent négligé la complexion sémantique des formes qui se sont développées, à partir du latin vulgaire impérial, au moyen d'une agglutination d'un infinitif avec l'imparfait ou le parfait d' *habere* – *cantare habebam*, d'où fr. *chanterais*, port. esp. *cantaria*, *cantare habui*, d'où it. *canterei*. On n'y a vu presque toujours que la création d'un mode conditionnel, dont les formes se sont substituées au subjonctif dans l'apodose d'un syntagme de condition. Quelques fois on a relevé – c'est vrai – une autre fonction de ces nouvelles formes verbales dans une oration complétive subordonnée à un verbe *dicendi* au passé: "il nous a dit qu'il viendrait nous voir"; mais on les y a interprétées le plus souvent comme un emploi à part, qui s'ajoute secondairement à leur fonction fondamentale de conditionnel. C'est, par exemple, la façon de voir de Brunot en français et d'Epifanio Dias en portugais. D'autres, au contraire, comme Andrés Bello en espagnol et Manuel Said Ali en portugais, ont parti de cet emploi dans les orations complétives et ont considéré les nouvelles formes romanes comme une sorte de futur "du passé", sans pousser pourtant trop loin leur thèse dans toutes ses implications.

Le but de cette communication est d'approfondir et de mieux établir ce dernier point de vue, en montrant qu'il s'agit en effet, bel et bien, d'un „futur du passé” et que l'apparition des nouvelles formes romanes marque la reconnaissance nette, dans le système grammatical, d'une catégorie verbale qu'on entrevoit en latin et ailleurs dans le domaine indo-européen.

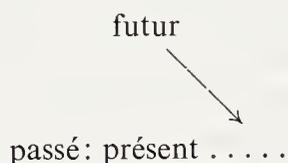
Pour bien le comprendre, il faut tout d'abord envisager la catégorie du futur comme située sur un plan significatif et fonctionnel tout autre que le présent et le passé.

Le présent et le passé sont dans la grammaire des langues indo-européennes une opposition binaire de formes non-marquées quant au passé et formes avec la marque du passé. Le soi-disant présent recouvre ainsi tout le champs temporel à l'exclusion du passé et sert tout naturellement à l'expression du futur.

Cet état de choses, qui date de l'indo-européen commun, se prolonge jusqu'aujourd'hui dans les langues indo-européennes: fr. "je pars demain", etc. À son côté, pourtant, il s'est développé dès la plus remote antiquité une catégorie de futur, qui n'a pas déterminé malgré tout une reformulation de la dichotomie passé-présent. Au lieu d'une triade passé-présent-futur, on a eu un futur sur un nouveau plan, superposé au plan passé-présent:



La catégorie du futur peut ainsi se projeter dans le passé tout aussi bien que dans le présent. Il y a un futur en relation avec un moment du passé qu'on évoque (comme dans la phrase – "il a dit qu'il viendrait", où l'on évoque le moment du passé où il a fait la promesse de venir), tout aussi bien qu'un futur en relation avec le moment où l'on parle ("il annonce qu'il viendra vous voir"). Quand il manque dans le système des temps verbaux la projection du futur dans le passé, on a une case vide avec le schéma suivant:

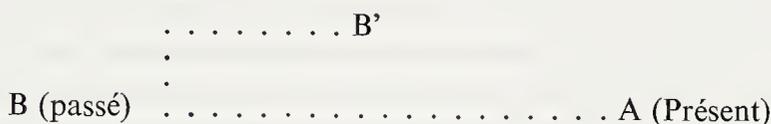


On peut se demander pourquoi on rencontre souvent ce dernier schéma au lieu d'un système symétrique complet avec un passé et un présent sur le plan du futur en corrélation avec l'opposition du plan primaire passé-présent.

En effet, le futur du passé, au sens strict, ou n'apparaît qu'au moyen de constructions en marge du système verbal fondamental ou, tout en entrant dans ce dernier système, y reste d'un usage rare et même un peu sporadique. C'est ainsi qu'on décèle en latin un futur "dans le passé" au moyen d'une locution de l'imparfait d' *esse* avec un participe du futur. Cfr. ce que nous dit Madvig, qui a même cherché un peu maladroitement à introduire cette périphrase dans le cadre général des temps verbaux en latin (*Gram. trad. Epifanio*, p. 270). De la même façon on a un futur du passé en grec au moyen de l'imparfait de *mellō* avec un infinitif: "Toùs ésploùs kleïsen émellon" (Thucydides). Cela veut dire que le latin et le grec, au moyen de formes periphrastiques, ont sporadiquement introduit dans l'expression des temps verbaux une catégorie de futur pourvue d'une opposition passé-présent (lat. *venturus sum: venturus eram*, gr. *kleïsen mellō: kleïsen émellon*).

Le sanskrit et les langues romanes ont fini pour placer un futur du passé dans le cadre même des temps simples. Le sanskrit, comme nous dit Whitney, "from the future-stem" a fait "an augment-preterit, by prefixing the augment and adding the secondary endings, in precisely the same manner as an imperfect from a present-stem in *a*" (*Gram.*, 334). Les langues romanes sont arrivées au même résultat quand l'ancien futur latin a disparu et à sa place on a développé un nouveau futur avec l'auxiliaire *habere*, au présent pour le futur du présent, et au parfait ou à l'imparfait pour le futur du passé. Mais l'emploi du futur du passé en sanskrit est remarquablement rare et dans les langues romanes l'emploi de la nouvelle forme dans le syntagme conditionnel a de beaucoup dépassé les phrases à futur du passé strict.

La raison de cette divergence entre le peu d'usage du futur du passé et son encadrement bien défini dans le système doit être cherchée dans le fait que la visualisation d'un moment passé comme futur ne s'impose que dans des situations linguistiques particulières. Le plus souvent tous les moments du passé ne sont que du passé tout court en relation avec le moment où l'on parle. Pour qu'il se crée le besoin d'un futur dans le passé il faut que le sujet parlant, en se rapportant au passé, s'y place par une sorte d'évocation (selon le mot de M. Togeby) et dans une ubiquité temporelle envisage de ce moment passé un autre moment qui lui est postérieur mais encore passé en relation au moment où l'on parle:



En dehors des orations complétives subordonnées à un verbe *dicendi* on rencontre peu de cas où l'on puisse sentir le besoin de cette évocation. Hereulano dans son roman historique *Eurico* nous en donne un exemple, quand il se place avec nous sur le champ de bataille de Chryssus pour contempler le cadavre du roi wisigoth vaincu: "Fugiam; Roderico, porém, estava aí! mas retalhado de golpes, mas sem vida. Já não seria debaixo de seus pés que o trono de Espanha se *desfaria* aos golpes dos machados dos árabes" (*Eur.*, 120).

Une fois ainsi compris le rôle du futur du passé dans le système des temps verbaux, il faut faire un pas en avant pour bien saisir l'introduction d'une catégorie de futur au-dessus de la dichotomie passé-présent. Le fait relève des nuances modales qui colorent l'idée d'un temps futur. L'analyse des formes de futur dans les langues indo-européennes le montre très clairement. Ce sont toujours les mêmes procès sémantiques qui se répètent depuis les langues anciennes classiques jusqu'à nos langues modernes. On a des formes desideratives en sanskrit, en grec et en latin archaïque (type *faxo*), des formes subjunctives (lat. *ero, legam*), des formes latines en *-bo* avec l'idée initiale de "devenir", tout comme le futur allemand avec *werden*; ou l'on a l'idée de volonté, d'intention, d'obligation etc. en grec byzantin, en roumain, en gothique et enfin en latin vulgaire imperial avec l'auxiliaire *habere* plus un infinitif. Ce sont en effet les nuances modales qui ont favorisé la consolidation de ces dernières périphrases tout comme l'emploi du participe futur avec *esse* (et du participe présent avec *Vas* en sanskrit).

Cela ne veut pas dire évidemment qu'on n'aie pas eu à la fin un futur de nature exclusivement temporelle. La parole oscille entre les deux pôles d'une expression psychique très large et d'un raisonnement sec et strictement logique. C'est ce dernier qui a le plus de poids dans certains styles très intellectualisés où l'on a affaire à l'information pure. On voit alors se développer un futur strictement temporel avec des formes qui sont sorties d'implications modales. Par contre, ces formes deviennent trop faibles pour le *sermo cotidianus* chargé d'intentions expressives et y cèdent la place à des nouvelles formes à coloration modale. Que tel a été le cas avec le futur latin

on le voit très nettement dans le fameux passage de Fredegair, qui témoigne l'existence du nouveau futur roman déjà comme un temps simple: la ferme réponse de Justinien avec ce nouveau futur (*daras*), exprimant la volonté impérieuse d'un vainqueur, s'oppose au futur latin (*non dabo*), qu'emploie le roi des Parthes dans sa timide parole de vaincu. "Et ille respondebat – 'Non dabo'. Iustinianus dicebat – 'Daras'".

On peut même avancer qu'à côté de leur emploi comme futur (à coloration modale ou non) les formes du futur acquièrent un sens tout à fait modal qui vaut comme un subjonctif. Dans cette nouvelle fonction le futur roman à auxiliaire au présent s'oppose au futur du passé (à auxiliaire à l'imparfait ou au parfait) sur un plan strictement modal: le futur du présent exprime le doute ou l'incertitude, tandis que le futur du passé se rapporte à ce qui était douteux et incertain au moment du passé qu'on évoque et maintenant se montre comme pas accompli. C'est cet emploi qui est à la base du soi-disant conditionnel. En effet ce qui explique la présence des formes du futur du passé dans l'apodose d'un syntagme conditionnel est le fait d'une hypothèse placée hors de la réalité. La valeur sémantique du soi-disant conditionnel est l'irréalité en opposition à l'incertitude du futur du présent et à la certitude du présent. C'est ainsi que *seria* ("serait") dans un passage d'Herculano suggère que personne n'a encore osé soutenir que got. *gards* veut dire "administrateur des palais royaux": "opinião que seria muito difícil de sustentar à vista dos vários monumentos hispano-góticos" (*Eur.*, 65). S'il admettait qu'il se pourrait bien qu'une telle opinion aie été avancée, il nous dirait: "opinião que *será* muito difícil de sustentar". Et si enfin il ne voulait que se refuser à accepter une opinion déjà émise, il nous dirait: "opinião que é muito difícil de sustentar".

La prothèse conditionnante qui accompagne souvent l'emploi modal des formes du futur du passé n'est pas un trait caractéristique de ces formes verbales, mais plutôt du futur au sens large. L'incertitude qui adhère à l'idée de futur apporte toujours, quoique souvent d'une manière implicite, la possibilité d'une condition pour son accomplissement. Brunot a tort de défendre la dénomination de conditionnel même pour l'emploi strictement temporel du futur du passé sous le prétexte que dans les phrases à verbe *dicendi* on peut aussi avoir affaire à une condition: "Elle disait qu'elle accepterait l'enfant en pension si on lui payait un franc par jour" (*La Pensée et la Langue*, 56). On pourrait avoir aussi bien la même condition avec *acceptera* ("Elle dit qu'elle acceptera l'enfant en pension si on lui paye un franc par jour"), parce que la condition, en dernière analyse, relève de l'idée du futur. Tout ce qu'on peut avancer là-dessus est qu'au cas d'une hypothèse pas accomplie l'explicitation de la condition se fait plus imperative. Dans un texte sanskrit de l'Aitareya-Brahmana on lit avec le futur du passé – *çatayum gam akarisyam* "je ferais la vache (vivre) cent ans"; mais dans le Gopatha-Brahmana il y a le développement d'une prothèse conditionnante – "si tu n'avait pas fermé ma bouche, vilain". Il ne faut pas oublier, cependant, les cas nombreux dans les langues romanes où le soi-disant conditionnel apparaît sans prothèse conditionnante et on ne parvient au syntagme conditionnel qu'en se secourant des ellipses les plus spacieuses.

On arrive ainsi à la conclusion que les formes romanes sorties d'un infinitif avec l'imparfait ou le parfait d' *habere* ne sont que des formes d'un futur dans le passé, lequel, comme il arrive au futur au sens large peut se placer sur un plan purement modal – celui de l'hypothèse pas accomplie. Et dans cet emploi modal ces formes s'opposent symétriquement à celles du futur dans le présent.

Il ne nous reste qu'à dégager une association très vive entre le futur du passé et l'imparfait. Souvent (en portugais, par exemple, d'une façon très nette) l'imparfait se substitue au futur du passé dans tous ses emplois. Dans la fonction temporelle stricte on n'y a que la contrepartie de l'emploi du présent au sens de futur pour le moment où l'on parle: "anuncia que vem" (*vem* "vient" au lieu de *virá* "viendra"), "anunciou que vinha" (*vinha* "venait" au lieu de *viria* "viendrait"). Dans l'emploi modal l'imparfait passe à exprimer l'hypothèse pas accomplie. On décèle cet usage dès le latin: "Quid magis his rebus poterat mirabile dici?" (Lucrèce) – At ille ait: Libenter te, sanctissime pater, *audebam* sed. . ." (Gregoire de Tours). En portugais le *sermo cotidianus* a développé cet emploi modal de l'imparfait d'une façon systématique. On ne peut pas dire que le futur du passé soit délaissé et ne se trouve que dans la langue littéraire. Dans tous les styles les deux formes modales vivent côte à côte et il y a entre l'une et l'autre une opposition d'ordre plutôt expressive: l'imparfait, en supprimant la projection de l'événement sur le plan d'un futur explicite, rapproche l'hypothèse du moment où l'on l'a faite en la rendant plus vive. Il y a là-dessus un joli passage de l'écrivain brésilien Machado de Assis, quand il imagine dans une boutade l'écroulement des bâtiments d'une fabrique: "Terrível, minha senhora? Não nego que fôsse feio, mas o mal seria muito menor que o bem. Perdão; não gesticule antes de ouvrir até o fim. . . Repito que o bem compensaria o mal". Et après l'emploi du futur du passé – *seria* "serait", *compensaria* "compenserait", il passe à l'imparfait pour nous faire voir l'écroulement, avec les formes *morria* "mourait", *havia* "il y avait", *era* "était": "Imagine que morria gente, que havia pernas esmigalhadas, ventre estripados, crânios arrebetados, lágrimas, gritos, viúvas, órfãos, angústias, desesperos. . . Era triste, mas que comoção pública! Que assunto fértil para três dias!" (*A Semana*, ed. M. de Alencar, 75).

Nous pouvons ainsi dégager dans les langues romanes le concept d'un futur du passé dans le cadre même des temps verbaux simples et en stricte corrélation avec ce qu'on appelle le futur tout court. Comme dans celui-ci, l'emploi modal n'est qu'un corollaire des implications modales qui adhèrent au futur. Et on voit au même temps que les valeurs sémantiques dès le latin, et voire le sanskrit, n'ont pas changé d'une manière substantielle. On y trouve toujours une dichotomie du futur en cherchant à s'établir dans le cadre des temps verbaux sans changer la dichotomie passé-présent mais en se superposant à elle et transposée souvent à un cadre purement modal de l'incertitude en face de l'irréalité.

CAN AND MAY, A PROBLEM OF MULTIPLE MEANING

YVAN LEBRUN

“The mechanical translation point of view”, B. Ulvestad writes,¹ “will compel linguists to examine in detail problems that have hitherto been regarded as trivial or inconsequential.” And somewhere else in the same article: “Traditional grammar is normally eclectic and vaguely formulated, and it often tends to overgeneralize or fails to state the range of validity for its rules.”

You need only try to design a machine program for translating such everyday words as English *can* and *may* to realize fully how many lexical questions have hitherto been considered trifling and not worth bothering about, and to see how often grammatical descriptions strain linguistic data.

If you try to make up a detailed list of the present-day meanings and uses of *can* and *may*, you will soon find that available dictionaries, even the best ones, are of little help: they account for only some — and not always the more frequent — senses or they dispose of the words by saying that they denote “possibility” or “contingency”, thus substituting one polysemy for another and explaining nothing.

If we turn to grammars and handbooks on English usage, we shall soon come across assertions that are at variance with the facts. Thus, in a recent, and in many respects quite remarkable, American grammar, one may read: “*Might, could, should, must, ought*, do not, in Present-day English, carry past tense meanings; all of them, when joined with the infinitive form of the verb, express a present or even a look towards the future. . . . To refer to the past these words are joined with the past participle form of the verb used with the function word *have*.”²

One only has to look through a few novels or short stories to see that present-day usage does not bear out the grammatical comment that has just been quoted. In fact, *could* is often, and *might* is occasionally, used before a present infinitive to refer to the past. The following excerpts from recent American works of fiction may exemplify this point:

When I saw him at the creek camp, he was loading his horse with everything he could tie on it.

He could read or write no more than a few simple words but felt no disadvantage at lacking an education.

¹ *Mechanical Translation*, 4 (1-2) (Nov. 1957), pp. 28, 29.

² C. C. Fries, *American English Grammar* (New York, 1940), p. 173.

The men inside could smell the odor as Underwood poured it [i.e. the kerosene] along the bottom of the door.

He [i.e. the hunter] was cautious in his approach; from the high rocks seals might be taken without the use of a skin boat.

In a widely used handbook on English usage a remark reads: "A careful analysis of all sentences where we cannot use *could* as simple past tense of *can* shows the following idea to be common to all of them. They deal with the attainment of something through some capacity . . . something attained through a capacity may not have *could*. Example: He was able to swim halfway before he collapsed. (*Could* is impossible here). *Managed to* also expresses this idea."³

The following examples, taken from recent writings, will show that *could* may be used to denote a past achievement:

He [i.e. the wounded outlaw] could ride three miles. That was all.

Could you get your company to settle the David Levy claim?
I believe that they're doing it today.

It was only with the greatest difficulty that I could persuade him.

From the state of affairs that has just been outlined, it results that the programmer of translating machines, if he wants to be successful, will have to fulfil a treble task. Firstly, he will have to test the soundness, and ascertain the range of validity, of each rule he proposes to take over from traditional grammar. Secondly, he will have to supplement the lexical and morpho-syntactical information provided by dictionaries, grammars, and handbooks on usage. By way of illustration might be mentioned here two gaps that will have to be filled before a program for properly translating *can* and *may* can be designed.

Most English grammars point out that *can* and *could* are frequently used before verbs of perception, as substitutes for the progressive tenses, some say, as equivalents of the simple present and past, others maintain. Now, what is it? Is there any semantical difference between, say, *he saw* and *he could see*? And if there is no difference, are the two turns of expression commutable in any position?

Some dictionaries assert that one of the meanings of *may* in present-day English is to denote *effectibility or potency*. Other dictionaries, however, ignore that sense. Does such a variance indicate that the meaning *effectibility or potency* is rare, or obsolete, or that it exclusively pertains to written English?

The third, and by far the most arduous, task that faces MT programmers is to find how the machine can be made to distinguish between the various meanings that a word may assume. Which, if any, contextual clues can the machine use to determine the intended sense whenever a term is utilized that has more than one equivalent in the target language? By way of illustration I may mention here some of the formal symbols that could be of assistance in translating *can* and *may*:

³ W. S. Allen, *Living English Structure*, third edition (London, 1951), p. 47.

a) the infinitive following the auxiliary:

in such groups as *can* (or *could*) *afford*; *can* (or *could*) *bear* (= stand); *can* (or *could*) *compare with* (if *compare* is intransitive); *can* (or *could*) *cope with*; *can detect* (or *be detected*); *can discern* (or *be discerned*); *can* (or *could*) *endure*; *can equal* (or *be equalled*); *can identify* (or *be identified*); *can* (or *could*) *manage*; *can* (or *could*) *stand* (= bear), the auxiliary always, or practically always, denotes *effectibility* or *potency*.

in such groups as *cannot* (or *could not*) *forget*; *cannot* (or *could not*) *miss*, the negative auxiliary always, or practically always, means *not to be liable to, there is no risk that*.

in such groups as *can expect* (or *be expected*); *can suppose* (or *be supposed*); *as could* (or *might*) *be* (or *have been*) *expected* (or *supposed*), the auxiliaries always denote an absence of moral obstacle.

before a perfect infinitive, negated *can* always means *there is no reason to presume that*.

b) typical phrases:

in incidental clauses beginning with *as* (or *so*) *far as*, the auxiliary always denotes *effectibility* or *potency*.

in *may I* and *might I*, the auxiliary denotes an absence of moral obstacle (except in exclamations).

may (or *might*) at the beginning of an exclamation marks the utterance as a wish. the group *as you could* (or *might*) *say* and the incidental *I* (or *you*) *could* (or *might*) *say* mean *in a way, so to speak*.

could do with means *to feel like*; and *could use* is a conversational synonym of *to need badly*.

c) syntactical patterns:

may and *might are* modal auxiliaries in content-clauses depending on such verbs or verbal phrases as *to fear*, *to be afraid* (or *scared*), *to hope*, or their allied substantives.

Extensive contextual analysis, such as will have to be carried out by MT research workers, is likely to bring out structural features that have hitherto escaped notice. These features may regard, among other things, the following three:

1. *Distribution*. — In a sample of over 5,000 sentences with *can*, *could*, *may* or *might*, collected from recent works of fiction and non-fiction, not a single affirmative clause has been found with *can* followed by a perfect infinitive. Neither has any assertive (as opposed to interrogative) clause with negated *may* or *might* before a perfect infinitive. This seems to suggest that in present-day English and American *can* is not used before a perfect infinitive whereas *can + negative*, *could* and *could + negative* are. It also indicates that in statements *may* and *might* can be negated before a present, but not before a past infinitive.

2. *Neutralization*. — The same sample of 5,000 sentences shows that *could + present infinitive* is used to denote a past effectibility or potency that was turned to use, where-

as *could* + *perfect infinitive* is utilized to express a past effectibility or potency that was not taken advantage of. This may be illustrated with the following excerpts from a recent short story:

Then I understood. Beneath this weird talk, this access of nobility, was sound logic, something I could get hold of.

That is when we could have got away without being seen, but we didn't.

Still, the semantical opposition between *could* + *present infinitive* and *could* + *perfect infinitive* is neutralized in final sub-clauses and in *before*-clauses depending on verbs in the past. In such subordinate clauses *could* + *present infinitive* is used whether the effectibility or potency was turned to account or not. As illustrations may be quoted:

He studiously worked to keep two Chicago locals in trusteeship for twenty-nine years, so that he could keep check on the contracts they negotiated with his management friends.

Stephen carefully held out the curtains so that she [i.e. the cat] could walk down them again. [But] Caroline [i.e. the cat] crouched even farther back [on the pelmet board].

3. *Structure*. — The sample mentioned above shows that, except in a few marginal cases such as wishes beginning with *may* or *might*, the correlation of *can* and *may* within the English language system can be defined in terms of commutability or complementarity.

If translating machines are ever to function satisfactorily, the problems that have been sketched above will have to be tackled — and solved. But once these questions are worked out, the accumulated lexical information will promote mechanical translation, and also widen the borders of structural semantics.

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THE NOTION "IDIOLECT": CONTRASTING CONCEPTUALIZATIONS IN LINGUISTICS

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For some years now I have been interested as a psychologist in the study of variability in language, particularly in the speech of psychiatric patients. My purpose has been to determine how variations in the language of each patient are related to changes in social context and how the pattern of these relations is associated with the patient's improvement.

The psychological techniques that were available when I first started working on this problem seemed quite inappropriate, both because they dealt with aspects of language available to the patient for direct manipulation and because I sensed a circularity akin to the "self-fulfilling prophecy", that is, the techniques seemed to be designed too specifically in the service of particular hypotheses. As a result of the lack of relevant psychological procedures, I became involved in linguistics and spent some time with the intricacies of descriptive techniques for the analysis of language. It was only after I became addicted to linguistics in a way that I still find difficult to explain even to myself that I realized that linguistics and I were headed in somewhat different directions. Techniques for linguistic analysis have been designed in general so that the structure of the language of a language community can be inferred from the data provided by a few speakers of the language. Where I was interested in variability within a speaker of a language, linguists were doing their best to obscure that aspect. Occasionally in the linguistic literature here and there I could get a glimmer of something relevant to my interests, enough to give me hope, anyway. The things that encouraged me were in various areas: dialect geography, stylistics, occasionally in the midst of arguments over phonological structure.

The term that kept recurring in these context was *idiolect*, and although it sounded like jargon at first, I came to appreciate the aptness of the word as a label. It seems to me that idiolect and other terms that involve the notion of a "source of uniform language data" have been appearing with increasing frequency in recent publications. I believe that there is a serious concern within contemporary linguistics about this notion.

In this paper I am presenting my view of the various conceptualizations of idiolect that I believe could be formalized within linguistics and asking some questions about them. My relative lack of sophistication in linguistics does not

permit me to assert the accuracy of my perceptions nor can I provide such formalizations myself. What I want to do, rather, is to get some reactions to the ideas, to ascertain their relevance for linguistics as well as for my psychological interests.

In many of the places in which the term "idiolect" has been used in the literature, it has had only a token value, indicating problems without pretending to resolve them. The use of the term in linguistic geography is a case in point. Here idiolect has been used as an extrapolated lower bound to a continuum formed from two other not-too-well-defined notions, "language" and "dialect". Although intended to provide some idea of a unit of language variability, the introduction of the word has not affected either the theoretical status of dialectology or the manner in which dialect studies are carried out. This is neither surprising nor inappropriate, of course, because the usefulness of idiolect would depend on the adequacy with which it was developed as a central concept in linguistics, and this remains to be discussed.

Another example of the use of idiolect as an illustration of problems in linguistics can be found in certain of the recent introductory textbooks. It seems to have become fashionable for authors of such texts to use the term in acknowledgment of the fact that they have used themselves as informants. They view this dependence on their own knowledge as speakers of the language as unfortunate, forced on them by the lack of an adequate sample of utterances from a large number of speakers. Since the authors themselves are not clear about how to formalize the differences between data coming from their own speech and those from other speakers, they blur the distinction between linguistic statements that reflect just their speech and those valid for English. Furthermore, where the term occurs at other places in such texts — for example, when conflicts in data are attributed to differences in the speech habits of several speakers — the resolution of the conflicts does not depend in any way on an elaboration of a concept of idiolect itself.

These examples of the use of idiolect indicate to me both the need for and the purpose of a more precise characterization for uniformities in linguistic data. It is obvious that the accumulated mass of data on a language can not be pooled together without constituting an uninterpretable mass; criteria are necessary to determine those sets of data that are equivalent in some sense. There are, however, three different conceptualizations of idiolect current in linguistics that might provide these criteria. Each of the concepts would have different consequences both for linguistics and for me as a psychologist interested in assessing variability in and among the speakers of a language.

At this point it might be helpful to discuss the topic from the vantage point of my need for measures of variability. In order to talk about variability at all in any domain, we need to be able to specify some unit or norm of uniformity with respect to which things can be said to vary. I am, of course, using the term "idiolect" to designate that reference point for work in language. However, variation can be assessed with respect to different parameters. The three I found most relevant for

my own research relate, first, to the actual language product itself — what was said or written, second, to the source of the production — the speaker, and, third, to the conditions under which the language was produced — the situation. I believe that within linguistics three theories or approaches can be delineated corresponding to these. In effect I see within linguistics what I will call a “theory of the sample”, a “theory of the speaker”, and a “theory of the situation”. In what follows I intend to describe each of these theories briefly and point out the areas in each in which a conceptualization of idiolect could profitably be developed — perhaps necessarily must be developed.

One further aside is in order here to clarify my choice of the word “theory” to designate these different orientations within linguistics. The use of this term in the present context may seem inappropriate because the referent is both more general and less structured than the one usually associated with the word. Since my concern is with generic orientations toward the subject matter of linguistics that have (at least in my estimation) theoretical and methodological consequences and since I find “metatheory” too pretentious a term, my choice was constrained to “theory”. In restricting myself to three such theories I do not claim to have exhausted all the possibilities, although these three do seem to have particular relevance for linguistics and for my own research.

The phrase “theory of the sample” designates the kind of approach found in Harris’ discourse analysis in its most general features (cf. Harris, 1951, 1952). In this work Harris was attempting to develop methods that would determine regularities present in a given body of data. For Harris, specifically, and certainly for what I am calling more generally a theory of the sample, the choice of the sector of linguistic relevance to be sampled is not critical. He might deal with texts, spontaneous utterances, elicited responses, and the like; and he could focus selectively on phonology, morphology, or syntax. What is necessary, however, is that each corpus of data be amenable to something like a distributional analysis. That is, the structure of the sample is to be determined solely on the basis of evidence internal to that sample, in particular by the co-occurrence of elements relative to each other. Within this approach, the corpus or sample *by being selected for analysis* is identified as the uniform aggregation of data; no criteria are available for rejecting a sample as heterogenous on internal grounds. It is at this point that the term idiolect seems to me to have some relevance. Idiolect, then, for a theory of the sample should clarify those characteristics of a particular corpus that would justify its selection for analysis.

A careful delineation of what idiolect entails — that is of the criteria for selection — is essential for interpreting the significance of any structure imputed to a sample. For my own interests and presuming that distributional methods do provide adequate structural representations — which it is not my purpose even to question here — I would know what data I could use as a sample and how they related to something called “language” only to the extent that idiolect was clarified. The distributional techniques as developed by Harris can be applied in principle to any kind of data

that has some internal sequential structure. Whether the results have any relation to language does depend at least in part on the appropriateness of the data as a sample of language. This is not to say that clarification of idiolect for a theory of the sample will guarantee the adequacy of the approach, but it is a *necessary* condition for its adequacy.

The second theory, the "theory of the speaker" is concerned with the person who produces and responds to language. Chomsky's characterization of the purpose of a grammar as specifying precisely the linguistic intuitions of the native speaker is directly in accord with such an approach (cf. Chomsky 1957, 1962a, 1962b). Rather than trying to formalize actual linguistic productions, Chomsky is dealing specifically with a speaker's ability to produce and respond appropriately to sentences he has never spoken or heard before. Similarly, any theory of the speaker would necessarily be clarifying *competence*, the person's knowledge about his language, rather than *performance*. In order to represent competence Chomsky has proposed that a grammar should be generative, consisting of an ordered set of rules that will recursively specify all and only the grammatical sentences in a language. Because of the complexity of a generative grammar conjoined with the ease with which children come to behave in accord with it, Chomsky has postulated an innate linguistic faculty that provides the framework for assimilating language. That is, the basic structure of a generative grammar is viewed as built into the individual rather than provided by learning or experience.

In a theory of the speaker it would seem at first glance that the uniformity of linguistic data was guaranteed by the fact that a single speaker was responsible for it. However, even if one were to accept the innateness condition, which I do not believe is essential to the theory, relatedness to a speaker would not by itself guarantee uniformity. A theory of the speaker differs from a theory of the sample precisely because of the distinction between competence and performance. Since all of our data about language result from performances of one kind or another, we require criteria that will identify the kinds of data from performance that will allow us to make inferences about competence. These criteria would be provided by an adequate conceptualization of idiolect; or, rather, idiolect for a theory of the speaker would define the classes of data that relate to competence.

In order to use generative grammars for my own interests in the assessment of variability, I would need an adequate treatment of idiolect. This would become even more important should it be necessary, as I think it would, to impute several generative grammars to one individual in accounting for multilingualism or for what are frequently called stylistic variations. Apart then from whatever arguments linguists might have over the relevance of generative grammars for linguistics, an adequate conceptualization of idiolect is necessary in order even to know what data are relevant for their construction.

The third theory, the "theory of the situation" focuses on the conditions under which language occurs. The possibility of such a theory within linguistics may be

only a fiction that I have created on the pattern of my model. Certainly there is no formalization that can be used to illustrate this theory as there is for a theory of the sample or of the speaker. Nevertheless, there are major problem areas marked out in linguistics which do provide a substantial body of material that could become part of a theory of the situation. The whole subject of style in language is undoubtedly relevant and those classes of problems designated by the terms "sociolinguistics" and "ethnolinguistics" are equally appropriate. The kinds of data to be explained would include the distinctiveness of "poetic language" and "literary style" on the one hand and the differences between forms of address directed toward people of superior as contrasted with those of inferior status. However, although the examples cited are usually striking, in the work done to date for a theory of the situation there has been neither consistency nor clarity in the attempts to relate features of language to features of context.

The absence of a formally developed theory of the situation does not preclude proposing a concept of idiolect. Quite the contrary, a careful statement of what idiolect would entail for this theory might actually further its formalization, because a precise conceptualization would delimit the conditions within which linguistic behavior can be related to situational factors. Once an exhaustive classification of contexts has been prepared, the linguistic concomitants can be explored more profitably. The development of an adequate classification system will certainly not be an easy matter; the sociological and anthropological efforts toward that goal have not been productive. The task of clarifying the kinds of linguistic data that should be considered will probably be as difficult. In spite of the problems encountered along the way, I do believe that a formalization of a theory of the situation would be valuable for linguistics. Its relevance for my psychological interests is obvious; it is easy to see how situational variables could be related to variability within and between individuals. Any work that could identify these variations as linguistics and could specify their dependence on contexts would be extremely useful.

I feel quite diffident about what I have had to say about linguistics. If I have been provocative as well, I will be content. As my competence in linguistics increases, I intend to concern myself more with ways of characterizing idiolects. I hope some proper linguists will be doing the same thing.

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DISCUSSION

FRANCESCATO:

The term "idiolect" has currency in dialectology in a meaning not accounted for by the speaker in his paper.

INFORMANT MORPHEMES VERSUS ANALYST MORPHEMES

R. S. MEYERSTEIN

Identifications on the morphemic level invariably derive from informant cues interpreted by the linguistic analyst, but to the extent of enhancing or de-emphasizing the informant's part in the functional evaluation of supra-phonemic forms, we may in relative terms speak, respectively, of informant morphemes as compared to the morphemes of the analyst.

Restriction to analyst-morphemics confines the informant to supplying the raw material of recurrent phonic expression and provides for morpheme inventories evolved solely by the analyst. Without benefit of semantic guidance which only informants could offer, the analyst proceeds on the basis of distributional function alone. This procedure has raised questions of practical feasibility.¹ Morphemic descriptions, in fact, may disclaim injection of the variable of meaning yet use "situational" variables or semantic criteria ostensibly disavowed.² Some presentations avoid taking a stand for or against meaning as a formal determinant or simply leave the morpheme undefined.³ There is, then, clearly no general commitment to asemantic morphemics in the strict sense.

¹ Cf. Murray Fowler, reviewing Harris, *Methods in Structural Linguistics*, in *Language*, 28 (1952), 505. "[Asemantic morphemics] provides for the tentative isolation of a 'morphemic segment' by contrasting it with another 'morphemic segment'. . . The procedure is not feasible. . . ; no morphemes at all have yet been isolated. . . The critical problem is the isolation of the first morpheme. . ." *Ibid.* 509: "The conclusion is that [asemantic] procedure cannot be used to isolate a single morpheme. . . Morphemic distinctions on a phonological level by the use of statistical - i.e. distributional - criteria alone [are] impossible."

² According to Charles F. Hockett, "Two Fundamental Problems in Phonemics", *Studies in Linguistics*, 7 (1949), 40, even when defining morphemes on a non-semantic basis we may use meanings as "hints" to tell us what morphemes may be, without thereby abandoning "the purely formal definitions". Cf. Zellig S. Harris, *Methods in Structural Linguistics* (Philadelphia, 1951): "In principle, meaning need be involved only to the extent of determining what is repetition. . .", but does meaning ever involve more than this? Cf. Fowler, *op. cit.*, 507: "The use of meaning even as a 'hint' is at once the use of a method in which distribution or arrangement is not the only relevant relation." Cf. also Einar Haugen, "Directions in Modern Linguistics", *Language*, 27 (1951), 219: "Those who eliminate meaning have brought it back under the covert guise of distribution." Cf., further, Fred W. Householder, reviewing Harris, *Methods in Structural Linguistics*, in *International Journal of American Linguistics*, 18 (1952), 263.

³ Cf. Charles F. Hockett, "Two Models of Grammatical Description," *Word*, 10 (1954), 210-231, defining a morpheme as a simple linguistic form, a term "defined by the other terms at least as much as they are by it", and assuming "a certain amount of our linguistic common sense". Cf. also H. A. Gleason, *An Introduction to Descriptive Linguistics* (New York, 1955), 52-53: "The morpheme was

Morphemes as minimal semantic functives, in contrast, are at present generally accepted entities. Semantically oriented procedure acknowledges the preëminence of informant views as a key to the statement of meaning in a specific or differential sense. Minimal units reflecting these views may thus be referred to as informant-morphemes.

Established conventions of semantic morphemics cannot, however, be supported uncritically. An examination of principal areas of vulnerability must focus on techniques of description as well as procedural practice. Morphemes determined by meaning might draw objections in the context of descriptive theory suggesting that meanings are determined by form. We may disregard directional contradictions of this nature, but there remain a number of additional issues arising from the semantic information conveyed, from the origin of the semantic data traceable to diverse classes of informants, from the extent to which meaning is supplemented or replaced by other criteria of form-identification, from the characteristics of these criteria, and from the degree to which incompatibility of determinant features leads to incompatible analytic results. This paper will study the combined effect of problems in these various areas as reflected in the wide range of disagreement among analysts, between analysts and informants, and among informants themselves, on whether a given form should or should not be recognized as a morpheme.

Expressions of possible morphemic relevance are here designated as *morpheme candidates*. Questions of claiming or denying morphemic status for morpheme candidates are sampled in the following review of some analyst's statements (Roman numerals, followed by page citations, in the left-hand column of the table)⁴ against

mentioned . . . as the second of the two basic units in linguistics. No definition was given, and it was stated that an exact definition is not feasible. Perhaps the best that can be done is to define the morpheme as the smallest unit which is grammatically pertinent. But it would then be necessary to define grammar as the study of morphemes and their combinations. This is obviously circular and hence is no definition. Nevertheless, it does serve to point out something significant. As a basic concept, a morpheme cannot be defined beyond some such circular statement. In place of a definition, therefore, we must merely describe certain features of morphemes and give some general rules for their recognition."

⁴ Roman numerals represent:

- I – Leonard Bloomfield, *Language* (New York, 1933).
- II – Eugene A. Nida, "The Identification of Morphemes", *Language*, 24 (1948), 414-441.
- III – Bernard Bloch and George L. Trager, *Outline of Linguistic Analysis* (Special Publications of the Linguistic Society of America, 1942).
- IV – Eugene A. Nida, *Morphology* (Ann Arbor, 1949).
- V – Dwight L. Bolinger, "Rhyme, Assonance, and Morpheme Analysis", *Word*, 6 (1950), 117-136.
- VI – André Martinet, "About Structural Sketches", *Word*, 5 (1949), 13-35.
- VII – Knud Togeby, *Structure immanente de la langue française* (= *Travaux du Cercle Linguistique de Copenhague*, Vol. 6) (1951).
- VIII – Murray Fowler, reviewing Togeby, *Structure immanente de la langue française*, in *Language*, 29 (1953), 165-175.
- IX – H. A. Gleason, *An Introduction to Descriptive Linguistics* (New York, 1955).
- X – Zellig S. Harris, *Methods in Structural Linguistics* (Philadelphia, 1951).
- XI – Murray Fowler, reviewing Harris, *Methods in Structural Linguistics*, in *Language*, 28 (1952), 505-509.
- XII – H. M. Hoenigswald, reviewing Sturtevant, *An Introduction to Linguistic Science*, *Language*, 23 (1947), 442.

a record (on the right) of informant views on semantic content and the localization of any such content in specific segments of a sequence potentially multi-morphemic. Repeated reference to certain sources results from the fact that a large number of morphemic discussions leave "marginal" issues either out of account or unsettled. Marginality, as will be seen, can scarcely be deduced from the number of questions presented, nor is it a tenable notion in the context of exhaustive description. The identification of a basic unit of linguistic description remains in doubt as long as the formulas of conventional morphemics fail to cover the fundamental problems of formal-semantic correlation illustrated in the table below.

MORPHEME CANDIDATES: STATUS, FUNCTION, SEGMENTATION AND LOCALIZATION

<i>Analyst's Decision on Morpheme Status</i>	<i>Informants' Identification of Semantic Content</i>
(A) Status claimed for: <i>as-</i> in <i>assure</i> (I, 239); <i>be-</i> in <i>behead</i> (I, 218); <i>nast-</i> (in <i>nasty</i>), with "clearcut meaning" (I, 242); <i>yell-</i> (in <i>yellow</i>), on same grounds as for <i>nast-</i> (I, 242); <i>we-</i> in <i>were</i> (II, 416); <i>s-</i> in <i>so</i> and <i>such</i> (I, 244); <i>-le</i> (in <i>bottle</i>), "extremely vague in meaning" (I, 240); <i>-kin</i> in <i>manikin</i> (III, 56); <i>-ceive</i> (in <i>receive</i>) (I, 242: with "very vague meaning"; III, 54, 55; IV, 1, 81, 98, 162: "the meaning cannot easily be defined"; cf. R below); <i>-ow</i> in <i>slow</i> , <i>grow</i> , <i>tow</i> (V, 120); <i>-er</i> in German <i>der</i> , <i>roter</i> , <i>wer</i> (by implication, I, 244; cf. G below).	Content denied, i.e. not localized in candidate.
(B) Status claimed for: <i>m-</i> in French <i>mon</i> (VI, 25, 26); <i>-st</i> in <i>best</i> (I, 275) <i>-ock</i> in <i>hammock</i> , <i>mattock</i> , <i>hassock</i> (I, 241).	Content localization doubtful in less than whole word (least likely for <i>-ock</i>), i.e. semantic segmentation doubtful. If localization in candidate, content of residue form denied.

XIII – Fred W. Householder, "On the Problem of Sound and Meaning", *Word*, 2 (1946), 83.

XIV – Charles F. Hockett, "Problems of Morphemic Analysis", *Language*, 23 (1949), 321-343.

XV – Dwight L. Bolinger, "On Defining the Morpheme", *Word*, 4 (1948), 18-23.

XVI – Fred W. Householder, reviewing Harris, *Methods in Structural Linguistics*, in *International Journal of American Linguistics*, 18 (1952), 267.

XVII – André Martinet, reviewing Nida, *Morphology*, in *Word*, 6 (1950), 84-87.

XVIII – Stanley S. Newman, "English Suffixation: A Descriptive Approach", *Word*, 4 (1948), 25.

- (C) Status claimed for:
-iche in French *barbiche* (VII, 232).
Content localized in candidate, but not identified (meaning of -iche = ?), i.e. semantic segmentation certain, but as noted not semantically motivatable.
- (D) Status claimed, in diversely comparable environments (cf. M below) for:
-m in *whom* and *him* (II, 423).
Content localization as "de-colloquializer" certain for -m in *whom*, with content of residue form (*who*) certain and identified. Content localization possible but far from certain for -m in *him*: if the word is at all semantically segmented, residue content almost certainly denied, *hi-* not being felt to derive from *he*; cf. E below.
- (E) Status claimed, in not directly comparable environments (cf. F, L, below) for:
-ais in French *niais* and *Marseillais* (VII, 236).
Content localization possible for -ais in *niais*, but residue content denied. Content localization certain for -ais in *Marseillais* with residue content certain and identified (cf. D above and Q below).
- (F) Status claimed or denied, of identical candidates, according to comparable or non-comparable environments (said to be determined, albeit subconsciously, by the informants), as being that of morphemically complex forms, for:
pas as French noun (claimed) or adverb (denied) (VII, 139; VIII, 169);
shoemaker as common noun (claimed) versus *Shoemaker* as personal name (denied) (V, 124; cf. O below).
Content localized or denied along the lines suggested by the analyst (granting the psychological reality of zero-modification in *pas* as noun), provided the two *pas* or the two -maker constructions are associated in the first place, which is doubtful. (Awareness of identity or recurrence may derive from formal or functional criteria.)
- (G) Status claimed, considered, or denied, of identical candidates in identical or comparable environments, according to individual analyst, for:
th- in *then, there, the*, (claimed: IX, 276; denied: I, 147, 244; cf. K, L, below); -er in *hammer, mother, embroider* (claimed; I, 240, 243 — "The descriptive analysis . . . may be extended to include such forms . . ."; IV, 60; undecided: II, 417; denied: X, 180; XI, 505, 508).
Content localization by semantic segmentation doubtful (for *th-*, cf. B above and L below) or denied (for -er, cf. A above), but according to candidate-form rather than analyst claim or denial. Semantic segmentation, if any, for *th-* leaves residue with content denied. (Cf. also J and K below.)
- (H) Status claimed, considered, or denied, of identical or comparable candidate-types in identical or comparable environments, according to individual analyst, for "phonestemes" of the type *fl-* in *flicker* or *flatter*, *sc-* in *scatter* or *scamper*, -atter in *scatter* or *flatter* (claimed: I, 244; XII 442; considered: XIII,
Content localization, i.e. semantic segmentation, certain regardless of analyst support or denial. Localization of residue content depends on candidate: certain for -itter or -atter, doubtful or denied for -amper.

- 83; V, 119, 130; denied: IV, 60; cf. R below).
- (J) Status claimed or doubted, according to individual analyst (cf. H above) for: *-way* in *away* (claimed: XIV, 322; doubted: XV, 19). Content possibly localized in one component only (*-way*), or semantic segmentation into two equally identified meaning-carriers (*a-*, *-way*).
- (K) Status claimed or denied by one and the same analyst:
I, 244, 245 (claimed for *j-* in *jump* or *juggle* or *b-* in *bang* or *bump*; denied for *th-* in *this* or *there*);
X, 177, 178 (claimed for *th-* in *this*; denied for *sl-* in *slide*). Content localization doubtful (for *th-*, and *sl-* in the environment cited) or denied, regardless of analyst's differentiations; but if the *j-* or *b-* forms are deemed significantly similar, they are associated as typologically alike (possibly on the basis of "target-connection", "audio-visible" in *j-*, "audible" in *b-*, and "visible or mental" in the *th-* forms cited).
- (L) Status denied, in non-comparable environments (cf. E - K above), for: *th-* in *there* and *though* (I, 147). Content localization, hence semantic segmentation, not impossible in *there*, but certainly denied in *though*. Residue content denied in both cases.
- (M) Status doubted, in comparable environments, for components of *disease* (XV 19); *dispute* (V, 121); *infect*, *affect*, *perfect* (V, 121, related as "having or keeping a state or condition"). Semantic segmentation doubtful, more likely in *disease* (cf. *away*, J above), less likely in *dispute* (with residue content denied for *-pute*) virtually denied (for *in-* in *infect*), or certainly denied (for *-fect* and for any of its prefixes; cf. *as-*, *-ceive*, A above).
- (N) Status undecided, claimed or denied according to individual analyst, for *tend* (free form) and *-tend* in *extend* or *intend* (claimed: III, 63; implicitly denied for *-tense* in *intense*: XV, 20). Semantic localization possible in *-tend* bound to *ex-* (though contents of the bound and free *tend* may be considered dissimilar; cf. E above), denied in *-tend* bound to *in-*.
- (O) Status varies, according to social differentiation of informant groups, for compositeness (said to be claimed by "sophisticated" informants and denied by other informants) of *breakfast* (V, 122) or *mistake* (XVI, 267). Content localization (semantic segmentation) present or absent as noted by analysts. (Analyst-informant conflict if the non-distributional "sophistication" variable is disregarded.)
- (P) Status varies, according to associative differentiation of informant-groups, based on respective "primary associations" said to determine segmentation of *dusty* as *dust-y* "covered with dust" or *d-usty* associated with *musty*, *crusty* "old" (V, 120, 121). Content localization (semantic segmentation) alternatives as noted by analyst, or overlapping so as to combine both associations. (Analyst-informant conflict if the non-distributional "primariness" variable is disregarded.)

- (Q) Status varies, according to formal (environmental) differentiations said to be made by all informants, for phonemically identical candidates: *-i/*, with "diminutive" meaning "obvious" in *doggie*, "dubious" in *honey* or *pretty*, "capable of creation" in *falsie*, "dead" in *bully*, and "non-existent" in *earie* (V, 122). Content localized (i.e. semantic segmentation made) with descending degree of certainty, as noted by analyst. (Analyst-informant conflict potential in deciding where *-i/* ceases to warrant recognition as separate entity.)
- (R) Status varies, according to formally or structurally comparable candidates as identified by the individual analyst, for:
fl- or *gl-* in *flimmer* or *glimmer*, with morphemic status said to be semantically better justified than for *-ceive* in *receive* (XVII, 87);
-ful in *handful*, said to be less tightly joined to the word-theme than *-id* in *horrid* (XVIII, 25). Content localization (i.e. semantic segmentation) more or less likely as noted by analysts. (Analyst-informant conflict if conventional criteria of distribution alone are considered.)

Analyst-informant disagreement as sampled above reflects the basic disparity of entities usually regarded as manifestations of one and the same type of unit, the morpheme. This disparity results from the heterogeneous features of meaning and form as a basis of morphemic analysis. The framework of conventional description does not accommodate morphemes based on disparate determinants. This entails elimination of some morpheme-candidates or else arbitrary reduction of diverse determinant features to the one of semantic distinctiveness, and a unit semantically irrelevant but deemed indispensable for considerations of structure must inevitably be assumed to have "very vague" though unverifiable meaning, simply because meaning is what morphemes are said to convey. We habitually encounter forms like *redo* and *receive* under the identical heading of binary semantic segmentations, an association clearly untenable: *-ceive* has no meaning whatever to any informant, and the analyst cannot classify *-ceive* among morphemes as meaningful forms. Structurally oriented description would stress the morphemic characteristics of *-ceive* or the componential similarity of *receive* and *redo*, but under prevailing definitions of semantic morphemics we are unable to assign comparable status to these two sequences.

We must therefore consider alternatives of descriptive revision or extension. We may adopt the point of view that fundamentally incompatible items do not lend themselves to treatment in similar terms. Structural and semantic identifications must in that event be regarded as representations of separate inventories. Accordingly, the non-semantic definition of the analyst's inventory would remain unrelated to the definitional basis of informant morphemes and the latter would ignore non-meaningful segmentations. *Receive* and *redo* would in terms of this analytic differentiation be forms of distinct morphemic sets. Variety of descriptive objectives contemplated

within storable limits of relevance would suggest an equivalent *number of inventories*, each of these with a *small number of units*.⁵

The opposite solution, examined in this paper, would take as a point of departure the traditional concept of *one inventory* only, but would have to allow for a *maximum number of units*. Reformulation of descriptive terms would in this case acknowledge the overlapping (hence partly complementary) nature of the identificational process. The large area of overlap where analysts and informants agree cannot detract attention from the two irreducible conflict areas pertaining, respectively, to segmentations of structural import beyond the informant's recognition and to identifications significant to natives but devoid of analytic value. Far from ignoring the structuralist's findings or else "letting the informant be damned", we acknowledge the dual nature of conventional morphemes as heterotypical units of comparable functional status. In this sense, *redo* and *receive* may on equal terms be regarded as bi-morphemic compounds. Informants and analysts will concur on the binary nature of *redo*; *receive*, in turn, will be multimorphemic to the analyst concerned with formal structure but not to the semantically motivated informant.

We may then trace specific componential or unitary solutions to analysts or informants as respective morpheme-*proponents*. Within either of these two groups of morpheme-proponents we must recognize proponent-*factions*. These factional differences further increase the scope of morphemic information. To accord equal stature to these additional identifications rather than preferential treatment to arbitrarily selected morpheme-candidates only, we proceed, within limits of acceptable proponent-qualifications, on the basis of maximum diversification, an established principle of phonemic and morphemic inventories⁶ which may extend to the number of valid determinants of additional morphemic segmentations.

A phonemic sequence subject to analyst-informant agreement on the number (though not necessarily the locations) of constituent morpheme boundaries may be termed a *homo-morphemic* structure, as opposed to *hetero-morphemic* constructions consisting of an unequal number of morphemic cuts when analyses of two proponent groups are compared. Relating, further, constructs to constructional devices, we may

⁵ This approach has been explored by R. S. Meyerstein, "Functional Parallelism in Descriptive and Applied Linguistics", to appear in a forthcoming issue of *General Linguistics*.

⁶ Maximally diversified phoneme-sets may contain proponent-determined units of reduced or even unique occurrence. This is noted in the educated speakers' articulation of certain sounds borrowed from another language, as in many Germans' pronunciation of such borrowings from French as *das Parfum* or *das Restaurant*. Idiolects or social dialects, considered representative of the speech community as a whole (and by what non-arbitrary process of elimination could we decide otherwise?) would call for the acknowledgment of such sounds as part of the total phonemic picture. We may also visualize structurally determined applications of the concept of maximum diversification on the morphemic level; thus we might describe the English "present" tense as being expressed by three verb-forms, encountered in one verbal paradigm (*am, are, is*), although two of these fall together in all other verbs of the language. We do indeed habitually transfer the diversification of a few irregular paradigms to the large number of regular verbs not so diversified; this prompts us to take our cues from *write-wrote-written* for such "three-form" conjugation as *call-called-called*. Cf. Bernard Bloch, "English Verb Inflection", *Language*, 23 (1947), 399-418.

notice the *sequential* composition of homo-morphemic constructs like *redo*, or the *overlapping* relationship of *d-usty* and *dust-y* as homo-morphemic sequences proposed by different informant-factions. Sequences such as *hammer*, in contrast, illustrate hetero-morphemic formation of the *incorporating* type in which *-er*, of morphemic significance to the structural analyst, is isolated, not against a residual *hamm-* which none of the proponent-groups is likely to sponsor as a morpheme candidate, but as included in the indivisible informant-form *hammer*. There are at least two distinctive hetero-morphemic constructions: in that of *hammer*, the analyst-form is part of the informant-unit, whereas combinations termed *phonestemes* model informant-units incorporated in analyst-forms.

This approach correlates diverse proponent-determined morpheme types and provides a basis for extending the definition of morphemes as units of minimal semantic distinctiveness or *equivalent structural function*, in accordance with relevant proponent statements. On this basis, we may meet the problems of conventional morphemics, as discussed above, in extending established descriptive concepts to new correlations of morphemic forms and functions.

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THE CURRENT STATUS OF DRAVIDIAN HISTORICAL AND COMPARATIVE STUDIES

ARUMUGAM SATHASIVAM

There are twenty Dravidian languages spoken in India and Pakistan. They are classified as South (SDr.), Central (CDr.), and North Dravidian (NDr.).

SDr.: Tamil, Malayalam, Toda, Koṭa, Kannada, Koḍagu (Coorg: Kod), and Tulu.

CDr.: Telugu, Kolami, Naiki, Parji, Gadba (Ollari, Poya), Kui, Kuvi (Kuvi), Pengo, Gondi (Go), and Konda.

NDr.: Kurukh (Oraon, Kur), Malto, and Brahui.

The terms South Dravidian, Central Dravidian and North Dravidian are used here to refer to the geographical situation of a given member of the family in the South, Centre, or North of India. Brahui, though spoken in West Pakistan, is included in the North Dravidian group. Telugu, by virtue of its geographical situation, occupies a midway position between the south and the centre. Before the discovery of so many central Dravidian languages in recent times, Telugu has been treated as a South Dravidian language. Now it appears to me that it should be included among the Central Dravidian group, for it shares more innovations phonologically, morphologically and lexically with the central Dravidian. Ollāri and Poya were treated by some as separate languages, but the recent researches of Burrow show that these are dialects of one language which could be named Gadba. New languages are still being discovered in Central India. Pengo is the latest addition to the Central Dravidian group.

Reconstruction in comparative Dravidian is now being seriously attempted. Work has yet to be started on Dravidian glottochronology. It is only then that historical studies in Dravidian on a scientific basis will be possible. Recent researches of Burrow and Emeneau show that the linguistic sub-grouping and the order of separation of the languages from the parent Dravidian could be worked out. On the bases of common innovation and of common retention of Proto-Dravidian features, an attempt is now being made by linguists to establish three sub-families within the Dravidian group. Only time will show whether these three proposed sub-families will coincide with the three geographical divisions of South, Central and North Dravidian. It can also be said, owing to the close affinities between certain of these languages, that there are seven sub-groups within the proposed sub-families.

- SDr.: 1. Tamil, Malayalam, Toda, and Koṭa.
 2. Kannada, Koḍagu, and Tulu.
 CDr.: 3. Telugu.
 4. Kolami, Naiki, Parji, and Gadba.
 5. Kui, Kuwi, Gondi, Konda, and Pengo.
 NDr.: 6. Kurukh, and Malto.
 7. Brahui.

Though the languages of each of these sub-groups resemble each other in many ways, it has not been established that these sub-groups possess many exclusive innovations or exclusive retentions of Proto-Dravidian features. An interesting phonological correspondence is found between the SDr. group (2) and the NDr. groups, i.e. the Proto-Dravidian *V- develops into b-. That this phenomenon is not found in any other group may be seen from the following examples: *vē- “to be hot”, “to burn”, “to cook”, etc.: *vē* (Tamil), *Vēka* (Malayalam), *vey* (Koṭa), *vēgu*, *vekka* (Telugu), *vēndi* (Kolami), *vī* (Naiki), *Vēp* (Parji), *Vē* (Gadba), *Vēva* (Kui), *Veyali* (Kuwi), *Vesne* (Konda).

*V- > b-: *bē*, *bēyu*, *bēsu* (Kannada), *be'y* (Kodagu), *bēyuni* (Tulu), *bi'ina*, *bīta'ana* (Kurukh), *bice*, *bite* (Malto), *bāsing*, *bising* (Brahui).

This shift of *v- to b- needs not be taken as two independent developments, one in the Kannada, Koḍagu and Tulu group of the South Dravidian, and the other in the Kurukh, Malto and Brahui groups of the North Dravidian. According to Narasimhia this shift occurred in Kannada about the 9th century A.D. The Kurukhs and the Maltos originally belonged to one tribe known as Urāōs. These Urāōs were really Kannadas, who emigrated from the Kannada country sometime after the 9th century and before the Muhammedan invasions. This is clear from the following statement of Ferd Hahn (*Kurukh Grammar*, 1900, p. iv): “According to the tradition of the Urāōs their ancestors formerly lived in the Karnātik, where Canarese is spoken. From there they went up the Narbada river and settled in Bihar on the banks of the Son river. Rohitas is their ideal place. Being expelled from Rohitas by one of the earlier Muhammedan invasions, the Urāōs split into two parts, one travelling north-east settled in the Rajmahāl hills, and the other following up the northern Kōel entered Chōtā Nāgpur. The former call themselves Maler men and their language Malto, belonging to men, and the latter are the Urāōs who call themselves Kurukh.” The marked similarity in phonology and morphology existing between Kurukh and Malto of the NDr. can thus be attributed to their recent separation.

Proto-Dravidian (PDr.) Phonemes

The following phonemes may be reconstructed for proto-Dravidian (PDr.).

Vowels: i, e, a, o, u. In Dravidian long vowels contrast with the corresponding short vowels.

Consonants:

	Labial	Labio Dental	Dental	Alveolar	Retro flex	Palatal	Velar
Stop	p		t	ʈ	ʈ	c	k
Nasal	m		n	ɳ	ɳ	ɲ	
Lateral Non-Fricative				l	ɭ		
Lateral- Fricative					ɮ		
Trill				r			
Frictionless continuant		v				y	

PDr. features:

The following PDr. features have been brought to light by recent researches.

1. The inventory of Tamil phonemes corresponds closely to that of the PDr. phonemes.

2. Only an unvoiced stop and not a voiced stop begins a word in PDr. Tamil alone among the Dravidian languages represents the state of affairs in the parent language in this matter. The word for “nest” in different Dravidian languages given below will show that voicing in word initial is secondary:

Kūtu (Tamil), *Kūtu* (Malayalam), *guṛ* (Koṭa), *ku.d* (Toda), *gūḍu* (Kannada), *guḍi* (Koḍagu), *gūḍu* (Tulu), *gūḍu* (Telugu), *gūḍa* (Parji), *gude* (Gadba), *gūdā* (Gondi), *kīranji* (Kui), *Kūḍa* (Kuwi). It is interesting to see that the verbal form from which this noun is formed appears in almost all the Dravidian languages with the initial k- and not with g-. **kūtu* “to come together”, “assemble”, “arrive at”: *Kūtu* (Tamil), *kūṭuka* (Malayalam), *kuṛ* (Toda), *kūḍu* (Kannada), *kuḍ* (Koḍagu), *kūḍuni* (Tulu), *kūḍu* (Telugu), *kūr* (Kolami), *kūr* (Parji), *Kūḍi kīnai* (Kuwi), *khōṇḍnā* (Kurukh). From these illustrations it is obvious that the initial k- of the verbal form originally prevailed in the nominal form too, and that the voicing in the nominal form of Kannada, Telugu and the other languages is secondary.

3. Only the SDr. languages and Telugu have alveolar phonemes distinguished from dentals and retroflexes. As pre-Parji possesses some archaic forms with alveolar phonemes, it is clear that languages of Central India which are related to it must have had these phonemes and later lost them. These alveolar phonemes are reconstructable for PDr.

4. PDr. had only a “c” phoneme and not “s”. In some of the languages c has

developed into s and in others into t or h. **Cuṭu* “to be hot”, “burn”, “to roast”, “to cook”; *Cuṭu* (Tamil), *Cuṭuka* (Malayalam), *tuṛ*, *cuṛ* (Koṭa), *tuṛ* (Toda), *suḍu* (Kannada), *cuḍ* (Koḍagu), *Suḍupini*, *cuḍu* (Tulu), *cūdu*, *suḍiyu* (Telugu), *suḍ*, *cuṛ* (Kolami), *surrānā* (Gondi), *hūdali* (Kuwi), *Kuṛnā* (Kurukh), *kuṛe* (Malto). In the NDr. languages, the PDr. *c- has developed into k- as in *kuṛnā* and *kuṛe*. An opposite development has taken place in Tamil, Malayalam and Telugu. In these languages the original *k- is palatalized to c- when followed by front vowels, e.g. *Cevi* “ear” (Tamil), *cevi* (Malayalam), *cevi* (Telugu). But in the other SDr. and CDr. languages the initial k- is preserved, e.g. *kev* (Toda), *kebi* (Tulu), *kawi* (Gondi), *Kēv* (Kolami), *kriu* (Kui), *Kiriyu* (Kuwi). In the NDr. languages the initial k- develops into kh- in Kurukh and Brahui, and into q in Malto, e.g. *khaf* “ear” (Brahui), *Khebda* (Kurukh), *qethwu* (Malto).

5. All the Dravidian languages, with the exception of Brahui, show a distinction between exclusive and inclusive in the 1st plural pronoun and in pronominal references in the verb; it is a feature that is reconstructable for PDr.

6. Of the Dravidian languages only Toda and Brahui show no distinction of gender. With these exceptions, the SDr. languages have a three-gender system, i.e. masculine, feminine and neuter, and the CDr. and NDr. languages have a two-gender system, i.e. masculine and neuter. As Parji of the CDr. shows archaic forms with feminine terminations, it is clear that the three-gender system of the SDr. languages originally prevailed in the other Dravidian languages as well and that the introduction of a two-gender system is an innovation of the CDr. and the NDr. languages. Therefore, a three-gender system is reconstructable for PDr.

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LE TOKHARIEN ET LES LANGUES INDO-EUROPÉENNES OCCIDENTALES

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Abstract

A partir de 1949 l'apport de nouvelles données linguistiques dans le domaine tokharien a permis de compléter et de préciser l'étude de cette langue au point de vue de la phonétique, de la morphologie et de la syntaxe, mais c'est surtout le vocabulaire tel quel qui se trouve enrichi sensiblement. C'est là un fait très important, puisque pour la question de la *position* du tokharien dans le *groupe indo-européen* on dispose désormais d'une base plus solide. Il y a donc lieu de réexaminer cette question à la lumière des concordances lexicales et de vérifier les hypothèses assez divergentes de Meillet, Charpentier, Hermann, Pokorny, Pedersen, Feist, Reuter, Benveniste, Petersen, Krause et Porzig.

Or les recherches s'étendant à une documentation lexicale qui n'est plus indigente, prouvent que si le tokharien accuse des correspondances de vocabulaire exclusives avec le sanskrit, l'iranien et l'arménien (avec le thraco-phrygien), elles sont plutôt rares. Elles sont un peu plus nombreuses avec le hittite. Mais c'est surtout avec les langues occidentales (y compris le grec) que les rapports lexicaux s'avèrent très riches: il y a des correspondances exclusives avec le groupe entier de ces langues, avec deux ou plusieurs membres du groupe, et finalement aussi avec l'une ou l'autre de ces langues prises séparément. Parmi celles-ci le *grec* et *surtout* le *germanique* entrent en première ligne.

Dans la famille indo-européenne il faut donc situer le tokharien à l'Ouest et le considérer comme un membre ancien d'un groupe qui confinait d'une part au germanique, de l'autre au grec.

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THE PRESENT STATE OF NEW GUINEA (NON-MELANESIAN OR PAPUAN) AND AUSTRALIAN HISTORICAL AND COMPARATIVE LINGUISTICS

S. A. WURM

Our knowledge of the very numerous non-Melanesian, or Papuan, languages of the New Guinea area is only just beginning to grow out of its infancy. For large sections of this comparatively small, but linguistically extremely diverse, region we have only very little, and at best unsatisfactory, information, or none at all, and even of the so-called "known" languages only a small fraction has been studied and described in some detail.

New Guinea has long been believed to be occupied by a great number of languages which were largely unrelated, only a few of the languages being recognizable as related and constituting small, mutually unrelated groups. Only in recent years the existence of large groups of interrelated non-Melanesian, or Papuan, languages has been established and the detailed study of the nature of their relationship initiated.

In some cases, the existence of the interrelationship between the languages constituting the groups has been postulated on the grounds of the results of lexico-statistical studies based on counts of apparent cognates only, without the detailed working out of the sound-shifts involved. The groups thus established must therefore be regarded as tentative, though the number of such apparent lexical cognates shared by the individual languages, and the range of agreements in morphological and other structural features between them, make the existence of interrelationship between them a near-certainty.

Apart from these cases, however, thorough comparative work has been undertaken, or is being carried out, in the case of several of the recently established language groups. Research on these lines is in progress in several language groups in West New Guinea, in particular by G. F. Larson of the Christian and Missionary Alliance in the Ndani Family in the Balim Valley area, and of the Kapauku-Migani(Moni)-Woda Family in the Wissel Lakes area. J. D. Ellenberger, also of the Christian and Missionary Alliance, is engaged on a similar study of the four major languages of the Uhunduni Family, also in the central highlands. A. Healey and Ph. Healey, both of the Australian National University, are at present working in languages of the Telefolmin group in the southwestern highlands area of the Sepik District in Australian New Guinea, with a view to carrying out both descriptive and comparative studies.

D. Laycock, also of the Australian National University, has completed his descriptive and comparative studies of the nine languages constituting the Ndu Family

established by him in detail, and located in the Sepik District of Australian New Guinea. He has finished writing up his results for his Ph.D. thesis and will publish them in the foreseeable future. In the part of his study which deals with comparison, he has given a discussion of the sound correspondences observable when comparing the various languages of the Ndu Family and can consequently put his lexico-statistical work on a sound basis. In addition to phonological criteria, he also gives attention to comparison of structural features. His study is likely to be the first of its kind involving non-Melanesian languages which has ever been published.

S. Wurm, of the Australian National University, has been assessing the materials which he collected in sixty languages spoken in the three Highlands Districts of Australian New Guinea, and could establish the existence of what he named the East New Guinea Highlands Stock comprising forty-seven of the sixty languages, and consisting of five families, four of which can be subdivided into four to five sub-families each. In his studies directed towards the establishment of the families and the stock, he employed lexico-statistical methods, first counting apparent cognates. After further analysis of the languages sharing a considerable number of apparent cognates, attempts were made to recognize regular sound correspondences and to study sound-shifts, and the lexical comparisons were re-examined in the light of the findings of this work. This was followed by a comparison of morphological and other structural features of the individual languages.

After the stock had been established, comparisons were made between languages of the stock and some languages adjacent to them which had been found to share less than 12% apparent basic vocabulary cognates with the former, but more than 5%. A few regular sound-correspondences could be found in these instances, and some cases of shared structural features. It seems therefore possible to include both the stock and these languages of which five have so far been found, into an East New Guinea Highlands Phylum.

The detailed results of this work are being written up for publication but a language distribution map in fourteen colours, which shows the composition of the phylum and the stock in great detail and is to form a part of the final publication, has already appeared.¹

A number of language groups have been tentatively established in the New Guinea area with which the state of knowledge of the individual members is now good enough, or nearly good enough, for allowing detailed comparative work in them without a large amount of additional field work. Such language groups are, for instance, in West New Guinea, the Kamoro-Sempan-Asmat Group on the south coast, and some groups recently established by J. Anceaux, and in Australian New Guinea at least one of the three groups located in the Huon Peninsula, the Binandere Group along most of the coast and in the hinterland of the Northern District, and the Toaripi Group along the coast of the Gulf District to the east of the Purari Delta.

¹ S. A. Wurm, *Languages of the Eastern, Western and Southern Highlands, Territory of Papua-New Guinea*. Map in fourteen colors (Canberra, Australian National University, 1961).

Owing to the complete lack of materials illustrating earlier stages of the present-day non-Melanesian or Papuan languages, work in New Guinea historical linguistics, if entered upon, can only follow the lines of reconstructing hypothetical proto-forms on the basis of dialect comparisons. The only attempt of this kind so far undertaken is S. Wurm's study of the Kiwai Languages carried out over a decade ago.² In the light of additional information on languages of the Kiwai Family which has since come to hand, Wurm's work and his results can only be regarded as tentative and preliminary.

As a digression, it may be mentioned that apart from the non-Melanesian, or Papuan, languages met with in the New Guinea area, languages belonging to the Malayo-Polynesian or Austronesian Stock are also found there chiefly in coastal sections of the New Guinea mainland and in the islands regions of the New Guinea area. Comparative and historical studies involving these languages which are usually referred to as Melanesian are on a somewhat firmer basis than those concerning non-Melanesian languages, because of the work gone into, and the results arrived at, in Malayo-Polynesian comparative and historical linguistics. Such studies have in fact been undertaken, chiefly by A. Capell, of the University of Sydney, and by W. H. Goodenough, at present at Cornell University. At the same time comparative and historical studies in them encounter some unusual problems in view of the special nature of the Melanesian languages which are, by some scholars, regarded as mixed languages consisting of a Malayo-Polynesian and a non-Malayo-Polynesian element. However, this field of study is outside the immediate scope of this paper.

The situation with regard to comparative and historical linguistics in Australia is rather different from that found in the New Guinea area. It seems fairly certain by now that most of, if not almost all, the several hundred languages spoken by the Australian aborigines are interrelated and constitute a phylum with about 85% of its member languages forming a single stock. Apart from agreements in basic vocabulary, the languages share many morphological and other structural features which simplifies their comparison.

The earliest attempt at Australian comparative linguistics was the study of P. W. Schmidt in 1919³ who tried to classify all the languages then known into a number of groups on the basis of the assessment of the number of probable cognates in the fragmentary lexical materials available to him. He suggested that the languages in the north-western third of the continent were unrelated to each other and to the rest of the Australian languages which he subdivided into a number of interrelated groups.

After Schmidt, A. Capell undertook a very considerable amount of comparative study in Australian languages, placing the main emphasis on the comparison of structural features. As a result of this work, he classified a great number of the Australian languages into several structural groups, and attempted to arrive at historical explana-

² S. Wurm, *Studies in the Kiwai Languages, Fly Delta, Papua, New Guinea* (= *Acta ethnologica et linguistica*, 2) (Vienna, 1951), 124 p.

³ P. W. Schmidt, *Die Gliederung der Australischen Sprachen* (Vienna, 1919), xvi+299 p.

nations of the distribution and occurrence of certain structural features in the present-day Australian languages. At the same time, he tried to recognize sound correspondences, but found that not infrequently sounds in lexemes of one language corresponded to, or differed from, the sounds of the apparently cognate and often very similar lexemes of another language in a rather random, irregular and unpredictable manner. The establishment of regular sound-correspondences was made quite difficult and often impossible by this phenomenon. It still remains to establish whether this feature is attributable to an extremely high degree of borrowing between many Australian languages, or perhaps to some other factor.

In spite of these difficulties, Capell suggested tentative proto-forms for thirty-five lexemes which are met with almost universally throughout Australia, and also listed lexemes showing regional occurrence only. He also drew conclusions regarding the possible nature of a proto-Australian sound system. At the same time he postulated that all Australian languages ultimately belonged to one group, even though some language groups within this Australian group differed considerably from the bulk of the languages contained within it. Capell published a summary of his findings in monographic form,⁴ and he is at present preparing a second volume on Australian comparative linguistics.

G. O'Grady, at present at Indiana University, carried out comparative studies in the languages of a section of Western Australia as a part of his B.A. thesis.⁵ He also encountered some difficulties with unpredictable sound-correspondences and random sound-shifts like those which Capell had experienced.

K. Hale, of the University of Illinois, undertook comparative studies in the Arandic languages of Central Australia. In this group he succeeded in establishing regular sound correspondences between the individual languages belonging to it which allowed him to put his lexico-statistical comparisons on a sound basis and also enabled him to reconstruct proto-Arandic forms. As an illustration of the degree of the relationship existing between Australian languages belonging to one particular group within the entire Australian phylum, it may be mentioned here that the cognate density for Arandic is based on 40% sharing. In consequence, Arandic constitutes a family.

Hale also discovered a number of sound-correspondences between Arandic languages and Australian languages not belonging to the Arandic family. At the same time he established the percentage of shared cognates between Arandic and non-Arandic languages to be maximally 25%. His findings on the Arandic Family are to be published in the near future.

In addition, Hale carried out similar comparative work in one of the language groups on Cape York Peninsula. This group which is located in the north-western portion of the peninsula and may conveniently be called the Leningitig Group after

⁴ A. Capell, *A New Approach to Australian Linguistics* (= *Oceania Linguistic Monographs*, 1) (University of Sydney, 1956), ii+103 p.

⁵ G. O'Grady, *Significance of the Circumcision Boundary in Western Australia*, B.A. Thesis, University of Sydney, 1959, 187 p.

one of its member languages, consists of a number of languages which are quite different in their sound systems from other Australian languages and which at first glance appear to constitute a non-Australian group. However, after establishing the quite regular sound-correspondences between languages of this group and reconstructing protoforms, Hale was successful in linking a number of these forms with those met with in other Australian languages by discovering some regular sound-shifts. He has almost finished his manuscript on this subject for publication.

At the same time Hale has embarked on a preliminary genetic classification of all Australian languages on the basis of lexical comparisons involving the establishment of regular sound-correspondences as far as these can be found. In the course of this work, he discovered that approximately 85% of the Australian languages were quite closely interrelated with over 90% of the remaining 15% constituting several small groups more distantly related to each other and to this large group. For the latter group which stretches right across Australia from the north-east and east to the west and southwest, Hale suggests the name Pama-Nyungan which is made up from the word for "man" in the south-western and north-eastern portions of this language group.

Less than 1% of the languages in Australia appear to be unrelated to the other Australian languages. One of these languages used to be spoken in eastern New South Wales, and another one in the rain forest area of North Queensland where the natives differ racially from the rest of the Australian Aborigines. The extinct Tasmanian language or languages also belong to this category.

A cooperation is planned between Hale, O'Grady, Capell and Wurm in the genetic classification and grouping of the Australian languages initiated by Hale and a joint publication is intended along these lines in the foreseeable future.

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THE PHONEMIC PATTERN OF SOUTH-LAPPISH

GUSTAV HASSELBRINK

The South-Lapp dialects differ from Northern Lappish through absence of stage-alteration and through the prominence of palatalization- and velarization-phenomena. The speech-sounds are multitudinous but we are able to reduce them to a moderate number of phonemes. As a representative I use the dialect of Vilhelmina in Sweden.

The STRESS-ACCENT serves no distinctive purpose but is of importance for the word-structure and the distribution of allophones. The main stress rests on the first syllable and indicates the word-boundary. A following odd syllable may have a secondary stress.

CONSONANTS (/ [C] /)¹ standing before or after a stressed nucleus are divided into two *main series*: palatalized (pz., / [C,] /)²: the not final consonants in /b,iss,edh/ [b,iss,eth] "roast", and velarized (vz., / [C] /): the not final consonants in /baskedh/ [basktəh] "prick, sting". They are characterized by pro- or retraction and secondary articulations of the back-tongue.

Final consonants and consonants standing after an unstressed nucleus are *neutral* (nl., / [C] /): /n/ in /g,ien/ "whose" sg., /g/ in /čakčege/ "in autumn".

Quantity is distinctive: / [b,ies,ie] / "bird's nest": / [b,iess,ie] / "birch-bark".

STOPS appear as more or less voiced lenes [b d g] or as voiceless fortes which may be unaspirated [p t k], preaspirated [hp ht hk], postaspirated [ph th kh] or interaspirated, e.g. [pht].

Lenes and unaspirated fortes form one group, *weak stops* or *leniores* /b d g/ and the pre- or postaspirated another, *strong stops* or *fortiores* /p t k/. In some positions there are *indifferent stops* for which I use the same symbols as for the strong.

Indifferent are all stops preceded by a dental fricative or an affricate. They are then mostly unaspirated fortes: /skaarjaa/ "scissors", /byst,e/ "spoon", /čeck,ie/ "hermine". When an /r/ follows they are often aspirated: /strompuo/ "brushwood used for fuel". Stops standing initially before a liquid or a nasal range from a voiced lenis to a postaspirated fortis: /pluov,ie/ [b ~ ph] "bog, swamp", /tr,eevгаа/ [d ~ th] "ski", /knoogg,edh/ [g ~ kh] "rub".

¹ Slants / / indicate phonemic, and square brackets [] phonetic transcription; / [] / indicates that both transcriptions are identical.

² A comma after a consonant marks that it is palatalized.

In other positions are the stops *differentiated*.

In an initial solitary position the weak stops are lenes with varying voice and the strong postaspirated fortes: /d,ijj,este/ "from you" : /t,ijj,este/ "from the time".

Internally after an odd nucleus all sorts of stops are found. The weak intervocalic stops are unaspirated fortes [p t k] and the strong preaspirated [hp ht hk]. Weak single stops stand in opposition to weak geminates: /loabuoh/ "soles" : /loabbuoh/ "they spread", and weak geminates to strong: /seedd,edh/ "send" : /seett,edh/ "immerge, submerge". Before a voiced consonant, except a homorganic nasal, there are only lenes with strong voice /[b d g]/: /l,ebjaa/ "a leafe-like ornament", /ebr,ie/ "rain", /gaagne/ "useful". A voiced /[b]/ is also heard before another weak stop: /rabgelidh/ "blink" : /rapkelidh/ "frighten" tr. Farther south there is instead an /m/. Before a homorganic nasal all stops are unaspirated fortes: /rebm,ie/ "corpse", /bedn,ie/ "bottom", /jugne/ "red whortleberry". Clusters of two strong stops are interaspirated: /vuopte/ "hair" and a stop followed by dental fricative postaspirated: /v,eepsaa/ "wasp", /aakšuo/ "axe". After a voiced consonant unaspirated fortes stand against preaspirated: /s,ijdj/ gpl. "nomades' camp" : /s,ijtij/ "he would, wished", /g,ierg,ie/ "stone": /g,ierk,ie/ "glutton (Gulo luscus)".

After an even nucleus single weak stops stand as voiced lenes against preaspirated fortes: /vuojede/ "he swims" : /vuoj,ete/ "he drives".

In a final position (except by vowel-elision) only /[b]/ is found: /muoreb/ asg. "tree", /guvleb/ "I hear". The southern dialects have here an /m/.

The AFFRICATES are treated in a similar way as the stops, but they are never postaspirated and stand never after fricatives. Voiced varieties are unknown in Vilhelmina but occur in the South (Härjedalen). Even the affricates can be weak /ʒ ʒ̥/, strong /c č/ and indifferent /c č/.

Initially only solitary affricates are found. In Vilhelmina they are voiceless fortes [ts tš] which must be regarded as *indifferent*: /caabm,edh/ "beat", /čuoppedh/ "cut". In the South they are lenes, often with a faint voice. Indifferent are also all affricates before another consonant: /gaack,edh/ [ts] "bite", /r,iečm,ie/ [tš] "cord, string".

Inside a word they are *differentiated*. Between vowels there are oppositions of weak single affricates against weak geminates: /v,iež,ere/ "hammer" : /v,iežž,ede/ "you (pl.) fetch", and of weak geminates against strong: /božžaa/ s. "grease" : /boččaa/ "he, she milks".

Oppositions also exist in clusters beginning with a voiced consonant: /j,ijže/ refl. pron. : /č,ijče/ "seven". Not quite analogous is an opposition like /laabž,ie/ "rein, bridle" : /gapčedh/ "cover", where there in the South is an /m/ instead of /b/. Other clusters of a stop and an affricate are interaspirated: /ukc,ie/ "eight".

After an even nucleus are also both weak and strong affricates found: /leddeže/ "little bird", /g,ier,ece/ "Lapp sleigh".

In a final position there are no affricates except by vowel-elision before an /h/.

SEMIVOWELS, LIQUIDS and NASALS are in principle to be regarded as voiced. When preceded by an /h/ they have however a voiceless ingress: /troohn,eh/ "queen, lady",

/r,ihr,ie/ "track of reindeer and sleigh", and before a strong stop a voiceless termination: /g,erkuo/ "church".

The *semivowels* /j v/ sound often as fricatives. The pz. /v,]/ resembles an Engl. *v* but the vz. /v/ [w] is like an Engl. *w*: /v,irre/ "blood" : /varredh/ "run".

Liquids are /l r/. The pz. /l,]/ is like a French *l* but the vz. /l/ [ʎ] reminds us of a Polish *l*: /l,ijn,ie/ "cloth" : /laajg,ie/ "yarn".

Nasals are /m n ń ŋ/. Overlapping exists between /ń/ and /ŋ/: /b,ieńe/ [b,iěńə ~ b,iěŋə] "dog".

Voiceless FRICATIVES are /f s š/, of which /f/ can be either bilabial or labiodental. Some have /hv/ instead of /f/ in an internal position: /raaf,ie ~ raahv,ie/ "peace", /derf,ie ~ derhv,ie/ "peat, turf". The pz. /s,/ and /š,/ overlap: /sk,ierr,ie ~ šk,ierr,ie/ "dwarf-birch". The same is the case before an /ń/: /sńočcaan ~ šńočcaan/ "pointed, peaked".

An initial /h/ is pronounced almost as in Engl.: /haarduo/ "shoulder" as in *hard*, /h,ierg,ie/ "horse" as in *hear*. Internally occurs a geminate with a rather rough timbre: /rehhaa/ "fell, rug". Before a voiced consonant /h/ says that it has a voiceless ingress: /troohn,eh/ [tʰoŋ] "queen, lady". A final /h/ consists in a voiceless termination of the preceding vowel or in something like a German *ich-* or *ach-*Laut. When an /h/ stands finally after a short vowel preceded by a stop or a voiceless consonant, the vowel becomes whispered or falls out. A stop then gets a "false aspiration": /geegg,h/ [gäekk,x] "kegs". If it is preaspirated the aspiration becomes doublesided: /čakkh/ [fšahkkx] "mountain tops". A voiced stop becomes voiceless: /gaamegh/ [gaaməkk] "shoes". After an affricate or a fricative is /h/ realized as a prolongation of that sound: /ruoch/ [ruohtss] "roots", /gaash/ [gaass] "geese", or is not realized at all: /vuossh/ [wuoss] "sacks, bags".

The VOWELS (/V/) show a great variation. To denote their PHONETIC VALUES I use the following symbols:

	Front	Central	Back
High unrounded	[i	ī i	
rounded	y	ü ü	u ʊ
Mid unrounded	e	ě ə	
rounded		ö	o
Low unrounded		ä	a]

Those symbols are used single or in combinations where they indicate the tongue-position at the beginning and the end of a diphthongal sound; [i] resembles the Polish *y*, [ě] is intermediary between it and *a* in *man*, to which [ä] comes near, [y] resembles the German *ü* und [ü] the Swedish *u* in *hus*; [i ü ə ʊ] are lax counterparts to [i ü ě u].

The short front and back vowels have a rather uniform articulation, but all others are more or less gliding. For that reason no definite boundary can be drawn between

monophthongs and diphthongs. So-called proper diphthongs are regarded as sequences of a vowel + /j v/.

Quantity is connected with quality and its distinctiveness is questionable. To represent long vowels I use double letters /[VV]/. The long counterparts of /i u/ appear like proper diphthongs and are written /ij/ [ij ij] and /uv/ [uw üv]. The contrast long : half-long depends on the length of the following consonant(s) and is not distinctive: /[b,ies,ie]/ "bird's nest": /[b,iess,ie]/ "birch-bark".

We can group the vowels together into *main series* (MS) interdependent with the MS of the consonants and unequal in number in different positions.

IN A STRESSED SYLLABLE OF POLYSYLLABLES there are four MS:

Palatal (pal.), front uniform /[,V,]/: /b,iss,edh/ "roast",

Velarized (vz.), gliding backwards /[,V]/: /b,issedh/ "wash",

Velar (vel.), back uniform /[V]/: /laddeste/ "from rachitis",

Palatalized (pz.), gliding forwards /[V,]/: /ledd,este/ "from the bird".

In that position the *phonemes* /V/ with their chief *allophones* [V] are distributed in the following way:

Short:

/a/: [a]: /sarveste/ elsg. "elk"; /gaske/ "middle";	/o/: [o]: /goččudh/ "call"; [o ~ a]: /dolle ~ dalle/ "fire";
/e/: [ä]: /b,essaa/ "he washes; roasts"; [ä ~ ,o]: /d,evvudh ~ d,ovvudh/ "repair";	/u/: [u]: /jukke/ "brook, small river"; [u,]: /guks,ie/ "wooden scoop";
[ä]: /serveste/ elsg. "reindeer bull";	/y/: [y,]: /d,yvv,eke/ "loon, diver";
[ë,]: /serv,ies/ coll. "the bulls of the herd";	[ü ~ ,u]: /b,yssuve/ "is washed";
/i/: [i,]: /b,iss,edh/ "roast";	[u]: /n,ymme/ (or as /n,umme/ to /u/?) "name";
[i] /b,issedh/ "wash";	[ü,]: /jytt,edh/ "migrate, nomad- ize".
[i,]: /gisk,e/ "between";	

Long:

/aa/: [aa]: /baaleme/ pp. "dig"; [aä,]: /baal,eme/ vbs. id.;	/oe/: [oä]: /boedeme/ pp. "come"; [oë,]: /boed,eme/ vbs. id.;
/ee/: [ee,]: /b,eejj,ij/ gpl. "day"; [eä]: /b,eejaan/ illsg. id.;	/oo/: [ö]: /v,ooldede/ "grows white"; [oo]: /soole/ "island";
[äe,]: /geegg,e/ "keg";	[öö,]: /gookt,e/ "two";
/ie/: [ie,]: /b,iejj,ie/ "day";	/ue/: [uä]: /vuenuve/ "mother-in-law";
[ië]: /b,iegge/ "wind";	/uo/: [uo]: /muore/ "tree";
/oa/: [oa]: /boaluo/ "button";	[uö,]: /guol,ie/ "fish".

In a non-initial odd nucleus a long vowel alternates with a similar short one: /h,ijv,enlaagaan ~ h,ijv,enleggaan/ adv. "well"; /jottaaj,ieb,ie ~ jottaaj,ibb,ie/ "we begin migrating"; /v,iššetuvvedh ~ v,iššetuvvedh/ "get tired".

IN AN EVEN SYLLABLE and IN STRESSED MONOSYLLABLES there are two MS: pal. /[,V]/: /gisk,e/ "between", /g,ien/ "whose" sg., and vel. /[V]/: /gaske/ "middle", /duon/ "istius". The *phonemes*, represented by symbols enclosed by slants /V/, and their positional allophones are:

In polysyllables:

Short:	Long:
/e/: [e]: /gisk,e/ "between";	/aa/: [aa]: /d,eevaa/ "hill";
[ə]: /gaske/ "middle";	/ie/: [ie]: /goed,ie/ "house, hut";
/i/: [i]: /bood,ib/ "I came";	/uo/: [uo]: /j,eenuo/ "river".
[i̥]: /guvlib/ "I heard";	
/u/: [u̥]: /oer,uve/ "squirrel";	
[u̥]: /vuenuve/ "mother-in-law";	

In monosyllables:

Short:	Long:
/i/: [i]: /m,ij/ "what";	/ie/: [ie]: /g,ien/ "whose" sg.;
/a/: [a]: /dan/ "huius";	/aa/: [aa]: /maab/ asg. "what";
/u/: [u]: /dun/ "illius";	/uo/: [uo]: /duon/ "istius".

UNSTRESSED MONOSYLLABLES have only one neutral (nl.) MS and only one phoneme, a lax /i/ [i̥]: /jih/ "and", /vij/ "or".

IN ODD UNSTRESSED SYLLABLES there are also only short neutral vowels but two phonemes: /e/ [e ~ ə]: /gaamege/ "shoe" and /i/ [i ~ i̥]: /gaamegij/ gpl. id.

VOWEL-ELISION occurs not only before an /h/ but even else by short vowels between two voiceless consonants in an unstressed syllable: /v,eelg,es ~ v,eelg,s/ "white".

During inflection and word-formation a stressed vowel often alternates with another. I call that METAPHONY. The concerned syllable I call the *main syllable* and the next the *side-syllable*. Monosyllables have no metaphony.

The main syllable alters in a regular way. The supposed base for the alterations is the *metaphony-base* (MB). Vowels going back to the same MB form a *base-series* (BS) and the actually occurring vowels I call *derivates* (D) wih I group together into *derivate-series* (DS). Main DS are:

The *palatalizing* DS (pzig.): main-syllable /[(,)VC,]/, side-syllable /[,V]/, with the sub-series *broad* (br.) and *narrow* (nw.).

The *velarizing* DS (vzig.): main-syllable /[(,)VC]/, side-syllable /[V]/, with the sub-series *broad*, *narrow* and *labial* (lb.).

The different BASE-SERIES with their DERIVATES are:

The i-BS:	The ie-BS:
Pzg. br.: /,i,/: /b,iss,edh/ "roast":	/,ie,/: /g,ies,edh/ "draw": /g,ies,eme/
/b,iss,eme/ vbs.,	vbs.,

- nw.: /i,/: /b,iss,ij/ 3 sg. pt., /ee,/: /g,ees,ij/ 3 sg. pt.,
 Vzg. br.: /e,/: /b,esseme/ pp., /ee,/: /g,eeseme/ pp.;
 nw.: /i,/: /b,issedh/ "wash" : /b,isseme/ pp. vbs., /b,issij/ 3 sg. pt.; /ie,/: /g,iesedh/ "wind in" : /g,ieseme/ pp. vbs., /g,iesij/ 3 sg. pt.;
 lb.: /y,/: /b,yssedh/ "be roasted" : /b,ysseme/ pp. vbs., /b,yssij/ 3 sg. pt.; /oo,/: /g,oosedh/ "be drawn" : /g,ooseme/ pp. vbs., /g,oosij/ 3 sg. pt.

The a-BS:

- Pzg. br.: /e,/: /beld,edh/ "frighten" tr., /beld,eme/ vbs.,
 nw.: /i,/: /bild,ij/ 3 sg. pt.,
 Vzg. br.: /e,/: /beldeme/ pp., /berguo/ s. "work";
 nw.: /a/: /bargedh/ v. id. : /bargeme/ pp. vbs., /bargij/ 3 sg. pt.;
 lb.: /u/: /buldedh/ "get frightened" : /buldeme/ pp. vbs., /buldij/ 3 sg. pt.

The aa-BS:

- /aa,/: /baal,edh/ "dig" : /baal,eme/ vbs.,
 /ee,/: /beel,ij/ 3 sg. pt.,
 /aa/: /baaleme/ pp. : /gaadudh/ "go away", vbs. /gaadume/;
 /aa/: /baajedh/ "let" : /baajeme/ pp. vbs., /baajij/ 3 sg. pt.;
 /oo/: /goodeme/ pp. "go away".

The u-BS:

- Pzg. br.: /y,/: /byčč,edh/ "milk" : /byčč,eme/ vbs.;
 /u,/: /luvl,ie/ "east";
 nw.: /y,/: /byčč,ij/ 3 sg. pt. "milk",
 Vzg. br.: /o/: /boččeme/ pp.;
 nw.: /lukkedh/ "read" : /lukkeme/ pp. vbs., /lukkij/ 3 sg. pt.
 lb.: /u/: /buččedh/ "give milk" : /buččeme/ pp. vbs., /buččij/ 3 sg. pt.

The uo-BS:

- /uo,/: /vuoj,edh/ "drive" : /vuoj,eme/ vbs.,
 /oo,/: /vooj,ij/ 3 sg. pt.,
 /ue/: /vuejeme/ pp.;
 /oa/: /voajudh/ "sink" : /voajume/ vbs.;
 /uo/: /vuojedh/ "swim" : /vuojeme/ pp. vbs., /vuojij/ 3 sg. pt.;
 /oo/: /vooje/ 3 sg. pr., /voojeme/ pp. "sink".

The oa-BS:

- Pzg. br.: /oe,/: /boed,edh/ "come" : /boed,eme/ vbs.,
 nw.: /oo,/: /bood,ij/ 3 sg. pt.,
 Vzg. br.: /oe/: /beedeme/ pp.;
 /oa/: /goarudh/ "sew" : /goarume/ vbs.;
 nw.: /oa/: /čoaŋgenidh/ "gather" itr.;
 lb.: /oo/: /goore/ 3 sg. pr., /gooreme/ pp. "sew".

The o-BS coincides with the a-BS except in vzg. nw. where it often has an /o/. Many have even here an /a/: /dolle ~ dalle/ "fire".

Metaphony Table

		i-BS	a-BS	u-BS	ie-BS	aa-BS	uo-BS	oa-BS
Pzg.	br.	,i,	e,	y, u,	,ie,	aa,	uo,	oe,
	nw.	,i,	i,	y,	,ee,	ee,	oo,	oo,
Vzg.	br.	,e	e	o	,ee	aa	ue oa	oe oa
	nw.	,i	a	u	,ie	aa	uo	oa
	lb.	,y	u	u	,oo	oo	oo	oo

The preceding consonant is distinctive in some oppositions between different BS: /geedd,ijste/ elpl. "shore" : /g,eedd,ijste/ elpl. "fence, enclosure".

The following consonant is distinctive in oppositions between different main DS: /g,ies,eme/ (pzg.) vbs. "draw" : /g,ieseme/ (vzg.) pp. vbs. "wind in". In oppositions between sub-series of the same main DS is the vowel of the main syllable distinctive: /g,eeseme/ (vzg. br.) pp. "draw" : /g,ieseme/ pp. vbs. "wind in" : /g,ooseme/ pp. vbs. "be drawn".

The relations between the phonemes are sometimes disturbed by CONSONANT-INFLUENCE of two kinds: *palatal-influence* which affects vowels standing before and after a /j/ or an /ń/: /j,ijje ~ j,ijj,e / "night", /b,ieńe ~ b,ień,e/ "dog", /vuońele ~ vuoń,ele/ "reindeer heifer", and *labial-influence* which affects vowels standing before or after a labial consonant: /bist,e ~ byst,e/ "spoon", /n,ymne/ < */n,imme/ "name", /d,evvudh ~ d,ovvudh/ "repair", /vueneve ~ vuenuve/ "mother-in-law".

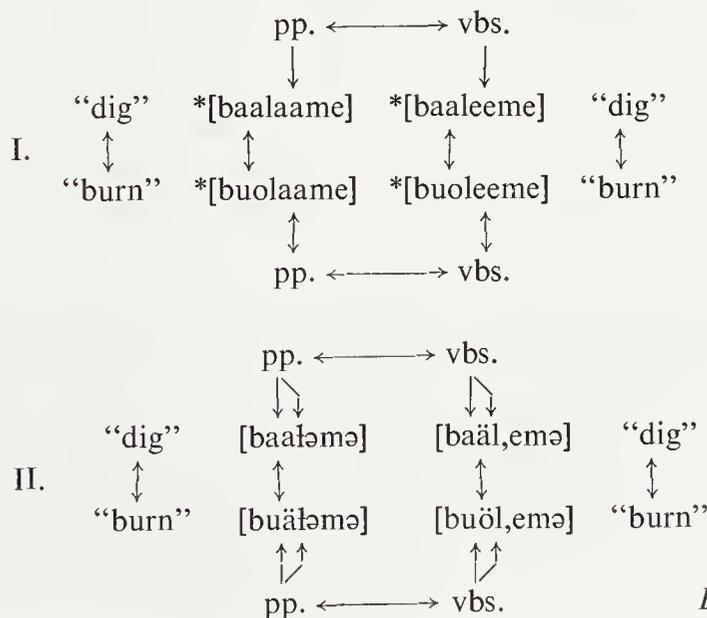
But do we really observe isolate phonemes when we hear a word? I think we mostly hear it as a unit, a "Gestalt" or CONFIGURATION. An opposition is really an opposition between configurations. Whatmough says in *Language*, p. 130: "it must not be supposed that the minimum differentiation of phonemes is what the brain interprets in terms of meaning. The economy of the brain seems to be concerned rather with the larger bounded units known as words."

Phonemes and conditioned allophones are features in a pattern which is a product of man's pattern-creating instinct and not consciently invented devices for directly practical purposes. Communication engineers must count with the phonemes as "discrete units", but from a psychological point of view I think they aren't it. If we see a pattern in black-and-white it may be a matter of taste whether we regard the black or the white features as forming it. But it is sufficient to talk about the black of them to describe it. So I have thought it most advantageous to regard the pz. and vz. qualities of the consonants as phonemes and a lot of vowel-qualities as allophones

conditioned by them. Others might perhaps go the opposite way. And I dare not to say that they are wrong.

In a South-Lapp word there exists a strong interdependence between all sounds and especially between those of a stressed syllable and its side-syllable. A word is a sound-configuration, and man has a disposition to make a configuration more pregnant. For that purpose he consciously or unconsciously tries to mark out its main point. In South-Lappish there is a strong trend to accumulate information in the main syllable at the cost of its side-syllable.

It seems that at an earlier stage information was distributed amongst the different syllables of a word. The first syllable gave semantic information and the following morphologic. In the opposition *[baalaame] : *[buolaame] the vowels of the main syllable showed that the first word had the semantic significance of "dig" and the second that of "burn". The suffix indicated that they both must be either pp. or vbs. The *[aa] of the side-syllable showed that they were pp. and stood in opposition to the vbs. *[baaleeme] and *[buoleeme]. But the *[ee] of the vbs. began to palatalize the main nucleus and the interjacent consonant, while the *[aa] in the side-syllable of the pp. affected *[uo] in delabializing direction and made *[l] vz. The morphologic significance was brought back to the main syllable. The significance of the side-syllable became redundant and its vowel was shortened, and so we got the now existing forms /baaleme/ [baaʔəmə] : /baal,eme/ [baäl,emə] and /bueleme/ [buäʔəmə] : /buol,eme/ [buöl,emə]. And even if the side-syllable still retains some significance, there is however a strong trend to confuse its vowels to insignificant vocal-murmurs. I do not say that this displacement of information is the only cause of metaphony. The fundamental cause is indubitably the predominance of the initial stress. But in every equation of a higher degree the unknown has more than one value. And we have to discuss the x-values to see if they satisfy the equation.



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“PHONEME OR CLUSTER” AND THE PHONEMIC STATUS OF THE RUMANIAN AFFRICATES

EMANUEL VASILIU

0. A sequence of two or more sounds $x, y \dots z$ may be interpreted either as a single phoneme or as a cluster a) if and only if x belongs to the phoneme X , y belongs to the phoneme $Y \dots$ and z to the phoneme Z , and b) if the sounds $x, y \dots z$ evince in their relation a certain degree of dependency. In other terms, it might be said that if x is an allophone of X and y is an allophone of Y , the sequence xy may be stated either as a phoneme or as a cluster if in some environments the occurrence of x allows the prediction of the occurrence of y .

1. If the first condition is not satisfied, i.e. if y may not be assigned to a phoneme Y actually existent in the described system, and if the occurrence of x allows us always to predict the occurrence of y , then the sequence xy must receive a compulsory monophonemic statement. In Spanish the sequence $[tʃ]$ is obligatorily a simple phoneme because $[ʃ]$ may not be assigned to an actual phoneme of Spanish and because $[ʃ]$ is always preceded by $[t]$. The sound $[ə]$ of Serbo-Croatian receives a similar phonemic status; $[ə]$ occurs automatically when an $[r]$ occurs, and the sound $[ə]$ may not be considered as allophone of some phoneme of the Serbo-Croatian.

Quite different is the situation of the sequence $[tʃ]$ of Rumanian. The two members of the sequence, $[t]$ and $[ʃ]$, may be considered as allophones of the phonemes $/t/$ and $/ʃ/$, respectively. In determined conditions the two sounds manifest a certain distributional dependence (see below, 3.2.2).

1.1. We are now able to formulate the following:

a) The question “phoneme or cluster” arises at *another step* of the sequence of the operations than the step of the “reduction of variants”, i.e. *after* the accomplishment of the reduction.

b) The question “phoneme or cluster” is the output of some *distributional peculiarity of the phonemes* and must be discussed in terms of *distribution of phonemes* and never in terms of distribution of allophones, i.e. in the terms of a lower level.

c) The question “phoneme or cluster” admits an optional solution and not an obligatory one, because the two possible solutions are based on a previous reduction of variants, on an establishment of the phonemic status of every class of sounds. The choice of a monophonemic statement is equivalent to a restatement of a previous phonemic statement.

1.2. Let us consider the allophones x, y, z, q . Every allophone is a member of a phoneme, different from the others: x is a member of the phoneme X, y is a member of the phoneme Y, z is a member of the phoneme Z and q is a member of the phoneme Q. The phonemes X, Y, Z, Q are not identical.

A sequence of two (or more) allophones may receive a monophonemic optional status if one of the two subsequent conditions are satisfied:

1) If there is at least one environment which admits the occurrence of xy and excludes the occurrence of x or y or both x and y .

2) If there is at least one environment which admits the occurrence of the sequences xq, yq, zq and excludes the occurrence of the allophones x, y, z, q , or of x, y, z only.

Note. – In the second case, *the order* of the two members of the sequences are immaterial from the theoretical point of view. Condition 2) may be stated in a theoretically equivalent manner as follows: If there is at least one environment which admits the occurrence of the sequences qx, qy, qz and excludes the occurrences of the allophones x, y, z, q or x, y, z , only.

1.3. If the monophonemic status of the sequences is chosen, we must establish the paradigmatic relations among the element xy and the elements x, y as well as among the elements xq, yq, zq and the elements x, y, z, q .

1.3.1. If \widehat{xy} is in complementary distribution with x and y , than \widehat{xy} must be interpreted as the manifestation of a coalescent syncretism of the phonemes X and Y; if $\widehat{xq}, \widehat{yq}, \widehat{zq}$ are in complementary distribution with x, y, z and q , than $\widehat{xq}, \widehat{yq}, \widehat{zq}$ manifests the coalescent syncretism of X and Q, Y and Q, Z and Q (Hjelmslev, 1953, p. 57).

1.3.2. If \widehat{xy} is in complementary distribution either with x or with y and never with both, than \widehat{xy} is an allophone either of X or of Y, respectively; if $\widehat{xq}, \widehat{yq}, \widehat{zq}$ are in complementary distribution with x, y, z , than $\widehat{xq}, \widehat{yq}, \widehat{zq}$ are allophones of X, Y, Z, respectively.

1.3.3. If \widehat{xy} is in contrastive distribution with x and y , than \widehat{xy} is a distinct phoneme as compared to X and Y. If $\widehat{xq}, \widehat{yq}, \widehat{zq}$ are in contrastive distribution with x, y, z and with q , then xq, yq, zq are distinct phonemes as compared to X, Y, Z, Q.

2. The model above constructed may be characterized as follows:

1) The proposed solutions are optional and not obligatory. This is the difference between our model and the rules established by N. S. Trubetzkoy (pp. 57-62) and André Martinet.

2) The rules above stated are purely *formal, structural*, and not phonetic. Here in lies the difference between our model and the rules stated by Trubetzkoy; his rules, as many scholars have noted, have often an evident phoneticist character.

3) The place of the model is explicitly established in the sequence of the operations of the analysis: *after* a first reduction of the variants, by an analysis of the distribution of the phonemes resulting from the reduction of the variants.

In this manner, the distinction between the optional solutions and obligatory solutions becomes possible; our model gives optional solutions only; the reduction

of variants gives obligatory solutions. The phonemic status of the Spanish affricate [tʃ] is obligatory monophonemically on the ground of the model of the reduction of the variants. The Rumanian affricate /ts/ admits an obligatory solution, on the ground of the reduction of the variants: the two-phonemic status and an optional solution, on the ground of our model: the monophonemic status (see below, 3.2.1).

4) In our model the relations of commutation are considered as immaterial, because the commutation test belongs to a lower level of the analysis: the reduction of variants. In this respect, our model is different from Martinet's rules.

5) In our model, the distributional types which allow a monophonemic statement of a sequence are explicitly formulated and enumerated.

L. Hjelmslev's (1935) and Ch. F. Hockett's (pp. 161-164) similar theories do not contain such an explicit enumeration. In this respect we consider the above constructed model as completing Hjelmslev's and Hockett's theories.

3. We shall describe the phonemic status of the Rumanian affricates in terms of the above constructed model.

3.1. The affricates of colloquial Rumanian [tʃ', ts, dʒ'] have as phonetic constituents a stop [t, d] and a continuous [ʃ', s, ʒ']. The continuous [ʃ', ʒ'] are palatalized. (This is a difference between Rum. [tʃ', dʒ'] and Engl. [tʃ, dʒ], without palatalization.)

The subsequent reductions are possible on the ground of the complementary distribution and of the phonetic similarity.

1) The sound [t] of such a segment as [tsap] "billy-goat", [tʃ'ás] "watch" may be identified with [t] which commutes with [d] in such pairs as [taj : daj] "I cut : you give".

The sound [d] of a segment as [dʒám] "window" may be identified with [d] of [daj].

2) The sound [s] of [tsap] may be identified with [s], which commutes with [z] in pairs like [sare : zare] "salt : horizon".

3) The sounds [ʃ', ʒ'] of [tʃ'ás, dʒ'ám] may be identified with [ʃ, ʒ] of pairs as [ʃale : zale] "loins : mourning", which test a commutational relation between [ʃ] and [ʒ]. The sound [ʃ', ʒ'] of [tʃ'ás, dʒ'ám] are followed by a palatal asyllabic element [ɛ̃], which may be identified with [ɛ̃] of a word as [tɛ̃akã] "sheath". The palatal asyllabic element of [votʃ', rodʒ'] may be identified with [ʲ] of a form as [lupʲ] "wolves".

On the ground of this identifications we must formulate a diphonemic status for the affricate [ts], and a triphonemic status for the affricates [tʃɛ̃, dʒɛ̃, tʃʲ, dʒʲ].

3.2. The sequences /ts, tʃ, dʒ/ satisfy the following conditions:

3.2.1. Between a vowel and an /i/ after the syllabic juncture /+ / (phonetically an [i]) the occurrence of the phoneme /t/ is excluded and the occurrence of the sequence /ts/ is admitted. Thus the cluster /ts/ satisfies the condition 1) of section 1.2.

3.2.2. Before a /+i/ (phonetically [i]), the occurrence of the phonemes /t, d/ is excluded and the occurrence of the sequences /tʃ, dʒ/ or of the phonemes /ʃ, ʒ/ is admitted: [votʃ+i, rodʒ+i, groʃ+i, vi'teʒ+i] "voices, you pray, big (pl. masc.), heroic (pl. masc.)". Thus the clusters /tʃ, dʒ/ satisfy the condition 1) of section 1.2.

3.2.3. The clusters /ts, tʃ, dʒ/ may be interpreted as monophonemic /t̥, č, ĝ/.

3.2.3.1. The phonemes /č, ġ/ manifest a peculiarity of distribution:

1) After /#/ and before /a/ the occurrence [čɛ, ġɛ], phonemically /če+, ġe+/ only is admitted, whereas the occurrence of /č, ġ/ and /e+/ is excluded.

2) After /#/ and /o, u/ the occurrences of [čj, ġj], phonemically /či+, ġi+/ and of [j], phonemically /i+/, are admitted while the occurrence of /č, ġ/ is out of question.

On the ground of condition 2) of section 1.2, the clusters /če+, ġe+, či+, ġi+/ may be interpreted as monophonemic.

After a consonant /če+, ġe+/ contrast with /e+/: /merġe+a, torče+a: nimere+a/. There is never contrast between /če+, ġe+/ and /č, ġ/.

After /#/, /či+, ġi+/ contrast with /i+/: /či+udă, ġi+ulġi+u: i+ute/ "spite, shroud: quick". There is never any contrast between /či+, ġi+/ and /č/.

On the ground of 1.3.2 /če+, ġe+, či+, ġi+/ may be considered as allophones of /č, ġ/.

3.2.3.2. The relations between /t/ and /t, s/, between /č/ and /t, ʃ/, between /ġ/ and /d, ʒ/ are the following: 1) /t/ is in contrastive distribution with /t, s/: /t̩ap, sap, tak/ "billy-goat, I dig, I am silent"; 2) /č/ is in contrastive distribution with /t, ʃ/: /'čină, 'jină, 'tină/ "supper, rail, mud"; 3) /ġ/ is in contrastive distribution with [d, ʒ]: /ġam, 'ʒale, dar/ "window, mourning, present".

The above stated facts (3.2.3, conditions 1), 2), 3)) allow us to consider that /t̩, č, ġ/ may be neither syncretisms of the phonemes /t, s/, /t, ʃ/, /d, ʒ/ respectively (cf. 1.3.1), nor allophones of the phonemes /t/ or /s/, /t/ or /ʃ/, /d/ or /ʒ/ (cf. 1.3.2), but only independent phonemes in relation to /t, d, s, ʃ, ʒ/ (cf. 1.3.3).

3.3. The cluster status of /tʃ/ implies some complications on the morphophonemic level: if /tʃ/ is diphonemic, then the plurals of /'lotkă, 'matkă/ "boat, queen-bee" would be /'lottʃ+i, 'măttʃ+i/, with a gemination of /t/. The diphonemic status of /tʃ/ implies a new type of inflexion, quite different from the others.

The monophonemic status of /tʃ/ is more suitable being more simple for the morphophonemic description. If the monophonemic status of /tʃ/ is admitted, then, on the ground of the principle of the pattern congruity, the same status must be admitted for all the Rumanian affricates.

The monophonemic interpretation of /ts, tʃ, dʒ/ implies a simplification of the consonantic cluster description, by reducing the number of the three phonemic cluster.

This simplification is advantageous for the establishment of the syllabic pattern.

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OBSERVATIONS ON PROTO-CHINESE MORPHOLOGY

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Abstract

The morphological processes observable in reconstructed Chinese have only been dealt with in a most sketchy way. Such work as has been published attacks the subject mainly from the basis of internal reconstruction. This method is very useful but must be supplemented by comparative evidence from related Tibeto-Burman languages. This relationship is generally assumed but the evidence so far cited has been meagre and only suggestive. I am at present engaged in a project of finding more evidence of the relationship of Tibeto-Burman languages with Chinese and am starting off in November, 1962 on a field trip to North India, Nepal, East Pakistan and Burma to obtain more conclusive data on the nature of the relationship of Chinese to Tibeto-Burman, and the establishing of regular sound correspondences. Results to date indicate that Lepcha (Rong) spoken in the Sikkim and Darjeeling areas offers very promising data to compare with Chinese. The present field trip is mostly to try to find other languages closely enough related to Lepcha so as to be able to set up a group of languages whose proto-form would be reasonably close to Proto-Chinese. Preliminary work indicates that both the Nung and the Abor-Miri groups stand in a close relationship to Lepcha. Data from other languages not necessarily very close to Lepcha must of course be utilized as well. My paper would stress the morphological processes in Proto-Chinese that have analogues in related languages.

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WORD CLASSES IN CLASSICAL CHINESE

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In this paper I am trying to use the text of Mencius as the sample material for a study of the word classes of Classical Chinese.¹

The text of Mencius consists of approximately 35370 graphs. Each graph represents one syllable, or, in most cases, one morpheme. The number of different graphs used is 1889, excluding variants. According to George A. Kennedy's study, the frequency with which particular graphs occur varies enormously, the highest being 1885, the lowest being, of course, one.² In table 1 of Kennedy's article the following statistics are of interest:

<i>Frequency</i>	<i>Number of Graphs</i>	<i>Total of Occurrence</i>	<i>Percentage of text</i>
500+	12	10,307	29
200+	27	14,499	41
100+	64	19,288	55
50+	125	23,415	66
20+	259	26,908	76
10+	417	29,680	84

In table 2 he lists 405 graphs which occur in the text of Mencius ten or more times, excluding the high-frequency graphs which represent proper names. For example, the graph 孟, Meng, representing the family name of Mencius and occurring 307 times, is omitted.

I choose from Kennedy's list the 125 most commonly used graphs which occur more than 47 times in the text of Mencius and try to classify them from the functional point of view.

There are two major types of sentences in Classical Chinese, the narrative sentence

¹ The author is especially indebted to Professors Y. R. Chao, F. K. Li, and T. H. Tung for valuable discussions. This paper is only a sketch. For further details and terminology not defined herein, see my *Historical Grammar of Ancient Chinese*, Part I: *Syntax* (Academia Sinica, 1961), pp. 17-54. In this paper, English translation for each graph or word is not complete. It is given merely for convenient indexing.

George A. Kennedy, "Word-Classes in Classical Chinese", *Wen-ti*, No. 9 (New Haven, 1956).

and the determinative sentence. Each of these may be divided into two parts, the subject and the predicate. Sometimes the subject may be zero. The kernel of the predicate of the narrative sentence is called the "narrative". For instance, 見 *chien* "visit" in 孟子見梁惠王 *Meng-tzu chien Liang-huei-wang*, "Mencius visited King Huei of Liang" is the narrative, followed by *Liang-huei-wang* as its object. The kernel of the second part of the determinative sentence is called the "determinator"; for example, 何言 *he yen* "What sort of talk" in 是何言也 *shih he yen yeh* "What sort of talk is this?" (James Ware, p. 66).

The subject, the object or the determinator is called the "substantive expression".

Most sentences can be analyzed into two or more syntactic units according to the principle of immediate constituents. A syntactic unit consisting of two or more words (i.e., syntactic words) is a phrase. A word (i.e., a syntactic word) can occur alone as a sentence, a substantive expression, a narrative, or in immediate constituent with a phrase of two or more words.³ For example, the graph 也 *yeh*, occurring 1227 times, can be bound to a whole phrase. Therefore, it is a syntactic word.

A graph may represent two or more different morphemes or words with or without different pronunciations.⁴ For example, while the graph 足 *tsu* (70 times) represents the homonymous words for "foot" (6 times), "be enough" (19 times), "be able to" (45 times), the words *yüeh* "music" (23 times), and *le* "happy" (58 times) are both represented by the same graph 樂.

A graph may represent a word (i.e., a syntactic word), a member of a compound or of a derived word (a morpheme) or, rather rarely, a part of a morpheme. With this understanding in mind, we may proceed to discuss the classes of words which the graphs represent.

Certain classes of words combine with certain classes of words to form certain kinds of syntactic constructions. Some of the classes are listable, others are not listable except in a dictionary. Members of the listable classes are called "listable words", while those of the unlistable classes are called "unlistable words".⁵

Besides, we may have another criterion of classification. A word which can be used in a sentence as a substantive expression or a narrative is a "full word", while a word which cannot be so used is an "empty word". Combining the two criteria, we may classify the words into three groups:

- Group I, unlistable and full;
- Group II, listable and full; and
- Group III, listable and empty.

³ Bernard Bloch, "Studies in Colloquial Japanese, II: Syntax", *Language*, 22:3 (1946), p. 205.

⁴ While pronunciations are given in modern Mandarin, the homophony or different pronunciation of different morphemes written with the same character has been roughly the same in the time of Mencius and the present.

⁵ Y. R. Chao, *A Grammar of Spoken Chinese*, 7.4 (unpublished); F. W. Householder, "Lists in Grammar, Methodology & Philosophy of Science", *Proc. of the 1960 International Congress*, E Nagel, et al. eds. (Stanford, 1962), pp. 567-576.

Now let us examine the members of Group I. A word which can be used as a narrative is a "predicative", while a word which cannot be or is rarely so used is a "noun".

For example, the graphs 人 *jen* "man" (occurring 603 times), 天 *t'ien* "heaven" (292 times), 民 *min* "people" (208 times), 心 *hsin* "mind" (124 times), 國 *kuo* "country" (123 times), 士 *shih* "gentleman" (94 times), 父 *fu* "father" (83 times), 侯 *hou* "marquis" (65 times), 日 *jih* "sun" (61 times), 地 *ti* "earth" (54 times), 水 *shui* "water" (47 times), and 母 *mu* "mother" (47 times) represent words and constituents of compound words, and are never used as narratives. The graph 子 *tzu* "son" (occurring 734 times) is in most cases a member of a compound word and occurs as a narrative only twice. The graphs 君 *chün* "lord" (251 times) and 身 *shen* "body" (57 times) are used as narratives only once each. They can be explained away by functional principles.⁶ The graph 道 *tao* (occurring 149 times) represents either the word for "say" (5 times) as a verb, or the word for "way" (144 times) as a noun.

Among the words which can be used as narratives we may further classify them into two sub-classes: the "verb" and the "adjective". Followed by a noun and forming with the following noun a phrase, an adjective usually is the modifier of the following noun which acts as the head of the phrase. On the other hand, a verb, followed by a noun, is the head of the phrase with the following noun as its object. In the class of the verb we have 有 *yu* "have" (461 times), 無 *wu* "have not" (265 times), 見 *chien* "see" (114 times), 問 *wen* "ask" (104 times), 使 *shih* "send" (103 times), 欲 *yü* "want" (96 times), 受 *shou* "receive" (72 times), 居 *chü* "live in" (68 times), 養 *yang* "cultivate" (68 times), 至 *chih* "arrive at" (66 times), 在 *tsai* "locate in" (65 times), 去 *ch'ü* "leave" (61 times), 求 *ch'iu* "seek" (53 times), 反 *fan* "go back" (56 times), 治 *chih* "rule over" (47 times), 取 *ch'ü* "take" (53 times), 悅 *yüeh* "be glad" (63 times), 殺 *sha* "kill" (53 times), 告 *kao* "tell" (50 times), etc. In the class of the adjective we have 大 *ta* "great" (165 times), 仁 *jen* "benevolent" (157 times), 善 *shan* "good" (114 times), 義 *yi* "righteous" (107 times), 賢 *hsien* "virtuous" (74 times), 小 *hsiao* "small" (73 times), 死 *szu* "dead" (52 times) and 聖 *sheng* "sage" (47 times), etc.

Some graphs represent two or more words in different classes with the differentiation of tones. For example, 王 *wang* (319 times) with the p'ing sheng and meaning "king" is a noun, but with the ch'ü sheng and meaning "to be the king (of)" it is a verb; 行 *hsing* (129 times) with the p'ing sheng and meaning "walk, practice" is a verb, but with the ch'ü sheng and meaning "behavior" it is a noun; 好 *hao* (57 times) with the shang sheng and meaning "good" (3 times) is an adjective, but with the ch'ü sheng and meaning "like" 54 (times), it is a verb; 惡 *e* with the ju sheng (> 4th tone

⁶ 子 *tzu* "son" in 父不得而子(也) *fu pu te erh tzu (yeh)* "The father cannot treat him as a mere son" (5A4), following the connective 而 *erh* and followed by pause or particle 也 *yeh*, should be used as a narrative mechanically. In 得百里之地而君之 *te pai li chih ti erh chün chih*, "Possessing 100 kilometers of territory, he had become the lord of it" (2A2, James Ware, p. 66) and 湯武身之也 *T'ang Wu shen chih yeh* "T'ang and King Wu embodied it" (7A30, James Ware, p. 154), 君 *chün* "lord" and 身 *shen* "body" are mechanically used as narratives before the substitute 之 *chih* "it".

in Mandarin) and meaning “bad” (15 times) is an adjective, but with the ch’ü sheng (pronounced *wu* in Mandarin) and meaning “dislike” (38 times) it is a verb; 知 *chih* (112 times) with the p’ing sheng and meaning “know” is a verb, but with the Ch’ü sheng and meaning “wise” it is an adjective.

Some graphs represent two homonymous words of two different classes. For example, the graph 事 *shih* (114 times) is a verb when its meaning is “serve” (61 times), but it is a noun when its meaning is “service” (53 times); and the graph 言 *yen* (112 times) is a verb when its meaning is “say” (over 50 times), but a noun when its meaning is “word”. Cases of class-cleavage in the text of Mencius are not so many as in modern Chinese.⁷

The classes of the words in Group II are as follows:

1. Substitutes, including pronouns, demonstratives, etc., for example 是 *shih* “this” (255 times), 何 *he* “what” (199 times), 我 *wo* “I” (158 times), 吾 *wu* “I” (127 times), 此 *tsu* “this” (114 times), 莫 *mo* “none” (58 times), etc.

2. Numerals, also a kind of substitutes, are 一 *yi* “one” (124 times), 百 *pai* “hundred” (87 times), 三 *san* “three” (68 times), 五 *wu* “five” (63 times), 十 *shih* “ten” (55 times).⁸

3. Classifiers usually occur after numerals to form modifier-head phrases which may in turn occur before or after a noun, modifying it and forming with it another modifier-head phrase of a higher rank. For example, 千里 *ch’ien li* “a thousand miles” (1A1), 一杯水 *yi pei shuei* “a cup of water” (6A18), 馬千駟 *ma ch’ien szu* “a thousand teams of horses” (5A7).

4. Localizers occupy almost all the usual positions of nouns, verbs, adjectives and adverbs (see group III). They are: 下 *hsia* “below” (237 times), 後 *hou* “behind” (110 times), 上 *shang* “above” (50 times), 先 *hsien* “before” (50 times), etc.

5. Auxiliary predicatives usually occur before a narrative to form a modifier-head phrase, but sometimes can be used as a narrative, for example, 可 *k’e* “may”, “can” (257 times), 能 *neng* “be able to” (135 times), 敢 *kan* “dare” (47 times).

The classes of the words in Group III are:

1. Adverbs are usually put before predicatives to modify them. They are: 不 *pu* “not” (1073 times), 亦 *yi* “also” (110 times), 皆 *chieh* “all” (97 times), 未 *wei* “not yet” (90 times), 87 times), etc.

2. Connectives connect two parts of a sentence or those of a syntactic unit. They are: 而 *erh* “and” (769 times), 則 *tse* “then” (422 times).

3. Prepositions ordinarily govern objects and form with them prepositional phrases. They are: 以 *yi* “with” (632 times), 於 *yü* “in”, “at” (505 times), etc.

⁷ There is still a small class of words called “descriptives”, in Group I. Descriptives, usually in reduplicated forms and/or with suffixes (e.g., 然 *jan*, 如 *ju*, etc.) can sometimes be used as narratives, though ordinarily as adverbs.

⁸ It is true, numerals cannot be exhaustively listed. They are, nevertheless, predictable and their ultimate components are listable. On this ground I put the numeral in Group II.

4. Interjections are used alone to form minor sentence – for example, 惡 *wu* (1st tone) “alas” (3 times).

5. Particles are always bound forms in immediate constituent with a word or a phrase. They are: 也 *yeh* (1227 times), 矣 *yi* (250 times), *tsai* 哉 (101 times), etc.

Academia Sinica

DISCUSSION

PENG:

If I understand Mr. Chou's title correctly, it implies dealing with classifications of words in order to define parts of speech in classical Chinese. But the paper presented here seems to merely give the audience the frequencies of occurrences of some graphs which have mixed 1) graphemes, 2) morphemes and 3) words together with no suggestions of determining the criteria of setting up parts of speech syntactically, of course in classical Chinese.

In view of this presentation, the title “Word-classes in classical Chinese” seems rather “misleading”. I believe that the set-up of syntactic parts of speech of classical Chinese can be done on the basis of distributional criteria and the like.

THE ORIGINS OF THE CHINESE TONAL SYSTEM

E. G. PULLEYBLANK

Abstract

Evidence, chiefly from Chinese transcriptions of foreign words during the Han period (206 B.C. – 200 A.D.), will be given to support the theory of A. Haudricourt that the third of the traditional four tones of Chinese, the *ch'ü-sheng* or “departing” tone, developed (like the corresponding tone in Vietnamese) out of a lost final -s. Words of those *Ch'ieh-yün* (6th century A.D.) rhyme classes which are confined to the “departing” tone – in Karlgren’s “Ancient” transcription; -âi, -ai, -jâi, -j i – and in the “departing” tone of other rhymes in -i diphthongs derived from earlier dental finals, are regularly used in transcriptions to represent syllables closed by a sibilant. In these cases -ts> -s>-i is to be reconstructed instead of Karlgren’s -d> -i. The reconstructed -s final was sometimes etymological but is often to be regarded as a derivational suffix analogous to the -s of Tibetan. (On the morphological function of the “departing” tone see G. Downer, BSOAS 1959.) There is much less evidence from early transcriptions as to the phonetic value of the later departing tone in other types of syllable but a few cases will be examined.

Haudricourt derives the rising tone in Vietnamese from a final glottal stop (which could exist after liquids and nasals as well as vowels). Though the evidence from transcriptions is hardly sufficient definitely to prove an analogous development in Chinese, what there is would seem to be consistent with such a theory.

Some further implications of the theory that the Chinese tones arose out of a previous non-tonal stage are briefly discussed.

Cambridge University

WORD ORDER RULES IN GERMAN

KEITH PERCIVAL

Abstract

Most German sentences are matched by other sentences in which the constituent words are arranged in a different order but in such a way that the meaning remains the same except for stylistic overtones. Thus the sentence *Ich habe heute eine deutsche Stunde* is matched by *Heute habe ich eine deutsche Stunde*, and by *Eine deutsche Stunde habe ich heute*.

There are two ways of accommodating this phenomenon in a generative grammar of German. One is to consider all the various permutations (in our case the three sentences quoted above) entirely different constructional types. In such an interpretation each type is generated by applying a different set of phrase structure rules. Each permutation has thus an entirely different derivational history.

A second method is to choose one permutation of each set as representing the whole set, generate this permutation in the normal way, and then subsequently generate the others by means of special transformational rules operating on the basic type. This method is at first glance preferable to the former – it can easily be shown that the first method is uneconomical. However, the second method has a disadvantage also. This is due to the fact that not all expected permutations of the constituents of a sentence are in fact grammatically permissible. Thus while the sentence *Zigaretten habe ich nicht* is grammatical, the permutation *Ich habe Zigaretten nicht* is not. Restrictions of this kind and their theoretical implications are discussed at some length.

M. I. T.

A PROBLEM IN NAVAHO SYNTAX

HARRY HOIJER

My studies of Navaho syntax are far from complete; the analysis presented below is therefore only a preliminary one, based in large part on textual data collected by Edward Sapir in 1929. Enough has been done, however, to disclose that Navaho has at least five major sentence types, as follows.

1. The verbal predication: a simple sentence which contains as its nucleus one verb.
2. The nominal predication: a simple sentence the nucleus of which is not a verb but a noun.
3. The interjectional sentence: a simple sentence made up of an interjectional phrase or simply of an interjection.
4. The complex sentence: a combination of two or more verbal or nominal predications where all but one of these predications contain a subordinating enclitic.
5. The compound sentence: a combination of two or more simple sentences (types 1, 2, or 3) united by a conjunction or by parataxis.

In this paper I shall consider only the simple verbal predication (type 1). This sentence type is not only the "favorite" sentence type (to use Bloomfield's terminology) but is as well the sentence type which is structurally most complex.

The simple verbal predication has seven positions only one of which (position VII, that of the verb) must always be filled.

Position I is filled by any one (or, rarely, two) of a set of uninflected particles. See, for example, the particle *ʔáádóó* "then" in the sentence *ʔádóó / dèèyá* "then / he started to go".

Position II is filled by a nominal (that is, by a noun, pronoun, or noun phrase) which functions as the subject of the sentence. See, for example, *màʔìì* "coyote" in *màʔìì / džòòldlòš* "coyote / he is trotting along".

Position III is filled by a nominal which is structurally identical with the nominal in position II, but which functions as the object of the sentence and not as its subject. Thus, in the sentence *dìné / tsìn / náìdììtá* "(the) man / (a) stick of wood / he has picked it up", the nominal *dìné* "(the) man" (in position II) is the subject and the nominal *tsìn* "(a) stick of wood" (in position III) is the object.

When a sentence contains only one nominal and the verb is transitive, the function of the nominal is sometimes unclear. Thus, the sentence *dìné / náìdììtá* (literally, "[the] man / he picked him up") may be translated either "the man picked him up" or

“he picked up the man”. An ambiguity of this sort, when it is not clarified by context, can be resolved by making the simple sentence into a compound sentence; e.g., *diné / náidiltí / diné / ʔádzàà* “(the) man / he picked him up / (the) man / he did so”.

Position IV is also filled by a nominal, which is structurally identical with the nominals of positions II and III, but which functions as the indirect object of the sentence – or, more precisely, as the referent of a postposition included in the verb. See, for example, the nominal *hòòγàn* “house” in the sentence *hòòγàn / bìʃšìʔdèèyá* “house / he has started to go toward it”, where *hòòγàn* “house” is the referent of the prefix *bì-* in *bìʃšìʔ-* “toward it”.

Some sentences of this kind may be ambiguous: thus, the sentence *diné / bìʃšìʔdèèyá* may be translated either “(the) man / he has started to go toward it” (where *diné* “man” is interpreted as a position II nominal), or “(the) man / he has started to go toward him” (where *diné* “man” is interpreted as a nominal of position IV).

Where an ambiguity of this sort is not resolved by the context in which the sentence occurs, it may be resolved, as in the preceding case, by making a simple sentence into a compound one; e.g., in the sentence *diné / bìʃšìʔdèèyá / diné / ʔádzàà* “(the) man / he has started to go toward him / (the) man / he did so”, *diné* is clearly defined as a position II nominal.

Position V is filled by a locative construction which is made up of a form (usually a nominal) plus a bound directional or locative enclitic. The function of the locative construction is therefore made clear by its structure; i.e., by the attached enclitic. So, for example, in the sentence *diné / tóò-gì / ʔıldèèʔ* “(the) men / at the spring (of water) / they arrived”, *diné* “(the) men” is clearly differentiated from *tóò-gì* “at the spring (of water)” by the presence in the latter construction of the postpositional enclitic *-gì* “at”.

Position VI is filled by an uninflected adverbial particle. An example: *hàžóʔó* “carefully” in the sentence *hàžóʔó / níńʔí* “carefully / you look at it” functions as an adverb.

Position VII is filled by an inflected verb. As noted earlier, the simple verbal predication must have a verb in position VII; the remaining positions (I to VI) may or may not be filled in a given sentence.

A model of the Navaho verb is given below; the numbers refer to the position of the morpheme relative to the stem. The morphemes of positions 1-12 are prefixes; the position 13 morpheme is always a bound stem.

1. Pronoun prefix (indirect object)
2. Postposition
3. Adverbial prefix(es)
4. Iterative prefix
5. Distributive plural prefix
6. Pronoun prefix (direct object)
7. Pronoun prefix (subject; see pos. 11)
8. Adverbial prefix(es)

9. Future tense prefix
10. Mode-aspect prefix
11. Pronoun prefix (subject; see pos. 7)
12. Classifier
13. Stem

Position 13, that of the stem, must be filled in every verb form. No verb has a prefix in all twelve of the prefix positions; some positions (e.g. 4 and 10, or 7 and 11) are mutually exclusive, and others are restricted to certain kinds of verb (e.g. position 6 is filled only in transitive verbs). A minimum verb form must contain one prefix that is not a zero morph – this prefix may belong to any one of the following positions: 3, 4, 5, 6, 7, 8, 10, or it may be one prefix of the set that fills position 11. The remaining prefixes of position 11 and all of those that fill position 12 never begin a verb form.

A maximum verb form may have one prefix each in positions 1, 2, 4 or 10, 5, 6, 7 or 11, 9 (if 4 is not filled), and 12, and one or as many as three prefixes each in positions 3 and 8. Needless to say, verb forms of such complexity are rare; it is probable that the more complex verbs rarely contain more than eight prefixes and that the average number of prefixes per verb is five.

The verb prefixes of most interest to the study of Navaho syntax are those which fill positions 1, 6, and 7 or 11. Prefixes of position 1 cross-reference nominals of sentence position IV, prefixes of position 6 cross-reference nominals of sentence position III, and prefixes of positions 7 or 11 cross-reference nominals of sentence position II. It will be noticed that the order of the pronoun prefixes of the verb is the reverse of the order of their nominal referents in the sentence.

The relationship between the verb and the three nominals which the sentence may contain raises difficulties with a sentence analysis based upon immediate constituents. It is of course clear that the particle in sentence position I is related as immediate constituent to the rest of the sentence. But that remainder, if it includes the three nominals which are cross-referenced by pronoun prefixes in the verb must, it seems to me, be divided into at least four immediate constituents: the position II nominal, the position III nominal, the position IV nominal, and the verb plus its modifiers in positions V and VI.

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THE INFLECTION OF ASSINIBOINE NOMINAL THEME

NORMAN BALFOUR LEVIN

Abstract

The Assiniboine are an Indian tribe of the Great Plains region, who originally occupied the territory of North Dakota, Montana, Manitoba, and Saskatchewan. Members of the tribe are now found not only in their original habitat, but in Alberta Province as well, where they are known as the Stoney. The aboriginal population of the Assiniboine is estimated at 3,000, of which 1,500 are on the Fort Peck reserve, Montana.

An analysis of the data reveals seven different types of morphophonemic changes, which treat the more productive alternations. These relate to nasalization, fusion of sounds, and loss of sounds.

Inflection of the nominal themes are defined in terms of morphologic and syntactic criteria. The nominal themes consist of four noun classes: the animate noun, the inanimate noun, the adjective noun, and the adverbial noun. Plurality, occurrence of compounds, diminutives, reduplication, locatives and possession occur in nominal themes.

The analysis of the four noun classes is based on the distribution of the plural suffix, *-pi*, and its position in compounds.

The adjective noun class falls into two sub-classes, descriptive and designative. The descriptive adjective noun is endocentric in construction. Pronominal adjectives are of two classes, the genitive which occurs as dependent and independent forms, and the demonstrative.

Derivative suffixes forming adverbial nouns are: adverbial negative, adverbial modal, adverbial locative, and interrogative adverbial.

A residue of non-analyzable or partially solved morphemes resulted from the material. Examples of the Assiniboine lexeme are given which will require future investigation.

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PRACTICAL LINGUISTICS: THE THEORY OF LANGUAGE PLANNING

VALTER TAULI

Since language is an *instrument*, it follows that a language can be evaluated, altered, corrected, regulated, improved, and that new languages can be created at will. In that we must take into consideration the other essential character of the language, that language is a *social* institution, i.e. we must take into account those we wish to communicate with. The prerequisite for normal, easy and efficient communication in a society is the possession of a common language. In the case of a nation it means that the persons from different parts of the country cannot use their own local dialect but must use an interdialectal, nation-wide common language, called *standard language* (SL).

The linguistic norm is inherent in the nature of language. This is the foundation on which an efficient and economic function of linguistic communication is based. This is felt instinctively by the speakers of a SL. It is on this normal instinctive feeling that the demand for linguistic correctness is based. And that is why we must deal with the problems of linguistic correctness and planning. It is not the task of language planning (LP) to prescribe norms or to decide which expressions are correct, but to try to point out and prove which expressions are preferable. But man is not satisfied with a language which is merely correct. As man is anxious to use the best tool and as he is perpetually improving his other tools and social institutions, so he wishes to use the best language and he is anxious to improve his language. It is the most natural and rational attitude towards language. And on this the higher and the most difficult task of LP is based: *the methodical improvement of language*. An urgent task of competent LP in the present situation is to eliminate the harm done to languages by incompetent grammarians in the past.

First of all the abolition of prejudices is needed. Among other things it also means the elimination of unnecessary differences between the colloquial language (CL) and literary language (LL). In various countries many newer more efficient CL forms have been repudiated, whereas archaic extinct and inefficient or unnecessary forms have been preserved in LL owing to the influence of grammarians.

SL means deliberate choice and planning. If this is to be done in the most competent and efficient way it must be done by linguists, not by amateurs or antiquated grammarians. That the problems of LP should be dealt with by linguists is as obvious as the handling of educational problems by pedagogic scientists or agricultural problems by agricultural scientists. This has been realized by many modern linguists.

Several scholars have also stressed the need of language improvement. I will quote only one linguist. A. Sauvageot in an article in *Vie et langage* (1960) asks why one should not perfect the language as one perfects transmitting or calculating machines. When linguists refuse to deal with concrete problems due to the needs of daily life, they fail in their duty. Sauvageot sees the LP problem in a new perspective. He foresees the time when the electronic machine will be superior to language 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. When there is too great a gap between the machine and language, another language, which serves the machine more efficiently will be chosen. This is the task future linguists must tackle. Sauvageot is sure that the role of the science of language in the history of civilization will thus acquire an importance that nobody has been able to imagine. To my mind there is no doubt that electronic machines in communication constitute a problem that language planners must consider.

LP presupposes language evaluation. The negative attitude towards language evaluation has been rather common among modern linguists. Some American linguists acknowledge that language can be evaluated from the viewpoint of its social prestige and from the aesthetical point of view. But they cannot imagine that linguistic features can also be evaluated from the structural point of view, i.e. from the viewpoints of efficiency; e.g. clarity, redundancy, economy and elasticity. In modern linguistic literature we meet incredible statements, such as "... one important outcome of proper linguistic training is a realization that one accent is as good as another; that one word is as good as another" (J. F. Gummere, *The Classical Journal*, 46, 1950-51, 268). Consequently a monosyllabic word is as good as a ten-syllable word from all points of view! Fortunately we meet also other views among linguists. It is essential to stress that evaluation of linguistic features is possible and is objectively verifiable, in many cases quantitatively measurable.

Several linguists have, in the past, voiced the opinion that one cannot deliberately direct or change a language. In recent times such a view is becoming rarer. Such a view overlooks some essential points in language as a social phenomenon, such as individual initiative, imitative instinct, authority, prestige, propaganda, and last but not least – power. This view also ignores the facts. The most obvious proof of the possibility of deliberate and arbitrary LP is the experiences of language reforms in such languages as Hungarian, Norwegian, Turkish and especially Estonian. The great Estonian language reformer J. Aavik has introduced into LL and colloquial SL not only arbitrarily constructed new root-words but also grammatical morphemes. The importance of these facts is also acknowledged by several modern linguists who maintain that individual intervention, deliberate planning and improvement of language are possible.

Unfortunately a prescientific mystical view has still been preserved in some circles. Behind this is the anachronistic view on language as an organism. It is high time to realize that man is free to alter his language. Theoretically there are no limits to language alteration. We have no practical experience proving that certain kinds of deliberate

alternations are impossible. Realization of planned changes in a language is only a question of tactics. LP is necessary as well as possible.

The need for LP is not the same in all languages. The opponents of LP are to be found particularly among speakers of English, a language with a long literary tradition, a simple morphology and relatively small dialectal differences. The situation is entirely different in languages with a young literary tradition, which did not become vehicles of a complex culture until the 19th or 20th century, and which have a complicated morphology, great dialectal differences and a great many competing variant forms in SL, and with the need of mass coining of new words in a few years. Still other problems face the countries where no national or regional common language has existed hitherto. An important task of LP is to establish linguistically sound principles for creating new common and literary languages where none yet exist. The creation of new national and regional common languages has become an acute problem all over the world. It concerns a great number of languages and peoples.

The enormous role which language plays in our life and culture has been rightly grasped by all, but unfortunately not the importance of the efficiency of a language. It should be obvious that the efficiency of such an important instrument is of the greatest concern for every nation. It is curious that while other instruments of culture and communication have been steadily improved by scientific methods, nothing like that has been done with language, the most important of tools. It should be clear that LP is so important a matter that it must be founded on proper scientific theory and methods, and cannot be left to amateurs. A proper solution of the intricate problems of LP is possible only if a new branch of science is established. We may call it *Practical Linguistics* (PL). The idea is not new. Among others, A. Peškovskij from Moscow suggested the establishment of PL in a reply to a questionnaire of the Second International Congress of Linguists in 1931. I had not read Peškovskij's suggestion when, in 1938, I outlined the foundation of PL in my book (published in Tartu) *Õigekeelsuse ja keelekorralduse põhimõtted ja meetodid* (with a summary: "Principes et méthodes de correction de langage et de règlement de la langue"). The translator of the summary used the term *linguistique appliquée*. More or less similar suggestions have been made by others, e.g. by B. Migliorini (1942) and H. Spang-Hansen (1946) who use the term applied linguistics (AL). The term AL is now mostly used not as a name for a special branch of science but for the application of the results of theoretical linguistics for various practical problems connected with language, often also for the application of mathematics and mechanical means in linguistics. In the USA the terms *practical* and *applied linguistics* were used as early as 1925 by H. Collitz.

PL may be defined as follows:

PL is the science of language planning. Or more explicitly: PL is a science which investigates methodically the ends, principles, methods and tactics of language planning.

Language planning I define as follows:

LP is the activity of regulating and improving existing languages or creating new com-

mon regional, national or international languages. Or in short: LP is an activity, whose aim is the improvement and creation of languages.

PL is an applied science in the meaning that it has a practical purpose, i.e. its results are applicable to practical ends. But only the results of PL are directly applicable in practice, not the results of theoretical linguistics (TL). It would be entirely wrong to give to PL the meaning of mere application of traditional TL.

PL is a *normative science* as opposed to a descriptive or factual science. It deals with values. Its task is to designate what ought to be, in conformity with an ideal. The propositions of a normative theory imply the following form: "an A which is B has the quality of C". PL has to solve the problem how to improve the factual state. The first thing is to establish the ideal norms.

A property of normative science is the non-uniqueness of its theory. As the postulates of a normative theory partly depend on subjective attitudes there may be several rival normative theories. Consequently more than one theory of LP is possible. One of them is discussed in my book *Outlines of Practical Linguistics: Introduction to a Theory of Language Planning*, as yet unpublished. It should be stressed that PL as a science is the theory, not practice. The practical application of the results of PL, i.e. the practice of LP does not belong to PL as a science. The problems of PL are teleological, methodological and tactical, corresponding to the ends (principles), means (methods) and tactics (strategy) of LP. It is necessary to stress the difference in principle between the teleology of PL, based on the ideal of language as an efficient instrument, and the tactics of LP, which must take into account the existing language and the social and other conditions of the language community. In practice, and often also in theory, it is difficult to separate these two points of view. A special branch of PL is *interlinguistics* which deals with the problems of interlanguage. Until the general theory of LP has been built up it is too early to tackle the special problems of interlanguage. It is no use launching new improvisations, or advocating old, deficient projects.

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DISCUSSION

ORNSTEIN:

It is no exaggeration to say that sociolinguistics has been one of the most neglected areas of our field. While the social scientists have not hesitated to intervene in virtually every aspect of human affairs, linguists, by contrast, have suffered from inhibitions as regards the study and analysis of social phenomena into which, as specialists in communication, they might have insight. As a result of this timidity, linguists have often remained outside the mainstream of events, concerning themselves exclusively with technical matters. In such spheres as language planning, only a few names, such as Jespersen and Sapir, occur. Bloomfield's desire to make of linguistics an exact science working only with observable phenomena has been in part responsible for this. While there is no quarrel with those who wish to occupy themselves solely with

phonetics and phonemics, morphophonemics, and other purely linguistic problems, there is a need, in the perplexing, fast-moving world of today, for some of our colleagues to undertake studies of current problems in which languages play a role. These may include language policy in such lands as India, Indonesia, or the Soviet Union; the matter of written and spoken "interlanguages" and their possible role in satellite communication; language and political unity in the emergent nations (such as those of Africa south of the Sahara); ethnic problems and language (Basque and Catalanian separatism in Spain, the Kurds in the Middle East, etc.); and finally, the applications of language in such fields as psychiatry, special education, and propaganda analysis (information theory, etc.).

HAUGEN:

The term "practical linguistics" seems to me synonymous with "applied linguistics", as used in this country, and its application to language planning alone is hardly warranted. I would also feel that the author underestimates the problems involved when he says that "realization is only a question of strategy"; the examples of Norwegian and Turkish show abundantly that it is easier to propose new language systems than to get them accepted.

HODGE:

Prof. Tauli spoke of the abolition of differences between spoken and written language. In my opinion it is more efficient to have such differences, since they convey a message. Lacking these differences, we would have to use other, more cumbersome, methods to convey the same message(s).

“AT HOME AND OUTSIDE”:
A SOCIO-LINGUISTIC STUDY (HOME LANGUAGE AND CULTURE LANGUAGE)

SUNITI KUMAR CHATTERJI

(Abstract)

Unilingual and multilingual countries: the impact of one or more cultural languages in either.

The situation in India – the language of the home in modern India everywhere subordinated by the language of the outside (the street and the market-place, the school and the administrative machinery, and the general forces of “progress”, industrial, commercial, scientific, and otherwise).

Primitive communities in India and their tribal speeches, and the action of outside languages.

The conflict of speeches in India leading to “linguism”, a new force against complete integration like religious exclusiveness and communalism as well as “groupisms” of various sorts.

“Outside” influences as forces of integration in India.

Sanskrit in the Indian scene.

Persian during the last 700 years.

English during the last 200 years and at the present day.

A new type of linguistic patriotism in the wake of the division of India into linguistic states.

Hindi *versus* Persian and Arabic and Urdu – Tamil *versus* Sanskrit and Hindi.

The conflict of two ideologies in India: integration *versus* segregation.

The home languages *vis-à-vis* English, Sanskrit (or an equivalent classical language), and Hindi.

The search of a common official language for the whole of India.

Interested or disinterested support, complacency and apathy, as well as opposition with regard to Hindi on the one hand and English on the other.

Proper adjustment between the home language and the outside language(s) a problem in India as a polyglot country.

Inroad of the outside language(s) into the home language, and in some cases, the internal disintegration of the latter.

Suggestions for mutual adjustment among the various home languages themselves and one outside language as a neutral speech, bringing about ideological unification with all the requirements of modern progressive life in science and technology, thought and the humanities.

Linguistic scholars fully informed of the situation should help to find the solution, and not politicians and “patriots” of various sorts.

DISCUSSION

QURESHI:

I should like to indicate two recent articles which complement Dr. Chatterji's illuminating talk: Paul Friedrich, “Language and Politics in India”, *Daedalus*, Summer 1962, and Munier Chowdhury, “The Language Problem in East Pakistan”, *IJAL*, July 1960. The latter deals particularly with Bengali, the richest modern language shared by the vast population of the Indo-Pakistan sub-continent. As Dr. Chatterji observes, Bengali and other languages in India are intentionally being loaded

with "borrowings" from Sanskrit and even Hindi. The same situation prevails in Pakistan as well, there has been a tendency to corrupt Bengali with "borrowings" from Urdu and Semitic languages. The Bengali Academy and the University of Dacca have been undertaking important research projects for the scientific study and standardization of Bengali. As a language of culture, English should be retained, but other languages, such as French, Russian and German, should be taught by the latest methods.



CONTRIBUTION À L'ÉTUDE SOCIOLOGIQUE DES BILINGUISMES

ANDRÉE TABOURET-KELLER

INTRODUCTION

La rapidité d'involution des différentes formes des parlers appartenant à la famille des langues d'Oc, dans le Sud de la France, a pour conséquence l'existence dans ces régions de situations de bilinguisme diverses qui dans l'ensemble se caractérisent par la prédominance toujours accrue de la langue française sur les formes patoisantes des différents dialectes gascons, languedociens et provençaux. Les villes sont à peu de choses près francisées, les campagnes présentent des situations de bilinguisme variables quant aux rapports d'usage de l'une et de l'autre langue.

Avec une assez remarquable stabilité de la situation de son dialecte, l'Alsace, au Nord-Est de la France, présente au contraire des situations de bilinguisme extrême-

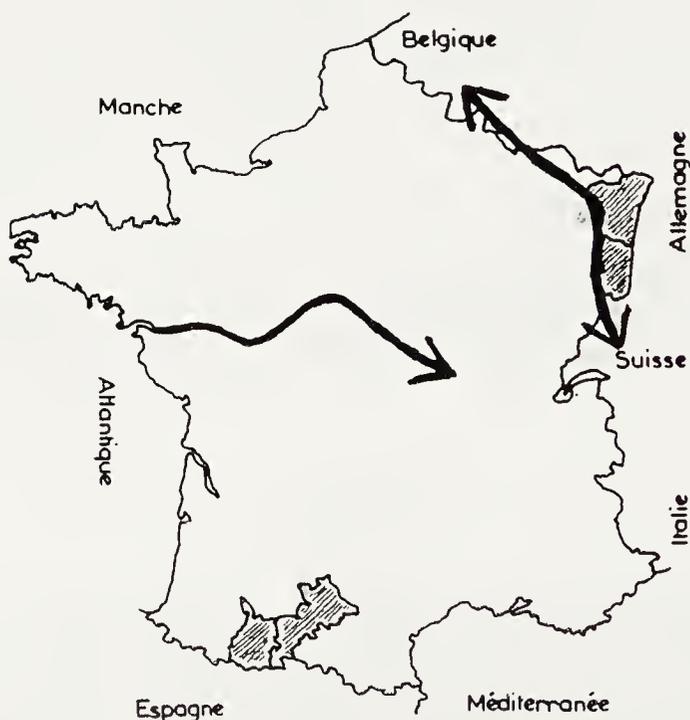


Figure 1. Les frontières linguistiques (en traits pleins) situent les limites des parlers occitans dans le Sud et les limites des parlers germanophones dans le Nord-Est de la France. Les enquêtes dont ce texte rend compte ont eu lieu dans les départements hachurés.

ment homogènes dans les campagnes où le dialecte alsacien, dialecte haut-allemand, est largement prédominant pour l'usage parlé et des situations de bilinguisme très variées dans les villes où français et dialecte sont dans des rapports d'usage très variables.

Ces deux situations de bilinguisme, Pays d'Oc et Alsace, peuvent se caractériser par la façon dont s'établissent les relations entre la langue dialectale et la langue française pendant la période d'acquisition du langage. Le graphique de la figure 2 montre qu'en Pays d'Oc le français, dont la connaissance est estimée ici par une épreuve de com-

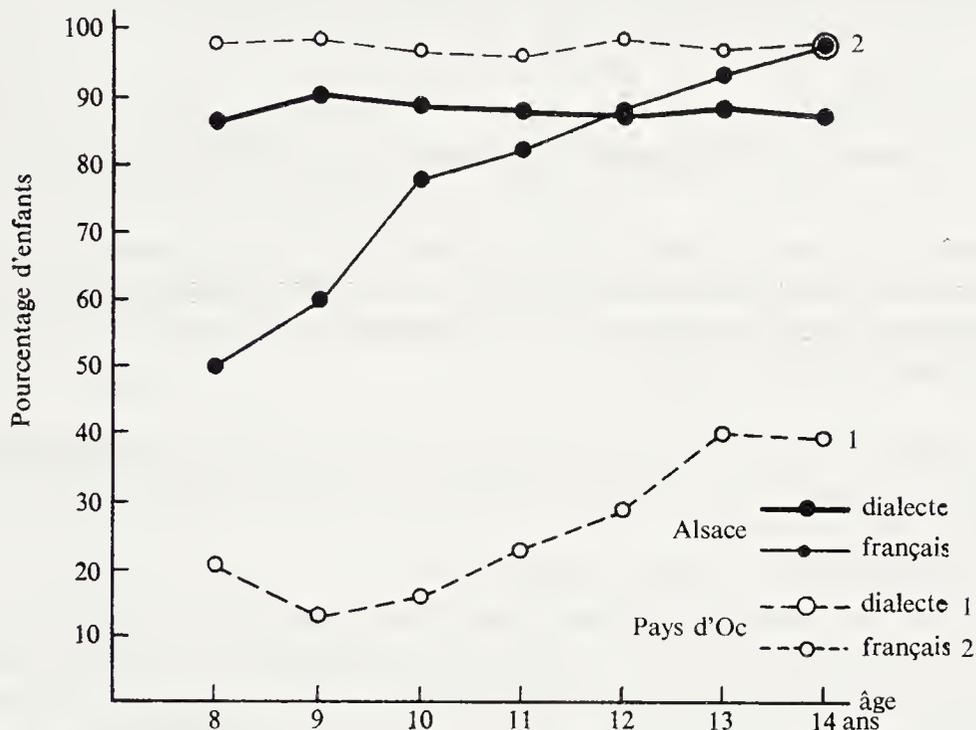


Figure 2. Variations en fonction de l'âge de la connaissance du français et de l'usage du dialecte en Alsace et au Pays d'Oc.

préhension et d'usage du vocabulaire, est langue courante alors qu'en Alsace la réussite à cette épreuve est fonction de l'âge c'est à dire de la scolarité, pendant laquelle l'enfant acquiert progressivement le français, deuxième langue. Le dialecte, au contraire, est langue d'usage courant en Alsace dès le plus jeune âge alors qu'en Pays d'Oc il est acquis progressivement et par 40 % des enfants seulement.

Ces faits sont révélateurs de la manière dont des rapports de langue évoluent. En Alsace, le dialecte a des positions fortes, il est la langue maternelle non seulement de la grande majorité des adultes vivants mais aussi celle des enfants de la génération présente qui acquièrent encore le français comme deuxième langue à l'école. En Pays d'Oc, au contraire, les parlers occitans qui ont encore été langue maternelle de la majorité des adultes âgés de plus de cinquante ans aujourd'hui, ne le sont plus pour

les enfants de nos jours pour qui le français se généralise comme langue maternelle, le dialecte étant appris comme deuxième langue par une minorité seulement.

Les faits présentés sont d'ordre statistique (populations de l'ordre de 1.000 enfants pour chacune des deux régions) et ne tiennent pas compte des populations des grandes villes où les rapports d'usage évoluent différemment. Ils recouvrent en Alsace une situation homogène avec une évolution vers la francisation actuellement encore latente et non-sensible dans la distribution du dialecte comme langue maternelle. Ils recouvrent en Pays d'Oc une situation en pleine évolution, où depuis cinquante ans les changements vers la francisation ont été rapides et importants. Ce sont les facteurs sociologiques de ces changements qui font l'objet de cette étude.

MODALITÉS D'ENQUÊTE ET ÉTENDUE DE LA DOCUMENTATION

L'enquête a été menée dans les écoles primaires des départements de la Haute-Garonne et des Hautes-Pyrénées. Elle portait sur les usages linguistiques des enfants d'âge scolaire de 29 localités rurales où les instituteurs ont remplis un questionnaire concernant les points suivants:

la localité: nombre d'habitants, répartition des habitants dans l'agglomération et en dehors de l'agglomération, nombre d'habitants étrangers, altitude, patois usuel: gascon, provençal, etc.

l'école et les classes: degrés, nombre d'élèves.

les élèves: âge, sexe, usages linguistiques. Les 11 questions suivantes sont posées au sujet de ces usages: parle et comprend parfaitement bien le patois; emploie le patois plus particulièrement avec sa mère; emploie le patois plus particulièrement avec son père; emploie le patois plus particulièrement avec ses grands-parents; parle un peu et comprend encore bien le patois; ne parle pas, mais comprend encore assez bien le patois; ne parle pas et comprend encore un peu le patois; ne parle ni ne comprend le patois; le patois s'emploie entre enfants de la même famille; emploie le patois dans la cour de l'école; emploie le patois avec les commerçants. Un système de colonnes permet de répondre par des croix pour chaque enfant à toutes les questions. Indication des enfants qui parlent italien, espagnol ou une autre langue maternelle dans leur milieu familial.

les parents: profession et lieu de l'emploi.

l'instituteur: durée de sa présence dans la localité et dans la région.

Les renseignements exploités dans cette étude concernent un millier d'enfants (1025) et une population globale de 24.081 pour le Pays d'Oc et un peu plus pour l'Alsace. Les enquêtes ont été menées entre juin 1961 et juin 1962 pour le Pays d'Oc et entre 1957 et 1962 pour l'Alsace.

DONNÉES DE L'ENQUÊTE ET ESSAI D'ANALYSE

1. *Données concernant les usages linguistiques
et l'âge des enfants*

La proportion des enfants qui parlent et comprennent parfaitement le patois augmente avec l'âge et passe d'un chiffre situé autour de 15 % à 8-9 ans à 40 % à partir de 13 ans; les enfants parlant et comprenant bien le patois à 8 ans peuvent être considérés comme l'ayant eu pour langue maternelle. On constate qu'à partir de 11 ans la proportion croît sensiblement, vraisemblablement à cause des rapports accrus avec les adultes à partir de cet âge. La proportion des enfants comprenant le patois mais le parlant peu ou pas du tout est toujours supérieure à celle des enfants le parlant couramment: ceci indique que plus de la moitié des enfants ont suffisamment fréquemment l'occasion d'entendre le patois pour être susceptibles de le comprendre. La situation alsacienne est indiquée à titre de référence.

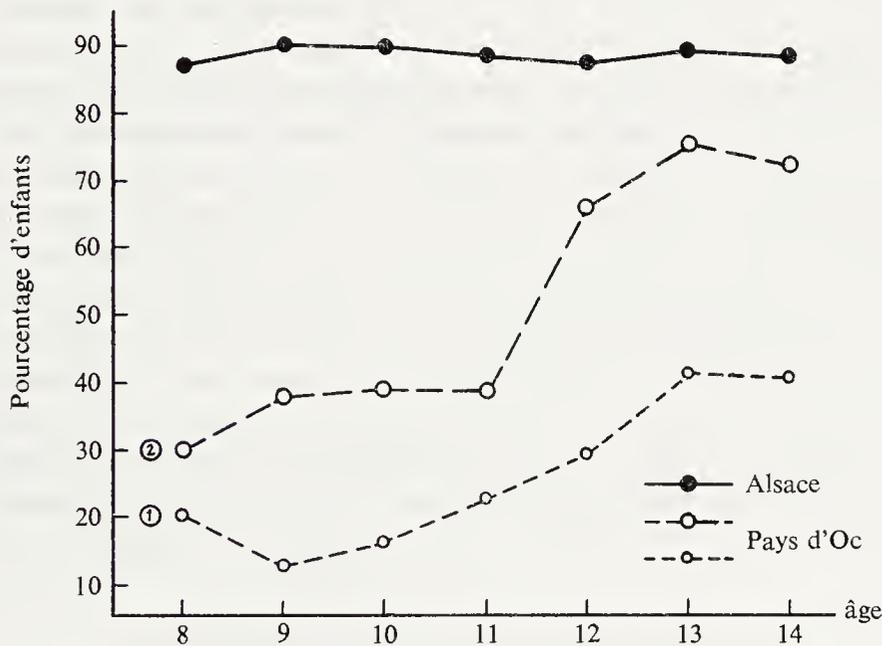


Figure 3. Variations en fonction de l'âge du pourcentage d'enfants: en Pays d'Oc (traits pointillés) parlant et comprenant bien le patois (courbe 1) et comprenant bien le patois mais le parlant peu ou pas (courbe 2); en Alsace (traits pleins) parlant et comprenant bien le dialecte.

2. *Données concernant les usages linguistiques
et le sexe de l'enfant*

La proportion des filles parlant couramment le patois est légèrement supérieure à celle des garçons: 28 % des filles contre 23 % des garçons. La différence entre les deux populations n'est pas statistiquement significative.

3. *Données concernant les usages linguistiques et les relations avec les parents*

Les enfants parlent un peu plus souvent le patois avec leur père qu'avec leur mère: 58,5% disent parler plus particulièrement le patois avec leur père et 45,7% seulement avec leur mère. Cette différence est statistiquement significative (.82) au seuil de $p = .0,05$ et indique une tendance qu'il n'est pas possible d'interpréter pour le moment. Quand on étudie le détail des distributions, on s'aperçoit que la proportion des

	Garçons	Filles	Total
Mère	40,8%	57,1%	45,7%
Père	55,1%	66,6%	58,5%
G.p.	—	—	51,4%

enfants déclarant parler patois plus particulièrement avec l'un ou l'autre des parents croît légèrement avec l'âge, mais de façon statistiquement non-significative. A partir de l'âge de 12 ans la signification (.91) de la différence d'usages patoisant en faveur du père s'accroît ($p = .0,01$) et traduit certainement une réalité liée au type de relations familiales: les relations toujours à dominance patoisante avec le père deviennent alors plus fréquentes. La différence très fortement significative entre garçons et filles pour les relations en patois avec la mère traduit elle aussi une réalité de type social: il paraît vraisemblable que les filles aient des relations plus fréquentes avec leur mère que les garçons. La proportion d'enfants déclarant parler plus particulièrement le patois avec leurs grands-parents n'apparaît pas avoir de signification particulière. La question "parle le patois plus particulièrement avec ses grands-parents" incitait à ne répondre que pour les grands-parents en vie; il est probable qu'une question formulée différemment et tenant compte de l'ensemble de la génération des grands-parents aurait montré comment l'usage du patois a involué au cours des trois générations successives, grands-parents, parents et enfants, touchées par cette enquête.

4. *Données concernant les usages linguistiques et certains critères socio-économiques*

L'étude de la situation linguistique en Alsace a montré que le milieu rural assurait le maintien du dialecte alsacien comme langue d'usage courant de la façon la plus stable et la plus homogène. On a émis l'hypothèse d'une liaison positive entre le maintien des usages linguistiques dialectaux et le statu-quo de la situation socio-économique et plus particulièrement socio-professionnelle à la campagne. L'importance de certains critères, définis comme caractérisant ce statu-quo, a été étudiée.

Le graphique représenté sur la figure 4 illustre la façon dont la présence d'une population d'enfants utilisant couramment le patois est liée à la densité en professions

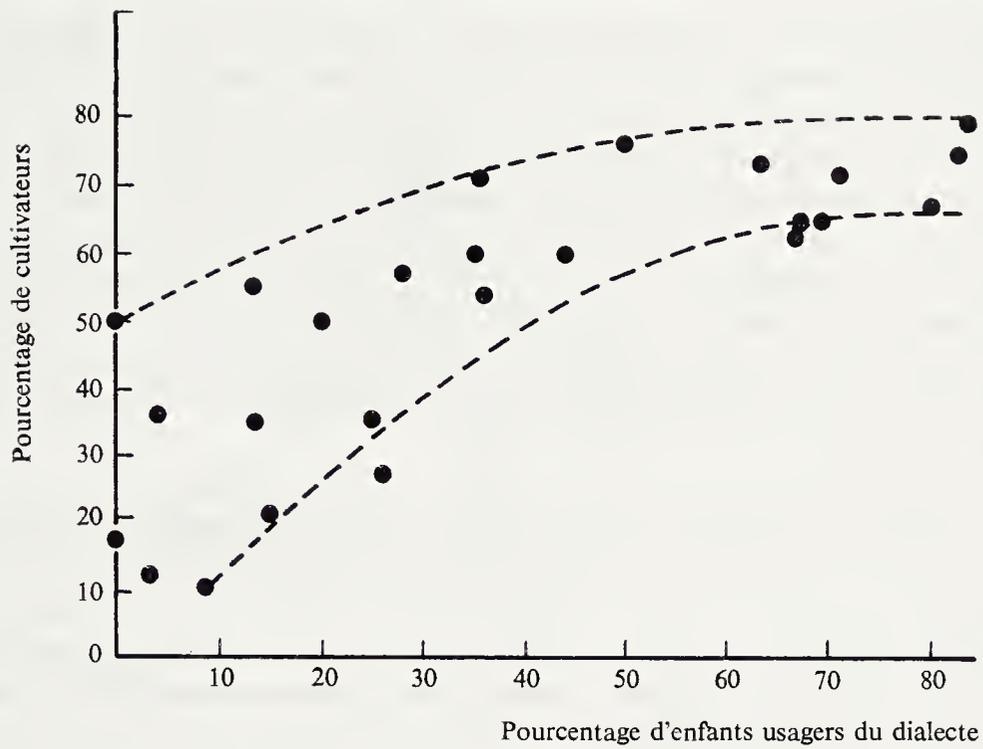


Figure 4. Variations du pourcentage de cultivateurs en fonction du pourcentage d'enfants usagers du dialecte. Chaque point représente une agglomération.

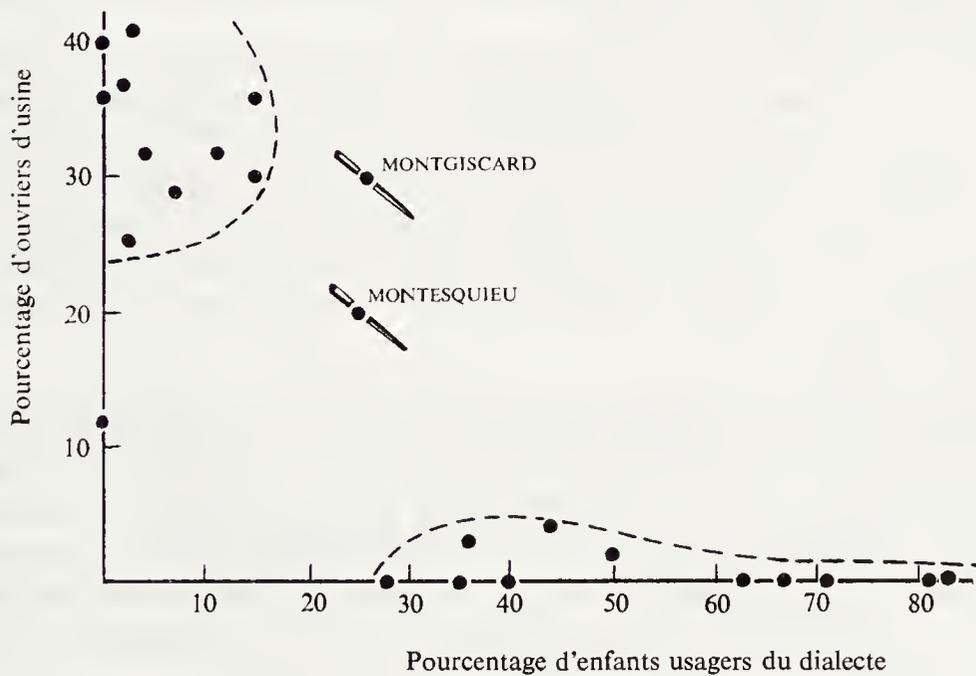


Figure 5. Variations du pourcentage d'ouvriers d'usine en fonction du pourcentage d'enfants usagers du dialecte. Chaque point représente une agglomération.

purement agricoles dans chaque agglomération. La présence dans une agglomération de 60 % de cultivateurs ou plus, va de pair avec un pourcentage significatif d'enfants usagers du patois, égal ou supérieur à 35 %, la liaison entre les deux facteurs devenant de plus en plus étroite. En effet les pourcentages de 50 % et plus d'enfants parlant couramment le patois correspondent à des pourcentages de 60 % à 80 % de professions agricoles mais aussi à des agglomérations de moins de 600 habitants. Pour des proportions de cultivateurs inférieures à 60 %, la dispersion du pourcentage en professions agricoles devient grande (0 à 60 %) par rapport à la variation du pourcentage d'enfants usagers du dialecte (0 à 35 %). Une faible densité de professions agricoles (moins de 25 %) va toujours de pair avec de faibles pourcentages d'enfants usagers du dialecte.

Le graphique représenté sur la figure 5 montre la liaison entre la présence d'ouvriers d'usine¹ et celle de populations d'enfants usagers du dialecte. Cette liaison est négative : la présence d'ouvriers d'usine dans une proportion supérieure à 25 % exclut l'usage du patois qui reste dans tous les cas inférieur à 15 %. Dans tous les cas où plus de 25 % des enfants utilisent couramment le dialecte, il n'y a pas d'ouvriers d'usine dans la population (moins de 5 %). A quoi correspond le petit pourcentage (0 à 15 %) d'usagers du dialecte dans les agglomérations à fortes proportions d'emplois industriels ? Vraisemblablement au fait que de petites localités, à proximité de centres industriels, sont devenues des cités dortoirs. On a pu vérifier que dans le cas de ces agglomérations, la très faible proportion d'usagers du dialecte correspond aux habitants des fermes toujours plus ou moins isolées en dehors de l'agglomération proprement dite. Les usages linguistiques des cultivateurs n'ont pas varié mais l'importance numérique relative de cette catégorie a diminué.

Les cas de Montgiscard et de Montesquieu-Volvestre méritent un examen particulier : dans ces deux agglomérations on se trouve en réalité en présence de deux populations, l'une agricole, l'autre industrielle ayant chacune ses caractéristiques : la population industrielle ne travaille pas sur place mais se déplace chaque jour vers son lieu d'emploi, la population agricole n'habite pas l'agglomération proprement dite mais ce qu'on appelle les écarts ; la population agricole est ancienne, la population industrielle est récente. Les deux agglomérations sont situées sur des voies de communication importantes.

La figure 6 montre sur l'exemple de la région de Toulouse l'importance des voies de communication pour l'évolution linguistique d'une région. Dans un rayon de 20 km autour de Toulouse les localités ont toutes plus de 30 % de la population travaillant en industrie ; dans ce même périmètre, plus de 40 % de la population travailleuse quitte chaque jour son lieu de domicile pour gagner le lieu de l'emploi. L'importance proportionnelle de la population agricole a extrêmement diminué, celle des usagers du patois a diminué de façon plus rapide encore puisque dans les agglomérations à

¹ On a distingué les ouvriers d'usine des ouvriers agricoles et des ouvriers des petits commerces artisanaux (garage, ferblanterie, forges, etc.).

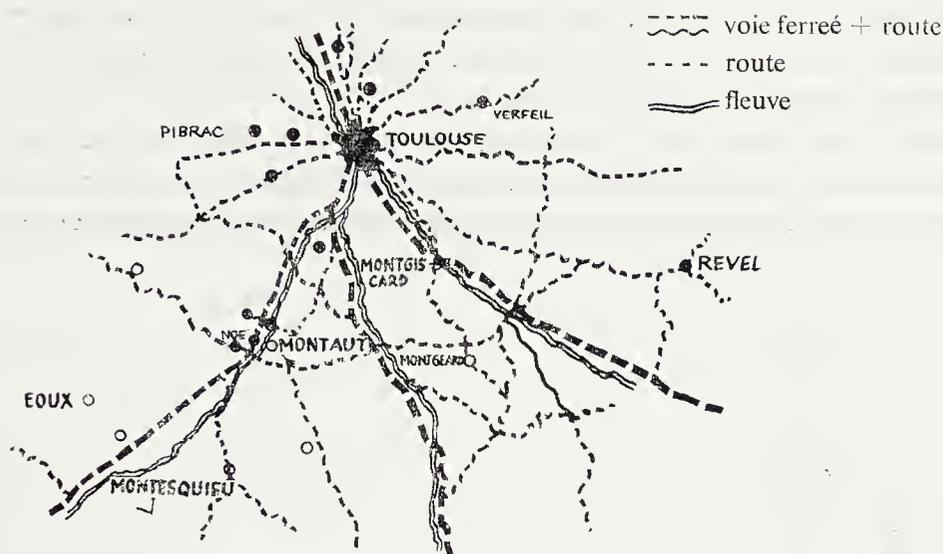


Figure 6. Les agglomérations à prédominance agricole sont figurées par des cercles blancs, celles à prédominance industrielle par des cercles noirs. Région de Toulouse.

l'intérieur de ce périmètre, elle est toujours inférieure à 15% alors que le pourcentage de la population agricole varie entre 8 et 36%. Le nombre d'habitants de ces localités varie de 374 (Pins-Justaret) à 1539 (Verfeil): il n'y a pas dans ce secteur de relation entre la grandeur de l'agglomération et l'usage du patois.

Le cas de Montaut est à mentionner séparément: ce village est également à

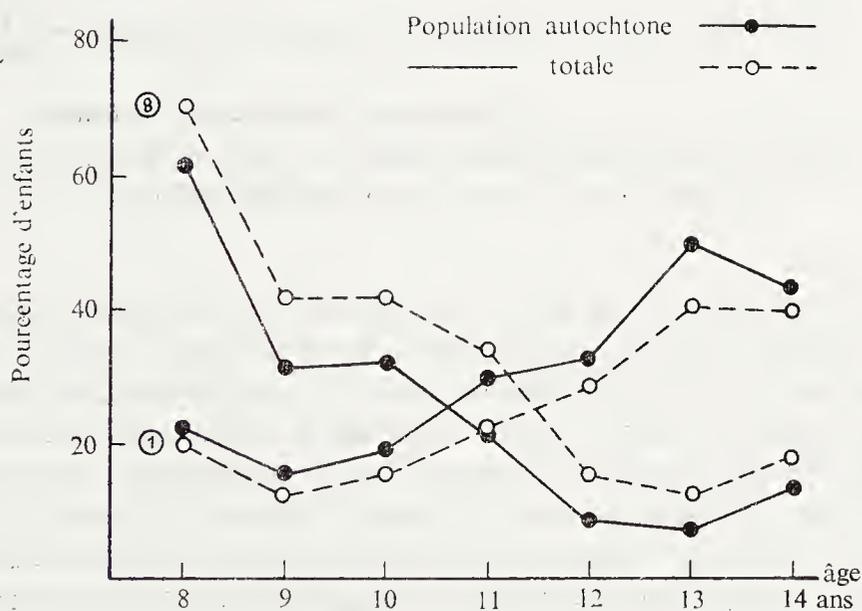


Figure 7. Variations du pourcentage d'enfants usagers du patois (1) et du pourcentage d'enfants ne parlant ni ne comprenant le patois (8) en fonction de l'âge.

proximité de Toulouse, mais séparé de la grande route et du chemin de fer par la Garonne qui, à cet endroit, n'est franchissable que par un bac: la quasi totalité de la population est employée dans des professions purement agricoles, le patois est d'usage courant.

Autre conséquence de l'industrialisation de cette région du Sud de la France: l'immigration d'ouvriers d'origine italienne ou espagnole. La figure 7 montre que la présence d'enfants ayant pour langue familiale une autre langue que le patois ou le

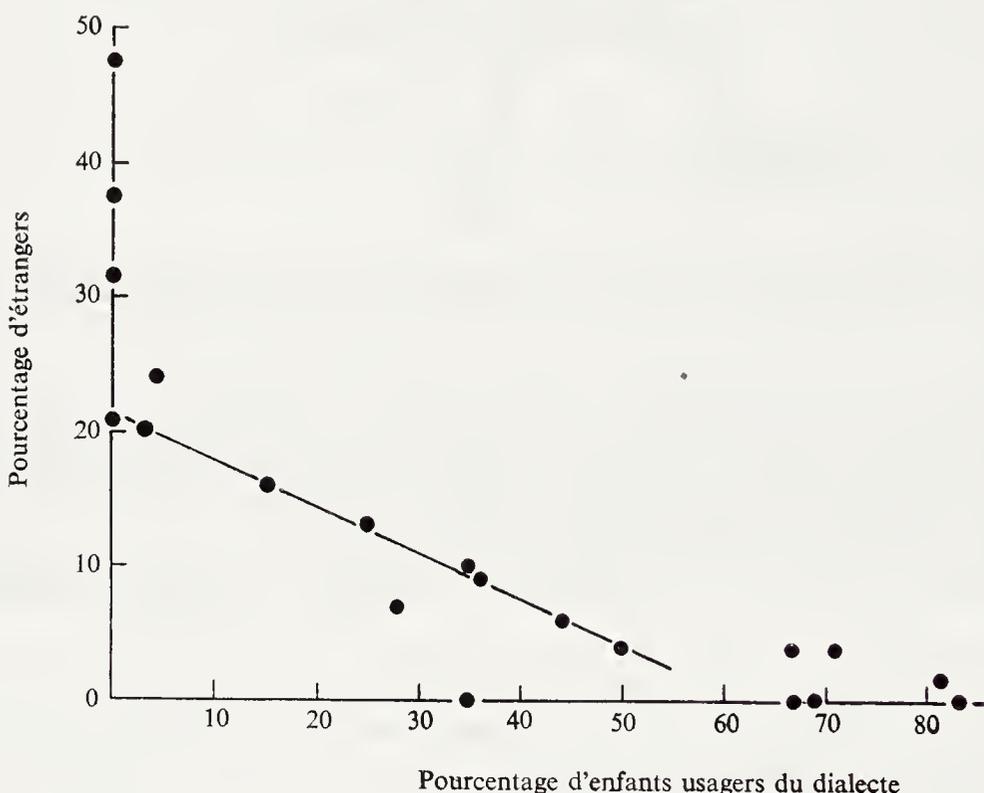


Figure 8. Variations du pourcentage d'enfants usagers du patois en fonction du pourcentage d'étrangers dans la population de chaque agglomération.

français intervient comme l'on pouvait s'y attendre pour diminuer le pourcentage d'usagers du dialecte. Ces enfants apprennent très facilement le patois et répondent positivement aux questions 5 et 6 du questionnaire mais ne le parlent pas couramment.

La figure 8 montre la liaison entre le pourcentage d'enfants usagers du dialecte et le pourcentage d'étrangers dans la population. Les agglomérations à fort pourcentage d'immigrés sont des agglomérations où l'usage du dialecte a fortement régressé. La liaison entre ces facteurs n'est qu'apparemment directe: les agglomérations à fort pourcentage d'immigrés sont généralement des agglomérations à forte proportion de populations employées dans l'industrie. La présence d'étrangers intervient comme un facteur d'accélération du processus d'involution des usages dialectaux.

CONCLUSIONS

Dans une situation de bilinguisme (dialectes et patois d'Oc) en évolution vers l'unilinguisme (français), certains facteurs d'ordre sociologique interviennent pour accélérer la transformation en cours. Cette étude montre l'importance des facteurs suivants: industrialisation d'une région, développement de son réseau de communications et proximité des agglomérations rurales par rapport à ce réseau, et celle des facteurs qui découlent de cette situation: transformation des proportions de main d'oeuvre agricole et industrielle, migrations quotidiennes (16 à 74% de la population active) vers le lieu d'emploi, immigration de travailleurs étrangers et de leur famille. En dehors de ces régions, les agglomérations qui gardent une forte proportion d'utilisateurs des dialectes et patois se caractérisent par les critères suivants: leur population est toujours faible (moins de 500 habitants), le pourcentage de cultivateurs est toujours élevé (plus de 60%), le lieu de l'emploi est toujours sur place (pour plus de 85% de la population active), il n'y a pas d'ouvriers d'usine, l'immigration est pratiquement nulle (moins de 4%).

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ONOMASTICS AND LINGUISTICS

FRANCIS L. UTLEY

Abstract

In Europe onomastics has become a highly rigorous discipline with unusual value for other disciplines. Of recent years in England the place-name study of Mawer and Stenton has, in combination with archaeology, strikingly rewritten the history of Celt, Scandinavian and Anglo-Saxon. A philological subject like Arthurian Romance has made significant use of personal and place names in determining the genesis of individual poems. Theory of names has earned a special place in semantics and in logic. The diachronic complexities have forced careful method on students abroad, and linguistic rigor has gone hand in hand with non-linguistic rigor and discovery. In the United States, with a few notable exceptions, name study has been largely a matter of entertainment and regional pride. Except in the case of Indian names, the etymological equations have been easy, and the proper study of Indian names has been notoriously scanted.

Successful application of onomastical science to other disciplines, then, depends upon systematic use of its linguistic components. These exist on all levels. Phonemics must be based both on accurate oral record and on a study of the complex graphemic problems from map and manuscript; graphemes (as in *Worcester*) differ from those of other word classes. Because of frequent use as sentence totals (traincalls, response sentences) proper names may be subject to special intonational effects. Folk-etymology, a rather unscientific area of investigation (*Cheboygan*), can benefit from the phonemics of bilingualism. The essence of serious place-name study has been the exhaustive census of combining elements and bound morphemes with their attendant morphophonemic questions (compare the four varieties of *-ing*). On the level of syntax the English proper noun lacks a determiner and the power *qua* proper noun of forming plurals. Names are barred from certain adjectival positions and verbal positions in English. The category of proper name is as universal as that of phoneme or morpheme, but the special signs and structures differ from language to language.

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ZWEI WORTGESCHICHTLICHE PROBLEME

JOHANN KNOBLOCH

1. Das urslaw. Wort für „Glas“, nämlich **stbko*, wird mit Recht als Lehnwort betrachtet. Wenn man es mit got. *stikls* „Becher, Kelch“ vergleicht, so macht der Bedeutungsunterschied wohl keine Schwierigkeiten und man hat schon seit langem dieses Wort mit anderen zusammen als eine Entlehnung aus dem Gotischen angesehen. Eine weitere Bedeutungsentwicklung, die aus dem Vergleich von ahd. *stihhil* „Stachel“: ahd. *stēhhil* „Becher“ hervorgeht, scheint für das Slawische schon ohne Belang zu sein. Dass das Trinkhorn (denn darauf führt aisl. *stikill* „Spitze eines Trinkhorns“) von einem Becher abgelöst wurde, sieht auf den ersten Blick nach „Sachwandel“ aus und die Annahme eines solchen erübrigt weitere Fragen. Diese Gedankengänge dürften allen bisherigen etymologischen Betrachtungen unseres Wortes zugrunde liegen.

In Wirklichkeit können wir uns mit diesem Sachwandel nicht zufrieden geben. Neben den Trinkhörnern, die bei heilbringendem Zutrink und im kulthaften Brauchtum der Tafelrunde ihre feste Rolle hatten, und schon vor ihnen gab es Trinkgefäße aus anderem Material, so dass hier keinesfalls die Ausdrucksnot – wie sonst beim Sachwandel – eine neue Bedeutung des Wortes entstehen liess: es gab Gefässbezeichnungen zur Genüge.

Aus einem zusammenhängenden Gebiet im Norden Galliens und des römischen Germanien kennen wir seit dem 3. Jahrhundert n. Chr. einen bemerkenswerten Exportartikel in die freien germanischen Länder, nämlich gläserne Trinkhörner. Noch im 4. Jahrhundert treten daneben Becherformen auf, die in ihrem Schmuck (es sind oft aufgelegte Glasfäden) die enge Verbindung mit der Glastrinkhörnern verraten. Es kann kaum zweifelhaft sein, dass der vom 5. bis ins 6. Jahrhundert erzeugte sog. „Spitzbecher“ die Rolle des alten Trinkhorns übernommen hat – und damit wohl auch seinen Namen, dürfen wir wohl hinzufügen. Auch nachdem die Römerherrschaft von germanischen Staatenbildungen abgelöst wurde, arbeiteten die rheinischen Glasmanufakturen weiter. Material und Technik werden gröber, aber die Nachfrage bleibt rege. An Hand von Fundstellen in Weimar und Prag-Veleslavín lässt sich der Handelsweg nach Böhmen eindeutig festlegen, ebenso die Zeit: denn hier geht es um die letzte germanische Fundgruppe (Reihengräber der Völkerwanderungszeit) vor dem Vorstoss der Slawen. Die Ergebnisse der Bodenforschung ermöglichen es, die Wortgeschichte zu rekonstruieren und der Sprachforscher kann den „Spitzbecher“ nunmehr zuverlässig mit vor-ahd. **stikl* benennen. An diesem Gefäss sah der Slawe zum ersten Mal

das ihm in dieser Verwendungsweise noch unbekanntes Material „Glas“ und nannte es mit dem german. Wort. Wer nach den vorstehenden Darlegungen die gotische der merowingisch-fränkischen Herkunft vorziehen will, muss eine Kette von Beweisstücken an die Stelle der bisherigen stillschweigenden Voraussetzungen – die sich als unbegründet erwiesen haben – setzen.

2. Auch bei einer anderen Wortgeschichte ist der Sprachforscher in der Lage, dem Urgeschichtler den Namen eines Gefäßes zu nennen. Von der schwäbischen Stadt Göppingen aus bietet der *Hohenstaufen* den Anblick eines Bergkegels. Zur Erklärung des Names könnte man sich auf ae. *steap* „hoch“ berufen und an ein tautologisches Kompositum denken. Kommt man aber in das Dorf Hohenstaufen, so hebt sich der Berg vom Himmel in einem einprägsamen Umriss ab und bildet, von hier aus gesehen, einen Kegelstumpf. Nun darf man daran erinnern, dass mhd. *stouf* die „Höhe“ und den „Becher“ bezeichnet. Es konnte also der Berg nach einem Gegenstand des Hausrats benannt worden sein, wie etwa im Tiroler Oberland der *Tschirgant* mit schaufelförmiger Spitze ein altes Dialektwort für „Schaufel“ bewahrt.

In der Tat lassen sich aus dem germanischen Bereich Deckschalen¹ für die Leichenbrandurnen nachweisen, die gerade in dieser Verwendung den gleichen trapezförmigen Querschnitt zeigen. Sie dienten, wie mir Prof. L. Franz mitteilte, sonst dem täglichen Gebrauch. Bezeichnend ist, dass sie wiederum in den germanischen Reihengräbern Böhmens gefunden wurden.

Innsbruck

¹ Ihren Nachweis verdanke ich Prof. L. Franz.

LINGUISTIC ASPECTS OF TRANSLATION

N. D. ANDREYEV

In discussing the linguistic aspects of translation I shall address myself to the following six questions:

1. What has been contributed by machine translation to the general theory of translation?
2. What constitutes an invariant in the process of translation?
3. What are the methods of confronting the elements of different languages?
4. What are the ways of transition from input structures to output structures?
5. What is the algorithmic linguo-typology, relative to the field of languages?
6. What is the future of translation?

1. The fundamental conclusion, which must be extracted from comparison between human and machine translation, consists in the following statement: Man translates, applying his understanding of the input and the output text; i.e., by correlating the given text and the formed one with his past and present *conscious and subconscious perception of reality*. The Machine on the other hand translates by passing from the input to the output text without any understanding of either; that is, merely by correlating the given text with a stored bi-codal vocabulary and with a prescribed routine of transitions from one code structure to another. This means that if we call *translation* the ensemble of operations executed by a translating machine, then a translating man does *more* than simply re-coding the input text into the output one, and this extra work, however much importance it has for human communication, is not necessary for the act of translation properly defined. Moreover, using such a definition we must conclude that actually Man *does not translate* at all, for he does not correlate the structures in two different codes. In practice a good "translator" first understands the heard (or read) message acting as a speaker of the input language, then he repeats the understood message now acting as a speaker of the output language. We may say that Man uses the input language and the output language on succession, whereas the Machine uses the two languages simultaneously. Surely, Man has the ability to translate in the same manner as the Machine (but not vice versa). A beginning student is compelled to act like the Machine because the inadequacy of his knowledge prevents him from understanding the foreign text directly; to him the

routinized correlation of the two languages is a forced substitute for direct comprehension. Understanding arises only after the correlation is done, and must be considered as a post-effect of translation itself. Even here we can see some difference between a student like a machine and a translating machine: in the latter we have no comprehension post-effect. From now on we shall speak of machine translation, of human translation (the beginner's case), and of human heterolingual rendering (the expert's case).

2. The creation of a numerical intermediary language (IL) for machine translation (MT) gives a new basis for treatment of various aspects of both translation and rendering. Even in binary systems of MT an input word is supplied with a number indicating the place where the corresponding output word is stored; those numbers constitute a kind of implicit *ad hoc* IL. From a theoretical point of view an explicit and universal IL, having the properties of autonomous functioning, is the only interesting case. Such an autonomous IL permits us to reveal one more distinction between MT and human heterolingual rendering (HHR): their invariants differ entirely. In HHR a set of thoughts and images serves as an invariant of a message rendered in two (or more) languages. Three messages: the French (F) – *les signaux de Mars sont déchiffrés*, the English (E) – *the signals from Mars are deciphered*, the Russian (R) – *signalys Marsa rassifrovany*, have the same invariant, the same thought induced in HHR, whatever direction of rendering we choose – FE, FR, EF, ER, RF, or RE. Turning to MT we find that the role of invariant sense is played by the *invariant text in IL*: let us go from the F message to some chain of numerical symbols in the IL, then further to the E message; during the RE translation we pass through the same intermittent chain of IL symbols. The paralinguage (input or output) text being compared with the corresponding IL text, one is obliged to expect that they will not be structurally identical (for instance, Russian genders are not represented in the kind of IL used by the Leningrad school of MT). This means that some elements of a paralinguage (PL) are *incongruent* when correlated to the IL. When elements of PL are congruent, translation is easiest: it reduces to simply replacing each PL element with the corresponding IL one, and vice versa. Serious problems arise when we have an incongruence to overcome. The same relation is evident when the IL or MT is compared with the logical language of information retrieval, which may be included in the field of paralinguages. To summarize the situation, let us say that a theory of translation must be essentially a theory of incongruences between PL's and the IL, and of a theory of algorithms for overcoming these incongruences.

3. Assuming a language space of two axes (syntagmatical, paradigmatic) and three levels (morphological, syntactical, semantical) we can develop a classification of incongruences. Because every paralinguage has those axes and levels, they must be represented in the Intermediary Language, too. The semantical units of the IL are *semoglyphs* (in the Leningrad MT they are five-digit octal numbers), denoting each a

ring of translational substitutes (R. *zajac* = E. *hare* = F. *lièvre* = D. *Hase* = I. *lepre* = . . .). There is no necessity to link semoglyphs directly with notions. When two notions, e.g., a radical in chemistry and a radical in politics, are expressed with a single word in all European idioms as well as in Indonesian, Turkish, Swahili, and many other non-European languages, it will be quite preferable to use a single semoglyph for both notions. By establishing two semoglyphs, we would be forced to make a choice each time we meet the word in any of PL's. By establishing only one semoglyph this task is eliminated without giving rise to errors since the probability of the two notions occurring in the same text is practically equal to zero.

The syntactical relations between semoglyphs are marked explicitly with *tectoglyphs*, the latter consisting of the phrase number of the governing node and adjoining to the governed one. The relations are provided with additional information by *formoglyphs*, which also adjoin the semoglyphs and indicate the role of the words in the sentence (subject, direct object, attribute, etc.). The other morphological characteristics, expressed by formoglyphs too, are those which exist in the majority of languages (the formoglyph of the plural, the formoglyph of the future tense, and so on). In our IL of MT there are twenty two entities included in the list of formoglyphs.

The incongruences between a PL and the IL, existing at the semantical level, are classified as various *semies*. On the syntagmatical axis we have *aposemy*, the case when the number of words in a PL term exceeds the number of semoglyphs in the corresponding IL chain; *macrosemy*, when vice versa; *metasemy*, the case when the numbers are equal, but a word-by-word translation of a PL term does not lead to the right IL chain, and at least one of the semoglyphs of the combination, obtained during "word-by-semoglyph" transition, must be replaced by another one. On the paradigmatical axis we have *polysemy* (embracing homonymy), when one and the same PL word under different circumstances is translated with different semoglyphs; and *synsemy* (formalized synonymy), when different PL words are translated with one and the same semoglyph. Each of these five semies needs its peculiar method of overcoming the incongruence, the routines being substantially dissimilar and depending also on the direction of the translation: synonyms give no trouble if we move to the IL, but choosing one of them is a difficult task if we move FROM the IL.

On the morphological level we find analogously various morphies: two on the paradigmatical axis and three on the syntagmatical one. Polymorphy is illustrated by the English ending *-s* (he fights, the fights), which splits into two different formoglyphs: the present tense and the plural; symmorphy – by the Russian nominal endings *-y, -a* (atomy, doma), both converging into a single dormoglyph of the plural; *apomorphy*, *macromorphy*, and *metamorphy* are the morphological parallels of their semantical namesakes. On the syntactical level one meets as an independent case mainly meta-tecty, for the other four types of tecties usually result from semies as their corollary.

Thus, the matrix of elementary incongruences consists of fifteen items. Of course, in many cases the incongruence between a PL and the IL group of elements belongs to more than one level and one axis simultaneously. Here the procedure of overcoming

the incongruences divides into two phases: first, resolving the complex incongruency into semies, morphies, and tecties, and then combining the standard routines prescribed for the corresponding elementary incongruences.

Complex incongruences produce relative wholes. In the input language they are produced with regard to the IL; in the IL, with regard to the output language. Taking as an example the English *in order to*, we see that each of the three words when used independently has its own semoglyph, but when they are dependently placed one after another they constitute a situative unit translated into the IL with a single semoglyph. This clear case of aposemy (combined with apotecty) well represents the dependent *valencies*, i.e., the possibilities of special links which exist inside the relative whole induced by an incongruence. We may say that a valency is potentially present in a word and is actualized as a link when the word is placed beside another word with the corresponding symmetrical valency. The preposition *in* possesses the right valency for the noun *order*, which in turn has the left valency for *in* and at the same time has the right valency for *to*, the latter possessing the appropriate left valency for *order*. All the valencies in the example become links creating the relative whole, incongruent to the IL (three words through four valencies into one semoglyph).

The system of dependent valencies is the most important result of projecting a paralinguage onto the Intermediary Language, and vice versa.

4. Translation via the IL consists of two principal stages: *analysis*, i.e. transition from the input language to the IL, and *synthesis*, i.e. transition from the IL to the output language. In both stages a symbolic sign system, called the *metalanguage* (ML,) is used for the description of a message in the paralinguage. The difference between a PL and its ML lies in the cardinal fact that the majority of syntactical relations and many morphological categories are given in the PL indirectly, implicitly, while in the ML all of them are expressed directly and explicitly. A symbolic element of the ML used in the description of morphology or *formeme* denotes one and only one grammatical characteristic of the PL word. For the morphological image of the French métaux we shall use three formemes: One for the substantive (symbol *S*), a second for the masculine (symbol *m*) and a third for the plural (symbol *p*). Only the last of the formemes will be converted into a formoglyph during passage to the IL, the first two formemes having no correspondents in the IL grammar. In the ML *tectemes* are used for the description of syntactical relations, whereas sememes are used for the description of word-building (to the German *Treibstoffsatz* three sememes correspond in the ML symbolization).

Because every PL has its own grammar and its own word-building which does not coincide with systems of other PL's we have to conclude that each PL requires its special ML. The IL on the contrary is one and the same for all the PL's of the translational field; this means that the IL is not identical with any of the ML's. Given the message in a PL, its description of it in its special ML must be totally isomorphic with the described: all the PL categories of the message are to be symbolized faithfully,

including those which do not exist in the IL. Here lies the obvious reason for the dissimilarity between the ML's and the IL: the same message in the IL will be normally non-isomorphic with the PL message.

The analysis contains two groups of operations: 1) the *descriptive group*, where we replace the chain of morphemes of the input PL and the implicitly given relations between them, with the set of the explicitly written formemes, tectemes, sememes of the corresponding ML; 2) the *forward-normalization group*, where we delete those symbols, which do not exist in the IL, add the lacking ones, and change those elements which are incongruent with regard to the LP. Symmetrically, the synthesis contains two groups of operations too: 1) the *backward-normalization group*, which transfers the IL set of elements into the output ML set of symbols; 2) the *enscriptive group* which replaces the set of the ML symbols with the chain of morphemes of the output PL.

Operations for overcoming incongruences belong naturally to one of the normalization groups. Having found among a cluster of ML formemes a symbol of valency, the analyzing machine (case of MT) or man (HT) has to check, whether the next cluster of the ML formemes includes the symbol of the pair valency. If not, nothing is to be done; if it does include the symbol, however, the sememes to which the valencies are attached must be searched for in the special list of sememe pairs, and after the positive result of the searching, the prescribed change of the semoglyphs must be performed. Once this subroutine has been finished, we do not keep the ML set of symbols intact; the isomorphism between the PL message and its ML description is lost at the very moment of the semoglyph change. Consequently since at that point of the algorithmic time the message is in a transitional state, it has already ceased to be a purely ML chain, but it has not yet become a purely IL chain. A similar thing can be said about the descriptive (enscriptive) group of operations: during the most part of its algorithmic time the message is in a transitional state which no longer represents a pure PL chain of morphemes and has not yet been converted into a pure ML chain of symbols. In practice those pure forms exist only before and after the analysis (the same with the synthesis). Usually it is much more convenient for the creator of the algorithm to interweave the two groups of operations within each stage combining some descriptive operations with certain forward-normalization ones, and some backward-normalization with certain enscriptive operations as well. It is self-evident, that the mixing up of the operations does not affect their theoretical status which in any conceivable combination and admixture remains absolutely unchangeable.

The analysis begins on a single-word level by splitting any word into a stem and a residue (in those languages that permit such treatment, of course); when necessary we make their grammatical description more precise by consulting the formemes of neighbouring words. Asemic (i.e. without corresponding semoglyphs) words, which are mostly the auxiliaries of various kinds, transfer their information to semic ones and get a special mark for future deletion; the valencies are utilized for indicated changes. After these operations the initial text becomes a kind of sieve with pendants: its sec-

tions must be shifted in such a manner that all traces of deletions and of insertions are eliminated, and the text becomes compact and linear once more. Here the pre-syntactical phase of the analysis ends.

The main purpose of the syntactical part of the analysis is to establish throughout the text the pair configurations with a governing node and a governed one. This is made by using the hierarchy of syntax: first the immediate neighbours are tried for pair configuration, then the checking is spread to some fixed distance (plus-minus 2, 3, 4 words) and only later the whole sentence is investigated without restriction for possible pair links. At each stage those words which have already got the governing node are excluded from further consideration; the only word that has not found its governor is fixed as the summit of the sentence tree. In the final phase of the analysis some post-syntactical rearrangement is executed with the aim of adjusting the received IL chain to its standardized form.

For synthesis we have much richer information than for analysis; that explains why the synthesis is essentially simpler and shorter than its counterpart. The main difficulty in the synthesis is the necessity to work out a good word order. And the best solution of the task lies not in the highest stylistic beauty of the output text, but in the minimum complexity of operations used to get it.

5. The best basis for constructing an algorithm of translation is provided by statistico-combinatorial methods which reveal the most important properties of a language. Two methods of the kind have already been developed: approximal analysis and the algorithmic statistico-combinatorial modelling.

Approximal analysis is the simpler and rougher procedure of the two. As its point of departure we take the functional classes. A part of speech in its function as a part of a sentence forms a separate functional class. All dubious groups of words are treated as temporary autonomous classes. Each class obtains its own symbol; then all the words in a given set of sentences are indexed with the symbols, thus filling five matrices. The first matrix shows the spectrum of indices which can be right and left neighbours of a given class. The second matrix pictures the spectrum of governor and dependent indices which can be linked with the given class in tree structures of the sentences. The third matrix reflects the cases of coinciding neighbourhood and syntactic link. The fourth is devoted to syntactic links existing without neighbourhood; and the fifth to neighbourhoods existing without syntactic link. Each matrix has two entries, thereby giving to the classes ten characteristic spectra. The spectra are compared according to a strictly determined routine. From the results of the comparison it is possible to judge whether a dubious group is a really independent class, or whether it must be united with another class having the similar set of spectra. Furthermore, one obtains a paradigmatic lattice of relations between parts of a sentence and parts of speech, the lattice being based on probabilistic spectra for each class. The results are considered as the first level of approximation. It can be used immediately for constructing an algorithm of translation or be accepted as a stopping

point for the second step of approximation, now based on a more subtle system of functional classes and relations between the latter. The results can be considered in turn as the second level of approximation and so on, until the $n + i$ -th step yields the results identical to those of the n -th one.

The data obtained by approximations analysis permit one to distinguish the *coer* of syntactic relations which includes the relations between those functional classes whose summary probability (among highest probabilities) amounts to 0.5. The pair configurations belonging to the core are established directly, their functional classes being represented in the standard subroutines of the translation algorithm. The more frequent of non-core pair configurations are established indirectly, through the use of special *valencies of the second* type; because they bear no relation to the IL, they are called independent valencies. They may be considered as incongruent with regard to the PL syntactical core. This fact produces additional evidence for the close connection between valencies and incongruences.

The algorithm for statistico-combinatorial modelling works *without any use of word and phrase meanings and without any kind of preliminary-grammatical information*. Only texts (divided into phrases, words and phonemes, or graphemes) are given and investigated within a frame of strictly determined programs which can be fully executed by computers. The statistical operations of the algorithm consist mainly in correlating conditional probabilities with unconditional ones, the set-theoretic operations mainly in grouping linguistic units according to their combinatorial properties with regard to the key point revealed by correlation of probabilities. The statistical and set-theoretic operations work in the algorithm alternately; transfer of control from one routine to another depends on the results obtained in the last executed subroutine.

The algorithm includes routines for investigating morphology, syntax, word-derivation, and semantics; it is fully described in N. D. Andreyev's paper published in *Materialy po matematičeskoj lingvistike i mašinomu perevodu*, tom 2 (Leningrad, 1963), pp. 3-44.

When the statistico-combinatorial modelling of morphology is started, we must first of all establish the probabilities of the paralinguistic elements. Conditional probabilities of phonemes (graphemes) correlated with unconditional ones permit one to find out the first affix of a morphological paradigm. A set of bases combining with the affix is compared with another set of bases combining with the second affix, and a subset belonging to the two sets is formed. A recurrent routine reveals all the affixes constituting the paradigm and the type of bases for which the paradigm is characteristic. When all the types existing in the language are established, the next routine investigates the co-occurrence of types. The results make it possible to group types into divisions analogous to parts of speech. Binary indices connecting affixes with the divisions are formed; neighbourhood of the indices in word-strings is used for splitting polysemic affixes into primary ones. A new routine is utilized for functional grouping of primary affixes which leads to obtaining formal categories analogous to grammatical ones. Each elementary functional group of affixes is connected with several formal

categories, thus yielding a formal image for words whose affixes belong to the elementary group. When a language possesses no inflexions (type of Vietnamese) the algorithm, having discovered the fact, turns to treating functional words, chosen again on a statistico-combinatorial basis, as separately written "affixes".

Procedures used for investigating syntax and word-building are based on very similar principles. Probabilistic modelling of semantics partly differs from them: it is founded on *semantic distances*. The latter are calculated according to formulas that require numbers for co-occurrences. The similarity between two words is measured in six different ways, and the weighted mean square of these is used as an overall estimate of semantic distance. The first component measures the mean separation of two words in the word sequence of each sentence. The second component is derived from the tree structures of the sentences, distance being measured by the number of links between words. The third component measures the tendency of words to occur together in one compound. The fourth measures the extent to which words tend to share common neighbours to the right and left; the fifth, the extent to which they share common governors and common dependents. Finally the sixth component measures their tendency to combine with common partners in forming compounds. The first three components are of a syntagmatic nature, the last three ones are of a paradigmatic type. Problems of synonymy, homonymy, polysemy, semantic groups, semantic regions, are approached by an *a posteriori* method, that involves taking some sets of words which, all would agree, constitute a semantic class of the given kind, discovering what properties these sets have in terms of semantic distance, and accepting all sets which show the same properties as representatives of the class.

Both the particular ways along which the universal algorithm for statistico-combinatorial modelling proceeds, and the resulting grammar, depend on the internal structure of the investigated PL. Comparing and classifying those different ways will inevitably lead us to the independent algorithmic typology of languages. Projecting PL grammars onto the IL grammar and evaluating the resulting systems of incongruences leads us not less inevitably to the dependent algorithmic typology. Here the IL serves as the origin of the typological space.

Both algorithmic typologies (AT) are necessary for the better solution of machine translation problems: the descriptive (enscriptive) operations are connected with the independent AT, the forward (backward) normalization operations – with the dependent AT. The theory of human heterolingual rendering differs from the two AT's functionally: a man repeating the understood message in another language needs the theory neither before the rendering nor during it – but after it, in order to evaluate the results obtained. This means that the theory of HHR is essentially a scheme for comparison of two messages, the input and the output one, with regard to their thought-and-image contents; it may therefore be named the *comparative psychoinvariant textology*. Only after the properties of psychoinvariants have been adequately studied will one be able to venture to construct a typology of languages correlated with thoughts and images.

6. To minimize the volume of normalization operations we must have the IL as close to the PL's as possible. It is, however, impossible to move the IL in the direction of the Chinese language, for example, without at the same time moving it away from the structure of Russian, and vice versa. We are, therefore, obliged to seek a middle course; i.e., to minimize the average quantity of incongruences for the translational field of the PL's taken as a whole. By appropriately weighting each language of the field, we are able literally to *calculate* the properties of the IL (the calculation has just been performed in Leningrad).

Not only the general properties but also the concrete elements of the IL – simple and complex ones – may be calculated with the help of the weighted means. Looking at a term through the entire PL field, we shall usually see that in the majority of its languages the term is represented in a similar cluster of sememes, and only a minority gives several deviating representations. Let us take as an example a subfield of 9 paralinguages and a term in it: Russian *obratnyj tok elektroda* = English *inverse electrode current* = French *courant inverse d'électrode* = Spanish *corriente inversa de electrodo* = Italian *corrente inversa d'elettrodo* = Dutch *omgekerede electrodestroom* = Polish *prąd elektrody wsteczny*; all seven are congruent with each other, and form the clear majority. Two remaining languages give incongruent equivalents: German *Kathodenstrom*, and Swedish *backström*; both need the dependent valencies for the case to be converted to the IL. Those three sememes of the case, found in the majority of the subfield, with the meanings (*current*), (*inverse*), (*electrode*), and their corresponding semoglyphs 00065, 14230 and 00665 – taken from the real IL dictionary – form a *basic* representation (BR) of the term in the IL. The procedure of getting the BR's is not always so simple and easy, and sometimes one must use a rather complicated routine of calculating the BR; nevertheless the routine is subordinated to the same law of minimizing the sum total of incongruences throughout the PL field. The BR's constitute the system of the IL units of the second order, called *koinoglyphs*. The koinoglyph system must be considered as an outcome in the sphere of retrieval language, that is of the sign code for information retrieval.

The retrieval language (RL) is not required to minimize the average level of incongruence: it is a logico-pragmatical code serving as an instrument for accumulating information. Its structure is fully determined by the classification of scientific facts, and cannot depend on any field of paralinguages and is therefore not identical with the structure of the IL. In spite of their dissimilarity, the very existence of the IL as a passdoor to the RL evidently facilitates creating a truly international network of information retrieval. Instead of translating from so many PL's into the RL and back, with the IL it is sufficient to have only two algorithms of translations: the IL-RL analysis and the RL-IL synthesis. Thus, machine translations (MT) becomes the first stage of information retrieval (IR).

The most advantageous scheme of MT will be that of establishing 200 national MT computers with only two algorithms of translation in each: analysis from the national PL to the IL, and synthesis from the IL to the national PL. Every important piece of

scientific, technological and public information will be fed into the computers, translated into the IL, 200 copies of the translations being spread among all the national centers and duly translated back into the output PL's. The most advantageous scheme of IR will be that of establishing two IR computers for every branch of science and technology, duplicating each other and situated in opposite hemispheres. The materials in the IL, produced by the national MT computers, will be incessantly put into the branch IR computers, according to their contents; the two international systems of computers (MT and IR) will be indissolubly interwoven with each other by means of the Intermediary Language of Machine Translation.

Mankind has had numerous languages for communication of class I, "humans-to-humans"; now we elaborate codes for communication of classes II, "machines-to-machines", III, "humans-to-machines", IV, "machines-to-humans". With this new development the linguistic aspects of translation are focussed around that unique language which serves the communication classes II, III, and IV simultaneously, that is around the Intermediary Language of MT. One may foresee a not very remote period of time when the IL, common to men and machines, will expand the sphere of its application to communication class I too. Men, getting accustomed to meeting one and the same language circulating between all the computers, may take that lead and begin to use the IL as a means of direct access to the electronic "brains", therewith saving the extra time and money which are spent for the MT, preceding and following the IL. To facilitate the process of direct communication with information machines, a phonetic form of the IL will be developed, because of the millenia old custom of men to utter and comprehend sounds easier than figures.

But even such development will not kill the theory of translation: we can press upon the terrestrial machines our Intermediary Language, but we shall not be able to do the same with regard to extraterrestrial civilizations. Being involved into a group of those cultures, each with their own IL, we shall be compelled to construct in cooperation with them the Intermediary Language of the Second Order, which will mean a new life to the theory of translation and make it a Science of Highest Rank and Importance.

Till now we, linguists, dared only *explain* languages. The time has come, when our chief occupation must be *creating* them.

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DISCUSSION

WINTER:

I would like to make two brief comments:

1. The basic assumption about the essential difference between human and mechanical translation is only partly justified. It is not true that all human translation implies comprehension of content except in the case of a beginning language student. Rather does it seem to be a very common experience in practical translation work that

the content one is asked to render in the target language is comprehended only in part; still, the human translator can exact the transfer. To give an example; there is no obstacle to my translating a text in nuclear physics or in advanced mathematics without my understanding it, provided that I know the linguistic structures involved and that I have been furnished with the appropriate vocabulary information.

2. One consideration which seems to let us favor the introduction of an artificial interlanguage is the resulting economy in the number of translation programs needed for the handling of a large quantity of natural languages. Obviously, if texts in ten source languages were to be translated individually into ten different target languages, we would need one hundred individual programs. If we used an artificial interlanguage, only twenty such programs would be needed. However, if one decided against an artificial interlanguage, but instead chose to select a natural language belonging to the twenty referred to, clearly only nineteen transfer programs would be called for. Thus, an artificial interlanguage does not constitute a source for maximum economy; moreover, and this is more decisive, reliance on an artificial interlanguage implies reliance on an entity which one cannot help considering ill-conceived in strictly linguistic terms.

HAUGEN:

I have had no experience whatever with machine translation. However, I have been translating all my life, both informally as a bilingual speaker, and formally as a teacher and writer. Not until reading Andreyev's paper, however, did I discover that what I have been doing is not translation at all, but "human heterolingual rendering". I wish to say right from the start that I protest strongly against this redefinition of the term "translation". It would be a melancholy step towards the dominance of machines over men if the word "translation" should come to be synonymous with machine translation. Andreyev has given us an instructive analogy by comparing the work of the machine with that of our schoolboy cribs. In my lexicon this is not translation at all; it is just what I have called it, a crib. At its most successful it carries over from one language to another that fraction of the message which is conveyed by the most obvious lexical equivalents and the least subtle morphemic features. Even the high redundancy of natural languages cannot save the message from a disastrous loss of information when it is filtered through this kind of distortion. If we must include mechanical translation within the range of our definition of translation, let us at least keep the modifier "mechanical", abbreviated MT, and contrasted with "human translation", abbreviated HT. Let us not use the latter term for that subhuman kind of work produced by our less gifted students, whose brains are still vastly superior to those of the computers. Perhaps their work could be called "mechanical human translation", or MHT.

Except for this purely terminological comment, I commend Andreyev's description of HT. He assumes "a set of thought and images" between the translator's input and output. This corresponds rather exactly to my own intuitive experience in translating

from Norwegian to English and back. The input in the SL does not trigger the output in the TL directly, except in routine messages for which direct associations have been established by previous experience. To put it crudely: the input forces me to recreate in my mind the social context of the utterance and to search my memory for the closest equivalent in the output language. Instead of the single S-R box which receives the input and generates the output, I have to have two such boxes, one for each language. The channel between them is not a mechanism which matches words and structures, though it can also do this, but one which matches the message contents. Permit me to clarify by an example: in older Norwegian plays one often finds cultivated ladies exclaiming "Gud!", which any mechanical human translator would unhesitatingly render "God!" This can be avoided only if the translator stops to recall the kind of person involved and matches the exclamation with one which similar English speakers might use, e.g. "Good heavens!" or even "Dear me!" I once translated a physics exam from Norwegian to English. When I submitted it to a physicist, he laughed and found it necessary to make a number of corrections in order to make it a correct message. Ideally translation should convey the whole message, without loss of information. This can only be approximated by a process of re-creation, in which a new utterance is stimulated by an imaginary analogue of the situational and linguistic context of the original utterance. The term information here includes not only the referents of the message, but also everything that a message can and does tell us about the speaker himself and his attitudes. It might be possible to set up a scale for measuring this kind of information, and in this case it is clear that the gap between any kind of HT and the best possible MT is not only vast now, but is likely to remain so in the foreseeable future.

FRANCESCATO:

The phrasing, "time has come when [the] chief occupation [of us linguists] must be creating languages", is inaccurate insofar as language has always been created by human beings.

GARVIN:

1. Prof. Andreyev's theoretical position is reminiscent of Hjelmslev's conception of the "purport of content". In Hjelmslev's terms, the construction of an interlingua would have to be based on an independent analysis of the purport of content.
2. The desirability of an interlingua for reducing the number of algorithms required for multiple translation has been asserted for several years now and can, in principle, not be disputed. The a priori construction of an interlingua, on the other hand, must be considered extremely unrealistic. It would require the comparison of a multiplicity of languages of diverse genetic origin in a degree of detail comparable to, and exceeding, that needed for historical reconstruction.
3. A more realistic view of the possibility of reducing the number of algorithms for multiple translation can be based on the separation of recognition routines (analysis)

from command routines (synthesis), which is already being followed by a number of machine translation groups. Instead of designing an interlingua a priori, separate recognition and command routines can be written for various languages. The information obtained by the recognition routine for a given source language can then be used by a number of different command routines for various target languages, and conversely. A master control program can be imagined that would act as a "switchboard" into which the different recognition and command routines could be connected as required. The advantages of such an empirical approach are that the various recognition and command routines can be written and checked out separately in the process of designing practical systems for individual language pairs, and the ambitious objective of a more general translating system is deferred until it can be based on tested experience.

HAHN:

Since no two languages make precisely the same distinctions, any translation, especially that provided by a machine, must be hopelessly inadequate. For instance, how can English represent the nuance denoting a change in human relationships presented in French or German by a shift from *vous* to *tu* or from *Sie* to *du*, or by the reverse shift? – It seems to me that a mechanical translation from any given language can be adequately interpreted only by one who knows the language which is being translated – and in that case he does not need the machine!

P. IVIĆ:

Every translation, human or mechanical, brings upon a certain loss of information (distortion of the message). This loss increases if we translate from one language to another, and then from this one to a third one. Would the use of the intermediate language in mechanical translation not also cause an augmented loss of information?

POURQUOI LES LOIS PHONÉTIQUES SONT SANS EXCEPTION

JEAN FOURQUET

I

Lors de chaque acte de parole, des figures phoniques (signifiants) porteuses d'une fonction d'information (signifié) sont assemblées selon certaines normes. C'est ce que A. Martinet appelle la première articulation du langage. Cette technique de communication suppose un code, auquel se réfèrent les participants de l'acte de parole. Ce code est caractéristique de la langue employée.

Les travaux inspirés par les notions de phonème et de système phonologique se fondent sur l'idée que les figures phoniques de première articulation résultent de l'assemblage, selon certaines normes, de signaux phoniques élémentaires, les phonèmes.

Chacun de ces signaux est caractérisé par un ensemble spécifique de traits phoniques distinctifs, de sorte qu'un phonème est distingué de tout autre par au moins un de ces traits. La production et l'identification des signifiants comme des séquences de phonèmes suppose que les participants se réfèrent aux mêmes signaux élémentaires, distingués par les mêmes traits phoniques. La collection de ces signaux constitue le système phonologique de la langue étudiée.

C'est ce que A. Martinet appelle la deuxième articulation du langage. Cette technique de construction des signifiants par combinaison d'unités discrètes est une solution économique du problème de la production de plusieurs milliers de signifiants distincts.

On peut penser que seule la première articulation est inhérente à la notion de langage articulé. Une écriture purement pictographique se réfère uniquement à cette première articulation. L'existence de la deuxième articulation est pour nous une donnée de fait; elle s'accorde avec la possibilité de délimiter par commutation les unités discrètes dont sont formés les signifiants; elle se vérifie par le comportement des sujets parlants devant des tests de correction de fautes, de transfert dans le système phonologique de la langue principale des sons d'une autre langue (Lautsubstitution), etc.

Les observations que nous avons pu faire en Alsace sur les parlers locaux confirment l'existence de la seconde articulation avec plus de netteté encore que celles qu'on peut faire sur les langues du type koiné (français, allemand).

Il ne semble pas qu'on ait vu jusqu'à présent que si la deuxième articulation existe, il en résulte logiquement que les lois phonétiques sont *sans exception*.

Qu'entendait Leskien lorsque vers 1875 il a proclamé ce qu'il appelait *Ausnahmslosigkeit der Lautgesetze*?

Si l'on compare deux états successifs I et II d'une langue donnée, dans *tous* les mots qui en I contenaient un segment affecté de certains caractères articulatoires (ainsi occlusion bilabiale, absence de voix= p) ce segment aura subi en II le même changement (ainsi un changement en f (constrictive labio-dentale); si on observe le changement $p > f$ dans un mot, on peut prévoir qu'on le retrouvera dans *tous* les mots contenant un p , c'est-à-dire un segment occlusif, bilabial, sourd.

On considérait alors les signifiants, unités phoniques de première articulation, comme les unités phoniques dernières. Certes le phonéticien reconnaissait des types de segments phoniques, consonnes, voyelles, de qualité diverse. Mais on avait le sentiment qu'il s'agissait d'une activité de l'esprit, capable de reconnaître des parties dans un objet continu, comme on distingue dans un pays des montagnes, des vallées, des plaines, des forêts.

On découvrait alors qu'une partie d'un mot, présentant certains caractères articulatoires, était affectée d'un changement, toujours le même pour des caractères articulatoires donnés, et on concluait que la cause du changement se trouvait dans ces caractères physiques.

Nous utiliserons ici, comme comparaison, le cas d'une écriture pictographique (un dessin indécomposable par signifié): si dans les dessins d'une telle écriture, tous les traits présentant une certaine courbure et une certaine inclinaison étaient atteints par le même changement, ce serait évidemment une constatation remarquable.

Si cette hypothèse (hautement invraisemblable) se réalisait, il serait logique de chercher une cause physique de la modification du tracé: position de la main du dessinateur, nature de l'instrument traçant, nature du support...

Mais pour nous, il s'agit de tout autre chose: le changement porte sur les caractères distinctifs attachés à l'un des signaux élémentaires: la norme de production d'un phonème a été modifiée; la nouvelle norme est valable pour tous les cas où ce signal élémentaire entre dans la composition d'un mot.

Nous pouvons illustrer ceci par la comparaison avec une écriture alphabétique. Si en France on se mettait à enseigner à l'école à tracer la lettre *e* en forme d'épsilon (ϵ), comme le font certains Anglais, nous n'aurions pas à nous étonner si dans tous les mots contenant un *e*, le tracé de cette petite boucle était remplacé par une boucle ouverte, ni à chercher une cause physique de ce changement, comme si les jeunes Français n'étaient plus capables de tracer une boucle fermée.

C'est que des mots comme *eau*, *venir*, *deux* sont des combinaisons de lettres, et non des dessins globaux pour notre norme d'écriture.

L'observation d'un changement phonétique revient donc à cette constatation: de l'état I à l'état II, la norme qui définit les traits distinctifs d'un phonème a changé. Le fait que ce changement atteint tous les mots où se trouve ce phonème est de la nature du principe d'identité: A est A; tout individu qui présente les caractères spécifiques de l'espèce A est un représentant de l'espèce A, où *qu'il se trouve*.

II

L'erreur qui consistait à méconnaître la deuxième articulation et à raisonner sur le signifiant des unités de première articulation comme sur des signaux phoniques globaux a eu pour conséquence de fausser toutes les hypothèses relatives à la cause des changements et à leur nature.

Les changements ont été conçus comme des altérations d'un segment de signifiant en tant que fait physique, articulatoire, et l'on a cherché des causes physiques capables d'agir sur l'articulation des sujets parlants.

On a proposé en conséquence des explications comme celles que donne Meillet dans son *Introduction à l'étude comparative des langues indo-européennes*: "Il se peut qu'il y ait dans chaque population certaines tendances héréditaires qui ne se modifient pas du fait que les sujets changent de langue et qui, s'appliquant à une langue nouvelle, y déterminent des altérations notables" (7e éd., p. 25).

Ou encore: "dans la France du Nord, à partir d'un certain moment, différent pour chaque localité, les enfants ont été incapables de prononcer l mouillée et y ont substitué le y qui en tient aujourd'hui la place dans les parlers français..." (p. 19).

Ce type d'explication ne résiste pas à la confrontation avec les faits: si une tendance héréditaire celtique avait rendu les Français incapables de prononcer u de latin murus, mulus, qui passait à ü de fr. mur, mulot, comment est-il possible que le son u ait réapparu à partir de o, ainsi dans *tour, nous, tout*? Quelle cause peut rendre les enfants incapables de produire des articulations que produisaient leurs parents, dans une population dont la composition raciale et les conditions de vie restent les mêmes?

En fin de compte, en imaginant qu'à un moment donné apparaît dans le comportement articulatoire des membres d'une communauté une cause spécifique, qui exerce une action sélective sur l'articulation de certaines parties des signifiants, on n'a fait que créer une entité, comparable à la vertu dormitive de l'opium.

Nous opposerons à cette conception une vue fondée sur l'existence de la deuxième articulation comme faisant partie des normes auxquelles se réfère la communication dans une communauté linguistique donnée, c'est-à-dire comme participant du caractère institutionnel du langage.

Tant qu'une innovation phonétique, par exemple une avance de l'articulation de u ne se présente que comme une déviation occasionnelle par rapport à la norme, elle n'a pas encore d'existence linguistique. Il n'y a passage d'un état de langue à un autre que si cette déviation est reconnue comme norme; il y a alors changement d'identité d'un phonème. Ceux qui acceptent ce fait parlent une nouvelle langue, distincte de la précédente sur au moins un point, sur le plan de la norme de la deuxième articulation.

La causalité des déviations occasionnelles se situe dans le domaine de l'anatomie et de la physiologie de la parole. Mais l'acceptation d'une innovation de ce genre comme *norme*, son institutionnalisation, est un fait social. Or dans les décisions d'une communauté joue une causalité complexe, qui n'est pas uniquement physique: la communauté réagit devant un conjoncture. Certes les facteurs articulatoires ne sont pas négligeables,

car ils déterminent les directions dans lesquelles se produisent des déviations occasionnelles, qui pourront éventuellement être acceptées comme la norme. Mais entrent également en ligne de compte la fonction distinctive des traits nouveaux par rapport aux autres phonèmes, le rendement lexical ou morphologique d'oppositions menacées par les innovations articulatoires; et il ne s'agit ici que de facteurs intralinguistiques: le prestige du parler d'une autre communauté, qui présente certains traits phonétiques, l'emprunt d'une masse de mots présentant le phonétisme d'une autre langue, la dominance des forces conservatrices ou novatrices dans une communauté sont des facteurs du plan social et psychologique.

L'étude de chacun de ces facteurs est facilitée dans les cas où l'un d'eux est nettement dominant, au point que les autres peuvent être considérés comme négligeables, ou dans les cas où une comparaison est possible "toutes choses égales d'ailleurs".

Le problème des changements phonétiques est ainsi ramené du plan des sciences de la nature au plan des sciences humaines historiques, à qui la notion de causalité complexe est familière.

L'étude des multiples facteurs en cause, longtemps retardée par la fausse analogie des lois phonétiques avec les lois physiques, s'esquisse actuellement à partir des expériences les plus diverses: étude des structures phonologiques (A. Martinet), du rendement lexical et morphologique, théorie des communications, atlas dialectaux, sociologie et psychologie linguistiques. Il importerait de coordonner toutes ces activités.

Les changements de la norme de la deuxième articulation ne concernent pas seulement le changement de l'identité d'un phonème (ceci correspond à ce que l'ancienne phonétique appelait changements spontanés ou inconditionnés); ils concernent aussi la réduction du nombre des phonèmes par coalescence, ou l'accroissement de leur nombre par phonologisation (phonemicization) de variantes (allophones); enfin la reconnaissance comme norme de faits de neutralisation d'une opposition, ou de variantes de position, c'est-à-dire des faits de phonétique combinatoire.

Il va donc falloir reconstruire toute la phonétique historique en termes de succession de systèmes phonologiques.

Une fois les faits caractérisés de façon plus adéquate, il sera plus facile de poser les problèmes de causalité: ici, bien que la connaissance de la deuxième articulation ait fait apparaître de nouveaux types de causalité intralinguistique (structure du système phonologique, fait de "rendement", c'est-à-dire d'interaction entre la première et la deuxième articulation), nous devons nous dire qu'ils n'épuisent pas une causalité complexe, et qu'il reste une part de causalité extralinguistique, qui exigera une liaison de la recherche linguistique *historique* avec les autres sciences humaines.

III

L'application du principe que les lois phonétiques sont sans exception (Ausnahmslosigkeit der Lautgesetze) a permis aux néogrammairiens de faire faire des progrès consi-

dérables à la grammaire comparée et à l'étymologie. Nous ne nous en étonnerons pas, puisque nous donnons à ce principe un fondement plus solide encore.

Cependant, dans la pratique de son application, les linguistes ont rencontré des difficultés, ont trouvé des *exceptions*. Les objections se sont multipliées aussitôt que les atlas linguistiques ont permis d'étudier de plus près les faits d'évolution.

On sait que Gilliéron, fort des observations que permettait l'ALF, n'a pas hésité à proclamer la faillite de l'étymologie phonétique, fondée sur l'idée qu'on se faisait alors des changements phonétiques. Les exemples accumulés par Gilliéron ont produit une telle impression qu'on n'ose plus, depuis, parler de lois phonétiques; on parle de *tendances*, qui se réalisent plus ou moins complètement. Depuis 60 ans, la phonétique historique a mauvaise conscience: elle continue, faute d'autre chose, à employer une méthode fondée sur un principe auquel elle ne croit plus.

Si le principe a le caractère absolu que nous lui donnons, celui d'une vérité déduite de l'existence de la norme linguistique (et non d'une proposition inductive, comme les lois des sciences de la nature), nous pouvons être certains que les exceptions sont apparentes: elles s'expliquent par des conceptions qui ont été indument associées à une notion de loi phonétique mal interprétée.

Un des filons exploités par Gilliéron a été l'étude des réfections de formes, celle de la "thérapeutique verbale", et du remplacement de signifiants devenus incommodes par d'autres: on trouve *avette*, élargi d'un suffixe, ou *mouche*, là où la loi ferait attendre *ée*.

C'est qu'ici entre en jeu non une causalité physique indépendante de la fonction de communication, mais l'exigence d'une technique de communication: les changements affectent ici, non des unités de la deuxième articulation, mais des signifiants, unités de la première articulation, et ce genre de problèmes peut être facilement disjoint.

Il en est autrement des changements qui restent dans le champ de la deuxième articulation: le normand dit *cat*, mais *chaine*, et a même *canchon* à côté de *chanter*. La loi selon laquelle l'occlusive vélaire latine donne une chuintante devant *a* s'applique-t-elle capricieusement? En fait les formes en *ch-* sont venues de l'Ile de France; il y a ici mélange de deux *normes*, c'est-à-dire de deux langues.

Chanter relève de l'histoire phonétique de l'Ile de France, *canchon* de celle du normand. Ici s'appliquent deux lois phonétiques, l'une et l'autre sans exception par nature, tant qu'il s'agit d'un fait intralinguistique.

Ce qui empêchait de comprendre cela, c'est l'ancienne conception *stemmatique* de la différenciation des dialectes (Stammbaumtheorie): une communauté se scinde en groupes à l'intérieur de chacun desquels l'évolution des signifiants obéit à des lois spécifiques de ce groupe. Dans ce cas, les exceptions ne pouvaient s'expliquer que par une défaillance de la loi.

Les atlas linguistiques ont donné raison à la Wellentheorie; les dialectes d'une aire différenciée continuent à communiquer entre eux, et un dialecte est perméable aux innovations et aux formes d'autres dialectes.

Des multiples collusions, dont le détail commence à être connu, sont possibles entre

dialectes, depuis des emprunts au phonétisme d'un autre dialecte jusqu'à celui de formes isolées (Formübertragung).

Il y a là, certes, un grave facteur d'indétermination, mais elle est déjà levée en partie par la certitude qu'il ne peut s'agir d'une défaillance de la loi, mais qu'il s'agit de l'action des lois d'un autre groupe.

Armé de cette certitude, le dialectologue pourra plus facilement reconstituer l'histoire de l'aire sur laquelle il travaille, et de sa différenciation, et séparer les emprunts, déceler les fausses régressions, etc.

L'attribution des changements phonétiques à des lois spécifiques d'un groupe ethnique impliquait en fait, par sa logique interne, l'association de la notion de loi phonétique sans exception avec une conception *stemmatique* de la différenciation dialectale.

La conséquence de cette liaison a été que l'observation directe des faits a produit un choc dont la loi phonétique a été la victime innocente. C'est la *Stammbaumtheorie* qui aurait dû être frappée, et l'interprétation des causes qui liait celle-ci à la loi phonétique.

Pareille à la lance du Graal, la dialectologie se doit aujourd'hui d'apporter le remède au coup qu'elle a porté.

Pour conclure, disons que nous pouvons rendre à la linguistique historique bonne conscience dans l'emploi de méthodes fondées sur la constance des correspondances phonétiques, tout en lui indiquant les causes qui limitent l'efficacité de ce bon outil de travail, et laissent subsister une marge d'indétermination. Les causes étant désormais mieux connues, nous pouvons espérer que cette marge sera progressivement réduite.

Sorbonne

DISCUSSION

BUYSENS :

M. Fourquet nous donne des exemples de prétendues exceptions tirés du dialecte normand, qui a emprunté au français central des mots comme *chaîne* et *chanter*; il est évident que dans pareil cas il ne s'agit pas d'exception. Mais je connais un cas d'un autre genre: en néerlandais, le suffixe diminutif était autrefois partout *-ken*; par exemple, *voetken*, petit pied. Ce suffixe a perdu l'*n* final et le *k* s'est palatalisé en *j*, ce qui a donné, par exemple, *voetje*. Ce phénomène a commencé dans une aire médiane allant du sud-ouest au nord-est, sans considération des variantes dialectales; dans cet aire, tous les mots ont à présent le suffixe *-je*. A la périphérie, il y a deux aires restreintes qui ont gardé la forme *-ken* dans tous les mots. Dans les zones intermédiaires, on constate qu'une partie des mots diminutifs a gardé *-ken*, tandis que les autres ont *-je*; cette variation ne tient pas à une influence du phonème qui précède le suffixe; d'ailleurs un mot qui a *-ken* dans une région a *-je* dans une autre et inversement.

Il me semble difficile de parler ici de l'influence du dialecte emprunteur; il me semble même difficile de parler de dialecte; car les dialectes n'ont pas de frontières: il n'y a que des isoglosses qui s'enchevêtrent. Et la propagation de la forme *-je* crée

précisément une isoglosse; l'isoglosse marque la limite au delà de laquelle commencent les exceptions: l'isoglosse crée le dialecte, si tant est qu'on puisse parler de dialecte. On ne peut donc pas dire, dans ce cas, que les différences dialectales ont créé les exceptions et fait apparaître l'isoglosse. C'est l'inverse: c'est l'isoglosse qui crée la différence dialectale; ou, si l'on préfère, ce sont les exceptions qui créent la différence dialectale.

ALLOPHONES, ALLOMORPHS, AND CONDITIONED CHANGE

HENRY M. HOENIGSWALD

Our assertions about linguistic change are not so much descriptions of events as they are formulae of replacement. Of sound changes this is immediately clear; but the same is true in principle of the types of change known as grammatical, syntactical, and lexical changes.

For practical purposes one might well divide these formulae into three kinds, each one with its own characteristic style of statement. The first kind is the *typological*. For instance, it may be said that the earlier stage of a language has a three-number system (singular, dual, plural), while the later, by comparison, has a two-number system. Or it may be said that an earlier stage has a three-vowel system while a later stage has a five-vowel one. Note that the "comparison" we have in mind is the kind that might be made among any two or more languages but is here specifically applied to two languages which are not only defined as related but as related in the special manner to which we refer as descent. Note further that "comparison" here carries its everyday, almost non-technical meaning which is quite different from the particular sense which the word has acquired during five generations of "comparative grammar".

The second kind of formula states a *replacement pattern*; or, if you will allow the expression, it states bits of translation from stage to stage. For instance, in the Romance languages the preposition *de* replaces both an older ("identical") *dē* "from" and an older genitive ending; Italian *méno* "less" replaces Latin *minus*, Italian *crédo* "I believe" replaces Latin *crēdō*, and generally Latin *i* as well as Latin *ē* in accented position are replaced by (or, as we say, "go to") Italian *e*. As it happens, these particular examples are both mergers, that is, the replacement is two-to-one. Other possible patterns are one-to-one, and one-to-two (or one-to-more).

The third kind of formula has to do with all sorts of *relations* that may exist *between the two partners* of a replacement process, provided only that they go beyond the defining relationship of replacement itself. For example, the replacing unit may be found physically similar to the unit which it replaces (in which case we may want to assert that "no change" occurs). Or the two may be found to be physically different in any one of a number of typical ways (thus the voiced stops of Indo-European are replaced by the unvoiced stops of Germanic). Again, the new and the old may either be found to occupy corresponding points in the two typologies, or else not; the first vowel in Italian *crédo*, while physically quite similar, for all we know, to that of Latin *crēdō*, belongs

to one vowel phoneme out of seven (*i e ε a ɔ o u*), whereas its Latin antecedent represents a vowel phoneme which is one out of five (*i e a o u*), plus a prosodeme of length. The Latin words *māter* "mother", *avia* "grandmother", *amita* are replaced by Italian *mādre*, *nōnna*, *zīa* respectively. *nōnna* and *zīa* are physically dissimilar from their ancestral counterparts. *mādre* and *nōnna* are the typological equivalents of *māter* and *avia* in the lexicon of kinship words, but *zīa* and *amita* are not equivalents, since *amita* means only "paternal aunt" and contrasts with *mātertera* "maternal aunt". And so on.

These, then, are the three styles of historical description: by typology, by replacement pattern, and by comparison between partners in replacement. As our examples have reminded us, these styles are applicable to all areas of language: phonetics, phonemics ("segmental" and "suprasegmental"), morphophonemics, grammar, and vocabulary. Conventionally, to be sure, certain styles of statement are more favored in some of these domains than in others; but although there are some good reasons for that it remains a matter of secondary importance. On the whole, the framework is valid throughout. We need not be surprised or suspicious that this should be so since we are quite used to letting certain fundamental concepts operate both in phonology and in morphology. One of these concepts I propose to examine here, namely the concept of contrast and one of its converses, the principle of complementation.

As it happens, this principle furnishes one of the more popular morphologic-phonemic analogs in synchronic theory. At least according to one rather widely adopted terminology, non-contrasting (and, in particular, complementarily distributed) phones are allophones and constitute, in the aggregate, a phoneme; non-contrasting morphs are allomorphs and constitute a morpheme. The relation between [n:] in *sin* and [n] in *sinner* is said to parallel the relation between the morph *ride* in *rides*, *rider*, *riding* and the morph *rid-* in *ridden*. The argument centers only on what other criteria than complementarity may be needed in addition, to establish membership in one phoneme or in one morpheme. Here, indeed, one discovers disparities. In phonemics many, perhaps most, will demand phonetic similarity of a sort among the phones in question. In grammar, phonemic resemblance among morphs is not crucial, even though it is of course a far more clear-cut affair than phonetic resemblance among phones, and even though it strongly predominates in many types of natural language. The mutual status of *go* and *wen(t)*, of *-er* and *more* (in *finer*: *more beautiful*, etc.) is not really called into question by the suppletive nature of these examples. Possibly, then, unless the whole analogy is radically wrong, the role of phonetic diversity among allophones in phonemic analysis is not the same as that of phonemic diversity of allomorphs in grammar, and it remains to discover the true analogs.

At this point it may prove instructive to have a close look at the behavior of allophones and allomorphs under change. Nobody doubts that the allophonic structure of phonemes is essential for an understanding of the dynamics of sound change. Whether or not one favors cause-and-effect formulations of change, the analysis of change processes as such clearly presupposes, reflects or, if you will, helps confirm a break-

down into allophones. The “sound changes” which our manuals list are, as we have said, assertions about phonemic replacement. Conditioned sound change formulae, in particular, state the replacement of a phoneme in different phonemic environments. The replacement pattern is one of split, where the entities replaced are positional allophones or blocks of allophones, since “phoneme in a given environment” and “positional allophone of a phoneme” are evidently equivalent terms. In a conditioned split, positional allophones are reassigned.

I have tried to show elsewhere that in the realm of morphemics, that is, in so-called semantic and grammatical change processes as well as in “borrowing”, the situation is indeed parallel. All we need to do is to choose a formulation which brings out the parallelism. In the history of English, *conscience* has replaced *inwit*: on the whole a neatly one-to-one replacement which we may liken to one-to-one sound changes of the “shift” variety, where the new phones are dissimilar to the old phones but where there has been neither merger nor split. The morphs are new, but the morphemes continue much in the old manner of contrast. The lexical categories are the old ones, regardless of the forms which fill them.

Just so, phonemic merger has its morphemic counterpart; the Italian example *zia* above is an example. But since we are interested in positional variants and their behavior, we must specifically turn to morphemic split processes. Take a simple example: Latin *nepōs* is in the long run replaced in some Modern French contexts by *neveu*, in others by *petit-fils*.¹ The event may be described as a conditioned change. In the particular text environments in which *nepōs* has the meaning “nephew” its later equivalent is *neveu*; where it has the meaning “grandson” it is now *petit-fils*, much as, say, in early Latin the particular instances of /s/ that occur between vowels “go to” *r* while the other positional allophones of /s/ “remain” *s*.

It is of course customary to present developments of this sort not as properties of morphemes but as properties of morphs. This particular instance is therefore ordinarily classified as (1) a semantic change (namely a change in the meaning, or morphemic location, of the morph *nepōs*), accompanied (2) by a neologism (namely the creation of a new expression, *petit-fils*).

We prefer to return to our own formulation. The morpheme *nepōs* (i. e., the morpheme whose unique morph is the phoneme sequence *nepōs*) has been split in two, much as the early Latin phoneme /s/ has been split in two. In phonemic split the role of the allophone seems evident. We think – in this particular instance we almost know – that such cases of phonemic split are the final consummations of phonetic divergence. It is said that intervocalic *s* must have passed through a stage [z] before merging with the old *r*-phones. Now, on this score our analogy breaks down altogether. Obviously, allomorphs have nothing whatever to do with morphemic split. Latin *nepōs* had only one allo-

¹ It goes without saying that the data are far more complex than the present oversimplification indicates. For a brief summary see Bloch and von Wartburg, *Dictionnaire étymologique de la langue française*, s.v. The fact that *petit-fils* is not a simple morpheme but a construct is here disregarded as irrelevant, although of course it is quite relevant for the origin of the neologism.

morph;² had it had more, these allomorphs would still have no relation to the split which later occurred in French.³

This teaches us an interesting lesson. It seems that the true analogy obtains not between the phonetically distinct positional variants on the one hand and the phonemically distinct but complementary morpheme variants on the other, but rather between parts of phonemes occurring in different classes of phonemic environments and parts of morphemes occurring in different classes of morphemic environments. This means that the appeal to shape or content is secondary while the criterion of distribution plays the central role.

On the phonetic-phonemic level it is easy to misjudge the relationship between the two criteria. Complementary allophones are not merely different from one another; they are also, as everyone knows, similar to their surroundings in the flow of speech. They are, on the whole, assimilatory in nature. What is more, allophonic differences within a phoneme are likely to be governed by a conditioning in such terms as for want of a better word I will describe as "simple" or "important". It is no accident that early Latin must have had a voiced allophone for /s/ "between vowels" – a conditioning which is indeed simple to name and which in turn lends importance to a grand division, in the Latin sound system, between one class of segmental phonemes called "vowels" (and, in part, semivowels j, v, :) and another class of non-vowels or consonants.

How are morphemic split processes pre-formed in the preparatory stage? Most assuredly not along allomorphic lines. These, as historians of language well know, are fossils rather than harbingers of the future. From all we know, the split of a morpheme, that is, the setting up of a grammatical or semantic contrast where there was none before, is nevertheless typically preceded by the development of subtle flaws within the homogeneity of its distributional existence. How can this breakup be expected to become recognizable until it becomes overt through two separate replacement processes – separate, that is, for each portion of the morpheme? It is notoriously and necessarily difficult to formalize this with anything like the precision to which phonemics can be subjected. But perhaps we can perceive the outlines. To oversimplify vastly, a morphemic split like that of *nepōs* into *neveu* and *petit-fils* takes place in these three stages. There is one (1) in which the area of meaning is homogeneous and "general"; where, in other words, the synchronic observer (working from the texts, or introspecting as a speaker of the language) finds no more reason to subdivide it into "(a) child's son" and "(b) sibling's son" than, say, into "(a) daughter's or sister's son" and "(b) son's or brother's son" or, for that matter, into "(a) redheaded *nepōs*" and "(b) non-redheaded *nepōs*". There follows a second stage (2) in which the meaning is no longer general but takes on the characteristics of homonymy such as exists to a much more extreme extent within

² Except for possible intonationally conditioned allomorphs about which we know nothing.

³ Of course it happens occasionally that allomorphs are utilized, as it were, to act as replacers for morpheme parts. They are then unrecognizably redistributed. The example of *shade: shadow* comes to mind. See also my *Language Change and Linguistic Reconstruction*, 39-40. The present paper is intended to correct undue emphasis given to the allophone-allomorph analog in that volume.

a morpheme like *swallow* ("a bird" vs. "gulp"). We may be confident that this state of affairs is recognizable distributionally – perhaps in the sense that the particular combination of characteristic text environments in which *swallow* occurs is not paralleled for any other English word.⁴ The case must necessarily be weaker for *nepōs* at the comparable stage (the history of the homonymy, for one thing, is essentially quite different); yet we may expect it to be similar in nature. Support would have to come from showing that the distinction between linear and collateral descent, or the distinction between one and two generations of descendants develops into an important dimension in other parts of the kinship terminology as well. We note in passing that ordinary lexicographic usage with regard to homonyms varies in an interesting way: some will speak of one word, *swallow*, with two meanings, while others will operate with two words, that is, with two morphemes both represented by the same morph, *swallow*. The case of *nepōs* during the second stage is perhaps a bit too weak to make us seriously consider setting it up as two separate morphemes. Still, it is conceivable. In any event the true third stage(3) is reached, and as it were openly proclaimed, by the separate morphic replacement (*neveu: petit-fils*) for each half of the entity in question.

The upshot is that the analogy between morphemic and phonemic split is valid enough. This is welcome because among many other things both can be shown to have a quite parallel usefulness for the operations of the comparative method of reconstruction. But to keep the analogy fruitful we must avoid mistakes in identifying the homologous factors. To match phonetically identified allophones and phonemically identified allomorphs would be to commit precisely such a mistake. The true correspondence is revealed by observing the dynamics of the change processes; it obtains between distributionally identified blocks of phoneme occurrences (within the phoneme as a whole) and distributionally identified blocks of morpheme occurrences (out of the whole morpheme).⁵ And "distributionally identified" means: defined in the "simple, important", self-justifying way to which we have alluded above (p. 648). Here is where the hierarchical build of language can lead us astray: it so happens that our distributionally identified phoneme occurrences do have a way of coinciding with the phonetician's phones or sound types, whereas distributionally defined subdivisions within morphemes must lack all intrinsic connection with the diversity of allomorphs founded merely on differences in shape.

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⁴ *LCLR* 19.

⁵ David Smith, to whom I am obliged for discussing this question with me, suggests the term "distributional allomorph".

THE NATURE OF SOUND-CHANGE

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The purpose of this paper is to consider some contradictions in currently expressed or widely held views on the reasons for the regularity of sound-change, and to suggest possible ways of resolving the contradictions.¹

The principle of the regularity of sound-change is basic to all aspects of historical linguistics. It is interesting to note the development of meaning between, say, Lat. *iunior* and Fr. *gindre*; but before we can do this we must know that one word is in fact the ancestor of the other, which is by no means obvious on the face of it. Or again, in discussing problems of linguistic geography, we assume that we are in a position to say that certain forms are simply dialect variants of what is basically the same word, while other forms are completely unrelated. But in many cases, perhaps, if we are to be rigorous, in all, such identifications rest on generalisations about the correlation of sounds in different stages or dialects of the same language. Such generalisations are made on the assumption that correspondences of sound are likely to be regular, that is, phonetically patterned. As a matter of common observation, many such regularities do exist; equally obviously there are many exceptional forms which do not fit into the patterns which can be observed. It is clearly essential to know the limits within which such patterning can be expected. Otherwise, interpolation and extrapolation become arbitrary, and we have no defence against the linguist who alters the rules of the game to suit his own convenience. But it is hard to see how these limits can be determined except by establishing the reasons for the regularity of sound-change.

Exceptional forms can, of course, be explained in various ways. They may be the result of borrowing from a dialect in which the development was regular, or of analogy with other forms related in meaning or function. The influence of other psychological factors, onomatopoeia or sound-symbolism, for instance, has been debated; but there would seem to be no objection in principle to such explanations, however much one may query individual examples. Analogy itself is a psychological factor; if one admits the possibility of association with words of related meaning or function, one cannot rule out association with, say, natural sounds or muscular movements. And

¹ The first part of this paper is founded on a talk given to the Linguistic Circle of St. Andrews University in 1954, a version of which will appear in a forthcoming volume of studies in language and literature by members of the Circle.

it is to be noted that all linguistic changes except purely semantic ones are changes of sound; if some instances of phoneme *x* merge with phoneme *y* in a given language the effect is the same whether the change is the result of a phonetically determined split or dialect borrowing or analogy. It is begging the question to limit the term sound-change to phonetically patterned sound-change. Nor is it clear that all regular sound-change is the result of phonetic pressures. This is particularly doubtful in the case of unconditioned sound-change; but it is very often just these unconditioned sound-changes that are most regular in their incidence.

Linguists have tended to approach this problem by setting up a distinction between the more or less sporadic type of change resulting from the disturbing factors mentioned above, and so-called "regular" or "natural" sound-change, which is assumed to proceed in a given population by gradual and imperceptible stages, and which is usually explained as resulting from the inability of the individual to reproduce exactly the sounds which he hears.² But why, if these variations are imperceptible, should they all work in the same direction? It is true that there are certain types of sound-change which seem to be natural in the sense that they are likely to appear spontaneously among different members of a population; sound-changes which make for easier articulation are an example. But, as we have seen, not all regular sound-changes can be explained in this way. It is particularly hard to see why speakers should embark independently on unconditioned changes. Even in the case of conditioned changes it is not plain that all speakers must follow the same path. It is usually considered that the tendency to voice voiceless spirants between vowels is a natural one, but the reverse process has occurred in the history of Spanish.³ Even if one development is in the majority, one would expect there to be minority groups of various sizes, resulting in an increasing fragmentation of language. If in fact a sound-change is carried through completely in a particular speech-community, it must, it seems to me, proceed by imitation due to social pressure, just like dialect borrowing.

A refinement of the theory of gradual sound-change is that such accidental variations in the realisation of phonemes are in fact perceptible, and are unconsciously imitated, there being a constant assimilation to whatever realisation is accidentally in the majority.⁴ It is even admitted that the imitation (unconscious, presumably) of socially favoured individuals may be of importance.⁵ The trouble with this approach is that it still does not explain why such accidental variations should be phonetically patterned. It is no doubt true that we do not consciously distinguish between sounds except as grouped in phonemes, or at the least allophones of phonemes determined by phonetic position. But the whole point of this theory is that we can, unconsciously at any rate, perceive and imitate far finer variations of sound; and in that case there seems to be no reason why such gradual variations should be bounded

² Gray, *Foundations of Language* (1939), p. 85 provides a good statement of this point of view.

³ Cf. Martinet, *Economie des Changements Phonétiques* (1955), p. 35.

⁴ Hockett, *Course in Modern Linguistics* (1958), p. 440.

⁵ Gleason, *Introduction to Descriptive Linguistics* (1955), p. 289.

by phonetic, rather than, say, semantic associations: why, for instance, the *t* of *taker* should not develop along different lines from the *t* of *taken*. To put it another way, this explanation assumes the phoneme; but an analysis of the role of perception in the development of sound-change cannot assume the phoneme; it must incorporate an explanation of the phoneme as part of itself.

It is no doubt the case that imitation of subphonemic variants is generally a good deal less conscious than imitation of phonemic variants, though in my experience even a difference of phoneme can appear in one's speech without conscious intention. On the other hand, I do not think that it needs to be unconscious. The difference between patterned and unpatterned sound-change does not lie here. After all, it is not simply the phonemes of a language which are constant from speaker to speaker in a single speech-community; the realisation of the phonemes in terms of allophones is also constant within reasonable limits. It is this use of a particular set of allophones which more than anything constitutes the pronunciation-pattern – the accent – characteristic of a speech-community. Such variations may not be significant for the differentiation of meaning, but they are socially significant – they are status symbols.⁶ The child not only learns to distinguish between phonemes; he learns to use the allophonic variations characteristic of the group.

But what I would deny is that it is possible to imitate such subphonemic variations in one word and not in another, unless this variation is purely phonetically determined. Imitation of such allophonic variations is all or nothing. A speaker may certainly vary between the old pronunciation and the new, but this variation will be uncontrolled, and will not be systematically connected with one word rather than another. This is a matter not of the ear, but of the tongue; it is an articulatory, not a perceptual difficulty. Once this point is understood, much of the difficulty over the regularity of sound-change vanishes. We can appreciate, and even consciously imitate, quite a small allophonic variation, but what we cannot do in ordinary, reasonably rapid speech, unless we are exceptionally skilful – and it is the unskilful majority who ensure the spread of a phonetic change – is to use allophone *x* in one word or set of words and allophone *y* in another, unless the variation is supported by the differing phonetic surroundings. Perhaps only controlled experiment can determine what phones can coexist in a language as phonemes and what cannot.

Regular, or phonetically patterned sound-change, then, is sound-change which is initiated and transmitted at the subphonemic level – and, strictly speaking, this does not include the same changes if and when they reach the phonemic level; but this is a matter which we will consider in a moment. The ultimate cause of a sound-change does not affect the issue, just as the geneticist is not concerned with the causes of the mutations which he studies (which is not to say that the cause of mutation is an extra-biological problem, or an unimportant one). Sound-changes may be “natural” in the sense already described, that is, they may be due to the phonetic incompatibility

⁶ So Gleason, p. 165.

of various sounds. They may be due to fashion; for instance, a particular language may favour a forward articulation or a backward one – English and Arabic may be examples of these two tendencies. The original impulse may be simply the whim or disability of some socially favoured individual; we cannot rule out this possibility. Or conceivably, as suggested in the theory already mentioned, the changes may be the result of pure accident, analogous to the biologists' "genetic drift". I am not so happy about explanations that imply a desire, conscious or unconscious, on the part of the individual to systematise language. Sound-changes often do have this effect, but they need not do so; and perhaps such phenomena are adequately explained by the fact that subphonemic alterations often, if not always, involve not a particular phoneme, but rather all the phonemes falling within a certain range. In other words, it is not so much a matter of fronting or backing a particular phoneme, say, but of a general tendency to use a forward articulation, or a backward articulation – or a forward one before front vowels and a backward one before back vowels, and so on. This would be enough to set up systematic groupings of phonemes according to shared phonetic features – and languages vary considerably in the amount of system which they show;⁷ contrast say the vowel-system of Turkish with Modern English. Again, the fact that phonemes must have a minimum phonetic distinctiveness to co-exist will prevent the number of phonemes in a language from becoming excessively great – further splits will lead to mergers – and in any case successive sound-changes are likely to cancel one another out in this respect. But whatever the cause of particular sound-changes, it is not this which makes them regular, but the fact that they are initiated and spread purely at the subphonemic level.

This formulation implies, I think, that subphonemic changes proceed by jumps rather than gradually, if not by such large jumps as in the case of change at the phonemic level, though this is perhaps not necessary to the theory. At any rate, it allows for the possibility. Plainly, however, one subphonemic change may be imposed on another, until the resulting sound is heard by speakers of another form of the language as belonging to a completely different phoneme. When this happens, dialect borrowing in the usual sense of the word, that is, borrowing at the phonemic level, becomes possible. Such dialect borrowing, no doubt, usually involves sound-substitution, but this is not necessarily the case. A new phoneme may be introduced into a language if it is heard as distinct from other phonemes in the language; thus a Southern English speaker may easily learn to use a Scots /x/ or /ɲ/, and introduce it into his speech – I have done this myself – though probably not in every case in which a Scots speaker would use it. Thus dialect borrowing may alter the phonemic structure as well as the phonemic distribution of a language. Equally, it seems to me, not every sound-change with a phonetic basis need be subphonemic, and so regular in its incidence. Thus, changes which lead to the loss of a phoneme, such as the omission of the middle consonant of a group of three in Old English, may not involve

⁷ Cf. Hoenigswald, *Language Change and Linguistic Reconstruction* (1960), p. 81.

any development at a subphonemic level – we can easily omit a sound in one word and keep it in another without doing violence to our powers of speech. I see no reason in principle why such a sound might not be preserved in a particular case, to preserve some feature of inflection, for instance. Dissimilatory changes are perhaps in the same position.

On the other hand, sound-change which proceeds at the subphonemic level cannot be influenced by disturbing factors; all sound-change which is not purely phonetically patterned must have proceeded at the phonemic level, or, if it proceeded at the subphonemic level, must have been interfered with after it had for whatever reason come to involve a phonemic distinction. Thus, not only dialect borrowing, but analogy and any other psychological factors involving associations of any kind – and this includes all expressive formations, e.g. expressive gemination – must operate at the phonemic level, and are not necessarily regular. Of course, it is possible for levelling to be carried to its extreme, and wipe out all signs of an earlier patterned sound-change, or for dialect borrowing to go on until the new phoneme has completely replaced the old; but there is no necessity for this to happen, and a relict group is usually found.⁸ But such instances of complete replacement must be based on some earlier subphonemic change, so the position is not really altered.

The principle that subphonemic changes are of necessity regular, and that no other changes are, is not in itself particularly novel. I suspect that most linguists act upon it, even if they do not explicitly formulate it. But I hope that the preceding explanation, based on the disharmony between perception and articulation, may help to indicate more clearly both why the principle holds, and what are the limitations to it. And the distinction between subphonemic and phonemic change has not always been drawn as clearly as one might expect. Thus, structuralists often make the statement that sound-change is regular in its incidence because the phonemic structure of language forms a system; but this, though suggestive, is not without further qualification an adequate answer to the problem, as it does not explain how in that case there can be any sound-change which is not regular.⁹ Bloomfield, again, says of the doctrine that sound-laws admit of no exception: “Historically interpreted, the statement means that sound-change is merely a change in the speakers’ manner of producing phonemes and accordingly affects a phoneme at every occurrence, regardless of the nature of any particular linguistic form in which the phoneme happens to occur”;¹⁰ but then goes on to say: “It is certain that these non-distinctive, sub-phonemic variants are subject to linguistic borrowing (imitation) and to analogic change

⁸ Hoenigswald, p. 54, n. 12. Another example is no doubt the form of the London place-name, and hence often of the appellative, *Strond*, in Early Modern English. This seems a better explanation than that given by Kökeritz, *Shakespeare’s Pronunciation* (1953), p. 166.

⁹ E. g. Pos, quoted in Rosetti, *Les Changements Phonétiques* (1948), p. 8; cf. Juilland, *Word*, 9 (1953), p. 200. Martinet, *Word*, 8 (1952), p. 3, simply assumes the existence of “regular” sound changes.

¹⁰ *Language* (British edition, 1934), p. 353.

(systematization)";¹¹ though the example which he proceeds to quote seems clearly a case of phonemic variation. Gleason goes in the other direction, and classes phonemic variation (by which he means, apparently, phonemic variation based on earlier subphonemic variation) together with phonetic variation as necessarily regular.¹² But this is not entirely satisfactory as a formulation, as the whole point about phonemic variation as opposed to phonetic variation is that it does not have to be regular, in the sense of phonetically patterned; the phoneme is not bound to any particular phonetic position; it can theoretically interchange with other phonemes, and therefore any regularity which exists is not necessary regularity.

The apparent contradiction in these cases can only be resolved by a closer look at what happens when a subphonemic variation becomes phonemic. This is a vital stage in the process which has received curiously little attention. In the case of unconditioned sound-changes there is strictly speaking no stage at which the change becomes phonemic, unless it results in a merger (which is not to say that unconditioned changes are unimportant; they are plainly of great importance from the point of view of dialect borrowing); this is no doubt why they are so often completely regular. In the case of conditioned sound-change the result will eventually be a phonemic split of some sort. Now, it is surely not sufficient to say that in such cases a subphonemic variation becomes phonemic as the result, for instance, of a loss of conditioning sounds. In the case of the split between back and front *c* and *g* in Old English there is little question that other consonants than *c* and *g* originally had fronted allophones, as the same sounds which produced fronting of *c* and *g* also produced front-mutation of the vowel of the preceding syllable, no matter what the intervening consonant. But only in the case of *c* and *g* had the variation between the two allophones reached such a degree that they could stand on their own without the support of the conditioning sounds. In other words, even before the loss of the conditioning sounds, the two allophones must have been potentially phonemic; that is to say, the two sounds were capable of being heard as two separate phonemes even before the loss of the conditioning sounds gave rise to minimal pairs. But why then, if the only reason for a sound-change to be regular is that it is subphonemic, should the variation remain regular when it reaches a stage at which the two phones are capable of co-existing in the language as two separate phonemes? Hoenigswald has attempted to solve this problem by an ingenious explanation of the way in which subphonemic changes reach the phonemic level.¹³ His theory, if I understand him rightly, is that a subphonemic change may be imitated by the speakers of a different dialect of the same language (or even a different language). They hear the variation as a phonemic one, and adapt it to the phonemic structure of their own language. One must then assume a further shift of social status, so that the substandard pronunciation now becomes fashionable, and drives out the former pronunciation.

¹¹ P. 365.

¹² P. 290.

¹³ P. 55.

The final result is that in the original dialect a subphonemic variation has been replaced by a phonemic one. Such a situation can certainly occur, but cases of a complete transfer from one dialect to another and then back again can hardly be very common. If they were, we should expect far more relict forms than actually appear. It seems more likely that the successive stages are stages in time rather than place. A change spreads by fashion at the subphonemic level; no doubt such fashionable pronunciations will tend to be exaggerated; and this will go on until a new generation of speakers hears the allophones as two sounds rather than one. We cannot suppose that children have to wait for a minimal pair before they decide whether two sounds form two separate phonemes or not; what matters is whether they can interchange them freely. But children learning the language, unlike speakers of another dialect, who already have some ideas on what the language should sound like, have no preconceptions, and simply imitate the sounds in the distribution in which they have heard them, a distribution which depends on phonetic criteria. At the same time, however, the way is now open for analogical interchange which will disturb the phonetic pattern. It is in fact the possibility of disturbance which indicates that the change has reached the phonemic level; the two things go hand in hand. Frequently, it is the appearance of analogical forms which leads to the recognition that the sound-change has occurred; thus in Modern Southern English, *jam*, "preserve", with a long *a*, but *jam*, "blockage", with a short *a* (from *traffic-jam* and verbal forms?).¹⁴ It may even be the case that a distinction can be made in articulation which is inaudible to the hearers; e.g. in the final syllable of *candied* and *candid*¹⁵ – another aspect of the fact that there is no one-one correspondence between articulation and perception. Perhaps all cases of merger proceed in this way.

It may be objected that the distinction between subphonemic and phonemic change is of little use to the historical linguist, as he has no means of determining whether a prehistoric sound-change took place at one level or the other. But there is one case in which the distinction is of obvious importance, and that is when it comes to relating a sound-change which has taken place in historical times to the historical evidence. A sound-change may remain dormant at the subphonemic level, perhaps for centuries. I think one must accept as a cardinal principle that, except conceivably under the most unusual conditions, no phonetic feature finds written expression unless it is also a phonemic feature (and not necessarily even then, of course).¹⁶ Thus, the variation between /u/ and /ʌ/ in Modern English is noted by foreign observers from the late 16th C., but by native observers not till the middle of the 17th C. (probably as a result of the shortening in words like *could* and *look*, which now con-

¹⁴ Cf. Jones, *English Pronouncing Dictionary*⁹ (1948), p. 237; other minimal pairs could be produced: e.g. *band* and *banned*.

¹⁵ Thus Jones makes no distinction between the vowels.

¹⁶ The fact that a script does not represent all phonemic distinctions present in the language does not make it a phonetic script, of course. To be this it would have to represent phonetic features which did not correspond to phonemic distinctions. This point is curiously misunderstood by Kratz, *JEPG*, 59 (1960), p. 471.

trasted with *cud* and *luck*). It may happen that one of a series of related changes comes to be represented a considerable time before the rest, if it results in a merger with an already existing phoneme. Thus in the case of front-mutation in Germanic it seems most economical to assume that there was a tendency from PG times to use a forward articulation before *i*-sounds; but, as is well known, the change only comes to be represented as and when the sound-change becomes phonemic in the individual languages. However, the mutation of *e* is represented in all the Germanic languages, because it had merged with the existing phoneme *i* in PG; and the mutation of *a* before a single consonant is represented in OHG, and also in later Visigothic, because it had merged with an existing *e*, when front-mutation is otherwise not shown.¹⁷ It may even happen that a secondary sound-change appears before the change on which it depends. Thus, the development of *wi* to *wu* before *u* in the next syllable in Old English seems to be a further development of the back-mutation of *i* to a diphthong, but is recorded earlier. The preceding *w* brought about a further development to a monophthong, which merged with the existing phoneme *u*, and so is recorded.

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DISCUSSION

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A specific example to illustrate my point of view. In the Viennese dialect, standard *Wald, gern* become [vəjt, geʁn], that is to say, the phonemes *r* and *l* become semi-vowels. The factor decisive in this change was an auditive one, as the phonetic result of the change is rather close to its antecedent on the auditive level, much nearer than on the articulatory level. The principle of lesser effort is behind this change, but it worked through auditive, not articulatory similarity. What was irrelevant in this change was the arrangement of the phonemes in their respective patterns.

¹⁷ The mutation of *a* merged with restored *e* before *i* in OHG; see Marchand, *Word*, 12 (1956), p. 86. *E* could be restored in OHG because the front-mutation of PG *e* had become phonemic.

TYPES OF SOUND CHANGE

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Like many linguistic phenomena in the past, sound change has been classified and labeled by non-linguistic criteria. Among criteria employed are articulatory mechanisms, psychological forces and incidence. As with the Germanic consonant shift, changes are classified for modifications in position of articulation, or manner, and the like. Other changes, such as metathesis, dissimilation and haplology, are related to problems of innervation. Changes have also been labeled as sporadic, such as dissimilatory shifts in individual words, e.g. NE *turtle*, or as general, even natural, such as the comprehensive changes described in Grimm's law. With other linguistic method not based directly on language structure, these classifications are inadequate. They should be subordinated to classification established from observation of the types of changes as they are effected within phonological structures.

The mechanism of sound change was the primary concern of the nineteenth and early twentieth century. During this period of development of historical linguistics the chief concentration concerned events involved in the carrying out of shifts – the raising of vowels, their rounding, moves towards greater closeness or openness in the articulation of consonants. With such a concentration it is scarcely surprising that the emphasis was on individual events, that for example the Germanic consonant shift was viewed as a group of isolated changes rather than as a set of modifications. Possibly even more serious has been the concentration in our handbooks on isolated phonetic changes, generally those which affect a small segment of the language.

By contrast, for an accurate understanding of linguistic change and the development of individual languages, we need to note retentions and changes of structure within entire phonological systems. It may have been stated sufficiently often by now that the shift from the three member Proto-Indo-European obstruent system to a two member system in Baltic, Slavic and other dialects involved a greater alteration of the Indo-European obstruent system than did the modifications in Germanic which have been the subject of so much attention. It has not been stated sufficiently that the shift in the Proto-Indo-European resonants in all of the dialects involved a far greater modification of the Indo-European phonological system. The Proto-Indo-European allophonic variation of resonants which distinguished them from other sets of Proto-Indo-European phonemes was gradually eliminated in all dialects, though in

different ways, as is evident from the gradual loss of the consonantal allophones of /w/ and /y/ in Greek, their retention in Latin, the conservative treatment in Vedic, Germanic and so on. If one restricts his investigations to individual entities, such as Proto-Indo-European [w] or [i], the extent of the disruption of the resonant system may be concealed. It becomes clear, however, when one examines the changes within the framework of the various phonological systems. The moves towards clarification of the change of resonants in the Indo-European dialects, owing in large measure to Edgerton's capable linguistic analysis, and the increasing understanding of other problems in historical linguistics through a structural approach suggests that we can propose procedures used for analysis and classification of sound change in language which will give us new insights into its development.

An essential requirement is that we classify sound changes by their effect on the structural system of the language in which they take place. After such classification it is profitable to deal with the mechanism of sound change, the direction and finally to venture statements about the causes.

There are two fundamental types of sound change, change by allophone and change by phoneme. It is unnecessary to elaborate on them here, for I have done so in *Historical Linguistics: An Introduction* (New York, 1962). Change by allophone is that generally known as sound change in past statements on historical linguistics. To the present, few such changes have been thoroughly documented, though the shift of intervocalic *-t-* in American English is being extensively reported. Apart from other such changes now going on we must rely on descriptions we can produce on the basis of periods when we have copious literary sources, as for the final stages of the Great English Vowel Shift. Yet the course of a change by allophone is clear. When it takes place, similarly articulated phones in a phoneme are modified so thoroughly towards the phones of another phoneme in the language that eventually there is a merger. In American English, for example, *t* following vowels and resonants came to be articulated as a short, lenis stop when preceding vowels that do not carry a primary stress, e.g. in *deter*, or when an *n* preceded *t* or when it followed, as in *scanty*, *button*. Examples in which the shift has been carried out for some speakers are: *sorted*, homophonous with *sordid*, *bitter* with *bidder*, *bottom*, *bottle*, and so on. This superficial outline of the environments in which the shift is taking place is given to point up some of the characteristics of changes by allophones.

1. The phones involved are severely restricted to those with a common articulation, i.e. to specific allophones. We unfortunately have no description of the articulation of *t* in the neighborhood of *n* before the shift began, or in the other environments, but from our knowledge of articulatory habits we suggest that *t* in the neighborhood of *n* was pronounced differently from that in the environment of *r l m* etc., possibly like its articulation in *anti* with stop released at the velum. It is because of this restriction that we select the term change by allophone.

After we view general or natural changes in this way, the situation in past changes comes to be clarified. The changes described in Grimm's law, for example, were so

consistent because in the phonological structure of Proto-Germanic obstruents were chiefly located initially in syllables before vowels or resonants. When they were not, as in the etymon of *stand*, a shift did not take place. Many further examples could be cited. It is highly important to observe in all of them the clarification resulting when we examine changes in the context of phonological systems.

2. A second characteristic of change by allophones is the possible disruptive effect on neighboring entities. The prominent difference in duration between vowels before voiced and voiceless consonants in English, for example, is disturbed by merger of *t* and *d* as in *latter*, with a relatively short allophone of *æ*, and in *ladder*, with a relatively long allophone. If the two entities merge completely, the allophones of neighboring phonemes must be rearranged. In the course of this rearrangement other changes may take place, such as the production of inverse forms, e.g. ready [reɪy], or the modification of the allophonic structure of further phonemes. The production of inverse forms has a relatively small effect on the language. The modification of other phonemes may have a complex effect, of the type which has been labeled a chain effect.

There is some evidence, for example, that the other intervocalic voiceless stops of English may undergo similar modification. One may hear pronunciations of *package* riming with *baggage*. Inverse spellings give further evidence that for some speakers intervocalic *p* and *k* may be on the way to change similar to that of intervocalic *t*.

3. Since change by allophone is gradual, it takes place over a long period of time. Evidence can be cited from the Great English Vowel Shift, dated about the fifteenth century, for poets in the eighteenth century were still uncertain of the phonemes in certain words; cf. Johnson's uncertainty whether to rime *great* with *seat* or with *state*.

In contemporary English too there are differing treatments of intervocalic *t* from area to area. Nor can we predict as yet what the phonemic alignment of the intervocalic dental stop will be when *t* and *d* coalesce. The different dialects of a language may favor one variant, as Irish did the lower variant of Middle English \bar{e}^2 , and lead to differing reflexes of an earlier phoneme in the mainstream of the subsequent language. For the interpretation of such varying strands, we need all the flexibility that the wave theory and the findings of dialect geography can provide.

Change by phoneme resembles change by allophones essentially only in dissemination and establishment of a shift. In change by phoneme there is no modification of specific allophones towards allophones of another phoneme. A phoneme of one item is replaced by another phoneme, as in *turtle* < Lat. *turtur*. Nor are other phonemes affected. Individual phones of *turtur* probably were modified upon the dissimilation of the second *r*. But this modification had at most an effect on the frequency and arrangement of members of the two phonemes. While change by allophone affects the structure of a phonological system, change by phoneme affects the frequency of individual phonemes or more fundamentally the phonotactics of a language.

The classification of sound change proposed here is not only of interest in providing improved understanding of the phenomena involved in linguistic change. If as outlined above, the two types of change affect phonological systems variously, any instance of them gives us information about the phonological system of the language under investigation. So that historical linguistics may utilize this information more fully, after compiling the results of past sound changes, we must analyze their results within a broad group of phonological systems.

Initial moves to such analysis have been undertaken in the languages that have been most thoroughly studied. For example, umlaut in the Germanic dialects has been related to the fixing of initial stress accent, which circumscribed phonological entities and apparently led to similarity of articulation for stressed vowels within such an entity. Related to the same shift are: modification of the voiceless fricative phonemes; loss of final elements; the reduction of movement in diphthongs, so that for example by the time of late Old English all the Proto-Germanic diphthongs have become monophthongal. We can suggest these relationships between these changes but they have an air of unreality today, for we have recorded no similar shifts in similar phonological systems, nor have we recorded converse shifts in differing phonological systems with which we might support our suggestion for Germanic. We can however assert of our Germanic evidence that in it we have data within a phonological system which has been extensively described. Yet when we note the various analyses of the Old English phonological system, we are aware that even for the Germanic languages much must be done before we can generalize from their changes. Eventually, however, we will be able to use these events and data as nuclei for further such material which we will assemble from other phonological systems available to us over a space of a millennium or more.

Will we be able to deal similarly with changes by phoneme, with changes that have often been called sporadic in the past, such as metathesis, epenthesis, and the like? Although fewer attempts have been made to deal with these within their systems, there is every likelihood that they too will seem more regular – less sporadic – when we do so. There was considerable metathesis of consonants in Old English, especially late West Saxon, such as vowel plus *r*, as in *burna* “stream”, cf. German *Brunnen*, *first* “period of time”, cf. German *Frist*, *tux*, cf. *tusk*, *wlips*, cf. *lispig*. These took place when the restructuring of syllables under way already in the so-called West Germanic consonant lengthening, came to involve clusters, leading eventually to the situation in Modern English where clusters differing from those initial in syllables are permitted finally in syllables. Similarly, epenthesis of consonants in English and German seems to be not wholly unrelated to the delimitation of words by the initial stress (final juncture) fixing, for the epenthetic consonant involved most extensively is a voiceless stop, as in *parchment*, *cormorant*, *peasant*, added in a weakly accented syllable. The much more widespread and frequently recurring epenthesis in German, as in *Axt*, *selbst*, *jemand*, *Habicht* may reflect the continued bisyllabism of German in contrast to the basically monosyllabic structure of English.

We therefore suggest that any instance of either type of change reflects the structure of a phonological system and gives us information about it. Until we have more information on sound change in general it is important to use restraint in venturing explanations for specific sound changes. Premature ventures to account for specific changes within their phonological system may otherwise seem as unfounded as now do earlier attempts to relate sound change with cultural or geographical, even climatic, change. Language is too complex to assume that a modification in one social or geographical dialect, or in one segment of one of its levels, will pervade the entire system, or that any modification in these can be pinpointed in origin to one of its systems, unless we should have a detailed description of the origin and development of such a change.

Our interest in dealing more precisely with change in language virtually proposed a program for one segment of historical linguistics. We must investigate more fully the subtypes of changes that have been documented. Their mechanism, widely studied in the nineteenth century, must be dealt with in the context of phonological systems and can then be outlined with greater precision. Recently the mechanism of sound change has been intensively analyzed, especially by Hoenigswald, again primarily without reference to phonological systems. The direction of sound change greatly interested Sapir, and has received illuminating comment from Martinet; but we have scarcely begun to compile material from an adequate number of languages to speak with any sophistication on drift. Until we do, statements about hypothesized causes of specific changes scarcely seem more profitable than have those of the past which we hardly take seriously. It may be a recurring challenge in historical linguistics to undertake broad study of sound change, but now at least we have recognized that we must carry it on within a framework which promises some success for such study.

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DISCUSSION

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It seems that there is a certain relation between the "change by allophone" and the "change by phoneme". The phonemic *split* entailed by the former presupposes an identification of the *phonemic* factors conditioning the allophone. The split of I.-E. *k* into Sanskrit *k* and *č* (*ka:ča*) is a *consequence* of the phonemic merger of I.-E. *e* and *o* ($>a$). The "change by phoneme" ($e = o$) leads to the "change by allophone", *k'* (i.e. *k* before *e*) becoming *č*.

ON THE UTILITY OF AN OVERALL PATTERN IN HISTORICAL ENGLISH PHONOLOGY

ROBERT P. STOCKWELL

The overall pattern concept¹ has met with a variety of responses.² I will try first to clarify what its characteristics are, and then try to see whether it provides anything useful to the investigator who wishes to introduce a degree of reason and pattern into the story of how English pronunciation came to its present state.

The strongest form of the concept holds that there exists in a language an inventory of phonemes from which speakers select a subset in producing utterances. It asserts that mutual intelligibility between dialects results from an awareness of the system as a whole on the part of speakers who themselves utilize one subset but are capable of interacting with speakers who utilize another subset. The pattern is assumed to be implicit within the linguistic awareness of any speaker of the language, whether or not he himself in fact utilizes more than some indeterminate fraction of the system. This strong form of the concept is untenable, asserting that because there are dialects of English in which a centralizing glide in phonemic (*can* /kéɹn/ "container" vs. *can* /kén/ "be able"), then all dialects must have such a glide transcribed phonemically wherever it occurs phonetically, even if it is never contrastive (i.e. even if it is always predictable). It is useful to compare two dialects and to find, for example, that one of them *has* such a contrast and the other does not; to discover that in spite of this difference, and presumably others, speakers of the two dialects communicate with each other efficiently; to seek to explain *why* the difference in structure does not inhibit such communication. But the explanation is to be found in such facts as syntactic coincidence, lexical coincidence, contextual probability, and the practice that speakers have had in making appropriate adjustments. Of the many hypotheses that might explain why the gap is easy to bridge, surely the least verifiable is the speculation that the speakers share an awareness of the entire phonological pattern, in terms of which each can place the other's phonological habits appropriately.³

¹ G. L. Trager and H. L. Smith, Jr., *Outline of English Structure* (= *Occasional Papers No. 3 of Studies in Linguistics*) (1951).

² For reasonably full bibliographical notes, see my "Structural Dialectology: A Proposal" (*American Speech*, 34, 4, 1959, 258-68), or James Sledd's review of Trager and Smith in *Language*, 31.3 (1955), 312-45.

³ This is not to suggest that I disapprove of the notion that the grammar is a kind of Platonic ideal lying behind actual instances of speech, in terms of which slips of the tongue, grammatical deviation, reduced constructions, etc. can reasonably be explicated. On the contrary, I see no very satisfactory

There is a weaker form of the concept⁴ which does not make the mistake of treating the units in the pattern as indivisible particles (phonemes), from which inventory a selection is made in each dialect. The weaker form instead views the units of the pattern as convenient labels for intersecting categories of distinctive features. In this view, one aspect of the vowel system which is "overall" for English is merely a set of features – *high, mid, low* intersecting with *front, central, back* – assuming that no dialect of English has a vowel system that is adequately characterizable by fewer than these six features. The features may be labeled in other ways, as with terms which are applicable to the feature analysis of consonants also. The other aspect of the system which is "overall" for English is that three of the categories of the system of simple nuclei – *front, central, back* – are distinctive features among the glides also. One of the most debated properties of the system has been the insistence on three such glides, since it is demonstrable that only two of them are distinctive in numerous dialects. But there is no *need* to insist on this: some dialects exploit the set of six features more fully than others, and whereas *central* is distinctive among the simple nuclei of all dialects, it is not distinctive among the glides in all dialects.

In this weaker form, it may be argued that nothing has been gained; we must still examine an adequate sample of tokens from each dialect and demonstrate what the system of contrasts and their distribution is. It may turn out that all English dialects, shorn of phonetic differences and reduced to a set of minimal feature oppositions, in fact are not reducible below this particular set of features – even though some exploit the set more fully than others. But we have gained nothing in explicitness, which resides in the detailed sub-rules that describe the phonetic exponents of the feature bundles, and in the differentiation of the bundling habits of different dialects. We have not gained any power to describe the phonological system of English as a whole until we have seen the results of several linguistic atlases now in progress to determine whether the feature analysis has any general validity. Dialect geographers have attacked the system in its stronger form, but they have not considered it in its weaker, but testable and more general, form.

So it is not immediately clear that for purely synchronic purposes anything has been gained. But I should like to remain cautious: it is generally the case that an analysis which appeals to so many linguists has elements of truth, though they may await correct formalization.

For diachronic purposes a good deal more has been gained. For one thing, only

approach to the characterization of utterance tokens other than in terms of underlying abstract rules to which the tokens conform in varying degrees and manners. But this notion, and the overall pattern notion, though in some surface ways similar, are notions sharing nothing that is really distinctive. Abstract rules of the types found in a transformational grammar or a stratificational grammar are quite explicit about the structure of the sentences they characterize; an overall pattern of vowel phonemes is explicit only about potential contrasts – not about the system of contrasts that characterizes a set of utterance tokens from a single dialect.

⁴ Found in A. A. Hill's exposition of the Trager-Smith nine vowels (*Introduction to Linguistic Structures*, New York, 1958), and in my "Structural Dialectology: A Proposal" (*loc. cit.*).

sets of oppositions are recoverable from historical documentation; there is no way at all to test the physical realization of these oppositions in the speech of any individual; there is no way to defend historically the assertion, common in synchronic discussion, that John has a contrast that James lacks. Documents from one area may consistently distinguish two items that are not distinguished in documents from another area or another period. But there is very little chance, except with an Orm or a Wilkins, that such distinctions are assignable to a single individual's awareness of his own speech habits. A very considerable degree of generalization about what oppositions were consistently maintained over a wide range of documents is required. Furthermore, the internal symmetry of alternative formulations of these oppositions counts for vastly more than it does in synchronic description. And finally, descriptions of phonological change cannot rest merely on synchronic descriptions of periodic slices through the time continuum, since the synchronic descriptions of historical systems depend not only on documents (a description of graphic contrasts is not a phonological description but the starting point for one), but also on the internal consistency of the transitional rules that link one slice with the next.

I hold that the laws of sound change are the simplest set of general rules, plus exceptions, which with earlier forms as input yield later forms as output; the most insightful analysis of the structure of the most investigable state of the language – contemporary speech – is evidence of a high order for the structure of earlier states. This does not mean that the phonological structure of OE or ME was identical with that of MnE. A simple untenable form of this assumption was made by R. F. Weymouth in his long forgotten dispute with A. J. Ellis.⁵ He assumed there had been no change at all from OE vowels to MnE vowels, that the graph ⟨ii⟩ or ⟨i⟩ was as reasonable a way to write [ai] or [əi] then as now.

The assumption as I wish to state it is not that the vowels remained unchanged, but that the *system* did; I believe this assumption should be overruled by only the most convincing evidence of systemic change, as, e.g., between IE and Gmc. By the *system* I mean the set of features and their bundling potential; even more generally, I mean the *types* of features and the *types* of bundles. I would differentiate four types:

- (1) Vowel plus length (i.e. oppositions of short vs. long vowels): a/ā, e/ē, i/ī, etc. This system also requires a small number of diphthongs: ai/au/oi. [Jones, Kökeritz, Dobson, etc.]
- (2) Vowel plus vowel (where, to be justifiable, a significant variety of the potential clusters must be shown to occur): a/aa, e/ee, i/ii, a/ai, a/ae, e/ea, etc. [Sledd, Jakobson, Lamberts.]
- (3) Vowel plus glide (where only direction of glide is significant, with a range of variation as to the extent of the glide): a/ay, e/ey, i/iy, a/aɔ̄, i/iɔ̄, a/aw, u/uw, u/uɔ̄, etc. [Trager, Smith, Hill, Bloomfield, Sweet, etc.]

⁵ R. F. W., *On Early English Pronunciation* (London, 1874); A. J. E., *On Early English Pronunciation*, EETS 1867-1889.

- (4) Vowel quality (where such features as height, tenseness, and offglides are considered to be inherent in the qualitative distinction): i/I, e/ɛ, u/ʊ, etc. Like (1), this system requires additionally ai/au/ɔi. [Pike, Kurath, Fries, Kenyon, etc.]

Within each of these types, certain features are taken as distinctive and others as redundant. All of them agree, for English, that at least three grades on the vertical axis (height) are distinctive, and three grades on the horizontal axis. But no two agree as to what *other* features are distinctive. For (1), length is distinctive, but off-glides (except in ai/au/ɔi) and tenseness are redundant. For (2), nothing is distinctive but vertical-horizontal categories and clustering. For (3), off-glides are distinctive, but length, tenseness, and grades of height beyond three are redundant. For (4), at least five grades of height are distinctive, but length, off-glides (except in ai/au/ɔi), and tenseness are redundant. (1) is the least realistic in terms of the phonetic facts of MnE, since length need not occur acoustically in the instances where it would have to be transcribed as distinctive, and it does occur acoustically across a spectrum of conditioning environments. (2) is the most economical of symbols but it provides for far more contrasts than can be shown to occur, and at the same time requires drastic *ad hoc* adjustments to account for non-contrastive variation in the system (such as the variability of the extent of the glides in *bite*, *boat*, *bout*, etc.). (3) requires that two symbols (*y* and *w*) be utilized which are not required by (2), but it allows for variation in extent of glide, and it is just as economical in terms of the features required for its specification. (4) is, in my view, the most acceptable competition for (3), though it is the most wasteful of symbols and of features required for specification. Synchronically, I view (3) and (4) as about equally defensible in terms of dialect studies, psychological validity, pedagogical application, and so on. Diachronically, I think the case for (3) is much stronger.

In historical studies of English it has always been assumed that (1) is the strongest of the four types.⁶ If it could reasonably be demonstrated that (1) is best for MnE, then, since it certainly corresponds best, on the surface, with OE and ME spelling practices, I should be less inclined to question the traditional analysis of English historical phonology.⁷ But (1) is not considered the best analysis for MnE by most investigators.⁸ The assumption that it is best for OE and ME must therefore be put aside until it can be shown that there is no reasonable alternative to the conclusion that OE-ME were type (1) and that they changed, not only in details within the type,

⁶ I believe we can reject quite flatly, however, Sherman Kuhn's assertion ("On the Syllabic Phonemes of Old English", *Language*, 37.4, 522-38) that in OE "Length was clearly phonemic, as Reszkiewicz has demonstrated by means of minimal pairs" (528). What R. demonstrated was only that there was a contrast, of undetermined nature, between two types of vowels. The contrast may equally well have been of any one of the four types, or perhaps of some combination of two or more types.

⁷ There would still remain certain exceedingly perplexing aspects to the Great Vowel Shift, such as [i] → [ai], aspects that do not seem so perplexing within an analysis of type (3).

⁸ Key references basic to documentation are the following: Einar Haugen and W. F. Twaddell, "Facts and Phonemes", *Lang.*, 18.3 (1942), 228-37; R.M. S. Heffner, "Notes on the Length of Vowels", *Am. Sp.*, 12.2 (1937), 128-34, with continued study through six parts up to 1943.

but changed from type (1) to some other type altogether. We *must* seek alternatives to this conclusion, since it is inherently more complex than an analysis which shows changes within a system, but no change of systemic type.

(2) is the type that has been proposed for MnE by James Sledd,⁹ and for the history of English phonology in a still unpublished account of the great vowel shift by J. J. Lamberts. Since Lamberts' work is not generally accessible, I cannot properly comment on it here beyond observing that on certain essential points he and I are evidently in agreement, though having arrived at them independently.¹⁰ The only advantage of (2) over (3) is that it provides for an enormously larger inventory of potential contrasts, at the cost of complicating allophonic statements and of having no dialect whatever realize more than a tiny fraction of the potential. It is less a systemic analysis than a grid for transcription of data to be systematized.

(4), while very popular as an analysis of MnE, has been proposed by no one that I know of as the basic type from OE down. Whatever the "long/short" contrasts of earlier English were, evidently no one thinks they were distinctively qualitative.

What, then, are the strengths of (3) as a type of phonological structure within which not only is MnE reasonably describable but also as a type within which the diachronic changes may be simply accounted for?

(a) In the OE spelling *-ig* (in *bodig*, *stig*, etc.¹¹), *ġ* is generally assumed to have represented a voiced palatal fricative, roughly a front-gliding [i] plus friction. In ME *-ig* falls in with the reflexes of inherited *ī*. If *ī* by this time represented /iy/, the collapse of *ī/ig* is fully accounted for merely by loss of friction in *-ġ*. In this way it appears to be precisely parallel with such developments as *hægl* → *hail*, *dæg* → *day*, *weg* → *way*, etc., where the assumption that *-ġ* → /-y/ is fully borne out by ME spelling and MnE pronunciation.

(b) In the OE spelling *-ug* (in *fugol*, *sugu*, *bugan*, etc.), *g* is generally assumed to have represented a voiced velar fricative with strong lip rounding (by assimilation to the preceding rounded vowel). In ME *-ug* falls in with the reflexes of inherited *ū*. If *ū* by this time represented /uw/, then the collapse of *ū/-ug* is fully accounted for merely by loss of friction and velarity – the voiced lip rounding components remaining as /-w/. The /-w/ which is present in *fowl*, *sow*, etc. now may thus be traced in a direct line to *-g*, without the complication of *-ug* → *ū* → /aw/, which posits total assimilation of *-g* to *ū* and then generation of a new /-w/ subsequently.

(c) The assumption that one of the three types of "long" vowel was V + /ə/ (or simply /ə/, or even /h/) makes at least reasonable, an account of the phenomenon known as lengthening in the 10th century before certain clusters (liquids or nasals plus homorganic voiced consonants, shown consistently in Orrm, but more generally

⁹ *A Short Introduction to English Grammar* (Chicago, 1959).

¹⁰ Particularly we agree on setting up, instead of length, three kinds of "length" (i.e. three kinds of non-simple nuclei), which he writes /-i/, /-i/, /-u/, which I have elsewhere written /-y/, /-h/, and /-w/. We agree on positing stages /iy/ and /iw/ as the first steps in the changes *ī* → [ai] and *ū* → [au].

¹¹ I use the dot over the *g*, as is the practice of modern editors, to distinguish the palatal fricative from the velar.

restricted to the groups *-ild*, *-eld*, *-uld*, *-old*, *-ald*, *-ind*, *-und*, *-imb*, and *-omb*). The mid-central resonance of /l/ in English is well-known. I know of no contemporary instances where a simple vowel falls in with a complex nucleus under this influence, probably because MnE complex nuclei are predominantly out-gliding, whereas those of OE were predominantly in-gliding. Instances of analogous phenomena in MnE include the influence of velar stops in Southeast Midland American where e/ey have coalesced in *leg*, *egg*, *beg*, etc., or of palatal fricatives in Southwest Midland American where ə/əy have coalesced in *wash*, u/uy in *push*, *bush*.

(d) The curious relation of ME \bar{e} to \bar{i} , and of \bar{o} to \bar{u} , such that 13th century "lengthening" of \bar{i} yields \bar{e} , \bar{u} yields \bar{o} , is readily accounted for without special *ad hoc* sound laws. The change OE → ME includes the series given below. Since I have presented elsewhere¹² the argument for this interpretation, I will omit further details of it here.

	OE		ME		OE		ME		
⟨ig⟩	iġ	→	iy	⟨ī⟩	⟨ug⟩	ug	→	uw	⟨ū⟩, ⟨ou⟩
⟨ī⟩	ī	↗	ī	⟨ē⟩	⟨ū̄⟩	ū	↗	ū	⟨ō⟩
⟨ē⟩	ē	↗	ē	⟨ē̄⟩	⟨ō̄⟩	ō	↗	ō	⟨ō̄⟩
⟨ēa⟩, ⟨ā̄₂⟩	ǣ	↗			⟨ā̄⟩	ǣ̄	↗		

(e) The "first step" in the Great Vowel Shift, which has been the subject of argument through hundreds of pages of the journals since the time of Ellis, becomes reasonably clear – at least as to *what* it must have been, if not why. Leaving the question of dating aside for the moment, we ask where ME \bar{i} /iy/ might "go", so to speak, without intersecting and coalescing with other units with which it was in opposition.¹³ For the vocalic element to lower, yielding /ey/, was impossible without coalescence (which did not occur) with the existing /ey/ from a variety of sources, including Norse loans (*they*, etc.). For the glide to become centralizing or backing rather than fronting, yielding /ī/ or /iw/, would result in coalescence with the existing /ī/ (long close \bar{e}) or /iw/ (from such sources as OE $\bar{i}ew$), and would be unlikely in any case since the glide is still /-y/. The only possible change that would maintain the oppositions already in existence was for the vocalic element to centralize, yielding /iy/, subsequently /əy/ and finally /ay/. The widespread present-day alternation between /iy/ and iy/ (Cockney, Philadelphia, etc.) gives dialectal support to this suggestion. Support for the parallel series /uw/ → /iw/ → /əw/ → /aw/ is even stronger: all four stages still exist, in the same words, in present-day dialects (Scots /huws/; Tidewater Virginia /hiws/; Piedmont Virginia and Eastern Canada /həws/; /haws/ ~ /hæws/ elsewhere).

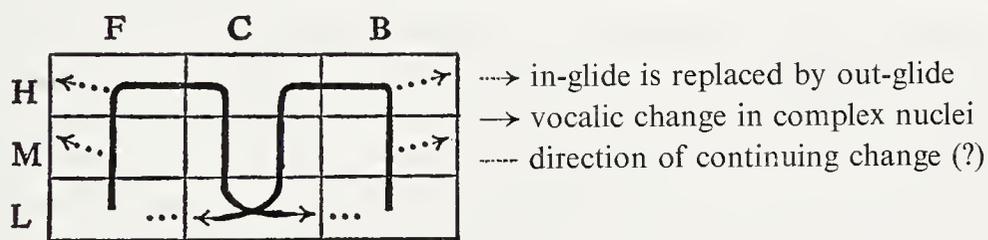
(f) The OE features distinctive in the vowel system were *high*, *mid*, *low* and *front*, *round*, and *back*. Loss of the front rounded vowels (which were, in any case, struc-

¹² "The Middle English 'Long Close' and 'Long Open' Mid Vowels", *U. of Texas St. in Lit. and Lang.*, 2 (1961), 529-38.

¹³ I take \bar{i} simply for exemplification; \bar{u} would do as well, since it is completely parallel.

turally *central* as between front and back) left a gap which was filled by the splitting off of the central allophones of high and mid front vowels (i.e. the OE system /i e æ ü ö u o ə/ had become /i e æ i ə u o/ by ME). In some dialects (Southwestern), the front round vowels fell in with the new central vowels rather than with front unround. The assumption of a high central vowel is more defensible in late ME than is the assumption of a high front rounded vowel at that time.

(g) Finally, the question of chronology. By some investigators the GVS has been assigned an almost cataclysmic timing (roughly 1400-1500), and a more generous spread by others (roughly 1350-1650). It is my contention that the series of changes of which the GVS is a part have been going on at a remarkably steady rate for more than 1500 years, and that the GVS part of that series was in no way more abrupt or sudden than the rest of the changes – which, one can demonstrate, are still going on in the same pattern. The pattern consists in two main types of change: (1) raising of the vocalic element in complex nuclei toward the extremes, and centralizing followed by lowering of the extremes; (2) alternation between out-glides and in-glides. Thus:



Limitations of space do not permit me to fill in the details of this chart, nor to list the pre-GVS and post-GVS changes that belong in the same series.¹⁴ I have tried only to show that the notion “overall pattern” is a useful and stimulating one in diachronic phonology.

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DISCUSSION

ROBSON:

1. Overall pattern has long been taken into account in linguistic reconstruction, in the attempt to attribute substantial phonetic values to the asterisked allophone categories set up by comparison: “substantial identifications have ultimately to be made *via* the individual languages, with some selection where they are not in agreement”, W. S. Allen, *TPS*, 1953, 82. The selection may involve favoritism, and depends in any case on the range of investigable languages available to the historical grammarian. Thus, the seven-vowel system postulated for VL and ME (four grades of height with front-back contrast for all but the lowest) appears to be extrapolated from Mo-

¹⁴ I hope to complete a monograph, *Structural History of English Phonology*, giving such details, within the near future.

dern Italian and Modern Dutch (on the low degree of stability of the system in the former cf. Malmberg, above, p. 80).

2. This method was often coupled with a monolithic *simplicity* as regards the phonology of synchronic stages, which did not allow for co-existent phonologies within a single koinè language or under the surface of a single set of graphic and grammatical conventions, and an undue *complexity* in the postulation of successive systemic changes.

3. That the Great Vowel Shift is best regarded as part and parcel of long-term changes between two major linguistic stages, OE and ME, is a conclusion suggested to me independently by Romance historic phonology. In each case we have two successive stages and a postulated intermediary:

	<i>a</i>	<i>b</i>	<i>c</i>
	CL	*VL	OF
	OE	*ME	MnE
systems	(1)	(4)	(3)

Here system (1), length distinction with a small number of diphthongs, is attributed to CL on the unanimous evidence of ancient tradition; type (3) is a favorite analysis of MnE dialects and is a *prima facie* interpretation of classical OF spelling (glides represented by *-i*, *-e* and *-u*, alternating with zero, e.g., *ui/u*, *ie/i*, *ou/o*). While I hesitate, on account of its superior graphic tradition, to place an asterisk against OE, all systems of ME phonology must be regarded as purely hypothetical.

4. Stockwell and I appear to be in agreement in tending to eliminate the intermediary stages in our respective fields; for I hold that OF diphthongs can be explained in neo-Diezian terms as reflexes of the free-blocked allophonic contrast and of cl. length, without reference to the *VL stage extrapolated from modern Italian, see *TPS*, 1955, 172–180. On the traditional hypothesis CL diphthongs and length contrasts merged into a system of simple vowels distinguished by quality only, and diphthongs re-appeared from “secondary length” in the VL stage; explanations of the GVS seem to proceed from related concepts current in the 1870’s and ’80’s. It is possible however to suppose that when CL *oe* merges with *ē* both the diphthongal and the non-diphthongal interpretation of the result are valid from the outset, the simple vowel being preserved in Italian *pena* and the diphthong in dial. OF *poine* (cf. Stockwell on the merger of OE long vowels with *-ig* and *-ug*).

5. Here again one can envisage a ‘strong’ version (“stage *a* is already best interpreted in terms of system (3)”) and a ‘weak’ version of the doctrine (“stages *a* and *c* are in direct contact, with both systems (1) and (3) interpretatively valid from an early stage”).

To defend even the latter will require an examination of the credentials of *ME as excogitated by Sweet, Sievers, Luick and others, and a study of the geographic distribution of overall patterns in NW Europe today with a view to a probable reconstruction of their distribution in medieval times.

LEHMANN:

Commending Professor Stockwell for his lack of dogmatism in presenting various analyses of the Old English vowel system I would like to point out that the balancing of vowels in the late Middle English period favors the assumption of vowel plus length. In closed syllables, for example, OE *cēpte*, NE *kept*, long vowels were shortened; in open syllables, for example, OE *stelan*, NE *steal*, short vowels were lengthened. Consideration of the entire phonological system may therefore indicate the make-up of some of its members better than does examination of sound changes which they have undergone.

SUBCATEGORIES IN TRANSFORMATIONAL GRAMMARS

EMMON BACH

1. INTRODUCTORY

Transformational theory has offered a general theory of linguistic structure in the form of descriptions of the various components that seem to be needed in a “grammar”, that is, a deductive theory which will enumerate an infinite set of strings in a finite alphabet and predict that the computed strings (when transformed into sounds) will be sentences of the language being described. It is to be expected that modifications in the general theory will arise both for theoretical reasons and also when the general theory is tried out in the detailed description of a wide variety of languages. The present paper is concerned primarily with a modification not in the general theory but in the particular way in which it has been applied and secondarily with two alternative modifications in the general theory which suggest themselves as a result of the proposed change in the mechanics of application.¹

Three main parts to a grammar have been proposed. The first – called variously the phrase-structure, constituent-structure, or IC-structure component (hereinafter “PS”) – consists of a set of initial strings, for instance “#S#”, and an ordered set of “rewrite rules” which are *simple* (that is, they apply to a given string by virtue of its shape alone) and *one-many* (they replace a single element by one or more elements). Because of the latter restriction it is possible always to construct a “derivational tree” to represent the structure of the derived strings. In addition, no PS element is derived from the identity element (except perhaps the initial or primitive strings), and no PS rule maps an element into the identity (that is, there are no deletions in the PS).

The second and third components are of less concern to us here. Transformational (T) rules are *complex* (that is, they convert strings with a given structure into new strings with a derived structure). They may be *one-many*, *many-one*, or *many-many* (in the above sense). They may perform deletions and permutations. Phonological (P) rules are again simple but otherwise unrestricted.

The lexicon of a transformational grammar has no special status, being merely the

¹ For a succinct statement of transformational theory and references to literature on theory and application see Noam Chomsky, “On the Notion ‘Rule of Grammar’”, *Structure of Language and its Mathematical Aspects, Proceedings of Symposia in Applied Mathematics*, XII (1961), 6-24.

final rules of the PS, in which symbols for various classes of lexical items are converted by long disjunctive rules into particular representatives of the classes. Morris Halle has introduced a further breakdown of the phonological rules into two sorts: (1) the "morpheme-structure" rules, which follow the lexical rules, take care of what is traditionally called phonotactics, and introduce into the lexical representations those redundant features which must be specified before morphological transformations choose the proper allomorphs for inflections; (2) the phonological rules proper which apply after the developing strings have left the transformational component.²

Thus, with the modifications proposed by Halle, a transformational grammar has the following structure: (1) a set of initial strings, (2) the PS rules, (3) the morpheme-structure rules, (4) the T rules, (5) the P rules. Before turning to the problem with which this paper is concerned, let us consider two questions that arise in this conception of a grammatical theory.

First, many morphemes enter into the developing strings only as the result of optional transformations. For instance, conjunctions such as *and* are introduced by generalized (two-string) transformations. In the very limited published examples of transformational grammars such rules introduce one and only one example of the class concerned. In a fuller grammar it is possible that such rules will introduce not the actual constant (say, *and*) but rather a class-symbol (say, *Conj*), which would be replaced in a later rule by any of a number of members of the given class (say, *and, or, as well as. . .*, or for a different class of clause connectives *because, since, although . . .*). But such rules would be exactly like the lexical rules which appear at the end of the PS. Moreover, the conjunctions (relative pronouns and so on) which are introduced in the T rules must obey precisely the same rules of morpheme-structure (phonotactics) which have been applied *before* the developing strings entered the T component.

Second, the arguments from simplicity which have been advanced both for particular solutions to problems and for general formulations (for example, the distinctive-feature version of phonological theory) have been based on what might be called "simplicity of rules", that is the number of symbols necessary for stating a set of rules. It seems to me that there is another important feature, which might be called "simplicity of derivation" and which has to do with the total number of symbols necessary to carry out a complete derivation. It is this consideration which leads us to postpone as long as possible (other things being equal) the development of any given element into two or more elements. I shall return to these two general considerations below.

2. THE PROBLEM

PS rules move from most-inclusive constructions (such as *S, NP, VP*) down to least-inclusive ones (i.e., one-member constructions: *Noun, be +Ing*), and from most-inclusive class-symbols (*Verb, Noun, T*) to least-inclusive ones (i.e. one member

² Morris Halle, *The Sound Pattern of Russian* (The Hague, 1959).

classes: *run, John, the*). In breaking down a general class-symbol like *Verb* a disjunctive rule allows the replacement of the symbol by one of several subcategories (e.g., *Verb*₁, *Verb*₂, *Verb*₃). Each of the latter may be further subdivided into smaller categories and so on. Because the PS rules are simple (in the sense given above) it is now no longer possible to state by a non-transformational rule anything which might apply to all the subcategories (e.g. to *Verb* in general) except by dealing with each one in turn (or by collapsing several rules into one with square brackets). Sometimes a reordering of rules (to allow the parallel development to take place before the breakdown of the general category) is possible, however, such a reordering often brings about the same problem in some other category. This difficulty arises when the criteria for making subclassifications do not coincide, some having to do with transformational developments, some with various co-occurrence relations (including government) and so on.³ The problem is not limited to the treatment of base-base co-occurrence, as the examples below will show.

Let us consider the following situation in the syntax of the German verb-phrase. German verbs must be subdivided according to at least the following characteristics: (1) Capacity to undergo the passive transformation (denoted below by "t", lack of this capacity by "i"). (2) Presence of nominal "objects": either none, one ("1"), or two ("2"). (3) Case of objects when present (or necessity for a particular preposition). (4) Choice of "haben" or "sein" as the perfect auxiliary. (5) All the other subclassifications according to the co-occurrence relations of verbs and nouns and so on (for instance, the restrictions on the subject of impersonal verbs like *geschehen, gelingen*).

In the following rules (ignoring 4 and 5 and considering only verbs governing dative –"d"– and accusative –"a"– objects for purposes of clarity) we analyse the verb-phrase according to the first three criteria in the order given.

$$\begin{array}{l}
 \text{Rule 1.} \quad \text{VP} \rightarrow \left[\begin{array}{l} \text{VP}^t \\ \text{VP}^i \end{array} \right] \\
 \\
 \text{Rule 2.} \quad \left[\begin{array}{l} \text{VP}^t \\ \text{VP}^i \end{array} \right] \rightarrow \left[\begin{array}{l} \left\{ \begin{array}{l} \text{Verb}^t \\ \text{NP} + \text{Verb}^t_1 \\ \text{NP} + \text{NP} + \text{Verb}^t_2 \end{array} \right\} \\ \left\{ \begin{array}{l} \text{Verb}^i \\ \text{NP} + \text{Verb}^i_1 \\ \text{NP} + \text{NP} + \text{Verb}^i_2 \end{array} \right\} \end{array} \right] \\
 \\
 \text{Rule 3.} \quad \left[\begin{array}{l} \text{Verb}^t_1 \\ \text{Verb}^i_1 \end{array} \right] \rightarrow \left[\begin{array}{l} \left\{ \begin{array}{l} \text{Verb}^{ta} \\ \text{Verb}^{td} \\ \text{Verb}^{ia} \\ \text{Verb}^{id} \end{array} \right\} \end{array} \right]
 \end{array}$$

³ Cf. Robert B. Lees, *The Grammar of English Nominalizations* (Bloomington, 1960), pp. 37 f., 48. See now also Paul Schachter's review of Lees' monograph, *IJAL*, XXVIII (1962), 134-146.

$$\text{Rule 4. } \begin{bmatrix} \text{Verb}^t_2 \\ \text{Verb}^l_2 \end{bmatrix} \rightarrow \begin{bmatrix} \left\{ \begin{array}{l} \text{Verb}^{taa} \\ \text{Verb}^{tda} \end{array} \right\} \\ \left\{ \begin{array}{l} \text{Verb}^{laa} \\ \text{Verb}^{lda} \end{array} \right\} \end{bmatrix}$$

Observe that rules 2, 3, and 4 produce exactly parallel changes in the two different types of verb-phrases, and that if one began with a breakdown according to the second characteristic, rules of the type of 2, 3, and 4 would still have to be used, again producing exactly parallel changes in several different subtypes.

3. THE PROPOSAL

A simple change will allow us to avoid the inelegant alternatives. Rather than producing subcategories by adding sub- or superscripts to class symbols, we can achieve exactly the same results by making these "diacritic" marks separately concatenated elements in the developing string. The advantage of this notation may be seen by the following rule for the breakdown of German verb-phrase types given above

$$\text{VP} \rightarrow \left\{ \begin{array}{l} \text{Verb (t)} \\ \text{NP} + \text{Verb (t)} \begin{Bmatrix} a \\ d \end{Bmatrix} \\ \text{NP} + \text{NP} + \text{Verb (t)} \begin{Bmatrix} d \\ a \end{Bmatrix} + \text{;} \end{array} \right\}$$

This rule abbreviates 10 minimal rules as compared to 16 above and uses 15 symbols as compared to 29. Simplicity of derivation, on the other hand, is considerably decreased since we must now carry along several symbols in the derivation where before there was one. There is thus good reason to restrict this sort of bifurcation to instances where fairly complex cross-classification is necessary. More important than the reduction in symbols and rules, perhaps, is the fact that we can still refer both in PS and T rules to the general class *Verb*.

One more example may be given, this time from English grammar (although the details have been worked out for a similar situation in German). From R. B. Lees' classification of nouns⁴ it is possible to abstract at least the following categories: *animate* (AN), necessary for verbal co-occurrence and for choice (by transformation) of the proper relative pronouns; *tangible* (TAN), necessary for verbal co-occurrence; *individual* (IND), necessary for restrictions on the indefinite article and plural formative – i.e. individual nouns cannot occur with *a/an* or in the plural. (Further categories for some of Lees' distinctions, such as that between factive and action nouns, are not considered here.) We may make each of these categories an element (leaving the opposite categories – inanimate, intangible, class or count – unmarked) and illustrate by the following (semi-facetious) chart:

⁴ *Op. cit.*, also a multilithed revision of his rules dated March 14, 1961, and distributed by Robert P. Stockwell in a seminar at the 1961 Linguistic Institute in Austin, Texas.

Example:	Mary	milk	Jehovah	physics	boy	table	angel	reason
IND	+	+	+	+	—	—	—	—
TAN	+	+	—	—	+	+	—	—
AN	+	—	+	—	+	—	+	—

The full possibilities for a noun phrase can be given by the rule:

$$NP \rightarrow T + \text{Noun} (AN) (TAN) \left(\begin{array}{l} \{IND\} \\ \{P1\} \end{array} \right)$$

(T_1 must subsequently be changed to \emptyset if either *IND* or *Pl* – plural – is chosen.) Such a rule would have to follow any special restrictions on types of nouns in the environment of various verb classes. For instance, in the sorts of environments that would lead eventually to “That surprised___”, “___believed it”, “I told___so” *NP* would become *T + Noun + AN* with or without the other possibilities, allowing *Mary, Jehovah, the boy(s), the angel(s)* but no others. Or a rule like this one might choose the proper (tangible) nouns for an environment like “___ felt cold (to the touch)” allowing *Mary, the milk, the boy(s), the table(s)* but not *Jehovah, physics* etc.:

$$NP \rightarrow T + \text{Noun} (AN) + TAN \left(\begin{array}{l} \{IND\} \\ \{P1\} \end{array} \right)$$

Symbols like *t*, *AN*, *TAN* represent categories and are thus less directly connected to the ultimate physical parts of sentences than are the usual symbols of transformational grammars. (We can interpret a string like *Noun + AN + TAN* as standing for the intersection of the two classes of animate and tangible nouns.) Nevertheless, as introduced here the category symbols do not transcend the limits of ordinary PS rules. However, the lexical rules must now be carefully ordered and are largely of the context-restricted type. For example, lexical replacements for the symbol *Verb* following the rules above for German must be of the following sort:

$$\text{Verb} \rightarrow \text{geb, zeig,} \dots \text{ in the environment } ___ +t+d+a$$

And this rule must precede the replacement of *Verb* by *dank, folg*, etc. in the environment “___ +t+d”. Similarly, the choice of *John, Mary, or Bill* for *Noun* in the environment “___ +AN+TAN+IND” must take place before the replacement of *Noun* (with no category symbols) by *reason, fact*, etc. We are left, then, with strings containing category symbols which must be deleted at some point after the lexical rules.⁵

The advantages of a technique like the one suggested here for nouns are especially great in a grammar of German, where at least as many subcategories as those given above must be used, but where it is in addition necessary to classify according to

⁵ The use of context-restricted lexical rules is not without precedent, since even the published versions of rules for English include rules that must be interpreted in this way. For instance, the lists of prepositional verbs like “flirt (with), float (on), object (to)” are a shorthand for rules in which the determining context is the (already chosen) preposition.

gender, a classification which has nothing whatsoever to do with the other categories. With an analysis like the above we can simply add another rule changing *Noun* into the three gender types. With subscripts or superscripts it would be necessary to produce this same change independently for each of the (eight or more) types of nouns.

4. IMPLICATIONS FOR THE GENERAL THEORY

Two possible modifications in the structure of transformational grammars suggest themselves.

(a) *Change in the restrictions on PS rules.* One may ask why is it necessary to keep the category symbols beyond the lexical rules only to have them subsequently deleted. Perhaps the proper restriction on PS rules should be that they either change one element to one or more elements or one or more to a single element, prohibiting only those which would replace several elements by several different ones. It is only if neither side of a PS rule is restricted to a single element that derivations appear for which it is impossible to construct a unique tree, and in which it is hence impossible to analyse strings unambiguously for T rules. Under such a modification, the lexical rules would be just those at the end of the PS in which several elements (class symbols plus category symbols) are reduced to single lexical items.

(b) *Change in the placement of lexicon.* One could avoid the above modification (which may be theoretically unsound⁶) and still avoid the awkward category deletion rules by moving the lexical rules (many-one rules) into the transformational component. There is a natural place for them, namely, just before the internal morphological transformations. This modification seems to me the more attractive and has several desirable consequences. To do this would mean, of course, that all T rules except the obligatory morphological ones would not apply to strings of lexical items, but to the next more abstract strings, that is, the strings (terminal with respect to the PS) consisting of the most detailed subcategories provided for by the grammar (together with their derivations or derivational trees). An optional transformation would then state a relation between detailed sentence types and only indirectly between particular sentences.

Three advantages would accrue. First, the more abstract statement of relationships would avoid the present necessity of introducing various items into derivations only to have them subsequently deleted or replaced. (One feels very uncomfortable about deciding just what particular noun is replaced by the italicized phrase to make a sentence like "*That she was pretty was obvious*".) Second, one would resolve the difficulty or inelegance mentioned above (end of section 1) which seems to arise because

⁶ Cf. Noam Chomsky, "On Certain Formal Properties of Grammars", *Information and Control*, II (1959), Theorem 3 and subsequent discussion on pp. 143 f.

certain parts of the lexicon are introduced only by transformations and hence escape (except by some *ad hoc* specification) the operation of morpheme-structure rules. Finally, what I have termed "simplicity of derivation" may be considerably increased just because we can wait to introduce parts of the finally produced strings until just before they are needed, that is, just before the part of the grammar which must deal with the morphology of lexical items. This last argument becomes especially compelling when one attempts to deal seriously with a fairly complicated morphology. For we must obviously carry along (or specify somehow) the information necessary for any non-automatic morphological alternations in order to obtain the proper final shapes in our derived strings (for instance, the information to change English /rɪŋ/ plus *Past* into one of the three shapes /rɪŋd/, /ræŋ/, /rəŋ/, i.e. *ringed*, *rang*, or *wrung*).

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DISCUSSION

HOUSEHOLDER :

Both of Mr. Bach's proposed changes in the structure of transformational grammar are sound, and have been used for some years in grammars written by my students. Similarly, his proposal to treat subscripts as elements has been used, e.g., by A. K. Ramanujan in his Indiana thesis on the structure of Kannada.

STUDIES IN THE CORRESPONDENCE OF PROSODIC TO GRAMMATICAL FEATURES IN ENGLISH

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J. P. L. RUSIECKI, A. J. T. COLIN

1. As K. L. Pike has said, "In each language . . . the use of pitch fluctuation tends to become semi-standardized, or formalized, so that all speakers of the language use basic pitch sequences in similar ways under similar circumstances."¹ The present paper reports an experiment in the use of a computer to correlate data on intonation and grammar, conducted within the framework of the Survey of English Usage. The limited aims of the experiment have been to investigate the extent of relationship between types of nucleus and types of grammatical unit; the grammatical relevance of the elements bearing nuclear tones; the grammatical junctions that occur between units of intonation; and the kind and degree of correspondence between units of intonation and units of grammar.²

2. The material used in the experiment consisted of two texts, each of 5000 words, of continuous unscripted speech, transcribed from recordings of panel discussions. There were four speakers and a chairman in each discussion, making ten different speakers in the material as a whole; they were all university-educated men of middle age or over, well accustomed to speaking in public. In each discussion, the panel was asked to comment on questions of general interest; speakers in Text 1 were broadcasting from a hall in the presence of an audience, those in Text 2 were televised in a studio.

3. The textual samples given below illustrate how the material was transcribed and reproduced on the 6"×4" slips on which all the Survey texts are filed. In general, orthography is used, but incomplete words are given in phonetic notation in square brackets, whereas the sounds of voiced hesitation are indicated only approximately by the use of ə, ə:, əm, without square brackets. Each slip has a number in the top left-hand corner: the first symbol indicates the type of material, the second is the number of the text within this category, and the third is the number of the slip.

¹ *The Intonation of American English* (Ann Arbor, 1945), § 3.1.

² Space forbids a consideration here of previous work in this field – for example, D. L. Bolinger, "Intonation and Grammar", *Language Learning*, 8, 31-7; "Contrastive Accent and Contrastive Stress", *Language*, 37, 83-96; F. Daneš, "Sentence Intonation from a Functional Point of View", *Word*, 16, 34-54.

Capital letters in the left-hand margin identify the speakers; continuation of speech from a previous slip or after interruption by another speaker is shown by enclosing the speech heading in round brackets. The two consecutive slips 5b.1.21 and 5b.1.22 show how an overlap of four lines is given on each slip in order to provide adequate linguistic context for each feature analysed: the starred lines at the top and the bottom of each slip are not treated as part of the material on that slip, but are analysed when they appear without stars on neighbouring slips. Interruptions of one speaker by another, and passages where two or more people are speaking at once, are marked by paired asterisks and crosses, as on slip 5b.2.56.

4. The system of discriminations in terms of which we make the present report has been formulated under the pressure of specific practical needs. We have aimed at a system which could be applied with reasonably high speed to large bodies of material and in which the distinctions were sufficiently broad to be heard with a high level of agreement by the team engaged in analysis. Since we wished to investigate the relation of grammatical categories to categories of intonation, we had to avoid using grammatical criteria in setting up our intonation units. We favoured a set of discriminations based on phonetic substance, which suggest their phonological status chiefly by promoting least disagreement in their recognition.

In common with most of our colleagues in this field, we find ourselves unable to use instrumental assistance to any more than a minor extent, and our work is based on auditory analysis. Play-back at half speed has, however, proved valuable, and of course a loop repeater has been quite indispensable.

5. It may be convenient if we state our intonation categories with some reference to those set up by K. L. Pike³ in what is probably the best-known treatment of English intonation. Our basis is the TONE UNIT (cf. Pike's "Total Contour", 3.5.3), marked as the stretches between the sign "#" (cf. Pike's "Ending Point", 3.5.2). Minimally, the tone unit comprises a NUCLEAR SEGMENT⁴ containing the NUCLEAR TONE (cf. Pike's "Primary Contour", 3.5.2), but may have:

- a) a PRE-ONSET, terminating at the ONSET, marked "/" (cf. Pike's "Beginning Point", 3.5.2); b) one or more PRE-NUCLEAR SEGMENTS, in addition to c) the NUCLEAR SEGMENT.

³ *Op. cit.*

⁴ Our *segment* is by no means the same as that of W. Jassem, *Intonation of Conversational English* (Wrocław, 1952). Although like him we work in terms of pitch-direction instead of pitch-level, there is less obvious correspondence between our units and his than between ours and Pike's. To some extent, our *tone unit* resembles Jassem's "tone group" and our *nuclear tone* his "tone unit"; in some cases our *nuclear segment* corresponds to his "nuclear tune", but the latter term has apparently a much wider reference than the former: cf. "I suppose", observed Lord Mansfield', which has for Jassem (p. 59) two nuclear tunes but for us only one nuclear tone. We obviously cannot attempt here to correlate our system with that of other linguists, for example G. F. Arnold and J. D. O'Connor, R. Kingdon, W. R. Lee, M. Schubiger, nor can we consider important contributions to intonation theory by M. A. K. Halliday, L. S. Hultzén, A. E. Sharp, R. P. Stockwell, and others.

A segment consists of at least one stressed syllable (which bears some correspondence to Pike's "Key Point", 3.5.3). After the first segment, which begins with the onset, a new segment begins where a stressed syllable is uttered on a higher pitch than the preceding syllable; this point is called a BOOSTER and is marked ":". ⁵ When such a syllable is uttered on a higher pitch than the first syllable of the preceding segment, this point is called a HIGH BOOSTER and is marked "!". Either type of booster may also obtain at the beginning of a nuclear tone. The basic nuclear tones are as follows: the simple fall (˘), rise (ˊ), fall-rise (˘ˊ), rise-fall (ˊ˘), and level (ˉ); and the compound fall-plus-rise (˘+ˊ), and rise-plus-fall (ˊ+˘). Tone units with simple nuclei are here called "mononuclear", those with compound are "binuclear". STRESS (with its variant HEAVY STRESS: cf. Pike 4.4.7) is recognised in contrast with absence of stress, which is never marked. Tonic marks are indications also of stress, and so stress as such (ˈ) is only marked when it does not occur at a nucleus or at the beginning of a segment. Heavy stress (ˉ), however, is marked wherever it occurs. A post-nuclear syllable bearing stress and continuing the nuclear tone is marked by a raised period (˙).

6. The system is illustrated in the specimens of material given below (p. 682: the corresponding recordings were available for conference members to hear). A question mark placed before a syllable bearing a tone-mark indicates a doubtful reading: in some cases an alternative reading is suggested in the margin. The symbol *m* in the text, used to indicate a modification in voice quality, is amplified each time in the margin. It will be noticed that our system (a) regards pitch primarily in terms of direction rather than levels, and (b) takes account of pitch movement as such only at "nuclei". Several further intonation distinctions (observed, for example, by Pike) are handled in terms of the segment distinctions, together with the notation of tempo, prominence, pause, modification, and "extended tone", ⁶ features which may receive only a bare mention here, since they were ignored in the limited computer experiment now being reported.

7. The material was described for the purposes of computer analysis in terms of nine sets of data, hereafter referred to as columns.

Column one was assigned to the description of the nucleus in a system of 27 terms – the basic seven already discussed, distributed over a number of sub-categorisations (for example, interrogation) that do not concern us here. In the 1880 tone units examined, simple falling nuclei were by far the most numerous (958); there were half as many rising nuclei (451). The next most numerous were the binuclear fall-plus-rise (170), the mononuclear fall-rise (125) and rise-fall (72). ⁷

⁵ Cf. Pike on "centers of attention", 4.1.2.

⁶ We lack, however, the useful distinction observed by some other linguists between a "2-3" and a "2-4" fall (Pike, 4.1.2).

⁷ Minor categories: 38 level, 11 rise-plus-fall, 9 extended, 46 doubtful.

SPECIMENS OF MATERIAL (actual size of each slip is 6" x 4")	
<p>5b.1.21 (TL) *group of councillors# . /in Cróydon# en/tirely on *their own bát# without /reference to :anybody :else have de:ecided to : "dò# - e I /just don't believe# that it'e/trùe# a that they're going to /hurl a# everybody into the :street tomorrow# . that /clearly couldn't be right# (?) /six months nòtice # they're 'giving 'them# /Bill# (TL) /six [man] are you :sure of 'that# is /that what it says in the páper# (?) /yès# I /think só# /yès# (TL) /six months nòtice# . /wéll# . a /?gáin# I /don't know [òe òe] /that would depend on :how many houses a:available there :are in :Cróydon# whether that's /fair or nòt# . I /don't know thát# and /neither do any of us# - /but [òí:] the : "principle : "surely of * . : "people : "paying . : what they can afford to páy# *an /economic rent# . is / "surely : "right# . and I</p> <p>agáin (Rq)</p>	<p>5b.1.22 (TL) /fair or nòt# . I /don't know thát# and /neither do any of us# - /but [òí:] the : "principle : "surely of * . : "people : "paying . : what they can afford to páy# an /economic rent# . is / "surely : "right# . and I /would have thought# as /we've had here in : "Bristol for : "many yéar# . a /differential : rént# - because /one of the great : probléms# of of pro/viding# /subsidized?hóusing# or /subsidized anything : else# - one of the : great : probléms# of the : Welfare : State ie# - that we tax ourselvee up the hilt# to [pré] to pro/vide benefits . : quite indis:criminate# for /those who néed# and those who : don't néed# - /with the result that the : whole benefit is : spread so : "thinly over the community# . you /can't : réally do enóugh# for /those who : réally : "áre in 'need# . and * /housing : subsidies is a : classical example : òf * . that#</p> <p>(JS) housing non-nuc.</p>
<p>5b.2.29 G *oh /dèar# e e - /this ie 'quite im:pòssible# /cer- *tainly for me to : ànswer# - /but - : if one : tries to draw an a:nalogy from ànimals# - - /their : acts . which : look . thoughtful# - m such as the : spider'e : spinning of a : wèb# m# or a m /bird : building of a : nest# m# - em . are /not réally thoughtful in 'our eénse# that they're . /concepts . : drawn out# . of a /large number of individual . ?items# - [en] . they're /concepts built in through a hereditary : mèchanism# . and I /think in the be:gínning# . /man# . /muet have# . aó in in/verted commas# aó# . /thought# - /in this eense of he : did certain things without knowing the reason : why# . /each as am a : child suckling ite : mòther# am# - with/out knowing : why# . /but . the : minute# /he wae : capable of : réal 'thought# . he was /also capable * I'm eùre# of com/municating that thought# by /some</p> <p>m = narrow & rhythmic</p> <p>ítéms</p> <p>m = rhythmic & narrow pre-nucl.</p>	<p>5b.2.56 (M) ie /'that a Robertson óne# + and and em . the e e ?F + yes + (M) there / "áre 'these 'things# that ap/pèar to be 'sexlinked# on the /Y chròmosome# * . but ... /see it ?F 'yee' (M) : even a more stróngly# because they're / "all inter- related in Scott'land# it's it's a /small country# and we've / "all intermarried for a : long time# a# and /yet . there are m : Cábbell . character'istics# m# there are . the a /main Cábbell character'istic was their great : couráge# a# . em if you /take the m : Cawdor Cábbell's# . /three Victoria Crócees# un/limited M m# : 'Ces# . the m /Lindsays# - /always very cívilized# /they write pláys# m# . they . o /Lord : Cráwford# is a very /good ex:ámple# o# - they * they're /very ártletic# . and /yet they're all * intermáried# - and I've /always wanted to : táckle</p> <p>m = high narrow</p> <p>m = narrow</p> <p>m = narrow</p>

8. *Column two* registered the length of tone units (measured simply by the number of institutional words). It was necessary to have as many as 23 terms, but in fact only one unit was as long as 23 words, and only for lengths up to 14 words were there as many as ten units, while there were over a hundred units consisting of a single word. The overall average length of a tone unit (c. 10,000 words, 1880 units) was 5.3 words, and in fact 61 per cent of the units were found to have lengths fairly evenly distributed between 2 and 7 words, but the highest number of occurrences clustered between 3 and 5: there were 251 units of 3 words, 248 of 4 words, and 252 of 5 words.

9. *Column three* registered in a system of 16 terms the form-class of the word bearing the nuclear tone. Six classes accounted for 95 per cent of the nuclei. In 847 cases, the nucleus fell on a noun – and this class did not include the “proper name” which accounted for a further 153 nuclei. The next most important form-class was the finite verb (310), followed by the adverb (262), the adjective (221), and the pronoun (126). Among the minor items, it is of interest to mention that prepositions bore the nucleus 25 times when the phrase-head was a pronoun, but only six times when it was a noun or name (though the latter types of phrase were more than five times as numerous as the former).

10. *Column four* dealt with two distinct systems with a total of 47 terms. First, a grammatical description of post-nuclear parts of mononuclear units: this phenomenon occurred in less than an eighth of the tone units, and the commonest form for “tails” of this kind was a pronoun (as in “. . . sàw him#”). Secondly, a registration of the number of internuclear words in binuclear units: most frequently (71 times) the tones were borne by consecutive words; there were 46 units with one internuclear word and 28 units with two; there were few internuclear stretches of more than two words.

11. *Column five*, in a system of 23 terms, handled the principal types of group structure into which the nucleus-bearing form-class of column 3 entered. About three-fifths of all nuclei fell on some part of a nominal group structure, and nearly 60 per cent of these are on the heads of “simple” nominal groups (“. . . the hèad#”, a fall of course here representing any type of nucleus). About 20 per cent of the nucleus-bearing nominal groups had the nucleus on a non-clausal post-modifier (“. . . the head of the rìver#”); about 8 per cent had the nucleus on the head of a post-modified nominal group (“. . . the hèad of the river#”), and a further 8 per cent had it on the premodifier of a simple group (“. . . the ònly head#”). Something over 350 tone units (18 per cent of the total) had the nucleus on a part of the verbal group. Of these, 157 were “simple” (one-element) groups: “. . . he wàlked#”. Where groups had more than one element, the nucleus was usually on the final element: there were 123 cases of the type “he wàs wàlking#” beside 15 of the type “he wàs walking#”; there were 25 of the type “he had been wàlking#” as compared with 5 of the type “he had bèen walking#” and none of the type “he hàd been walking#”. There were 14 cases of nuclei on anaphoric verb forms (“he did#”).

12. *Column six*, in a system of 18 terms, dealt with the place in clause-structure⁸ of the form-class and group-structure (columns 3 and 5) bearing the nucleus. Of the 80 per cent of nuclei that did not fall on the verb, three-quarters fell on elements that were post-verbal in the clause structure. Whereas subjects bore the nucleus 170 times, adverbial phrases did so 457 times, and complements 553 times.

13. *Column seven* categorised in a 27-term system the clause-structure in which the nuclear element operated. The most important clauses numerically were the "free", e.g. "He came yèsterday#"; "independent terminal", e.g. "(He came yésterday#) and we had lùnch#"; "independent non-terminal", e.g. "He came yésterday# (and we had lùnch#)"; "bound as complement with *that* or zero", e.g. "(He said) he was tired#"; "relative", e.g. "(the man) who was thère#"; and "bound as adverbial with the main elements of clause structure following", e.g. "After he cáme# (he had lùnch#)". No other type occurred with as many as one hundred instances, though three came near it: "bound as adverbial with main elements preceding", e.g. "(He had lunch) after he càme#"; "non-finite verbal postmodifier in nominal group", e.g. "(the man) having lunch in the còrner#" (cf. "relative clause" above); and "verbless free", e.g. "A pèn# pléase#".

14. *Column eight*, in a 45-term system, assigned a grammatical description to the contents of the tone unit. This was primarily (and where relevant) done with reference to column 7, since, as we shall see, clause limits and tone-unit limits coincided in a large number of cases. Additional terms had however to be brought in to accommodate the several types of "clause fragment" that were found co-extensive with tone units. The commonest of these were prepositional phrases with adverbial value in a clause occupying a preceding tone unit (150), nominal group as subject (82), and verb with post-verbal elements (109: this type was obviously in the main correlative to the preceding type).

15. Finally, in *Column nine*, a system of 41 terms categorised the grammatical junction that obtained between tone units. About a quarter of the tone units terminated at the end of grammatical units which had no grammatical relation with what followed in the next tone unit (486). A further 97 might be added to these, since any grammatical link at the tone-unit junction was very weak and even questionable. A further 206 tone units ended at points of major grammatical break where the linkage was only through "sentence co-ordination". Thus about 800 of the roughly 1900 tone units terminated at points of grammatical junction no more binding than that between the two following clauses: "we were together befóre# and we're together nòw#". Other statistically important unit-terminals were as follows: between co-

⁸ Although the abstraction here referred to as "clause" includes what is commonly understood by the term, it also comprises units with non-finite verbal groups ("Walking slowly, he . . .") and verbless units ("His coat on his arm, he . . .").

ordinate bound clauses (75); between co-ordinate nominal groups as complement (71); between subject and verb (108); between the head of a nominal group (not operating as subject) and a clausal postmodifier (93); between the main elements of a clause and a following non-clausal adverbial (125); between the main elements of a clause and a following clausal adverbial (80); and between a preceding clausal adverbial and the main elements of a clause (82). No other type of grammatical junction was represented more than fifty times.

16. When each tone unit had been described with reference to these nine sets of variables, the total data were coded and transferred to punched tape in the University of London Computer Unit. The material was fed into a Ferranti Mercury computer and subjected to analysis by means of a sorting and counting programme, the first output being a simple inventory from which was abstracted the quantitative information so far presented. The inventory itself is enough to show areas of strong relationship between intonation and grammar, but is not enough to show the terms of such a relationship. This can best be done, we would claim, by carefully plotting the *co-occurrence* of variables,⁹ a task which is greatly facilitated by the use of a computer.

The programme involved the computer in noting and measuring several thousand co-occurrences among the many variables in the nine columns,¹⁰ and the notes that follow draw attention to some of the significant co-occurrences that were revealed and to the conclusions that may be drawn from them.

17. Comparison of *columns 1 and 3* (with supplementary comparison with *columns 4, 5, 6 and 7*) shows that in general the nucleus types occurred on the same range of form-classes in the same proportions. We would draw attention to certain exceptions which are presented in the table below:

(a) a relatively high proportion of rising tones on names; this is explained by the study of columns 3 and 7 which shows that the greatest clustering of nucleus-bearing names was in address-forms: "Jóhn# what do you sày#";

(b) a relatively high proportion of rise-fall nuclei on adverbs, as in utterances like "Rêally#" and "He does it bêautifully#";

(c) a relatively high proportion of falling nuclei on premodifying adjectives in fall-plus-rise units, as in utterances like "The òld man isn't#"; comparison of columns 1, 3 and 4 shows that out of 71 instances of fall-plus-rise on juxtaposed words, the first of these was an adjective in 24 cases.

(d) a significant correlation between the falling nucleus in fall-plus-rise units and a pronoun as subject, as in utterances like "Hè doesn't wórk#".

⁹ See *Transactions of the Philological Society*, 1960, p. 51.

¹⁰ Since some analytic categories depend on correlation between columns, and since for practical reasons it was necessary to cut out the computation of numerically small co-occurrences (less than ten in two-column comparisons, and less than five in three-column comparisons), it has been necessary on stated occasions below to give figures which are approximations.

Nucleus type	Nucleus on					
	Noun	Name	Adverb	Adj. premod. head of Nom. Gp.	Adj. not in Nom. Gp.	Pronoun as Subject
Fall	382	56	103	29	57	13
Rise	202	50	47	(<10)	22	(<10)
Fall-rise	55	(<10)	17	(<10)	10	(<10)
Rise-fall	20	(<10)	17	(<10)	(<10)	(<10)
Fall (of ' +')	25	(<10)	18	27	(<10)	17
Rise (of ' +')	82	14	16	(<10)	(<10)	(<10)

We shall return to the association of nucleus types with features of grammar in considering columns 1 and 9 below (§ 25).

18. In view of points (a) and (d) in the preceding paragraph, it is of interest to point out (from columns 3, 5 and 6) that:

- 1) 10 per cent of nucleus-bearing subjects had a name as exponent, but only 3.5 per cent of nucleus-bearing complements; and that
- 2) 25 per cent of nucleus-bearing subjects had a pronoun as exponent but only 9 per cent of nucleus-bearing complements.

19. *Columns 3, 5 and 7.* In most types of clause, the nucleus fell within a nominal group structure in a ratio of between 2:1 and 4:1 as compared with the verbal group. In a few clause types the position was reversed:

Type 1: *I think that he did it.*

Type 2: *He did it, I think.*

Type 3: *I wondered how he did it.*

Type 4: *I asked about what he did.*

The figures in the material are given below, and for comparison we add those for a Type 5: *I think that he did it.*

Nucleus on	Clause type				
	1	2	3	4	5
Nom. Gp.	13	<10	11	<10	150
Verb.	26	19	15	12	24

20. *Columns 2, 8 and 9.* Whatever the type of grammatical unit, there was a considerable correlation between the length of tone units and their grammatical contents. The length tended to be a single word with certain types of adverbial contents; 3-4 words when the tone unit was co-extensive with end-placed prepositional phrase as adverbial or with nominal group as subject; but tone units consisting of verb plus post-verbal elements tended to be longer, 4-5 words. Tone units co-extensive with one full clause averaged 5-6 words, and the length increased to 8 words when the tone unit was co-extensive with two clauses. The dominant clause pairs were of the type

“Clause plus clause as complement”. The study of co-occurrences indicates some tension between (a) the tendency for such clause sequences to occupy single tone units, (b) the tendency for the end-point of a tone unit to be the end-point of a clause, and (c) the tendency for tone units to maintain a roughly constant length. Clause sequences of the type in question are of course sometimes longer than even the 8 words prosodically “allowed” them, and this may explain why tone units comprising a clause plus a clause fragment occurred in the material 20 times with a nucleus on a clause having a clause as complement but did not reach double figures with any other grammatical type. For example: “Everyone knew that his concentration# was the secret of the man’s success#”. (See further § 24.) The table below shows the major correlations between the length of the tone unit and its contents:

Two Clauses (8 words)	I really think that he will come tomorrow
One Clause (5-6 words)	He will arrive by train tomorrow
Verb Gp. plus Adverbial (4-5 words)	will arrive at ten thirty
Nom. Gp. as Subject (3-4 words)	The train from London
Prep. Phrase as Adverbial (3-4 words)	at this platform
Miscellaneous Nom. Gp. (2-3 words)	and the South
Adverbial (1 word)	obviously

21. *Columns 5, 6 and 8.* Another indication that a tendency to constant tone-unit length was in conflict with the tendency for tone unit and clause to be co-extensive is shown by the study of columns 5, 6 and 8. Two types of nucleus-bearing nominal groups were most frequent: (1) “the head”, (2) “the head of the river”. Clauses with the nucleus on a nominal group were more frequently co-extensive with the tone unit when the nominal group was type 1 than when it was type 2.

Nucleus as in	Tone unit co-extensive with Clause	Tone unit co-extensive with Nominal Group as S or with Prep. Phr. as A
Nominal Group (1)	342	123
Nominal Group (2)	90	82

This distribution reflects the fact that complex nominal groups (type 2) were generally longer than those of type 1 and might often therefore make a tone unit longer than normal; they thus tended to be separated off into separate tone units.

Nominal groups of type 1 were always numerically predominant, whatever the place in clause structure occupied by the nominal group; but the proportions differ, revealing a tendency for type 1 nominal groups to be used in prepositional phrases as adverbial, while type 2 had a higher ratio than usual when the nominal group was subject:

	As Subject	As Complement	In Prep. Phr. as Adverbial
Nominal Group (1)	60	201	265
Nominal Group (2)	46	99	76

22. *Columns 6 and 8.* Where the nucleus fell on the verb or complement, the tone unit in a large majority of cases was co-extensive with the clause. Where it fell on a prepositional phrase with adverbial value, over a third were in tone units co-extensive with a clause, while a quarter of the prepositional phrases themselves were co-extensive with the tone units. Where the nucleus was on the subject, only a small minority of the tone units were co-extensive with a clause, and a majority were co-extensive with the subject (having a nominal group as exponent). Salient points of distribution:

Nucleus on	Total nucleus-bearing	Tone unit co-ext. with Clause	Tone unit co-ext. with Nom. Gp. as S	Tone unit co-ext. with Prep. Phr. as A
Verb	366	200
Complement	553	296
Subject	170	19	76	...
Prep. Phr. as Adverbial	457	169	...	115

23. *Columns 7, 8 and 9* show that free clauses were co-extensive with the tone unit in about half their occurrences and that a large number of clauses of various types were so in about one-third of their occurrences. Two clause types deserve a special comment. Sequences of "Clause plus clause as complement" ("I think he came", "I know how he did it", for example) were the only clause-pairs with a frequency (61) of more than 10. The figures make clear that the clauses which operated as complement had a distinctly lower than average co-extensiveness (about 1/7). Moreover, since we know from the material that there were 177 clauses as complement, it is very likely that there were almost as many clauses having such clauses as complement ("almost", allowing for co-ordination of complements). It must therefore be remembered that when table I gives one-third of such clauses (18) as co-extensive with a tone unit this does not mean one-third of all such clauses in the material. There would be a much smaller proportion of co-extensive instances if we were not restricting our comparison to those clauses (54) which had a nucleus.¹¹

¹¹ Since, through our tone-unit orientation, we register a clause's existence only if a nucleus falls within it, our totals for all types of clauses are inevitably incomplete. The disparity, therefore, between a 50 per cent clause tone unit co-extensiveness (in the case of free clauses) and a 14 per cent co-extensiveness (in the case of clauses as complement) may be in part illusory, since the former permits no inference as to the existence of clauses which do not bear a nucleus. It is unlikely, however, that many clauses of other kinds occur without a nucleus, though no doubt the phenomenon occurs with the parenthetic type.

TABLE I

Clause type	Total Clauses bearing a nucleus	Total Clauses co-ext. with tone unit	Proportion
Forms of address	46	38	3/4
“Clause + Clause as C” sequences	79	61	3/4
Verbless free	75	46	2/3
Free	160*	95	3/5
Parenthetic	36*	16	1/2
End-placed Fin. Vb. Clause as A	82*	35	1/2
Front-placed Fin. Vb. Clause as A	101*	39	2/5
Indep. non-terminal	263*	104	2/5
Indep. terminal	120*	49	2/5
Clause as S	15	5	1/3
Clause having Clause as C	54*	18	1/3
Front-placed Verbless Cl. as A	34	12	1/4
Cl. as postmodifier in N. Gp.	162*	35	1/4
End-placed Verbless Cl. as A	22	6	1/4
Cl. correlative to anticipatory <i>it</i>	19	5	1/4
End-placed Non-Fin. Vb. Cl. as A	22	<5	<1/5
Cl. appos. to N.Gp. or comp. to. Adj.	30	<5	<1/6
Clause as C (<i>that</i> or zero)	134*	19	1/7
Clause as C (“dep. question”)	43	<5	}
Non-Fin. Vb. Cl. postmod. in Nom. Gp.	86	<5	

* Approximate: see footnote 10.

24. Comparison of *columns 8 and 9* shows the relation of unit contents to inter-unit breaks. As can be seen from the table on p. 14, units co-extensive with one clause (879) or two (55) end in 422 cases at a point which had no overt grammatical relationship to the next. In a further 174 cases, the only grammatical link was clause co-ordination. There seems to be a tendency for a tone-unit break to come before an adverbial postmodifier in clause structure (109). In 16 cases, a clause co-extensive with a tone unit and operating as subject had a unit break before the verb (as in “What worries me most# is his laziness#”). When a tone unit was co-extensive with a nominal group, the group in the majority of cases (50) operated as subject and the tone unit break was between subject and verb.

A unit co-extensive with verb plus complement had a break before a postmodifier as often as at a point of no overt relationship.

Clauses operating as postmodifiers in nominal groups and clauses operating as complement in another clause showed a tendency to have inter-unit junctions thus:

The man I knów# is over thère#

I think this mán# will dò it#

In all, 46 per cent of the total number of tone units ended at points where overt grammatical relationship ceased or with the only grammatical relationship being clause co-ordination. Only 5.6 per cent were at points of group or phrase co-ordina-

TABLE II

Inter-unit break occurs between	Unit contents							Inventory of each type	Percentage of tone units in material
	Two Clauses	One Clause	Nominal Group	Verb plus Complement	Prepositional Phrase	Special Fragments	Other Minor Categories		
Units with no overt grammatical relationship	40	382	13	30	39	—	79	583	46%
Co-ordinate Independent Clauses	15	115	10	—	22	—	44	206	
Co-ordinate Dependent Clauses	—	44	—	—	—	—	31	75	
Co-ordinate Complements	—	34	12	—	—	—	25	71	5.6%
Co-ordinate Adverbials	—	14	—	—	—	—	20	34	
Subject and Verb	—	16	50	—	—	24**	18	108	8.4%
Verb and Complement	—	24*	—	—	—	11***	15	50	
Nominal and Postmodifier	—	58	15	14	27	—	27	141	7.5%
SVC and end-placed Adverbial	—	109	—	15	—	—	81	205	18%
Front-placed Adverbial and SVC	—	68	—	—	21	—	43	132	
Clause and parenthetic Clause	—	15	—	—	—	—	4	19	1%
Totals	55	879	100	59	109	35	387	1624	

* 18 of type SV and Clause as C, e.g. "I think he's going".

** 12 of type N.Gp. + Rel. Cl. and 12 of type SV $\frac{1}{2}$ C, where the " $\frac{1}{2}$ C" is the S of a Cl. operating as C, e.g. "I think this man \neq will do it".

*** Chiefly SV or VS.

tion. Breaks between major elements in clause structure (subject, verb, complement) occurred in only 8.4 per cent of all cases (tone unit breaks within nominal group structures represented almost as high a proportion of the material), whereas breaks between subject-verb-complement on the one hand, and adverbial on the other, constituted 18 per cent of the tone-unit junctions.

25. *Columns 1 and 9.* While the data examined in this experiment would offer no evidence that a particular type of nucleus has a specific grammatical function, the table below shows that particular nuclei occur with particular types and degrees of grammatical relationship in significantly different proportions. There is most notably an association of a falling nucleus with a termination in the chain of grammatical dependence. Group (a) represents the tone-unit terminals corresponding with greatest grammatical independence; it will be seen that this group accounts for over *half* the falling nuclei, but only a *quarter* of the rising ones, and only a *sixth* of the fall-rises. In contrast, group (b), which comprises the remainder of the frequently

Nucleus		Inter-unit break occurs between						
Type	Total shown in correlation of columns 1 and 9	Units with no overt grammatical relationship	Co-ord. independent Clauses	Co-ord. dependent Clauses	Co-ord. Subjects, Complements, or Adverbials	Subject and Verb	Nominal and Post-modifier	Front-placed Adverbials & SVC
Fall	907	365	122	22	54	30	53	37
Rise	424	97	43	22	34	30	36	41
Fall-rise	123	13	(<10)	(<10)	(<10)	16	17	(<10)

(a)
(b)

occurring categories, represents unit terminals at points of close grammatical relationship, and the proportion of falls to rises and fall-rises is considerably lower.¹²

26. To judge from the present limited material and limited analysis of it, it would seem that:

(a) there is in some measure an association between certain nucleus types and certain form-classes (§ 17), and between certain broad categories of grammatical relationship and tone units having certain nuclei (§ 25);

(b) there is a very considerable connexion between the point of nuclear tone and specific items of grammar, whether as regards form-class, group, phrase, clause, or clause-sequence (§§ 9, 11ff, 18ff);

(c) there is a tendency for certain constants to be observed in the length of tone units according to the types of grammatical content (§§ 20f);

(d) there is a high degree of co-extensiveness between tone-units and grammatical units of group, phrase, and clause rank (§§ 21ff);

(e) the point at which one tone unit ends and the next begins tends to be one of a fairly small number of grammatical junctions (§ 24).

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¹² Among the numerically minor categories not shown in the table, one might mention the tone-unit break between non-co-ordinate adverbials: falling nuclei 4, rising nuclei 12.

KERNEL AND NON-KERNEL SENTENCES IN TRANSFORMATIONAL GRAMMAR

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Pairs of constructions such as the English declarative-interrogative, affirmative-negative, and active-passive are often used to illustrate the nature of transformationally-oriented generative grammars. From a tactical point of view the choice of such pairs of constructions is an effective one; for the regularity of the relation between the members of the pairs is easily demonstrated, and the transformational account of this regularity quite convincing – so convincing, in fact, that many linguists who are otherwise skeptical of transformational grammar concede its explanatory power for such cases. From another point of view, however, the use of illustrations of this kind is unfortunate. For it tends on the one hand to blur the important distinction between sentences (including the products of optional one-string transformations) that have only one phrase-structure tree in their derivational history and sentences whose derivational history includes more than one tree, and on the other to give undue prominence to the distinction between the products of optional one-string transformations and so-called kernel sentences. The importance of the distinction between sentences with one-string and those with multi-string derivational histories has already been amply demonstrated.¹ It will be the purpose of the present paper to demonstrate the relative triviality of the distinction now made between kernel and non-kernel sentences.

In *Syntactic Structures*, Noam Chomsky defines kernel sentences as “the set of sentences that are produced when we apply obligatory transformations to the terminal strings of the [phrase-structure] grammar.” Kernel sentences, in other words, are sentences whose derivational history includes no optional transformations; all other sentences are non-kernel. “Every sentence of the language”, Chomsky continues, “will either belong to the kernel or will be derived from the strings underlying one or more kernel sentences by a sequence of one or more [optional] transformations.”²

While there is no doubt that it is possible to make a distinction between kernel sentences so defined and all other sentences, there is considerable doubt that this distinction is, as Chomsky asserts, “fundamental” – at least as regards those non-kernel sentences which may be derived from a string underlying a single kernel

¹ Most recently by R. B. Lees, in “On Reformulating Transformation Grammars”, presented at the December 1961 meeting of the Linguistic Society of America.

² Noam Chomsky, *Syntactic Structures* (s-Gravenhage, Mouton and Co., 1957), p. 45.

sentence. For in a great many such cases, it is possible to generate the given sentence-type as either kernel or non-kernel without loss of explanatory power in the grammar.

The English interrogative may be taken as a case in point. In existing transformationally-oriented generative grammars, the interrogative is derived from the declarative by an optional transformation that moves part of the verbal auxiliary to a position before the subject of the sentence. The rule may be stated as follows:

(1) *Optional transformation: interrogative*³

$$\begin{array}{l} \text{Structural analysis: Noun Phrase-} \left\{ \begin{array}{l} \text{Tense} \qquad \qquad \qquad - \text{Verb} + \text{X} \\ \text{Tense} + \left\{ \begin{array}{l} \text{Modal} \\ \text{have} \\ \text{be} \end{array} \right\} - \text{X} \end{array} \right\} \\ \text{Structural change: } 1-2-3 \rightarrow 2-1-3^4 \end{array}$$

It is, however, equally possible to derive the interrogative by means of a combination of an optional phrase-structure element and an obligatory transformation. Thus:

(2) *Phrase-structure rule*

$$\text{Sentence} \rightarrow (\text{Interrogative} +) \text{Noun Phrase} + \text{Verb Phrase}$$

(3) *Obligatory transformation: interrogative*

Structural analysis:

$$\begin{array}{l} \text{Interrogative - Noun Phrase-} \left\{ \begin{array}{l} \text{Tense} \qquad \qquad \qquad - \text{Verb} + \text{X} \\ \text{Tense} + \left\{ \begin{array}{l} \text{Modal} \\ \text{have} \\ \text{be} \end{array} \right\} - \text{X} \end{array} \right\} \\ \text{Structural change: } 1-2-3-4 \rightarrow 3-2-4 \end{array}$$

As an alternative to (2), a series of phrase structure rules requiring a choice between declarative and interrogative can also be devised:

(4) *Phrase-structure rules*

$$\text{Sentence} \rightarrow \text{Mood} + \text{Noun Phrase} + \text{Verb Phrase}$$

$$\text{Mood} \rightarrow \left\{ \begin{array}{l} \text{Declarative} \\ \text{Interrogative} \end{array} \right\}$$

“Declarative” may then be rewritten as \emptyset by obligatory transformation, with obligatory transformation (3) applying whenever “Interrogative” is chosen.

³ The format of the transformational rules that appear in this paper is essentially Chomsky's. The sentence-elements that function as units in the transformation are separated by hyphens in the *structural analysis*. In the *structural change*, these elements are represented by numbers, the numbers to the left of the arrow referring to the original sequence of elements in the structural analysis, the numbers to the right of the arrow representing the modified sequence that results from the given transformation.

⁴ Where 2 = Tense, an obligatory transformation operates upon the resultant string to insert *do* before the tense morpheme. In other cases, an obligatory transformation operates to invert the order of the tense morpheme and the immediately following base. See *Syntactic Structures*, p. 113.

While it cannot be argued that the alternative derivations of the interrogative just suggested are in any way superior to the derivation by means of (1), neither can it be argued that they are significantly inferior to it. All three derivations – by means of (1) by means of the combination of (2) and (3), and by means of the combination of (4) and (3) – produce the desired interrogative sentences, and relate these sentences to the relevant declaratives. The point to be observed, however, is that, while derivation of the interrogatives by means of (1) results in the classification of the interrogatives as non-kernel, derivation in either of the other ways makes the interrogatives kernel sentences; for, derived in these ways, the interrogatives clearly belong to “the set of sentences that are produced when we apply obligatory transformations to the terminal strings of the phrase-structure grammar”.

Since the aim of this paper is to show that the distinction now made between kernel sentences and the products of optional one-string transformations is a relatively insignificant one, it would seem appropriate to investigate just how generally it holds that construction-types derivable by means of optional one-string transformations are alternatively derivable as kernels. Construction-types usually derived by means of optional one-string transformations, then, may conveniently be divided into four classes, according to the ways in which the underlying strings are altered by the transformation. These four classes are: a) rearrangements; b) additions; c) deletions; d) some combination of a) and/or b) and/or c). Type a) has already been illustrated with the interrogative: *John should eat the apples* → *Should John eat the apples?* Type b) may be illustrated by the negative: *John should eat the apples* → *John shouldn't eat the apples*; type c) by the deleted object: *John should eat the apples* → *John should eat*; and type d) by the passive: *John should eat the apples* → *The apples should be eaten (by John)*.

It has been shown above that rearrangements can generally be handled in either of two ways: by an optional transformation, or by the inclusion of an optional “rearranger” in the phrase-structure rules, with an obligatory transformation becoming applicable whenever the optional rearranger is chosen. In general, additions can also be handled in either of two ways, the English negative being a case in point. Chomsky generates simple negatives by means of an optional transformation,⁵ which may be stated as follows:

(5) *Optional transformation: negative*

$$\begin{array}{l} \text{Structural analysis: Noun Phrase} + \left\{ \begin{array}{l} \text{Tense} \\ \text{Tense} + \left\{ \begin{array}{l} \text{Modal} \\ \text{have} \\ \text{be} \end{array} \right\} - \text{X} \end{array} \right\} \begin{array}{l} - \text{Verb} + \text{X} \\ - \text{X} \end{array} \\ \text{Structural change: } 1-2 \rightarrow 1 + \text{not} - 2 \end{array}$$

R.B. Lees, on the other hand, in *The Grammar of English Nominalizations*, generates negatives from an optional phrase-structure element, the “Preverb”, with certain

⁵ *Syntactic Structures*, p. 112.

obligatory transformations becoming applicable if the negative morpheme is chosen.⁶ Both Chomsky's derivation of the negative and Lees' are quite plausible – a fact that would certainly be puzzling if the kernel-non-kernel distinction, as this distinction is now understood, were a significant one. For there is no doubt that simple negatives are kernel sentences in Lees' grammar, non-kernels in Chomsky's.

Construction-types now derived by means of deletion transformations are, once more, susceptible of an alternative derivation as kernels. In Chomsky's "A Transformational Approach to Syntax",⁷ a class of transitive verbs is established (verbs such as *eat, drink, smoke*), which allow optional object-deletion. The deletion is accomplished by means of a one-string transformations, which may be stated:

(6) *Optional transformation: object deletion*

Structural analysis: X + Transitive Verb: optional object – Noun Phrase

Structural change: 1 – 2 → 1

Clearly, it would be equally possible to handle such cases in the phrase structure, as follows:

(7) *Phrase-structure rule*

$$\text{Transitive Verb Phrase} \rightarrow \left\{ \begin{array}{l} \text{Transitive Verb: optional object} \\ \quad (+ \text{ Noun Phrase}) \\ \text{Transitive Verb: obligatory object} \\ \quad + \text{ Noun Phrase} \end{array} \right\}$$

Once more, such a solution would make the construction-type part of the kernel.

It has thus far been shown that certain construction-types derivable by optional one-string rearrangement, addition, or deletion transformations are alternatively derivable as kernels. Such alternative derivations are also possible in the case of construction-types like the English passive, which are at present derived by an optional transformation that combines rearrangement, addition, and – in one version of the rule – optional deletion. This transformation may be stated:

(8) *Optional transformation: passive*

Structural analysis: Noun Phrase – Auxiliary – Transitive Verb – Noun Phrase

Structural change: 1-2-3-4 → 4-2+be+en-3(+ by +1)

The alternative derivation of passives as kernels would require the introduction of certain optional elements in the phrase-structure rewriting of transitive verb phrases:

(9) *Phrase-structure rule*

Transitive Verb Phrase → (*be + en + by +*) Transitive Verb + Noun Phrase⁸

⁶ Bloomington, Indiana University Research Center in Anthropology, Folklore, and Linguistics 1960, pp. 5-6, 18-19, 43-44.

⁷ Third Texas Conference on Problems in the Analysis of English, May 9-12, 1958 (to be published).

⁸ The differences between the rewritings of "Transitive Verb Phrase" in (7) and (9) would, of course, have to be reconciled in a grammar which seriously proposed to make both deleted objects and passives part of the kernel. While such a reconciliation is certainly not difficult, it would unnecessarily complicate the present exposition.

Then an obligatory transformation would apply whenever these optional elements were chosen:

(10) *Obligatory transformation: passive*

Structural analysis: Noun Phrase – Auxiliary + *be* + *en* – *by* – Transitive
Verb – Noun Phrase

Structural change: 1-2-3-4-5 → 5-2-4(-3-1)

Again, then a construction-type has been shown to be derivable in two different ways, one of which would require its assignment to the kernel, the other of which would not.

Just as it is true that many constructions usually derived by means of optional one-string transformations may, alternatively, be derived from optional phrase-structure elements (plus, in most cases, obligatory transformations), so is it true that many elements usually introduced as optional in the phrase structure may, alternatively, be introduced by optional transformation. Thus the modal auxiliaries are generally treated as optional elements in the phrase-structure rewriting of “Auxiliary”:

(11) *Phrase-structure rules*

Auxiliary → Tense (+ Modal)

Modal → *can, may, must, shall, will*

But the modals may also be introduced transformationally:

(12) *Optional transformation: modal*

Structural analysis: X + Tense – Y

Structural change: 1-2 → 1 + $\left. \begin{array}{l} \textit{can} \\ \textit{may} \\ \textit{must} \\ \textit{shall} \\ \textit{will} \end{array} \right\} - 2$

Admittedly, such a solution involves no gain in the explanatory power of the grammar. But neither does it involve any appreciable loss of such power.

To summarize, then: the distinction made by Chomsky between kernel and non-kernel sentences is a trivial one in the case of those non-kernel sentences derived by optional one-string transformations. Proof of the triviality of the distinction may be seen in the fact that many types of constructions may be derived, more or less indifferently, as either kernel or non-kernel. In any given case where such dual derivations are possible, the choice between derivations rests largely upon such criteria as simplicity or elegance, and very little, if at all, upon anything that may properly be considered fundamental to the nature of language.

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DISCUSSION

HOUSEHOLDER:

This point is made also in my review of Lees' *Grammar of English Nominalizations*, which has appeared in *Word*, 18.3. Closely related is the generation of special kernel strings which can only become sentences upon the application of some double-base transformation (see rules containing "C" or "Comp").

HODGE:

Professor Zellig Harris, who is the originator of transformation grammar but obviously not the formulator of the Chomskian variety, has stressed that any sentence in the language may be used as the basis for transformation.

Further, I should like to note that the affirmative-negative correlates in Hausa are in part so different morphologically that I would hesitate to 'transform' one into the other. I would even have difficulty equating what is the negative of what in certain cases. Maybe I'm not intuitive enough.

W. HAAS:

To comment on Mr. Postal's intervention: If simplicity and economy are to decide what is to be described as "kernel" and what in terms of "transformations": then it might well appear that the simplest and most economical description would be one which would choose primarily hypothetical ("artificial") sentences as kernel, and derive all, or almost all, the real sentences of the language by transformation. What would then happen to the claim that generative grammar explains the native speaker's linguistic intuitions – that is another matter.

HAHN:

I am shocked at the suggestion that we are to trust to intuition! *Is this science?* And what does it matter which of two different systems of transpositions we elect to follow? Suppose we are asked to classify human beings. We have two principal patterns open to us. We may elect to follow a system bases on age, in which case our outline would run as follows:

- 1, children: a, male – boys; b, female – girls.
- 2, adults: a, male – men; b, female – women.

Or we may elect to follow a system based on sex, in which case our outline would run as follows:

- 1, males: a, children – boys; b, adults – men.
- 2, females; a, children – girls; b, adults – women.

But the two systems amount to the same thing in the end; we cannot say that one is preferable to the other.

SUPRASYNTACTICS

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To be judged satisfactory, an explanatory model of linguistic structure must meet three conditions: it must be COMPLETE, it must be CONSISTENT, and it must be CONVINCING.¹ To be complete, it must account for a maximum number of (ideally, all) observed language phenomena, leaving a minimum number of loose ends to be dealt with by poetics, psychology, etc., but without attempting to force extra-linguistic phenomena into its own frames. To be consistent, such a model will not shift its terms of reference as it moves from one level to another, but will describe all phenomena within a maximally simple framework which illuminates the parallelisms of arrangement and process that obtain on different levels, without however confusing parallelism with isomorphism and "mixing" these levels. Finally, to be convincing, an explanatory model must really *explain*, i.e., it must satisfy the intuition of the sophisticated observer that his knowledge or understanding of at least some part of the structure of some language (ideally, all parts of language structure in general) has somehow been deepened or sharpened as a result of inspecting the model. All partial models (i.e., models of subsystems within the "ensemble of partial systems" of which language consists [Trubetzkoy]) should strive toward this general goal, and should be capable of being integrated into more and more general models as these are developed. The following paper suggests a tentative model designed to explain one small but not uninteresting part of the syntax of some modern Indo-European languages. The model employs descriptive devices already in common use, namely the immediate constituent or phrase structure "tree" as a description of certain basic sentence types, and the concept of operations which, when performed upon various units within the IC tree, yield sentences which differ in regular ways from the original sentences. It may turn out that these operations can be most economically formalized as transformations within a generative grammar of the now familiar type, but this has not yet been demonstrated and is in any case unimportant here, since the detailed formulation of "structural description", "structural change", etc., is a relatively trivial matter once the forms and meanings of the grammatical categories involved

¹ This paper is based on RM-3161-PR, *Suprasyntactics*, The RAND Corporation, May, 1962. Although this research was sponsored by the United States Air Force under Project RAND, this paper does not necessarily represent the views of the sponsoring agency.

are thoroughly understood. The present paper has only the restricted goal of attempting to approach more closely such an understanding of these categories.

The model suggested below is designed to explain the function of the traditional "status" categories of NEGATION and INTERROGATION, and especially of a third category which for want of a better term will be called EMPHASIS. Certain individual manifestations of emphasis have been referred to by such terms as "affirmation" on the sentence level (*He went* : : *He did go*) or "logical" or "contrastive" stress on the word level (*He walked home* : : *He WALKED home* : : *He walked HOME* etc.), but neither the functional identity of these operations on various levels, nor the striking parallelism between emphatic and negative or interrogative structures on any given level, has been adequately recognized. The view adopted here is that emphasis is a grammatical category manifesting the same high co-variance of form and meaning as negation and interrogation and like them susceptible to systematic description; emphasis as discussed here therefore excludes all those emotional or expressive nuances (anger, irony, astonishment, apprehension, etc.) which are perhaps "emphatic" in a broader sense, but which can be superimposed upon almost any utterance and which are formally expressed by such a complicated and unstable set of intonational and timbre shifts as makes it impossible to describe them in terms of recurring constants. The formal markers of emphasis as the term is used here differ from language to language, and from level to level within a given language, but are usually restricted to emphatic stress (*Hat er DIESES Buch gelesen?*), rearrangement of word order (*Za granicu poexal on*), periphrastic expressions (*C'est Pierre qui l'a fait*) and combinations of the foregoing. The function of the emphatic category is to SEPARATE OUT SOME LINGUISTIC UNIT (sentence, noun phrase, word, morpheme, sub-morphemic segment, etc.) AND BY MEANS OF THIS SEPARATING-OUT TO ESTABLISH AN AD HOC OPPOSITION BETWEEN THE GIVEN UNIT ON THE ONE HAND AND ALL LIKE UNITS (i.e., all units belonging to a form class determined by the environment of the given unit) ON THE OTHER HAND. On the sentence level, emphasis (like negation and interrogation) operates not on the entire sentence but on the predicative connection between its major elements: the emphatic sentence *John DID go home* is opposed only to the neutral *John went home* and the negative *John didn't go home*, but not to sentences with different lexical content, e.g., *Peter went to the movies*. An emphasized noun phrase will be opposed to all and only those noun phrases which could occur in the same place in the same sentence, e.g., in the sentence *It was the man in the grey coat who robbed the bank*, the emphasized NP *the man in the grey coat* is opposed to *some other man, a little old lady, John*, etc. Similarly, an emphasized adjective is opposed to all and only those adjectives which could fill the same "slot," e.g., in *It was the man in the GREY coat who robbed the bank* the emphatic GREY is opposed to *red, brown*, etc., but the NP in which the adjective occurs is no longer opposed to anything (cf. the impossibility of **It was the man in the GREY coat who robbed the bank, not a little old lady*). The morpheme *in-* in the sentence *I said he was INSIDE the house* is opposed only to those morphemes which can occur in the frame . . . was ()side . . ., namely *out-*;

that the ad hoc oppositions created by emphasis obtain only among units of the same level is shown by the difficulty of opposing *in-* in the example just given to a sub-morphemic unit such as *be-*, e.g. (*)*I said he was INSIDE the house, not BESIDE it.* Sub-morphemic components can, however, be opposed to each other: the segment *ob-* in *You call that an OBJECTIVE appraisal?* is opposed only to *sub-* which can occur in the given frame. The strictly ad hoc nature of the oppositions created by emphasis is demonstrated by the fact that the *ob-/sub-* opposition just discussed is by no means universal; cf. the impossibility of emphasizing the segment *ob-* in **I don't OBJECT to your statement,* an impossibility rooted in the fact that the verb *subject* has different co-occurrence features than the verb *object*; cf. however the perfectly possible *That word isn't the OBJECT of the verb, it's the SUBJECT,* where the nouns *object* and *subject* are distributionally similar. Every given sentence therefore carries its own "emphatic potential" which defines the operations which can and which cannot be performed upon its various parts; this potential is itself defined by preexisting factors not only of grammar but also of derivational morphology and of the lexicon.

Tactic rules usually define the arrangements and rearrangements of items on some given level (arrangements of phonemes within the syllable or of morphemes within the word; rearrangements ["transformations"] of terminal strings in a phrase-structure grammar, etc.). Because the rearrangements caused by the categories of negation, interrogation and emphasis are so strikingly similar over the entire range of levels from sentence to submorphemic component, one may refer to these categories as SUPRASYNACTIC. It would be artificial simply to relegate them to a "transformational level" distinct from but correlated with the "phrase structure level", because these three categories operate in exactly the same way on items which arise only in the course of transformations as on items within the kernel. *The picture was painted by Matisse* may be a passive transform of *Matisse painted the picture,* but the fact remains that the negative emphasis of *The picture wasn't painted BY Matisse* (, *but only under his supervision*) functions exactly as in the kernel *Matisse didn't PAINT the picture* (, *he only sketched it in charcoal*). Cf. *He wasn't brought in BY the police* (, *he was brought in TO them*), *I didn't say the child was sleepING, I said it was sleepY,* etc. In other words, suprasyntactic operations remain the same regardless of whether the sentence they are applied to is a kernel or a transform.

A simple sentence such as (1) *John reads the local paper* (*Johann liest die hiesige Zeitung, Jean lit le journal d'ici, Ivan čitaet zdešnjuju gazetu,* etc., etc.) can be represented by the following phrase structure tree, in which upper-case symbols (N, A, V) refer to form classes (noun, adjective, verb) and lower-case symbols (n, a, v) to specific tokens of these classes, i.e., the individual lexical items belonging to these classes; certain inadequacies of IC trees (obligatory binarism, artificially imposed hierarchies, difficulties with discontinuous constituents, etc.) are well known but irrelevant for our present purposes:

Suprasyntactic operations on individual words are expressed either by emphatic stress or by periphrasis, e.g., *It is . . . who*, *It's not . . . which*, etc., the latter containing in some cases obligatory emphatic stress as well. The simplest operations in the sample sentence are those applied to the unmodified first noun: $-(n^1)$ = either (9a) *It's not* or (9a2) *It isn't John who reads the local paper* (with redundant stress JOHN possible) or (9b) *JOHN doesn't read the l.p.*, contrasting with (2) and implying that Bill, Jim, etc. do read it; $?(n^1)$ = (10a) *Is it John who* or (10b) *Does JOHN read the l.p.?* (the latter contrasting with (3)); $!(n^1)$ = (11a) *It's John who* or (11b) *JOHN reads the l.p.*, the latter contrasting with (1) and implying "not Bill, Jim, etc."

Suprasyntactic operations on the verb token are expressed by a *do*-form and/or obligatory emphatic stress: $-(v)$ = (12) *John doesn't READ the l.p.*, contrasting with (2) and implying "he only looks at the pictures"; $?(v)$ = (13) *Does John READ the l.p.?*, contrasting with (3); $!(v)$ = (4b) *John READS the l.p.*, contrasting with (1) but homonymous with $!(\Sigma)$, cf. above, and for some speakers perhaps *John does READ the l.p.* as well. It is interesting that there are no periphrastic forms of $-(v)$, $?(v)$ or $!(v)$, e.g., **It's not reading that John's doing to the l.p.*; this fact is not unrelated to the absence of interrogative-relative pro-verbs in Indo-European.

Operations on the article are possible only when the article is lexicalized and appears in its full form: /ðij/ "the only good or proper" vs. /ej/ "inferior, indiscriminate", e.g. $-(a)$ = (14a) *It's not THE local paper (that) John reads* or (14b) *John doesn't read THE l.p.*, $?(a)$ = (15a) *Is it THE. . .* or (15b) *Does John read THE l.p.?*, $!(a)$ = (16a) *John does read THE l.p.* or (16b) *John reads THE l.p.*, contrasting with (1). Cf. the re-lexicalization of the pronoun in Pěskovskij's example (*Izv. russk. jaz. i slov.*, 1928, I, 2) *v odnoj rubáške* 'in just a shirt' but with $!(a)$ *v odnój rubaške* 'in one shirt (not two, three, etc.).' We can thus draw the conclusion that SUPRASYN-TACTIC OPERATIONS CAN BE APPLIED ONLY TO ITEMS WHICH CONTAIN, WHICH ARE, OR WHICH ARE CONSTITUENTS OF LEXICAL MORPHEMES.

Operations on the adjective are straightforward: $-(a)$ = (17a) *It's not the LOCAL paper that John reads* or (17b) *John doesn't read the LOCAL p.*; $?(a)$ = (18a) *Is it the LOCAL p. that J.r.?* or (18b) *Does John read the LOCAL p.?*; $!(a)$ = (19a) *It's the LOCAL p. that J.r.* or (19b) *John reads the LOCAL p.*; n.b. the obligatory emphatic stress LOCAL in all cases, necessary to distinguish $!(a)$ etc. from $!(NP^2)$ etc.

Suprasyntactic operations on n^2 are ambiguous, since as has been noted above the emphasized head word PAPER can express either $!(n^2)$ or $!(NP^2)$, e.g. $-(n^2)$ = (20a) *It's not the local PAPER that John reads* or (20b) *John doesn't read the local PAPER*, the latter contrasting with (2) but both homonymous with $-(NP^2)$, since *the local PAPER* is opposed not only to *the local literary magazine*, etc. but also to *a book by Salinger, the Paris Review*, etc. Similarly, $?(n^2) = ?(NP^2)$, both expressed by (21a) *Is it the local PAPER that J.r.?* or (21b) *Does John read the l. PAPER?*, and $!(n^2) = !(NP^2)$, both either = (22a) *It's the l. PAPER that J.r.* or (22b) *J.r. the local PAPER*. Such ambiguity will obtain in many cases where the head word of a multiword syntagm stands in syntagm-final position, since an emphatic multiword syntagm must in

English carry the stress on its final unit; cf. *It was the man in the grey COAT who robbed the bank*, which = either !(coat) or !(the man in the g.c.), whereas in non-final position the syntagm-head *man*, if emphasized (*It was the MAN in the g.c. . . .*), is opposed only to *woman, child*, etc., i.e. is not homonymous with the !(NP). N.b. that this situation is different from that obtaining in intraword syntagmata; cf. the discussion of *two-by-fours* above.

The more rigid formulations of suprasyntactic operations which the present model offers can be seen in a reexamination of Jakobson's German glosses to the Czech phrases of Gebauer and Ertl (*TCLP* 4, 1931): (23a) *DONES Janovi tuto knihu* = (23b) *bringe Jan dieses Buch*, *TU ES doch*, (24a) *dones JANOVI tuto knihu* = (24b) *es ist Jan, dem du das Buch bringen sollst*, (25a) *dones Janovi TUTO knihu* = (25b) *es ist DIESES Buch, das du Jan bringen sollst*, and (26a) *dones Janovi tuto KNIHU* = (26b) *bringe Jan dieses Buch, und nicht irgend ein anderes Ding*. Although the essential features of the suprasyntactic mechanism were clear to Jakobson, one may point out that, e.g., (23a) represents not only !(Σ) but also !(v): German *BRINGE es ihm, schicke es nicht per Post*; similarly, (26a) represents not only !(NP: *tuto knihu*) as in Jakobson's gloss but also !(n²: *knihu*) alone, i.e., *bringe Jan dieses BUCH, und nicht diese Zeitung oder diesen Brief*. In any case, one must protest against Karcevski's denial of a grammatical role to what he calls (*TCLP*, 4, 1931) the "accent logique" in the four varieties of Russian *Ty byl včera v teatre?* which he glosses as *C'est toi qui a été hier au théâtre?*, *Tu as bien été hier au théâtre?*, *C'est hier que tu as été au théâtre?* and *C'est au théâtre que tu as été hier?* The very fact, made clear by Karcevski's and Jakobson's examples as well as by those adduced in this paper, that emphasis is similar in form and function in several modern Indo-European languages, and that this category, like negation and interrogation, is systematically implemented on many different levels, demonstrates that we have to do not with logic or with psychological concepts (Šaxmatov, *Sintaksis russk. jaz.* 1941², p. 258), but with a genuine grammatical category, and a vital one at that.

Considerations of space prevent discussion of several further problems connected with the suprasyntactic categories and with the suggested explanatory model. Such, e.g., is simultaneous double emphasis (*It was JOHN who was reading the BOOK*, creating the opposition *John : book :: Bill : paper*), or pyramided emphasis as in *John does NOT read the local paper*, which can occur only after a previous emphatic sentence *John does read the l.p.*; the latter is part of the complex problem of the co-occurrence of negation, interrogation, and emphasis within one sentence, one syntagm, one word, etc. In some cases, suprasyntactic operations provide test frames for semantic investigations (e.g., "List the items which can occur in *I don't want the (...) shoes, I want the red ones*"). The emphatic category may be used to refine Hockett's "topic and comment" (*A course in modern linguistics*, 191, 201) and to distinguish emphatic reversals of word-order as a linguistic operation from the various and subtle varieties of verbal "panning" and "zooming" which are artistic, extra-linguistic devices. The specific differences by which related languages implement

the suprasyntactic categories should be investigated in more detail, as should the question of whether e.g. emphasis can apply to grammatical as well as to lexical items (i.e., can *John WAS here, but he's gone now* be interpreted as !(past), or only as !(v)?). Finally, the proposed model may be adapted to explain pronominalization operations including but not limited to Chomsky's *wh*- questions (*Syntactic structures*, 69 f.), i.e. all the *who, which, what, what kind of* (Russ. *kakoj*, Pol. *jaki*, etc.), *some-, any-*, etc. structures, specifying the particular nodes to which each operation can be applied. The present paper, however, must content itself with indicating the existence of certain of these problems, and with hoping that emphasis itself can now be recognized as a grammatical category without an account of which no structural syntax can be considered complete.

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DISCUSSION

HOUSEHOLDER:

In his second paragraph Mr. Worth says that *John DID go home* is opposed only to the neutral *John went home* and the negative *John didn't go home*. This is not quite correct, though the rest of Worth's remark is all right; the sentence is also (and often) opposed to sentences of the types *John will go home, John can go home*, etc., in which the stressed item is commuted. In regard to Worth's point of the parallelism between negation and emphasis, it is worth emphasizing the negative aspect of this, namely that while lexical devices and even affixing for *emphasis* are quite easy to find in languages all over the world, the converse, intonational or suprasegmental devices as the sole bearers of *negation* are extremely rare (my friend Don Laycock finds an approach to this situation in one New Guinea language).

BUYSSENS:

First a remark about examples: they are mostly taken from English, which possesses that curious auxiliary *do*. That auxiliary has no equivalent in French or German, and even in English certain verbs are incompatible with the use of *do*. To give his theory a more general value, Mr Worth should have chosen sentences without that exceptional verb. Let us consider the sentence "John is here". I can stress the verb *is* in three cases at least. If I oppose that sentence to *John isn't here*, the stress on *is* emphasizes the meaning of the indicative mood. If I oppose the sentence to *John will be here in a moment*, I emphasize the meaning of the present tense. If I oppose the sentence to *John is coming here*, the stress on *is* emphasizes the notion of localization. The three meanings are united in *is* and each of them can be emphasized in its turn.

I now come to the idea of suprasyntactics. In his diagram on p. 701, Mr Worth analyzes the sentence *John reads the local paper* into two parts: the subject and the

rest of the sentence; that is how logicians have analyzed sentences. But this analysis is no longer acceptable: the verb is the basis of the sentence, the principal element, and the subject is, like the object, a dependent element. The consequence is that if one stresses the verb to emphasize the meaning of the mood, one seems to emphasize the meaning of the whole sentence. We are right to speak of suprasyntactics when referring to the meaning of the intonation; there is an assertive and an interrogative intonation. That is why we are tempted to say that when stressing the modal meaning of the verb, we stress the meaning of the whole sentence. But objectively, it is only the modal meaning of the verb that is emphasized. And that explains why it is the verb which is stressed when we want to emphasize the assertive character of a sentence, why the negation accompanies the verb, and why an interrogative sentence bearing on the truth of an idea begins with the verb.

My conclusion is that there is a certain parallelism between the three kinds of sentences, but that suprasyntactics only concerns facts like intonation.

POTTER:

Like Professor Buysens, I doubt whether *emphasis* may be usefully regarded as a "genuine grammatical category". It is indeed closely involved in the prosodemes of duration, stress, pitch and juncture. It is also an important determiner of word order. It would be helpful, I think, to distinguish four near-universal factors determining sequence: (1) inherited sentence patterns; (2) limited variations within those patterns; (3) emphasis; and (4) euphony. In (1) the position of predication in declarative sentences, constituting some nine-tenths of all discourse, is fundamental. In French and Spanish, for instance, the inherited sequence is SVO; in Latin SOV; in Celtic and Arabic VSO. In (2) interrogation and negation generally demand variations, as also, to a less extent, condition, concession, etc. In (3) it is the speaker's subjective attitude to what he wishes to communicate to and impress upon his hearer that determines sequence, e.g. *Out you go*. In (4) the innate love of rhythmic harmony leads to unusual order even in frequent everyday expressions like *all the year round* and *all the world over*. While appreciating Mr. Worth's valuable paper, I would suggest that we dispense with the term *suprasyntactics* altogether and that we retain *emphasis* as a useful general term in the sphere of semantics but never as a precise term in analytic syntax.

P. IVIĆ:

Prof. Buysens' implication that the negation and interrogation particles are obligatorily combined with the predicate and not the subject is not universally valid. In various dialects of South Slavic it is possible to say, e.g., *Ne Petar, već Marko dolazi* "It is not Peter, but Mark who is coming" or *Marko li dojde?* "Is it Mark who came?".

THE IMPACT OF LANGUAGE DATA PROCESSING ON LINGUISTIC ANALYSIS

PAUL L. GARVIN

A number of linguists have in recent years become increasingly involved in language data processing activities. The purpose of this paper is to assess the effect of this new area of interest on the field of descriptive linguistics.¹

From a linguistic standpoint, language data processing can reasonably be defined as the application of data processing equipment to natural language text. Of greatest interest is of course the application of computing machinery.

Language data processing can either serve linguistic ends, as in automatic linguistic analysis, or more practical ends, as in the fields of machine translation, information retrieval, automatic abstracting, and related activities. The latter can be summarized under the heading of linguistic information processing.

All areas of linguistic information processing are concerned with the treatment of the content, rather than merely the form, of documents composed in a natural language. This emphasis on content constitutes one of the major differences between this aspect of language data processing and the field of descriptive linguistics as presently constituted, where the main emphasis is on linguistic form (although more recently, descriptive linguists have become increasingly interested in problems of meaning.).

Within the field of linguistic information processing, a major division can, from a linguist's standpoint, be made between on the one hand machine translation and on the other hand information retrieval, automatic abstracting, and related activities. This division is based primarily on the manner in which the particular activity is concerned with the treatment of the content of the document. In machine translation, the major objective is one of recognizing the content of a document in order to render it in a different language. In the other activities, for which I have proposed the cover term content processing, recognition of the content is only the first step. More than simple rendition is required; the content of the document has to be processed further for some such purpose as the inclusion of its entirety or portions of it under index terms, or the retention of certain portions and rejection of others in order to create an extract or abstract. Content processing thus involves not only the recognition but

¹ Work on this paper was done under the sponsorship of the AF Office of Scientific Research of the Office of Aerospace Research, under Contract No. AF 49(638)-1128.

also the evaluation of content, since for both indexing and abstracting, pertinent relevance judgments have to be applied.

There are two areas of language data processing in which linguistic work has found serious application in fact as well as in theory. These are automatic linguistic analysis and machine translation. While automatic linguistic analysis is as yet still in the planning stage, some of the proposed approaches are set forth in sufficient detail to deserve discussion. Machine translation has progressed beyond the planning stage some time ago, though it is still far from being fully operational. In other areas of language data processing, linguistic contributions are as yet so ill-defined that their discussion from the viewpoint of this paper is premature.

In assessing the impact of language data processing on linguistics, the empirical principle as stated by Hjelmslev² provides three generally acceptable evaluation criteria by its stipulation of the requirements of consistency, exhaustiveness, and simplicity. It is the contention of this paper that language data processing has served to pinpoint the difficulties that are encountered in carrying out what everybody agrees on as a desirable goal: a maximally consistent, exhaustive, and simple linguistic description.

This is most evident in the case of consistency. In data processing, inconsistency is not merely undesirable, it carries a severe penalty: it is almost trivial to make the point that a computer program just will not run unless the set of instructions is consistent within itself. The linguistic information underlying the program must obviously be equally consistent. The avoidance of inconsistency therefore becomes an overriding operational objective. The means of meeting this objective is explicitness, since this is the mechanism by which inconsistencies are uncovered for correction.

Language data processing applications require the formulation of linguistic information with a degree of explicitness that is often not met in ordinary linguistic discourse. I should like to exemplify this from the area of automatic linguistic analysis.

One of the basic assumptions of my conception of automatic linguistic analysis is that linguistic techniques can be made computable. Let me here discuss the difference in explicitness between the verbal statement of a technique and its formulation for purposes of automation.

A technique which I have found extremely useful in the syntactic analysis of an exotic language is the dropping test, serving as an operational means of ascertaining the presence of a relation of occurrence dependence (one in which a unit A presupposes a unit B for its occurrence). At the Congress in Oslo, I defined this test as follows:

The procedure is what I have called dropping: that is, in an utterance containing both A and B (or both A, B, and C), omit one of the units and inspect the resultant truncated utterance.

For occurrence dependence, the dropping test will work as follows: A is dependent on

² See Louis Hjelmslev, *Prolegomena to a Theory of Language*, Francis Whitfield transl. (Baltimore, 1953), p. 6.

B in an utterance containing both, if an otherwise identical utterance, but from which A is dropped, is also occurrent in the text, or is accepted by the informant as viable. B is dependent on A if the utterance from which A is dropped is non-occurrent in the text, or is not accepted by the informant as viable.³

For the purposes of the linguistic analyst, "utterance" can be accepted as a common-sense behavioral unit, it can be assumed that units A and B will have been previously specified by some linguistic method, and the statements "occurrent in the text" and "accepted by the informant as viable" seem to be adequate enough descriptions of the conditions for the positive or negative result of the dropping test.

For purposes of automatic linguistic analysis (even though at present only the processing of text but not the computer simulation of informant work can be envisioned) all of the above factors have to be specified in considerably more detail in order to formulate a computer subroutine simulating the dropping test:

The dropping test can be simulated by an essentially cumbersome series of comparisons (which . . . can be simplified, once certain conditions are met). For each comparison, the [computer] routines will have to identify a pair of unequally long strings of elements, such that the longer of the two strings contains all the elements of the shorter one, plus one additional element. The one element present in the longer string and absent in the shorter one can be said to be 'droppable' from the longer one, if both strings have been found to recur in the text sufficiently frequently to allow the assumption that the difference in their length is not due to chance. For every identified longer string, the droppability of each element will have to be tested by finding an appropriate shorter string. Those elements for which shorter strings are not found in the input text can then be assumed not to be droppable, provided enough recurrences have been found so that the absence of a particular shorter string is not attributable to chance. In order to allow for the necessary recurrence, [an extremely] large input text would . . . be required. . . .

For a dropping routine to operate within the logically prescribed restrictions – that is, without an initial dictionary or grammar code – each trial would have to compare every string of n elements present in the input text to all appropriate strings of $n - 1$ elements. A series of passes could be envisioned, with the value of n increasing for each pass from a minimum of 2 for the first pass to the maximum found in the text, for the last pass. Assuming a punctuated text, this maximum value of n could be made very much smaller than the total number of elements in the entire text by requiring that no string be allowed to contain a period or other final punctuation mark – this would restrict the permissible length of a string to the span between two such punctuation marks, or between one such mark on one side, and the beginning or end of the entire text on the other. That is, the maximum permissible length of a string would be that of the longest sentence in the text.

The reverse procedure, in which the program first ascertains the maximum value for n and then decreases it with each pass, is equally thinkable.

Either procedure for applying the dropping test, to be carried to its logical conclusion, seems to require a program of quite unmanageable proportions.⁴

³ Paul L. Garvin, "Syntactic Units and Operations", *Proc. VIII Internat. Congress of Linguists* (Oslo, 1957), p. 629.

⁴ Paul L. Garvin, "Automatic Linguistic Analysis – A Heuristic Problem", *1961 Internat. Conf. on Machine Translation of Languages and Applied Language Analysis, National Physical Laboratory, Symposium No. 13* (London, Her Majesty's Stationery Office, 1962), p. 663.

Let me now compare the above description of the dropping subroutine for automatic linguistic analysis to the dropping test as practiced in linguistic field work.

The first observation that can be made is that the computer simulation of the test reduces itself to a series of comparisons of strings of unequal length. Next, it may be observed that the units to be tested for droppability are in both instances either observationally given or defined by prior procedure. In the case of automatic linguistic analysis, the units dealt with are simply printed English words which by virtue of being "observable" to the input mechanism become the proper units for processing by the program. The most significant difference between the test as used in field work and its automation lies, however, in the practice of the linguistic investigator of selecting particular utterances and particular units within these utterances as the objects upon which to perform the behavioral test. In the computer subroutine as described above, this is clearly not possible, and hence its execution would result in a computer run of quite unmanageable proportions. It is, in other words, necessary to explicitate not only the conditions of the test itself, but also to explicitate the set of conditions under which the test becomes capable of execution. In my plan for automatic linguistic analysis, I have attempted to do this by deferring the use of dropping routines in the analysis program to a stage in the process at which the conditions for its application have been created by the use of subroutines based on other linguistic techniques. In particular, I am proposing not to use dropping routines until after the program has led to the specification of certain linguistic classes and to use the dropping routines then to test for occurrence dependences of classes of words, which can be expected to be quite finite in number, rather than for occurrence dependences of individual words, the number of which can be expected to be unmanageably large.

The essential features of the dropping test, by virtue of which it is diagnostic of a dependence relation, thus remain unaltered when the test is automated. Some important attendant conditions, on the other hand, have to be adjusted to the constraints imposed by automation: firstly, form classes which in linguistic field work are implicit in the systematic similarities of questions to informants and informant responses, must be made explicit; secondly, field work allows the random access to the readymade store of the informant's memory, whereas in automatic linguistic analysis the necessary store of accessible forms has to be prepared by previous procedures.

Let me now turn to the questions of exhaustiveness and simplicity. I should like to discuss these on the basis of some illustrations taken from machine translation.

First, the matter of exhaustiveness. This is one of the most difficult requirements to define in linguistic analysis. I have commented on it in a previous context, and at that time I stated that a significant consideration is whether or not the analyst is dealing with classes of unrestricted or restricted membership.⁵ In the first case, exhaustiveness can only be achieved by a listing of classes, since obviously a complete

⁵ Paul L. Garvin, "On the Relative Tractability of Morphological Data", *Word*, 13 (1957), 22-3.

listing of an unlimited membership is self-contradictory. In the second case, a listing of not only all classes but also of all members is possible. This is, however, a purely theoretical definition of exhaustiveness. It does not touch upon the heart of the matter, which is the extent to which exhaustiveness can be achieved, or must be achieved, in a particular task of linguistic analysis and how the requirement can be met in practice. In machine translation, the program as was stated further above is to a significant extent based upon a linguistic description of the source language. Since the aim of machine translation research is to produce ultimately a program that will be capable of dealing with randomly selected text, the question of exhaustiveness is of extreme practical importance and has to be faced from the beginning. There are two areas in which the problem of exhaustiveness arises: first, the lexicon, where in machine translation the problem concerns the machine dictionary; second, the grammatical description of the language, where in machine translation the grammar code and the syntax routines of the translation algorithm are involved. The field has not yet progressed sufficiently to allow the inclusion of problems of semantic equivalence and multiple-meaning resolution in the present discussion.⁶

Operationally, the problem is that the research has to be conducted, and the system developed, in stages. This remains equally true if the point of view is adopted that a complete linguistic description must precede the development of a machine translation system; such a prior complete description must still be prepared gradually. In either case, research in stages means dealing with one linguistic problem area at a time, without violating the requirement of exhaustiveness. The planning of the research stages thus becomes the primary question.

The details of planning for exhaustiveness involve the following:

In machine translation research, it is not possible to use the convenient scholarly device of the "et cetera", or of suggesting, "If this technique is carried further, it will then allow the treatment of the rest of the data."⁷ The computer will not accept this kind of instruction. It is therefore necessary to make other provisions for exhaustiveness in spite of the limited scope of the dictionary and syntax program that the realities permit at any one given stage before the final aim has been achieved.

In regard to the machine dictionary, the question of exhaustiveness as to the number of dictionary entries is not of great theoretical interest, since it differs very little from the problem of exhaustiveness faced by lexicography in general. The only problem faced by machine translation researchers is that of having efficient procedures for

⁶ Don R. Swanson, "The Nature of Multiple Meaning", *Proceedings of the National Symposium on Machine Translation*. H. P. Edmundson, ed. (Englewood Cliffs, N. J., 1961), p. 386: "Now that the stage has been set in previous discussion by the picturing of polysemia as a 'monster' or a 'blank wall', let me say that there isn't a great deal more to be said about multiple meaning that isn't either obvious or else wrong. ..."

⁷ Cf. George L. Trager and Henry Lee Smith, Jr., *Outline of English Structure (= Studies in Linguistics, Occasional Papers No. 3)* (Norman, Okla., 1951), p. 55: "A full presentation would [include a complete description of phonemics, morphophonemics, morphology, and syntax]... No such full grammar is attempted here. The purpose is to present enough material for discussion to illustrate the procedures and techniques involved."

dictionary updating, and this problem has been solved satisfactorily by most groups.

The question of making provisions for exhaustiveness in the actual translation algorithm is much more interesting. It must first be noted that there are essentially two technical aspects to every computer program: first, a table-lookup procedure, in which the program looks up information in a table for use in later processing; second, a logical-tree-type algorithm, in which the program goes through a series of yes-no decisions (often based on information looked up in a table), in order to arrive at an appropriate end result. Provisions for exhaustiveness here consist essentially in writing a program that allows room for later additions as more information becomes known and can be included. The "et cetera" is replaced by a more explicit device. In the tables of a program, provisions for the addition of further information are made by leaving sufficient blank fields, that is, spaces to be taken up by later instructions and information. An important consideration in the design of the program then becomes the size of these fields that are to be left blank for later use. In a logical-tree-type algorithm, provisions for later additions can be made by building into the algorithm end points to which further branches can be added by which to treat information that may later turn out to be of importance. Thus, the linguist's statement that "additional data can be handled by the same technique" is replaced by an open-ended exit in the program.

In the machine translation program with which our department is operating, this can be exemplified by the blank fields that are contained in our grammar code for the addition of grammatical information for which we have planned, but which we have not yet been able to pin down sufficiently to include in the program. The open-ended exits are exemplified by the provisions of our syntax program to print out under certain conditions "notices of syntactic difficulty" whenever the algorithm for coping with such a difficulty has not yet been written.

Now to consider the matter of simplicity. I have on a previous occasion pointed out the difficulty inherent in a criterion of simplicity.⁸ At that time I raised the question of defining simplicity more clearly by stipulating whether or not it is to be gauged in terms of minimizing the inventory of units, or by minimizing the number of rules. It seems to me now that the question cannot be answered in the abstract. It is impossible to specify the simplicity of a sequence of procedural steps or of the logical structure of a description in an objective way. It is too much a matter of esthetics.

It is not unreasonable, on the other hand, to attempt to specify simplicity in terms of the attainment of a particular aim, such as efficiency in the use of equipment. I should like to exemplify this by a brief discussion of the grammar code used in our machine translation program.

The purpose of our grammar code is to provide all the grammatical information that is required for the efficient operation of the syntax routines. One important task

⁸ Paul L. Garvin, review of *Prolegomena to a Theory of Language* by Louis Hjelmslev, in *Language*, 30 (1954), 70.

of these routines is to carry out an agreement check, that is, to ascertain on the basis of the grammar codes that have been furnished to the program whether or not certain adjacent words are in grammatical agreement with each other, such as for instance a noun and preceding adjectives. For purposes of maximum efficiency of operation, we have devised a grammar code which takes up more space in the computer memory but allows the rapid completion of agreement checks by a computer operation similar to ordinary subtraction of one digit at a time, an operation called "masking". The drawback of this type of grammar code is that it takes up a considerable amount of memory space. From the standpoint of storage, a grammar code compressed into the minimum amount of space is obviously vastly preferable.

It turns out, therefore, that in this particular case the requirement of simplicity has to be formulated quite differently in terms of the different purposes to which a particular set of elements is put. For purposes of the operation of agreement checks, a grammar code spread out over more space but allowing rapid completion of the check, is the most efficient and consequently the simplest. For purposes of saving memory space, the maximally condensed code is most efficient. It is thus possible to formulate a requirement of simplicity – if one equates efficiency with simplicity – quite clearly in terms of a particular purpose.⁹

The above discussion may explain why I have come to regard language data processing a very important application of linguistics. It is a challenge to linguistics as a science. The challenge is not theoretical, but operational – it is directed at both the methods and the results of linguistics. The strong requirement of exhaustiveness forces the treatment of minor subpatterns of a language, and not merely of its major patterns.

By enforcing its requirements, the computer has become an analytical instrument for linguistics, where previously only recording instruments were available. This may have important theoretical implications.

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⁹ We use both solutions in our machine translation program. We store the grammar code in its condensed form, and we have a simple subroutine which transforms the condensed grammar code into the spread-out grammar code for use in agreement checking.

STATISTICAL DETERMINATION OF ISOTOPY

HENRY KUČERA

This paper is intended as an outline of a procedure of linguistic analysis which aims at establishing a more accurate descriptive method for a phonemic typological comparison of languages and which shows some potential of leading to new approaches in historical linguistics as well. Because of space limitations, only the principal methodological features are described here; a more detailed discussion of the procedures and of the project in which they were developed and tested is in preparation.¹ This paper is divided into five sections: 1. an introductory statement; 2. definition of principal concepts and terms, primarily of isotopy and isomorphy; 3. the description of the analytical procedures used and of the linguistic material employed; 4. the derivation of the isotopy index of Russian and Czech; 5. some comparisons as tests of the method.

1. The emphasis in typological classification of languages has been predominantly on morphological criteria, and it is on this basis that various classificatory typological schemes have been developed. Such typological classifications have certainly accentuated the linguist's awareness of the fact that morphological patterns and types are not very durable features in the diachronic profile of a language; classifications of languages as to morphological types and as to genetic relationships are often drastically different. The phonemic level, on the other hand, is subject to more regularly patterned and, at the same time, less rapid change in time, and phonetic correspondences have thus become the principal operational tool of historical linguistics. Synchronic phonemic typology would thus appear to have a much closer correlation with the genetic classification of languages and could perhaps even provide significant clues in historical research. In spite of its promising role as a link between synchronic and diachronic linguistics, phonemic typology has been limited to relatively elementary analyses. Most such studies have been concerned with the comparison and classification of phonemic inventories. Valuable as such information is, it barely touches the surface of the problem. It is self-evident that two languages may have very similar

¹ A significant portion of this research was done during my tenure as a Guggenheim Foundation Fellow. I was also aided by a grant from the Howard Foundation and by an appointment as IBM Research Associate at the M.I.T. Computation Center.

or identical inventories of phonemes, differentiated even by the same distinctive features, and still be quite far apart in many distributional and combinatory aspects of their phonologies, such as constraints on the occurrence of phonemes in specified positions of relevant segments (syllables, words), restrictions on phonemic sequences, or relative frequency of occurrence of individual phonemes and phoneme strings. Moreover linguists, while paying a great deal of attention to positional distribution of phones (the concept of complementary distribution is, after all, one of the cornerstones of phonology), have viewed it primarily in terms of the opposition *can occur vs. cannot occur*, i.e. zero probability as opposed to all non-zero probabilities of occurrence. Again, substantial differences in relative frequency of occurrence of two phonemes which are both admissible in a specified distributional position are obviously very relevant in the total descriptive view of phonological systems. It is thus important to introduce into comparative analysis some criterion which would measure the differences among non-zero probabilities.

In Russian and Czech, for example, the affricates /c/ and /č/ are quite comparable in terms of distinctive features and, by and large, in terms of their distributional properties, if these are described non-quantitatively. However, a comparison of the relative frequencies of these phonemes in the two languages shows substantial differences. In Russian, the mean frequency, per 1000 phoneme occurrences, of /c/ is $\bar{x} = 5.62$, and of /č/ is $\bar{x} = 16.33$. In Czech, the order of magnitude of the frequency of the affricates is reversed, and /c/ has $\bar{x} = 13.69$ while /č/ has $\bar{x} = 9.91$. Or another illustration: if the relative frequencies of the phoneme /j/ in the two languages are compared, their ratio is less than 4:3 in favor of Russian ($\bar{x} = 41.38$ in Russian, $\bar{x} = 33.27$ in Czech). However, if one compares the frequency of /j/ in various positions within a phonological word, a substantially more complex and, at the same time, more revealing situation comes to light. In initial word-position if directly before a vowel, /j/ is almost twice as frequent in Czech as in Russian ($\bar{x} = 8.13$ in Russian; $\bar{x} = 14.48$ in Czech). On the other hand, directly after a vowel in final word-position, /j/ in Russian has $\bar{x} = 11.90$, in Czech $\bar{x} = 1.37$, i.e. a ratio of almost 9:1 in favor of Russian.

A reasonable diachronic explanation of these examples can be readily found; in the illustrated cases, the synchronic observation only confirms well known historical developments. But it is easily conceivable that synchronic analysis of a similar nature may, in other instances, furnish important clues in historical linguistic research.

Any investigation which aims at a quantitative description and comparison of distributional and combinatory aspects of phonological systems requires, of course, the examination of large corpora of linguistic data and many complex and tedious statistical counts. The availability of digital computers has, for the first time, made such detailed examination of languages feasible. This paper attempts to outline a method of quantitative phonemic typology which takes into account some of the most important distributional and combinatory criteria. Standard contemporary Russian and Czech served as the material for the development of the procedures.

2. *Principal concepts*: The following concepts and terms are central to the methodology: distributional position and phonological syllable, isotopy, and isomorphy.

2.11 *Distributional position* is the place of a phoneme, defined in relation to a specified environment, which the phoneme occupies in the string; environment may be stated either in terms of other phonemes which precede or follow in the string, or in relation to the boundaries and components of a relevant linguistic segment, such as syllable, word, or phrase. In this paper, distributional position is defined relative to the boundaries and the structure of the phonological syllable.

2.12 The concept of the *phonological syllable* utilized here is substantially that suggested by Hockett² and defined subsequently by Haugen as "the smallest unit of recurrent phonemic sequences"³ (including, under the notion of phonemic, not only segmental but also prosodic and configurational elements). The phonological syllable has the following structure: onset + nucleus + coda; these are the *constituents* of the syllable. The *nucleus* is the irreducible minimum present in any recurrent phonemic unit; the optional remainder constitutes the *margin* of the syllable. *Onset* is the portion of the margin between a disjuncture⁴ and the first following nucleus, *coda* the portion between a nucleus and the subsequent disjuncture. The margin between two nuclei, uninterrupted by any occurrence of disjuncture, is the *interlude*; interludes are divided into subsequent codas + onsets by a procedure described below.

A syllable constituent may be represented by one phoneme or by a cluster of phonemes. Russian and Czech onsets, for example, may consist of one, two, three or four phonemes, or be absent entirely. The four different *constituent types* which are thus possible in a non-zero onset are designated as ON1, ON2, ON3, and ON4. Each constituent type has a specific number of slots which are filled by segmental phonemes; the slots are the *syllabic positions* and the set of phonemes which can fill a slot is the *membership* of the position. Margin positions are differentiated by alphabetic notation, A being the position nearest to the nucleus, B the next nearest, etc. The notation ON3A thus refers to the syllabic position immediately preceding the nucleus in an onset consisting of a three phoneme cluster.

Membership of different syllabic positions may vary substantially, even in the case of positions which are in the same place relative to the nucleus. In Czech, for example, the position immediately preceding the nucleus in a one-position onset (ON1A) has a membership of 25 phonemes; on the other hand, the position immediately preceding the nucleus in a four-position onset (ON4A) has a membership of only six phonemes.

An interlude is divided into coda + onset by two ordered rules: (a) the division must not enlarge the membership of any onset or coda position; (b) if the first rule

² C. F. Hockett, *A Manual of Phonology* (Baltimore, 1955).

³ E. Haugen, "The Syllable in Linguistic Description", *For Roman Jakobson* (The Hague, 1956), p. 216.

⁴ An explanation of the concept of disjuncture, as employed here, can be found in my book *The Phonology of Czech* (The Hague, 1961), pp. 43-65.

can result in more than one interlude division or if it does not provide a division, interludes are divided as follows: one-position interludes are considered as onsets, two-position interludes are divided as 1 + 1, three positions as 1 + 2, four positions as 2 + 2, five positions as 2 + 3, and six-position interludes as 3 + 3. No seven-position or longer interludes were found in the Russian and Czech samples analyzed.⁵

The syllabic structure of Russian and of Czech can be represented by the following scheme:

$$\begin{array}{cccc}
 & & \text{ON1A} & \left. \vphantom{\begin{array}{c} \text{ON1A} \\ \text{ON2B} \\ \text{ON3C} \\ \text{ON4D} \end{array}} \right\} \\
 & & \text{ON2B} & \text{ON2A} \\
 & & \text{ON3C} & \text{ON3B} & \text{ON3A} \\
 \text{ON4D} & \text{ON4C} & \text{ON4B} & \text{ON4A} & \left. \vphantom{\begin{array}{c} \text{ON1A} \\ \text{ON2B} \\ \text{ON3C} \\ \text{ON4D} \end{array}} \right\} \text{N} \left\{ \begin{array}{l} \text{CO1A} \\ \text{CO2A} \quad \text{CO2B} \\ \text{CO3A} \quad \text{CO3B} \quad \text{CO3C} \\ \text{CO4A} \quad \text{CO4B} \quad \text{CO4C} \quad \text{CO4D} \text{ (only Russian)} \end{array} \right.
 \end{array}$$

Czech has no CO4's. The membership of onsets and codas, in both languages, consists of consonants and the semi-vowel /j/. The membership of the nucleus consists, in Russian, of a single vowel; in Czech, of a single vowel, a two-vowel chain, or /r/, /l/ and /m/. The nucleus is the irreducible part of the syllable, but either onset or coda or both can be zero. Both Russian and Czech also contain phonological segments which do not constitute syllables (consonantal prepositions and prefixes such as /v/, /s/, etc.) but which it is best to consider separately in the analysis. These non-syllabic segments, referred to here as *isolated consonantal microsegments*, are treated as separate syllabic positions in the determination of isotopy.

2.2 Isotopy. Two phonemes which can occur in identical distributional positions are said to be isotopic. The *isotopy index I* is a statistical measure based on the difference in the occurrence probability of isotopic phonemes which are being compared. The greater the difference between the occurrence probability of two phonemes in comparable positions, the smaller the isotopy index and vice versa. A formula for the computation of an overall isotopy index of two phonological systems is suggested in section 4.⁶

2.3 Isomorphy. The comparison of two phonemes of different languages obviously requires a measure of their phonemic similarity which is referred to here is the *measure of isomorphy M*. This measure is based on the matrix of distinctive features of the two systems; the fewer the distinctive features in which the corresponding phonemes differ, the greater will M be. The isomorphy measure (cf. 4.2) is expressed in this paper on a scale from 0 (no isomorphy) to 1 (identity).

⁵ The second rule favors an interlude division which results in the most common constituent types; commonness is defined in terms of the distinct phonemes and phoneme clusters of a given type before interludes are divided. Cf. also J. D. O'Connor and J. L. M. Trim, "Vowel, Consonant, and Syllable — A Phonological Definition", *Word*, 9 (1953), pp. 103–122.

⁶ The isotopy index I is in actuality not only a measure of isotopy but also of isomorphy (cf. 2.3), i.e., both a quantitatively distributional and a qualitative measure of the typological proximity of the two systems. It would be perhaps more appropriate to call I an isotopy/isomorphy index or even create for it such a name as iso-index. For reasons of simplicity on one hand and of terminological descriptiveness on the other hand, the term *isotopy index* is used in this paper as a compromise.

3. *Material and analysis.* The linguistic data in the project consisted of a Russian sample of 100,000 phonemes and a Czech sample of 100,000 phonemes; both samples were selected at random from twentieth century texts. No effort was made to choose Russian and Czech samples which would be translations of each other, but the general character of the two samples was made comparable: approximately 60 per cent of the data was from belles-lettres prose, 20 per cent from journalistic prose, 10 per cent from poetry and 10 per cent from scholarly and scientific publications. The data were transferred onto punched cards in conventional orthography with numerals and some special characters used for those graphemes for which there were no alphabetic characters available on the key-punch. The Russian text was punched in transliteration in Latin characters. Two computer programs for the IBM 7070 data-processing system were then written which contained the algorithms for the transformation of the graphemic representation of Russian and of Czech into consistent phonemic representation. Since both orthographies are morphophonemically oriented, it was possible to write computer programs which performed the transformation accurately, although the Russian program, especially, turned out to be quite complicated. Moreover, some pre-editing of the graphemic data had to be done before it was transferred to the punched cards. This included marking of prepositional and prefix boundaries which the program then interpreted as internal disjunctures, and, when punctuation did not provide an adequate clue, the marking of phonemic phrase boundaries to be interpreted as terminal disjunctures. This information was essential because the distribution of some distinctive features (voiced vs. voiceless, sharp vs. plain) can be affected by disjunctures. In Russian, vowel accent marks also had to be supplied. Pre-punching markings also had to be entered in the case of graphemic irregularities, occurring especially in loan words. The grapheme-to-phoneme transformation programs, in addition to performing the phonemic analysis, also counted the frequency of each individual phoneme per each 1000 phoneme occurrences, generated a table of all phonemic digrams found in the samples and counted their frequency per each 10,000 phoneme occurrences. The phonemic transcription was written on magnetic tape but could be also printed on the on-line printer if desired; the results of the frequency counts were punched on the on-line card-punch. The phonemization and the statistical counts, including all the printing and punching of results, were performed by the machine at the rate of approximately 1000 phonemes per minute.

Subsequent computer programs were written which used the phonemic transcription on magnetic tape as input; these programs analyzed the phonemic string into syllables, in accordance with the principles given in 2., generated tables of all the phonemes and phonemic sequences for each syllabic constituent and computed the frequency for each such table item.

4. *Isotopy index of Czech and Russian.*

4.1 With the aid of an appropriate computer program, the frequency of occurrence

of each phoneme in each constituent position of the phonological syllable was ascertained. Altogether 24 such positional frequencies were thus computed for each phoneme: N, ON1A, ON2A, ON2B, ON3A, ON3B, ON3C, ON4A, ON4B, ON4C, ON4D, CO1A, CO2A, CO2B, CO3A, CO3B, CO3C, CO4A, CO4B, CO4C, CO4D, ISOL1A, ISOL2A, ISOL2B (isolated consonantal microsegments can contain one or two positions, designated as A and B from right to left). In some instances, of course, the frequency of a phoneme in certain positions was zero; in Czech, all CO4's have zero frequencies.

4.2 The phonemes of Russian and Czech were then grouped into isomorphic sets in such a way that the resulting sets had the maximum possible isomorphy. The underlying principle of the grouping procedure was the distinctive feature analysis: the Russian and the Czech distinctive feature matrices were compared and, first of all, those phonemes grouped into isomorphic sets which, in both matrices, had identical feature specifications. This creates, for example, the isomorphic set consisting of R. /č/ and Cz. /č/ which are both identified, in their respective matrices, as non-vocalic, consonantal, compact, non-grave, non-nasal, non-continuous, and strident.

The remaining phonemes were then grouped with those phonemes of the other language from which they differed by the smallest number of distinctive feature specifications. This principle sometimes resulted in the grouping, in one isomorphic set, of more than one phoneme of one language with a single phoneme of the other language. For example, R. /p/ and R. /p'/, and Cz. /p/ constitute one isomorphic set; all three are non-vocalic, consonantal, non-compact, grave, non-nasal, non-continuous, and voiceless. However, the Russian /p/ and /p'/ are also differentiated as plain vs. sharp, i.e. by a feature which is not relevant in the Czech system.

The isomorphy measure M for each set is given by

$$M = (1 - \frac{d}{f}) \quad (E1)$$

where d is the number of distinctive features by which the Russian subset and the Czech subset of the isomorphic set differ; f is the maximum number of features needed to define that phoneme, in either of the two languages under comparison, which requires the largest number of distinctive features for its identification of all phonemes in both matrices. It should be noted that f is not necessarily the total number of features found in either matrix, nor is it the number of features needed to define the specific phonemes for which M is being computed. Once f has been determined for a pair of languages, it remains constant in the computation of M of all isomorphic sets. For Russian and Czech, $f = 8$.

Returning to the previous examples, we now find that for the set consisting of R. /č/ and Cz. /č/,

$$M = 1 - \frac{0}{8} = 1$$

and for the set consisting of R. /p/ + R. /p'/ and Cz. /p/,

$$M = 1 - \frac{1}{8} = 0.875.$$

If a distinctive feature is suspended in certain distributional positions (as the feature sharp vs. plain is suspended in some consonantal clusters in Russian, for example), this fact is taken into account in the computation of *M*.

4.31 The Russian and the Czech subsets of each isomorphic set were then assigned the probabilities of occurrence of the member phonemes for each constituent position of the syllable. In case that either subset consisted of more than one phoneme, the probabilities of all the phoneme members of the subset were totaled. For example: In the determination of the probabilities to be assigned for position ON1A in the isomorphic set consisting of R. /p/ (which in ON1A has the probability of occurrence 0.01357) + R. /p'/ (which in ON1A has the probability 0.00428), and of the Cz. /p/ (with the probability 0.01605 in ON1A), the probability value assigned to the Russian subset $p(R) = 0.01357 + 0.00428 = 0.01785$, that assigned to Czech subset $p(C) = 0.01605$.

4.32 The *isotopy index* of Russian and Czech is given by

$$I = \sum_{i=1}^n \left\{ \frac{p(R_i) \cdot p(C_i)}{\left[\frac{p(R_i) + p(C_i)}{2} \right]^2} \cdot \frac{p(R_i) + p(C_i)}{2} \cdot M_i \right\}$$

$$= \sum_{i=1}^n \frac{2p(R_i) \cdot p(C_i) \cdot M_i}{p(R_i) + p(C_i)}$$

(E2)

where *n* is the total number of isomorphic sets established for the two languages, multiplied by the number of syllabic positions.⁷

There are 29 isomorphic sets and 24 syllabic positions (cf. 4.1) specified for the Russian/Czech pair, which gives $n = 696$. In some isomorphic sets, of course, either $p(R_i)$ or $p(C_i)$ or both may be zero; all CO4 positions, for example, have $p(C_i) = 0$. Isomorphic sets consisting of vowels have $p(R_i) = p(C_i) = 0$ for all but the N position; $p(R_i) = 0$ in N position for all Russian subsets containing a consonant.

4.33 An examination of (E2) makes it apparent that *I* depends on three factors:

- (a) on M_i (cf. 4.2) which can assume values from 0 to 1.
- (b) on the difference between $p(R_i)$ and $p(C_i)$; the expression

$$\frac{p(R_i) \cdot p(C_i)}{\left[\frac{p(R_i) + p(C_i)}{2} \right]^2}$$

⁷ A somewhat similar measure was used, in the analysis of Russian morphemes, by R. H. Abernathy in *The Structure of Russian Roots* (Ph.D.dissertation, Harvard, 1951).

will reach its maximum value of 1 only if $p(R_1) = p(C_1)$, and will decrease as the difference between $p(R_1)$ and $p(C_1)$ increases and reach zero when either or both p values equal zero.

(c) on the ratio of the summed probabilities in corresponding positions of a given isomorphic set to the sum of the probabilities of all Russian subsets (which is 1) plus that of all Czech subsets (which is also 1), i.e. on the ratio $p(R_1) + p(C_1) : 2$. The greater this ratio, the greater the effect which the particular sum $p(R_1) + p(C_1)$ will have on I . If both $p(R_1)$ and $p(C_1)$ are zero, they have no effect on I . It should be noted that the values of I will be always between 0 and 1.

The isotopy index of Russian and Czech, based on probabilities derived from samples of 100,000 Russian and 100,000 Czech phonemes, is

$$I = 0.77221670$$

5.1 The isotopy index expressed by (E2) attempts to take into consideration the variables which can be viewed as the most significant factors in the determination of the "similarity" of two phonological systems. The isotopy index is, of course, a purely relative measure and its usefulness as a typological indicator or its potential as a tool of historical comparative research can be assessed only after it is applied to a significant number of language pairs. Such a project is now in progress at the Brown University Computing Laboratory. Some refinements in the isotopy determination, such as the introduction of transition probabilities of phonemes into the procedure, are also being considered.

5.2 Nevertheless, in order to be able to make some judgement about the value of I for Russian and Czech, three artificial phonological systems X , Y , and Z were constructed and an isotopy index computed for Russian/ X , Russian/ Y , and Russian/ Z . The following examples illustrate the notation used in describing these three constructs: $P_x(f)$ is the occurrence probability of the phoneme $/f/$ in construct X , $p_y(f_{ON1A})$ the occurrence probability of the phoneme $/f/$ in position ON1A in construct Y , and $p_z(CO2B)$ the probability of position CO2B being present in a non-zero syllable margin in construct Z .

X , Y , and Z have the same phonemic inventory as Czech and the same syllabic structure, i.e. syllables consisting of onset with a maximum of four positions + nucleus + coda with a maximum of three positions; ISOL positions are also assumed in all three constructs.

5.21 In X , only vowels can occur in a nucleus position, only consonants in margins. Each phoneme has the same probability of occurrence as the corresponding phoneme in Czech but the occurrence probabilities of syllabic positions in margins are all equal, i.e. $p_x(ON1A) = p_x(ON2A) = p_x(CO1A)$, etc. In this last respect, X differs from Czech where the probability of a given position being present in a syllable margin is as follows:

$$p_c(ON1A) = 0.55517, p_c(ON2A) = p_c(ON2B) = 0.10019, p_c(ON3A) = p_c(ON3B) = p_c(ON3C) = 0.00767, p_c(ON4A) = p_c(ON4B) = p_c(ON4C) = p_c(ON4D)$$

$= 0.00005$, $p_c(\text{CO1A}) = 0.19041$, $p_c(\text{CO2A}) = p_c(\text{CO2B}) = 0.00611$, $p_c(\text{CO3A}) = p_c(\text{CO3B}) = p_c(\text{CO3C}) = 0.00002$, $p_c(\text{ISOL1A}) = 0.01679$, $p_c(\text{ISOL2A}) = p_c(\text{ISOL2B}) = 0.00088$

Consequently $p_x(f_{\text{ON2B}}) = P_x(f) \cdot p_x(\text{ON2B})$ where $P_x(f) = P_c(f)$ but $p_x(\text{ON2B}) \neq p_c(\text{ON2B})$.

The isotopy index of Russian and X is

$$I = 0.45597325$$

5.22 In Y, only vowels can occur in a nucleus position, only consonants in margins. The occurrence probability of a position in a syllable margin is the same as that of the corresponding position in Czech but the probabilities of all phonemes are identical, e.g. $P_y(b) = P_y(d) = P_y(s)$. The occurrence probability of the phoneme /f/ in position ON3A is then determined by $p_y(f_{\text{ON3A}}) = P_y(f) \cdot p_y(\text{ON3A})$ where $p_y(\text{ON3A}) = p_c(\text{ON3A})$, but $P_y(f) \neq P_c(f)$.

$$I (\text{Russian/Y}) = 0.69976705$$

5.23 In Z, the consonant and vowel distribution as well as the occurrence probabilities of phonemes and syllabic positions are the same as in Czech. In contrast to Czech, however, the occurrence probability of a phoneme in a given syllabic position is the same as its overall probability in Z. Consequently, $p_z(f_{\text{CO2A}}) = P_z(f) \cdot p_z(\text{CO2A})$, where $P_z(f) = P_c(f)$, and $p_z(\text{CO2A}) = p_c(\text{CO2A})$.

$$I (\text{Russian/Z}) = 0.73860365$$

5.3 The fact that none of the last three isotopy indices reaches the value of I for Russian and Czech is an indication of the significant typological relationship of the two languages. None of the I values for Russian/construct represents, of course, a chance correlation of two phonological systems. I of such a chance correlation would be much smaller than any of those obtained here and would depend on the limitations on syllabic structure and phonemic repertory which would be introduced into its determination.

5.4 Preliminary indications are that the isotopy approach may have some interesting possibilities in the investigation of genetic relationship of languages and in measuring the dynamism of phonological change. Further experiments are necessary, however, before any such usefulness can be claimed.

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CONTENT RECOGNITION AND THE PRODUCTION OF SYNONYMOUS EXPRESSIONS

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1.0 The Linguistics Research Center of The University of Texas is developing several models for machine translation. It is the purpose of this paper to explain in part certain aspects of the linguistic work now in progress and the direction of the work projected for the future.¹ The efforts of the linguistics, mathematics and programming groups of the Center are so directed as to develop the most general translation schemes possible. The computer programming group is developing a system of programs which allow the linguist to work independently of considerations for the computer. The mathematics group is concentrating on formal proofs of the working assumptions of the linguistics group. In this way we hope to anticipate and circumvent unduly expensive operational problems with the computer.²

2.0 To prepare a language for use in the translation system, we begin with the description of its phrase structure. We accomplish this by writing a grammar of replacement type rules of the general form $X \rightarrow Y$ where X is interpreted as a syntactic class name, the symbol \rightarrow is read as "may replace/may be replaced by". Y represents either a sequence of alphabetic characters, one or more syntactic class names or a combination of both alphabetic characters and class names. Thus, we provide replacement rules of the two basic types: terminal and non-terminal. Terminal rules are those of the form $X \rightarrow a$, where the lower case a represents an alphabetic expression. The non-terminal rules are represented by rules such as the following

$$\begin{aligned} P &\rightarrow Q^1 \\ X &\rightarrow Y^1 + Z^2 \\ M &\rightarrow N^2 + b + P^1 \end{aligned}$$

The upper case symbols denote variable, syntactic classes.

2.0.1 As in other systems utilizing replacement rules, the restriction on replac-

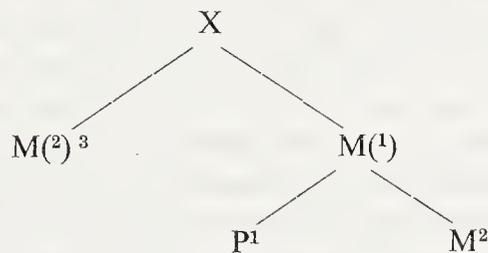
¹ Support for work on this paper was provided by the U.S. Army Signal Corps.

² For a more detailed accounting of all aspects of the work at the Linguistics Research Center see *Machine Language Translation Study*, quarterly progress report 8 and later, 1 February 1961 —, The University of Texas. Support for work in the various areas of investigation is provided by the National Science Foundation and the U.S. Army Signal Corps.

ing one of the right-hand variable elements of a rule with the right-hand element(s) of another rule is that the right-hand variable being replaced must be identical with the left-hand element naming the elements being substituted. In the phrase structure of the languages which we are analyzing, we require that an additional condition be met. Any rule to be inserted must be substituted at the lowest order superscript. For instance, suppose we are given the following set of rules:

$$X \rightarrow M^2 + M^1, \quad M \rightarrow P^1 + M^2$$

The second rule must be substituted into the first rule at M^1 . After the substitution has been made, a branching structure diagram would have the appearance below.



Each time a substitution is made the superscripts associated with the variable classes are re-evaluated in the following manner. The right-hand elements of the inserted rule retain their original superscript values, and the unfilled variables of the rule substituted into have their superscript values increased by $n - 1$, where n is the number of variables in the right-hand side of the inserted rule. The parentheses above show the original superscript values of the first rule. The numbers without parentheses denote the superscript values resulting from the substitution. If we were to make further substitutions into the above network, the next rule would have to be insertable at the variable element P^1 .

2.0.2 The superscripts associated with the right-hand variable elements of non-terminal rules have another function as well as that of specifying the order of substitution. In the two rules $X \rightarrow Y^1 + Z^2$ and $M \rightarrow N^2 + b + P^1$, we find a like number of variable elements. Superscript 1 denotes the correspondence of semantic content for the respective variables Y and P . Superscript 2 likewise denotes a similar relation between the variables Z and N . In other words, the superscripts guarantee that the same semantic content found to fill one variable slot will be substituted into only those other variable slots which are appropriate correspondents.

2.1 Any two or more rules which stand in this semantically equivalent relation to one another are said to be in the same semantic equivalence class. We require that all rules in the same equivalence class be composed of the same number of variable elements. The variable elements of rules in the same equivalence class and bearing the same superscript do not necessarily have identical syntactic class names.

2.1.1 Since an equivalence class must contain only rules which have the same number of variable elements, we have thus specified a particular kind of transfor-

mation. We have established the relation which allows for the permutation of elements, whether the respective correspondents have identical class names or not, and for the correlation of semantic content among correspondents having the same class names, whether or not the corresponding elements are permuted.

2.1.2 Equivalence classes of non-terminal rules provide us with variety in the order of expressions. Equivalence classes of terminal rules provide variety of expression in that they allow us a choice of one or more morphs or "words".

2.1.3 As the terminal expressions are initially classified in the translation system, each terminal rule is placed in a unique equivalence class. Whenever two expressions, or more precisely, terminal rules are found to be mutually substitutable in some context, each rule is assigned to the other's equivalence class. For example, suppose the rules $A: N_x \rightarrow$ abridgement and $B: N_y \rightarrow$ abstract are found to substitute for one another in some context. The upper case symbols which are in italics denote the equivalence class name of each rule. If the rules are substitutable we assign the rule $N_y \rightarrow$ abstract to equivalence class A , as well as to B , and $N_x \rightarrow$ abridgement to equivalence class B , as well as to A . Class assignments made on this basis will not necessarily result in the two classes A and B containing the same list of member rules. A representative list of the rules in A might be as follows:

$A:$	$N_x \rightarrow$ abridgement	$N_b \rightarrow$ digest	$N_f \rightarrow$ contraction
	$N_y \rightarrow$ abstract	$N_c \rightarrow$ abbreviation	$N_g \rightarrow$ truncation
	$N_z \rightarrow$ condensation	$N_d \rightarrow$ curtailment	$N_h \rightarrow$ summarization
	$N_a \rightarrow$ brief	$N_e \rightarrow$ shortening	$N_i \rightarrow$ reduction

i.e., the rules substitutable for $N_x \rightarrow$ abridgement. Similarly, a representative list of member rules for equivalence class B might read:

$B:$	$N_y \rightarrow$ abstract	$N_b \rightarrow$ digest
	$N_x \rightarrow$ abridgement	$N_j \rightarrow$ essence
	$N_z \rightarrow$ condensation	$N_k \rightarrow$ distillation
	$N_a \rightarrow$ brief	$N_l \rightarrow$ extract

i.e., the rules substitutable for $N_y \rightarrow$ abstract. Every rule in the grammar will belong to at least one equivalence class.

2.1.4 Whenever a rule is found to apply in the analysis of an input expression, this fact is registered along with the information as to what other rules are members of the same equivalence class, i.e., are substitutable for the given rule. For output, then, we are able to call not only upon this rule but all the rules in the same equivalence class, assuming that we are synthesizing equivalent expressions in the same language as the input. Not all such rules should be considered as likely candidates for substitution. Therefore, we must provide some way of establishing a preference for selecting among the various alternatives.

2.2 We are able to provide a measure of synonymy between any two rules by

considering the set of all equivalence classes in which either rule occurs. The synonymy S is given by

$$S = \frac{N(C_x \cap C_y)^3}{N(C_x \cup C_y)}$$

C_x is the set of all equivalence classes in which, say, the rule

$$N_x \rightarrow \text{abridgement}$$

occurs. C_y is the set of all equivalence classes in which the rule

$$N_y \rightarrow \text{abstract}$$

occurs. If we compare each of the above rules which are substitutable for

$$N_x \rightarrow \text{abridgement}$$

against this rule in the manner indicated, we arrive at numerical values for each of the rules such that $0 \leq S \leq 1$. Any rule compared with itself will give the value for identity, i.e., 1. Rules which stand in the relationship of being allomorphs will have the value 1. Rules which are not mutually substitutable will have the value 0 with respect to each other. All the rules which are in equivalence class A can thus be assigned a value between 0 and 1 which will allow us to establish a preference among the various synonyms for the expression *abridgement*.

3.0 None of the above computations are a part of the process of translation as such. Rather, such computation is carried out on the linguistic data as they are compiled for use in the system. The linguist is not himself concerned with making these computations. They are carried out by the compiling facilities of the program system. Once the computations are made, the values are available at all times for the translation process. The values are not absolute, since they depend on the size of the grammar and the partitioning of rules into equivalence classes. They are constant, however, within the grammar for any one language and at any one time.

4.0 In order to carry out the transfer of information from one language to another, we establish a table of interlingual equivalence classes which give the correspondences among semantically equivalent equivalence classes of the several languages in the system. This is to be accomplished in essentially the same manner in which we set up equivalence classes of rules. Let us consider again the rule

$$N_x \rightarrow \text{abridgement}$$

We shall denote its equivalence class as A_e to indicate that it is an English equivalence

³ Cf. "Work in Mathematics", *Machine Language Translation Study* (= *Quarterly Progress Report* 9), p. 18.

class name. Suppose we find that the English expression may be translated into French by the rule

$$L_f: N_q \rightarrow \text{abrégé}$$

and into German by

$$M_g: N_s \rightarrow \text{Abkürzung}$$

We place each of the above equivalence classes A_e , L_f , M_g in a unique interlingual equivalence class 01, 02, 03, respectively. Since the above rules are considered translation substitutions for one another, we have the respective assignments:

$$01: A_e, L_f, M_g$$

$$02: L_f, A_e, M_g$$

$$03: M_g, A_e, L_f$$

So far, the membership of the three interlingual equivalence classes appears to be identical. But if we continue building up the membership assignments as we did above, with rules in equivalence classes, we will arrive at similar results. Likewise, we may state a preferential ordering of one equivalence class compared to another equivalence class by considering the set of all interlingual equivalence classes to which the two belong. The computation of such "synonymy" values is similar to that for rules as described above.

5.0 Briefly, the translation process functions as follows. A certain set of rules is found to apply in analyzing the text of the input language. The equivalence classes to which each of the applicable rules belongs, respectively, are recorded. The interlingual equivalence classes to which each equivalence class belongs are likewise recorded. Assuming that we have selected the language into which we want to translate, we transfer on a one-one basis from the given equivalence classes through the interlingual classes to the equivalence classes of the target language. We pick the latter set of classes on the basis of highest synonymy values and in turn select rules from each of these equivalence classes on a similar basis. The resulting synthesis is a close translation of the original. We may refer to this kind of translation as a rule-for-rule translation.

6.0 The translation model described thus far consists only of the phrase structure descriptions of the various languages in the system. It takes into account only the overt, formal characteristics such as inflection, case and number agreement, etc. Furthermore, it provides only for transformations of the type which permute elements. It does not take into account transformations which delete or add elements. Nor does it provide for any restrictions¹ such as semantic agreement, e.g., animate actor with animate verb.

7.0 We may provide for these additional requirements by expanding our model.

We shall refer to the grammar which describes the phrase structure of a language as the first order grammar. The second order grammar is a description of the transformational structures. The input for a first order grammar is the sequence of alphabetic characters, numerals, blank spaces and punctuation occurring in a language. The input for the second order grammar is the sequence of first order rules found to apply in the analysis of an alphabetic input. If we visualize the replacement rules of a phrase structure as lying in a plane, we may think of the transformation rules as extending into the third dimension.

7.1 In the original model, the equivalence class serves a double purpose in that it brings together rules which stand in the relation of being allomorphs as well as bringing such morphemic subsets of rules together as alloemes. In the extended model, the equivalence class is obviated, for the terminal rules of the second order grammar take over the load of allomorphic classification. The next stratum of rules up from the terminal rules may be interpreted as those second order rules which classify alloemes.

7.1.1 To illustrate, let us consider such first order rules as

$$\begin{aligned} \text{AJ}_x &\rightarrow \text{good} \\ \text{AJ}_y &\rightarrow \text{bett} \\ \text{AJ}_z &\rightarrow \text{be} \end{aligned}$$

which are used in the analysis of the expressions *good*, *better* and *best*. Each rule in the first order grammar will have assigned to it a unique and permanent reference number. I shall represent these numbers here as *m*, *n*, and *o* — these tags are not to be confused with equivalence class names above. Thus, as each of the above rules is coded into the first order grammar, it receives the respective tag:

$$\begin{aligned} m: & \text{AJ}_x \rightarrow \text{good} \\ n: & \text{AJ}_y \rightarrow \text{bett} \\ o: & \text{AJ}_z \rightarrow \text{be} \end{aligned}$$

In the second order grammar we now code each of the first order tags *m*, *n* and *o* as second order alphabetic expressions. We write the second order rules

$$\begin{aligned} \{\text{GOOD}\}_m &\rightarrow m \\ \{\text{GOOD}\}_m &\rightarrow n \\ \{\text{GOOD}\}_m &\rightarrow o \end{aligned}$$

where $\{\text{GOOD}\}_m$ denotes a second order terminal class which we may interpret as a morpheme class.

7.1.2 The next higher stratum in the branching structures of the second order grammar may be interpreted as a sememic classification of the second order terminal classes. For instance, we would find the second order rules

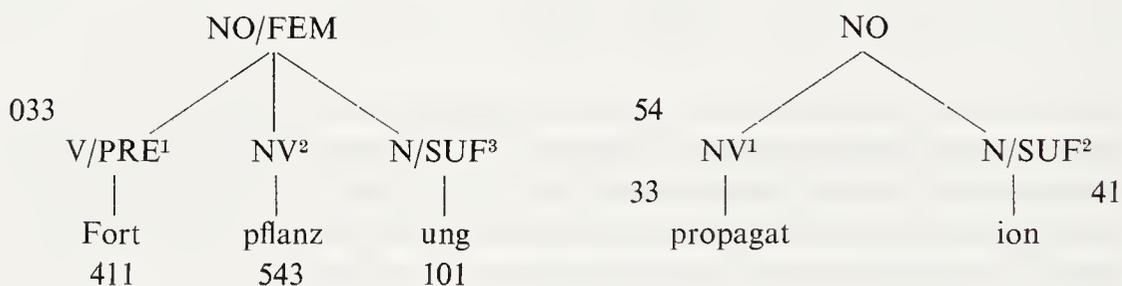
$$\{\text{GOOD}\}_s \rightarrow \{\text{GOOD}\}_m$$

where $\{\text{GOOD}\}_s$ denotes a sememe. In addition to the above rule, we might also expect

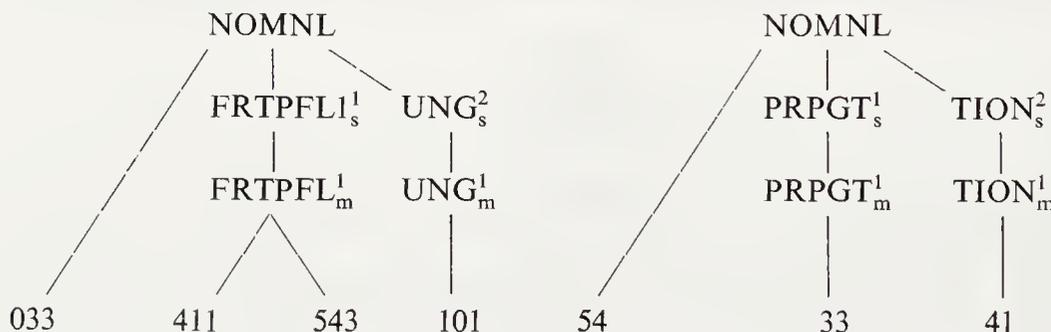
$$\begin{aligned} \{\text{GOOD}\}_s &\rightarrow \{\text{KIND}\}_m \\ \{\text{GOOD}\}_s &\rightarrow \{\text{COMPETENT}\}_m \\ \{\text{GOOD}\}_s &\rightarrow \{\text{BENEFICIAL}\}_m \end{aligned}$$

etc. Each of the above morphemes $\{\text{KIND}\}_m$, $\{\text{COMPETENT}\}_m$, $\{\text{BENEFICIAL}\}_m$, etc. will likewise appear in its own sememe along with its own list of substitutable morphemes. As earlier, we are able to arrange a set of morphemes in decreasing order of preference of substitution for a given morpheme. Instead of comparing two rules for their distribution over sets of equivalence classes as earlier, we now inspect their distribution over respective sets of sememic classes.

7.2 I have said that the two main functions of the second order grammar are to provide a semantic grammar and a transformation structure: In the interests of space I shall restrict consideration of the transformational problem to an example which relates an unlike number of elements in two nouns, one from German and one from English. The first order analyses for each noun are given below:



The numbers associated with each rule are the first order tags. If we read the tags in the order of the superscripts, the German second order alphabetic sequence is 033, 411, 543, 101. The English sequence is 54, 33, 41. We may perform a second order analysis on these sequences as below:



Note that the second order rules $\text{NOMNL} \rightarrow 033 + \text{FRTPFL}_s^1 + \text{UNG}_s^2$ and $\text{NOMNL} \rightarrow 54 + \text{PRPGT}_s^1 + \text{TION}_s^2$ are composed of the same number of

corresponding variable elements. In fact each German-English pair of rules which corresponds in the above illustration is composed of the same number of variables. We may, therefore, establish an interlingual correspondence between each such pair of rules for purposes of translation. The approach for relating transformationally all such dissimilar first order constructions is essentially the same.

8.0 As of the writing of this paper, extensive linguistic coding has been carried out only on the phrase structures of German and English — two languages about which we know a great deal, compared to many other languages. Even so, much remains to be done before we can say that we have, for machine translation purposes, a reasonably complete phrase structure description of the two languages. Recently, we have just begun compilation of synonymy lists which will provide us with the necessary information for coding the sememic structures of the second order grammar.

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DISCUSSION

DE TOLLENAERE:

It was fascinating for me to recognize the content recognition in the lecture, but I failed to see “the production of synonymous expressions”, which is mentioned in the title of his paper.

THE DECLINE OF LATIN AS A MODEL FOR LINGUISTIC ANALYSIS

GERD FRAENKEL

In the recent past scholars have begun to take cognizance of the fact that too many sweeping statements, based on too few facts, have been made on the linguistic incompetence of our precursors in the centuries preceding the middle of the 19th century.

In 1960 Hanzeli wrote in the introduction to his thesis: "Attempts to discover in linguistic works of the past something more or something different from what they have been believed to contain is not so much an attempt to rehabilitate authors but rather to rehabilitate linguistic science in its historical continuum."¹

Even during the initial stages of research on this paper it became clear that (a) the subject could hardly be treated in isolation as it is inextricably interwoven with two other questions presently to be mentioned, and (b) it is almost impossible to set a time limit for the starting point, as historical ramifications tend to lead us further and further into the past the deeper we probe into the subject. For this reason, the appearance of Herder's *Philosophy of Humanity* has been chosen as a starting point, i.e. roughly the year 1750. As the approximate cut-off date I have selected the year of Kleinschmidt's *Eskimo Grammar*, which gave the original impetus to this paper, i.e. the year 1851.

Before treating the Latin model in connection with the general intellectual climate between the two poles mentioned, the two questions hinted at in (a) above ought to be brought up. Neither can receive more than cursory treatment in this paper, however the first will provide us with some historical background data pertinent to the subject discussed.

1. What was the methodological development of the teaching of a foreign language?
2. What was the development of the idea that so-called primitive peoples speak primitive languages?

In 1935 Sanford B. Meech published a paper in which he described the earliest existent evidence of applying Latin grammar to English.² Although the first English grammar in English containing a latinized terminology appeared in London in 1586,

¹ Victor Egon Hanzeli, *Early Descriptions by French Missionaries of Algonquian and Iroquoian Languages, a Study of Seventeenth and Eighteenth-Century Practice in Linguistics*. Unpublished doctoral dissertation, Indiana University, December 1960, p. 7.

² Sanford B. Meech, "Early Application of Latin Grammar to English", *PMLA*, 1935, pp. 1012-1032.

research has established that about one hundred years earlier the "borrowing of concepts of Latin grammar" (mainly those of parts of speech) "was common in grammars of Latin written in English". That early, then, it seems to have been a characteristic of England that Latin inflections were pointed out to the student of the classic languages as existing in his mother tongue as well in order to facilitate his study of the former. It may thus be assumed that the teachers of Latin unwittingly were the instigators of this practice.

It is of historic interest to point out here that early in the 17th century at least one man rose who was ready to dispense with all formal grammar in language teaching and who constitutes a gap in the continuum of that school of thought whose not unchallenged preeminence this paper tries to trace.

This man, on whom little has been written but who in turn can be shown to have practiced the ideas of a 16th century forerunner, is Joseph Webbc. Vivian Salmon quotes him as saying: "Custom, concurring with the pleasure of hearing, is the surest rule and foundation of the Latin language", and again: "By exercising of reading, writing, speaking after ancient customs we shall continue three things which are of the greatest moment in any language. . . . Thirdly, we shall get the judgement of the ear and retain the same forever which grammar cannot help us to in that it is imperfect and beguiles us."³ Truly, this man would not be in the line of those trying to apply the grammar of any one language to another.

The first traces of outspoken criticism of applying the Latin model to other languages comes to us from the turn of the 19th century. Although a great number of such critics belong to those groups of missionaries whose interest in foreign lands extended to their wards' language, there is also evidence of this anti-Latin trend in Great Britain at about the same time.

In this connection William Hazlitt is to be quoted who first in his *New and Improved Grammar of the English Tongue* in 1809 and later in his *The Spirit of the Age* in 1825 had the following to say on the subject:

It is a circumstance which may at first excite some surprise, that amidst the various improvements in books of modern education, there has hitherto been no such thing as a real English grammar. Those which we have are little else than translations of the Latin grammar into English. We shall, however, no longer wonder at this circumstance, when we recollect that the Latin grammar was regularly taught in our schools several centuries before any attempt was made to introduce the study of the mother-tongue, and that even since some attention has been paid to the latter, the study of the learned languages still having the precedence, our first notions of grammar are necessarily derived from them. Those who have written on the subject have not been exempt from the influence of early prejudice, and instead of correcting the error have strengthened it.

The following is an attempt to explain the principles of the English language, such as it really is. We have endeavoured to admit no distinction which, but for our acquaintance with other languages, we would never have suspected to exist. The common method of

³ Vivian Salmon, "Joseph Webbe: Some Seventeenth Century Views on Language Teaching and the Nature of Meaning", *Bibliothèque d'Humanisme et Renaissance*, 1961, pp. 324-340.

teaching English grammar by transferring the artificial rules of other languages to our own . . . occasions much unnecessary trouble and perplexity.⁴

(Lindley Murray) maintains that there are six cases in English nouns, that is, six various terminations without any change of termination at all, and that English verbs have all the moods, tenses, and persons that the Latin ones have. This is an extraordinary stretch of blindness and obstinacy. He very formally translates the Latin grammar into English (as so many have done before him) and fancies he has written an English grammar; and divines applaud, and schoolmasters usher him into the polite world, and English scholars carry on the jest.⁵

Although strong credence must be given to the statement of some scholars, including Hanzeli, that the study of languages very remote from Latin and other Indo-European languages was the prime reason for the realization that Latin concepts were not universally applicable, other reasons must have played a role as well. Despite the number of explicitly non-latinizing grammars written by missionaries, and despite a statement made by Von der Gabelentz ("Manche dieser Geistlichen quälen wohl erst sich und ihren Stoff durch alle Kapitel der lateinischen Grammatik hindurch, teilen aber dann in Form eines Anhanges mit, was eigentlich den Geist der Sprache ausmacht."⁶) it is possible to find a common denominator for these signs of a new age in the overall spirit of the time.

Basing many ideas of his on Leibniz but adding a renewed interest in the origin of human speech, Johann Gottfried Herder wrote his *Abhandlung über den Ursprung der Sprache* (1772). This book, however, does not contain any ideas relevant to this paper. On the other hand his preliminary work on the Philosophy of History (*Auch eine Philosophie der Geschichte zur Bildung der Menschheit*, 1774) written when he was about thirty and his main work *Ideen zur Philosophie der Geschichte der Menschheit* (1784-1791) both contain helpful statements.

Die Abgötterei, die Griechen und Römer so viele Jahrhunderte genossen; der fanatische Eifer, mit dem alles bei ihnen aufgesucht, ins Licht gesetzt, verteidigt, gelobt worden – welch grosse Vorarbeiten und Beiträge. Wenn der Geist der übertriebenen Verehrung wird gedämpft . . . sein – Ihr Griechen und Römer, dann werden wir Euch kennen und ordnen.⁷ Arm und klein wäre es also, wenn wir unsere Liebe zu irgend einem Gegenstande menschlicher Kultur der allwaltenden Vorsehung als Riegel verzeichnen wollten, um dem Augenblicke, in dem er allein Platz gewinnen konnte, eine unnatürliche Ewigkeit zu geben. Es hiesse diese Bitte nichts anderes als das Wesen der Zeit zu vernichten und die ganze Natur der Endlichkeit zu zerstören.⁸

⁴ William Hazlitt, "A new and Improved Grammar of the English Tongue", in *Works*, edited by P. P. Howe (London, 1931), vol. II, p. 5.

⁵ William Hazlitt, "The Spirit of the Age, The Late Mr. Horne Tooke", in *Works*, edited by P. P. Horne (London, 1931), vol. XI, p. 57.

⁶ Georg von der Gabelentz, *Die Sprachwissenschaft*² (1901), p. 26.

⁷ *Herders Philosophie, ausgewählte Denkmäler aus der Werdezeit der neuen deutschen Bildung*. Herausgegeben von Horst Stephan (1906), p. 105, from the 1774 essay "Auch eine Philosophie der Geschichte zur Bildung der Menschheit".

⁸ *Ibid.*, p. 158, from the *Ideen*.

These theorizings found their projection in Herder's compatriot, some thirty years his junior, Barthold Georg Niebuhr (1776-1831). There is no evidence of any personal contact between the two men, just as in many other cases only general mutual influences between intellectual currents of the time can be assumed. Niebuhr, however, must have been aware of Herder, particularly as Fr. H. Jacobi, who was of Herder's school and his personal friend, was also Niebuhr's teacher.

Niebuhr was born in Copenhagen and lived there for thirty years except for two years of study spent at the University of Kiel. This accidental domicile creates an interesting geographical closeness to Kleinschmidt whose grammar was published in 1851. I have no knowledge of Kleinschmidt's private life, but a direct line of ideological transfer is not to be dismissed although there is no evidence that Kleinschmidt ever left Greenland.

It could, by the way, be worthwhile to study another possible chain of transmission of ideas: from Herder's works to the Danish philosophers Heiberg, Martensen, Rasmus Nielsen (who, though being followers of Hegel, would have presented Herder's ideas alongside their own) to Kierkegaard (1813-1855), who was Martensen's student, to Kleinschmidt whose contact with these circles is, however, unfortunately still unknown to us.

Niebuhr, who mastered some 20 languages, directed his main scholarly efforts towards giving a new perspective to ancient history. By taking it down from its pedestal of the ideal he put the stamp of application to Herder's ideological request for the same step.

An admirer of his, who only eight years after his death in 1831, published one of his works accompanying it with a long introduction on the man and his ideas, Karl Georg Jakob, mentions that there is no evidence as to how far Niebuhr was late in life influenced by comparative philology and its initial results. From his own biased attitude, the author claims he does not believe that Niebuhr fell victim to the charms of these new-fangled ideas. He adds, however, that Niebuhr advocated writing Latin and thus does not belong to one of three classes of people who are in favour of abrogating this old-established tradition. By quoting some of these opinions the author reveals to us some additional facets of the anti-latinistic attitude of the second and third decade of the 19th century. One Arnold Ruge, called a radical reformer by the author, voices the following opinion: "Es ist gewiss ein Schritt zur Bildung selbst, dass uns die kalte Bildung der lateinischen Eleganz und überhaupt die Versenkung lediglich in den Sprachgenius des Griechischen und Lateinischen, dieser schwelgende Dilettantismus, nicht mehr genügt und es ist ohne weiteres auch hier die Entwicklung in ihrem Rechte zu begreifen."⁹

The question now arises which cross-currents carrying similar ideas from country to country can be established. Germany and Denmark were mentioned earlier.

⁹ *Barthold Georg Niebuhr's Brief an einen jungen Philosophen, mit einer Abhandlung über Niebuhr's philologische Wirksamkeit und einigen Excursen*. Herausgegeben von Dr. Karl Georg Jacob (1839), pp. 157-158.

Loebell, in an essay on Niebuhr written in 1852, i.e. twenty-one years after his death, mentions his influence abroad. "The writers who were incited through the influence of Niebuhr to new researches into the Roman history occupy very different positions in relation to him. With some, reverence, admiration, and agreement preponderate. Among these is your fellow-countryman, Dr. Arnold, who, had a longer life been granted him (1795-1844), would no doubt have been the most worthy to carry forward the immortal work."¹⁰

Dr. Arnold and Hazlitt were contemporaries. There is no shred of evidence on any mutual influence or literary contact. Again we are faced with one of these links in the chain of intellectual concatenation which are forged but may just as well remain invisible. Such contacts with England attain even greater significance when we consider that most missionaries writing on exotic languages were British or closely allied with them.

However, before reviewing these works a paragraph will have to be devoted to France. It is interesting to note that French and German scholars pay each other the compliment of ascribing to their colleagues the earlier realization of the desirability of non-Latin grammatical analysis. While in a German source I found a note in passing which mentioned that the French merited the palm in this respect, the famous historian of the French language F. Brunot writes in 1905: "Tandis que, en France, tout le monde s'attardait dans les conceptions à priori de la grammaire philosophique du XVIIIe siècle l'Allemagne avait poussé très loin l'étude scientifique fondée sur l'observation des langues indo-européennes."¹¹ Although this passage does not specifically refer to Latin as a model it gives the Germans a considerable head start in general linguistic analysis.

In fact, it seems that all pronouncements on this matter were more or less simultaneous. Although earlier, yet untapped, sources may exist, the earliest French statement having come to my notice is from Ch. P. Girault-Duvivier's *Grammaire des Grammaires*.¹² In the chapter treating the article in French, there is violent polemic against earlier grammarians who wrote on the subject. Here we read: "Encore une fois, les *cas* et les *déclinaisons* sont étrangers à la langue française: les noms qui se déclinent en latin . . . demeurent invariables dans notre langue; et c'est abuser des termes que d'induire les *cas* et les *déclinaisons* de l'identité des vues ou des rapports, quand les mots sont privés des *terminaisons* et des *désinences* qui constituent, à proprement parler, les *cas* et les *déclinaisons*."

However, for the time being this statement has to stand alone and cannot be connected to any organic development in French language study. France, being politically united much earlier than Germany, also had her standard language at a

¹⁰ Professor Loebell, "On the Character of Niebuhr as an Historian", in *The Life and Letters of Barthold George Niebuhr with Essays on his Character and Influence* (1852), vol. II, p. 421.

¹¹ Ferdinand Brunot, *Histoire de la Langue Française des Origines à 1900* (1905), vol. I, p. 14.

¹² Ch. P. Girault-Duvivier, *Grammaire des Grammaires*, nouvelle édition 1837 (first edition 1811), vol. I, p. 127.

much earlier time. Accordingly, French grammarians concentrated on purifying and standardizing the prestige dialect rather than on describing it structurally, which activity could have led up to an anti-Latin sentiment. Neither did France possess a school of philosophers of history who could have reached a position of judging antiquity the way Herder did. It can only be mentioned in passing here that the exchange of ideas with England and the strong influence on France emanating from her can in no way be compared with the almost non-existent flow of ideas between Germany and France at the time.¹³

Finally, only a short list can be appended, accompanied by some quotations, of works written by missionaries on non-Indo-European languages.

The earliest reference I could find is the French Jesuit P. Premare, who, about 1730, wrote against the introduction of Latin terminology into Chinese and argued that students be introduced right away to the genius of the language.¹⁴ Jonathan Edwards in 1788 mentions that the Mohegan Indians do not have the same parts of speech as English¹⁵ (or as Latin, for that matter). C. W. Schürmann, a German missionary writes in the introduction to his *Parnkalla Grammar and Vocabulary*, published in Australia in 1844: "To those who may undertake to study the Australian dialects from curiosity or philological interest, I should recommend to divest their minds as much as possible of preconceived ideas, particularly of those grammatical forms which they may have acquired by the study of ancient or modern European languages."¹⁶

In 1851 Samuel Kleinschmidt published his *Grammatik der grönländischen Sprache mit theilweisem Einschluss des Labradordialekts*. In his introduction Kleinschmidt states, among other things, that among his predecessors in the field "Fabricius [dahingegen hat] sich in manchen Stücken in einen für seine Zeit [i.e. 1791] allerdings bemerkenswerthen Grade von der damals noch fast unbestrittenen Autorität des Lateinischen als alleinigen Sprachmusters frei gemacht." (Fabricius nowhere explicitly states that.) "Mehr jedoch kann in dieser Hinsicht jetzt erwartet werden, wo man beides das Eigenthümliche und das Verwandte verschiedener Sprachen zu unterscheiden besser gelernt hat, und sollte daher der wesentlichste Unterschied zwischen jenen früheren Grammatiken und der gegenwärtigen darin bestehen, dass der Ausgangspunkt, statt bei jenen europäisch, bei dieser grönländisch ist."¹⁷

¹³ Cf. also Henri Tronchon, *La fortune intellectuelle de Herder en France* (1920).

¹⁴ Von der Gabelentz, p. 25.

¹⁵ Hans C. Aarslaeff, *The Study of Language in England 1780-1860*, University of Minnesota Dissertation, 1960, quotes Horne Tooke's *Diversions of Purley* which refers to Jonathan Edwards's 1788 remark. (From a personal communication by Professor Fred H. Higginson of Kansas State College.)

¹⁶ C. W. Schürmann, *Vocabulary of the Parnkalla Language spoken by the Natives Inhabiting the Western Shores of Spencer's Gulf. To which is Prefixed a Collection of Grammatical Rules, hitherto Ascertained* (Adelaide, 1844), p. VI.

¹⁷ S. Kleinschmidt, *Grammatik der grönländischen Sprache mit theilweisem Einschluss des Labradordialekts* (Berlin, 1851), p. v.

One other work will be mentioned although it appeared later than Kleinschmidt's whose book I had marked as the end-point of my study. I mention it because it treats Maori and thereby effectively completes a line running through the non-Indo-European languages of Amerindian, Australian, Chinese, Eskimo, and now Polynesian. About this book by W. L. Williams, published in London in 1862, Bruce Biggs has to say the following; "Missionary endeavour and thorough knowledge of the language was matched by a degree of linguistic insight remarkable for his time. His 'First Lessons in Maori' show an emancipation from traditional categories and some appreciation of the use of formal criteria for classification that was not matched by contemporary work in other Polynesian languages."¹⁸ However, again I could find no explicit statement about the method.

Speaking only about such sources as make specific reference to the subject matter discussed I should like to draw the following conclusions, scanty though the time-spanning material in some instances still is.

1. Assuming that the treatment of European languages along the Latin model really started in about the middle of the 15th century in England and not in the late 16th century as earlier said by scholars, we still find that the variedly maligned centuries of Latinized linguistic analysis lasted at most three hundred years and possibly less. If research included not only grammarians who stated their anti-Latin methods, a further reduction of this period may very well become possible. The worn generalization of recent decades on the supreme reign of the Latin model seems no longer acceptable although it must, of course, be admitted that it dominated linguistics for a long period, in fact much longer than the one under study here.

2. Some scholars have assumed that the ascent of comparative linguistic studies following the intensive research in Sanskrit precipitated the decline of Latin as a model. This proved to be incorrect. The newly detected languages were too similar to suggest such thoughts. Of much greater importance was the widening horizon of global interest in exotic peoples and languages. This interest led many missionaries who were of no consequence in the mainstream of linguistic thought of the century, to revolutionary yet untapped realizations.

3. No one idea exists detached from parallel ideas in other disciplines. Even if the proof for mutual fertilization is not always possible the evidence of simultaneous expressions in various fields permits us to assume cross-currents making for an overall intellectual climate of an age.

These results are by necessity preliminary. Further investigation – planned for a later stage – will have to probe deeper, mainly in order to bring to light personal contacts which could have had additional influence on this trend.

Whenever such new ideas are in the air, many of their facets may have been transmitted orally, hinted at in the classroom, or resulted from a personal discussion.

¹⁸ W. L. Williams, *First Lessons in Maori* (London, 1862). Described in Bruce Biggs, "The Structure of New Zealand Maori", *Anthropological Linguistics*, III/3, p. 3.

As such they may never have been committed to writing by the persons involved and may thus escape our notice for ever. However, such a pessimistic statement can only result from an earnest conviction to have reached the limit of possible detection after a prolonged investigation. Until this limit is reached there remains a great deal to be done. May I add that various people have lately furnished me with additional information often very interesting but too marginal to be incorporated in this short paper. New vistas opened up by this material may enable me to enlarge the scope of the investigation of these ideas into a wider study.

May I, finally, suggest that a fascinating addition to an enlarged study on this subject could be obtained by investigating the invention of the term "genius of a language" and the history of its changing application, from being an emotional to its becoming a scholarly term.

Such a complete study would certainly be a welcome addition to the somewhat neglected history of linguistics during an exciting period of inchoative changes in the concept of linguistic analysis.

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DISCUSSION

ENKVIST:

Dr. Fraenkel speaks of "at least one man" in the early 17th century who was ready to dispense with all formal grammar in language teaching, Joseph Webb. In this phrase Fraenkel is erring on the side of caution. As Webbe states in his *Appeale to Truth* (1622), he was indebted to the *Restauratio Linguae Latinae* of Georgius Haloinus Cominius (George Halewyn, Lord of Comines) for many of his antigrammarian ideas and principles. There were anti-grammarians of other kinds as well. For instance, many of the modern-language teachers who lived at St. Paul's Churchyard in Shakespeare's London taught French and other languages, sometimes even Latin, by anti-grammarian methods. Part of the explanation should be sought in their background: many of them were Huguenot refugees whose training in formal grammar left much to be desired. Thus Webbe should be regarded as one representative of a trend rather than as a solitary reformer.

Further, for instance in his *Pueriles Confabulatiunculae* Webbe developed new ways ("lines") of illustrating sentence structure in Latin and in English. Though he boasted his readiness to dispense with all grammar, he in fact developed a new and very interesting approach to the analysis of sentences.

ON THE PRESENT STATE OF INDO-EUROPEAN LINGUISTICS

VLADIMIR GEORGIEV

After the decipherment of Hittite and the pertinent researches in the thirties, IE linguistics entered its *third period* of development.¹ During the last thirty years our material has increased considerably in quantity as well as in antiquity. Now we know many more IE languages than before. Thus, formerly only Greek, Macedonian, Thracian, and Illyrian were known in the Balkans from antiquity. At present it is clear that the so-called Thracian linguistic material belongs to three different IE languages: (genuine) Thracian, Dacian or Daco-Mysian (the predecessor of Albanian) and Phrygian; and in the south of the Balkans a special pre-Greek IE language was discovered.²

Thracian is known from a few inscriptions, about 20 glosses and 1000 proper names. Its main characteristics are: *a, ē, un, ur (or)* from *ǵ, ā, ŋ, ʀ*, consonant shift, diphthongs and *s* preserved, assibilation of "palatals". Thracian is very different from Illyrian.³

Dacian (or Daco-Mysian) is known from an inscription, about 60 glosses and 400 proper names. Its main characteristics are: *ie, a, ā > o, a, e, a, ri* from IE *ǵ, ǵ, ē, aw, ew, ŋ, ʀ*, assibilation of "palatals". Daco-Mysian is the predecessor of Albanian.⁴

Phrygian is known from about 100 inscriptions, 30 glosses and 200 proper names. Its main characteristics are: *o, an, ar, w* from IE *ǵ, ŋ, ʀ, sw*, consonant shift. Phrygian is very closely akin to Greek.⁵

Pre-Greek IE ("Pelasgian") is known from a special analysis of the Greek vocabulary. Its main characteristics are: *a, un, ur, (or)* from *ǵ, ŋ, ʀ*, consonant shift, *s* preserved. "Pelasgian" is closely related to Thracian on the one hand, and to the Hittite-Luwian group on the other hand.⁶

¹ See V. Georgiev, *Issledovanija po sravnitel'no-istoričeskomu jazykoznaniju* (Moscow, 1958), p. 7 ff.

² See V. Georgiev, *La toponymie ancienne de la péninsule Balkanique et la thèse méditerranéenne* (Sofia, 1961), p. 5 ff.

³ See V. Georgiev, *Bălgarska etimologija i onomastika* (Sofia, 1960), p. 76 ff.

⁴ See V. Georgiev, "Albanisch, Dakisch-Mysisch und Rumänisch", *Linguistique Balkanique*, II (1960), p. 1 ff.

⁵ See O. Haas, "Die phrygische Sprache im Lichte der Glossen und Namen", *Linguistique Balkanique*, II (1960), p. 25 ff.

⁶ See V. Georgiev, *Vorgriechische Sprachwissenschaft* (Sofia, 1941); A. J. Van Windekens, *Le pélasgique* (Louvain, 1952).

“Most of the old languages of Asia Minor, as Lydian, Carian, Lycian, are not IE but belong to a group (. . .) which it is convenient to call Anatolian,” wrote C.D. Buck in 1933 in his *Comparative Grammar of Greek and Latin*, and this was the general conception at that time. Now we know that Lycian and Lydian are of IE origin and belong to the Hittite-Luwian group. Recently it was brilliantly proved by E. Laroche that Lycian is of Luwian (and Hieroglyphic Hittite) origin.⁷ There are also very strong arguments that Carian belongs to the same group.⁸ The Etruscans being of NW Anatolian provenance, there are also important considerations that their language is of the same descent. The Etruscan declension has its closest correspondences in the Hittite-Luwian one:

Etr. Gen. *-(e)s* beside Possessiv *-(e)si*, *-(e)sa*, corresponds to Hier. Hitt. Gen. *-as* and Possessiv *-asa-*, *-asi-*, Lyc. Gen. *-h* (*-he*) and Possessiv *-ehi*, *-ahi*, older (B) *-esi*;

Etr. Possessiv *-al* corresponds to Lyd. Possessiv *-(a)hi-*, Hitt. pronominal Gen. *-el*;

Etr. Dat. *-i*, *-e* corresponds to Hitt. Luw. Dat. *-i*, *-ja*;

Etr. Loc. *-(a)th(i)*, *-(a)t(i)*, *-(e)th(i)*, *-(e)t(i)* corresponds to Luw. Abl. -Instr. *-ati*, Lyc. Abl. -Instr. *-edi*, *-adi*, Gr. (“Pelag.”?) Loc. *-othi*, *-ēthi*.⁹

Several IE languages from the second millenium have become known too: Hittite, Luwian and Hieroglyphic Hittite, Palaic, Mycenaean Greek. Furthermore, in the last three decades new methods and possibilities for the investigation of languages and their development have been discovered.

The new data and the recent researches have permitted us to go deeper into the *history of PIE* and to refer the formation of the separate languages far before the third millenium. They contributed to the refutation of the pan-Mediterranean thesis which dominated for a long time and hindered the correct formulation of many problems, especially that of the original home of IE.

With the establishing of the “laryngeals” the picture of the *phonematic system of PIE* and its word structure have been fundamentally changed. The unconvincing Brugmannian “phonemes” *k̑*, etc. are already superseded: it has turned out that they represent special ancient phonetic changes:

IE **dheghom-*, **dhghom-* > **(gh)dhom-*;

**tetkon-* (cf. Gr. *tiktō* from **titkō*), **tetketi* > Indo-Iran. **tetseti* (Skr. *takṣati*); **Hṛtko-s* > **Hṛ(k)kos* (Gr. *árkos*) and **Hṛktos* (Gr. *árktos*) > **Hṛttos* (lat. *ursus*), **Hretkes-* > Indo-Iran. **retśes-* (Skr. *rākṣas-*), etc.¹⁰

However, the erroneous but still authoritative tripartition of the gutturals con-

⁷ E. Laroche, “Comparaison du louvite et du lycien”, *Bulletin de la Société de linguistique*, 53 (1958), p. 159 ff.; 55 (1960), p. 155 ff.

⁸ See V. Georgiev, “Der indoeuropäische Charakter der karischen Sprache”, *Archiv Orientální*, 28 (1960), p. 607 ff.

⁹ See V. Georgiev, *Linguistique Balkanique*, IV, 1 (1962), p. 16 f.; *Die sprachliche Zugehörigkeit der Etrusker* (Sofia, 1943).

¹⁰ See W. Merlingen, *Gedenkschrift P. Kretschmer*, II (Wiesbaden, 1957), p. 70 ff.

tinues to hamper etymology, since more than one third of the IE words contain some guttural. Brugmann's authority prevents many linguists from adopting the correct view. The bipartition (velars and labiovelars) and the palatalization are corroborated phonologically ($k - k^w$ or $k' - k$, but not $k - k' - k^w$) and statistically. On the basis of *Idg. etym. Wb.* by J. Pokorny (with some corrections and additions) the following statistics may be compiled. There are 748 stems that contain a velar (according to the tripartition plain velar or palatal) which in the so-called *Satem* languages is preserved or appears as sibilant. If we mark preserved velar by K, assibilated by K', liquid, nasal, and w by L, front vowel and y by E, back vowel or consonant by A, we obtain the following distribution:

K'(L)E (e.g. **key-*, **kley-*, **kwey-*) = 186

KA (e.g. **kakud-*) = 380

sk (e.g. **sker-*) = 41

K - K' (e.g. Lith. *klaus-* = Slav. *sluš-*) = 133

KE (e.g. **gerebh-*) = 8.

The velars were palatalized not only before e , i , y , but also in such clusters as kw , kl , kr , km , kn , gw , gl , etc. before e , i , y . We have here the following phonetic change: $kli > kl'i > k'l'i > č'l'i > Slav. š'l'i > sli$. Palatalization and assibilation in the clusters kv , gv is typical of most of the Slavic languages, cf. Russ. *cvet* from Common Sl. **květŭ*. Palatalization of velars in such clusters as kl , kr , kn , km , gl , etc. is typical of Lithuanian, cf. *kniaūstis* = $k'n'iaūstis$, *ugnis* = $ug'n'is$.¹¹ It is obvious that in this respect the situation in IE and the changes in the so-called *Satem* languages was the same as in Latin and the Romance languages, i.e. French is as much a *Satem* language as is Sanskrit. This conception gives us the possibility to make many new etymologies, e.g.:

**Hag-no-s* "driven" > "lamb" (lat. *agnus*) PPP from **Hag-* "drive" (Lat. *ago*, Av. *azaiti*), see below;

Lith. *rāgas*, Slav. *rogŭ* "horn" from IE **rogo-s* to OIcel. *rakr* "straight, upright, erect", Av. *arəzu-* "straight, upright, erect";

Lith. *vaikas* "child, son" = Gr. (w)*oikos* "house; family" = Skr. *veśāḥ* "house", *viśāḥ* "men";

OCSl *vragŭ* "enemy, devil", Lith. *vařgas* "misfortune, adversity", OPruss. *wargš* "evil, wicked" = ONorse *vargr* "wolf, evil-doer, felon", OE *wearg*, OHG *war(a)g* "brigand, criminal, felon".

The discussion on the Armenian consonant-shift is not yet definitely concluded,¹² but it has raised an interesting general question: to what extent can modern dialects preserve archaisms that have already disappeared in much older periods of develop-

¹¹ Cf. J. Otrębski, *Gramatyka języka litewskiego*, I (Warszawa, 1958), p. 270 f.

¹² A. S. Garibjan, *Voprosy jazykoznanija*, 1958, 5, p. 95 ff.; 1959, 5, p. 81 ff.; B. Agajan, *ib.*, 1960, 4, p. 37 ff.; V. Georgiev, *ib.*, 1960, 5, p. 35 ff.; G. B. Džaukjan, *ib.*, 1960, 6, p. 39 ff.; E. Benveniste, *ib.*, 1961, 3, p. 37 ff.; H. Vogt, *ib.*, p. 40 ff.; J. Otrębski, *ib.*, p. 44 ff.; V. Pisani, *ib.*, 1961, p. 46 ff.; W. P. Lehmann, *ib.*, p. 56 ff.; L. Zabrocki, *ib.*, 1961, 5, p. 34 ff.; F. Feydit, *ib.*, p. 46 ff.; E. A. Makaev, *ib.*, 1961, 6, p. 22 ff.; V. V. Ivanov, *ib.*, 1962, 1, p. 37 ff.

ment of the same language? A similar question has already been positively solved in connection with the Indian Kāfir languages.¹³

In the field of *morphology* the new data have helped to explain many problems. Here are two examples. The Mycenaean medial ending *-(n)to(i)* made it possible to analyse Hitt. *-ta*, *-tari*, Lat. *-tur* as *-to + ri*, Gr. *-toi* and Skr. *-te* as *-to + i*. Hittite verbal forms like *kurur-iya-* “to be hostile, to be at enmity with” which can be analysed as *kurur* “enemy, enmity, war” and *iya-* “to do” facilitate the explanation of the (factive) verbal class in *-eyō* (= Luw. *aya-* “to do”), *-(ī)yō* (Hitt. *iya-* “to do”) and reveal a pre-synthetical period of PIE.

A similar pre-synthetical formation hides in the verbal class in *-ew/u-*. This class is formed on the basis of agent nouns (also action nouns and similar adjectives) in *-ú-*, cf. Skr. *bhikṣ-ú-* “beggar” to *bhiks-* “to wish for, desire, beg”, *pāy-ú-* “protector” to *pā(y)-* “to protect, preserve, keep”, Ved. *tāy-ú-* “thief”, Skr. *jay-ú-* “victorious”, *dār-ú-* “destroying”, *nṛt-ú-* “dancer”, *aś-ú-* “swift”, *ragh-ú-*, *lagh-ú-* “swift, light”, *dhṛṣṇú-* “bold”, etc. Therefore, the original meaning of Skr. *karómi* “I do, make, perform” from IE **k^wor-éw-mi* has been literally “doer I”; Skr. *dhṛṣṇómi* “I am bold” from *dhṛṣṇú-* “bold” has signified literally “bold I”. This type of formation is preserved in most other IE languages. Thus, the Greek verbs in *-eúō* (from *-ew-yō*) are derived from agent nouns in *-eús* (IE *-ú-*), cf. *basileúō* “I am king”, literally “king I”, from *basileús* “king”. The closest correspondences of this Greek verbal class are the Slavic verbs in *-újǫ* (inf. *-ovati*) and the Lithuanian ones in *-áuju* (inf. *-áuti*) from IE *-éw-yō* (inf. *-ew-ā-těy* and *-ew-těy*), cf. OCSl *vojujǫ* (inf. *vojevati*) “I am at war, make war” from Slav. *vojǫ* “warrior” (in Slav. *voj-inǫ* “warrior, soldier, fighter”) < IE **woy-ú-*, OCSl *darujǫ* (inf. *darovati*) “I give a present” from IE **dōr-éw-yō*, literally “giver I”, Lith. *kariáuju* “I am at war, make war” from IE **kōri-ew-yō*, cf. *karǫs* “warrior”. The same formation is probably Hitt. *watku-z(z)i* “he springs”, literally “springer he”, *eku-mi* “I drink”, literally “drinker I”.¹⁴

Various problems of IE *etymology and word formation* have been the subject matter of numerous investigations in the last twenty years, so that the dictionary of J. Pokorny has proved to be out of date, the same being true of dictionaries of the same type (e.g. *Gr. etym. Wb.* by H. Frisk) which represent the neogrammarian period in the development of IE linguistics.

Nowadays attention is focused not only on correspondences, but also on an insight into the reason of the denominations. Thus in Pokorny's dictionary there is not even a hint about the link between **Hew-* “to dress, to clothe, to put on” and **How-i-s* “sheep, ewe”, while for us the etymological connection between “the woolly animal” and “the clothing” is also important. The same is true of **Hag-nó-s*

¹³ Cf. H. Sköld, “Die sprachliche Stellung der Kāfir-Sprachen”, *Wissensch. Bericht über den Deutschen Orientalistentag* (Hamburg, 1926); N. Morgenstierne, *Norsk Tidsskrift for Sprogvidenskap*, II (1929), p. 192 ff.; T. Burrow, *The Sanskrit Language* (London, 1955), p. 32.

¹⁴ See V. Georgiev, “Die griechischen Nomina auf *-eús* und die baltisch-slavischen Verba auf *-áuju/-újǫ*”, *Lingua Posnaniensis*, VIII (1960), p. 17 ff.

“lamb”, which represents PPP “driven” from *Hag- “drive” and whose strange phonetical peculiarities in the various IE languages are well explained through contamination of *Howi-s + *Hagno-s, cf. Celt. *ogno-s, Germ. *awna-, Gk. *amnós* (*Hagwno-s), Lat. *avillus*.

Today we urgently need a new *Grundriss* and a new etymological dictionary which will reflect the present state (the third period) of IE linguistics.

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DISCUSSION

BONFANTE:

The problem of the connections between Etruscan and Indo-European is not new. It has been examined thoroughly, especially by Italian and German scholars (Skutsch, Pallottino, etc.). While there are some features of Etruscan that seem definitely Indo-European (e.g. *mi* “I”, the -s- genitive, the feminine in -i, et al.), as a whole Etruscan cannot be called Indo-European in the same sense as Slavic, Latin, Germanic, Hittite, or Tocharian. It is at best a distant relative of Indo-European (a third cousin, let us say, of Latin, Hittite, etc., whereas the latter can be called “brothers”). In particular, the Etruscan numerals – known to us in part through the famous dice – cannot be squeezed into the Indo-European mold without unacceptable tricks, whereas e.g. the affinity of Lat— *duo*, *trēs*, etc. with the corresponding Greek, Slavic, Germanic, or Keltic numerals is obvious, even to the layman, at first sight. Of the examples presented by Prof. Georgiev for the asserted connection between Etruscan and Hittite, some cannot be accepted at all (for reasons that I cannot well discuss here). Some can be accepted, but (overlooking the fact that they are not new) they do not prove in the least a connection between Etruscan and Hittite, since they appear in other Indo-European languages. They prove, if anything, a (very distant) connection between Etruscan and Indo-European as a whole, which is an entirely different proposition. I am therefore not convinced in the least by Prof. Georgiev’s arguments.

THE STRUCTURAL FRAMEWORK OF PANINI'S LINGUISTIC ANALYSIS OF SANSKRIT

VIDYA NIWAS MISRA

1. Panini does not explicitly enunciate the underlying principles of his analysis of the speech, which he refers to as "bhāṣā"¹ (=spoken language) and which would correspond to "language" in the Chomskian sense, so that it is a set of sentences and is not merely "type".² Nor has he defined hierarchical levels in explicit terms. Nevertheless we can reconstruct the basis of his analysis. Let us first take up units of utterance. Panini has in view two languages, one spoken in a restricted sphere of ritual activity (chandas) and the other spoken elsewhere (bhāṣā). Sentence he has defined both explicitly and implicitly in terms of (a) phonological juncture boundary, i. e. pause³ (b) sentence accent-patterns⁴ (c) its components called *pada*⁵ (= word) and (d) its types⁶ present in different ways at the morphemic or the morphophonemic or the phonemic levels respectively. In fact sentence is the starting point in his description and it is the syntactic relationship, which is expressed through different types of substitution and which therefore is inherent in compounding and suffixation as well.⁷ A sentence in the third person singular present indicative form has always been used as a kernel sentence in generation of secondary derivation forms, e.g.

"tad adhīte tad veda" (=that studies that knows)

can be substituted by a substitute for *tad* (= that) + a suffix *ika*, so that

"vedam adhīte vedam veda"
(he studies Veda and knows it)
> *veda* + *ika* > *vaidika*

Since it is the verb or an enclitic which is the marker of a clause, Panini has described his sentence types terms of these constructions and their accent morphophonemics.⁸

¹ A.III.2.108, IV.1.62, IV.3.141, VI.1.177 etc. A=Aṣṭādhyāyī, the title of Panini's work.

² S.M. Lamb, On the mechanization of syntactic analysis (private communication).

³ A.I.4.110.

⁴ A.VIII.2.82-108.

⁵ A.VIII.1.16-74.

⁶ E.g. active and passive, imperative, interrogative etc.

⁷ A.II.1.1

⁸ A.VIII.1.16-74 and A. VIII.2.82-108.

The second unit is the word (*pada*) which has been defined in terms of (a) phonemic boundary⁹ (which prevents operations of internal Sandhi rules), sometimes in terms of morphophonemic boundary (as in 1.4.14) or in terms of its external morphemic distribution class i.e. the inflectional endings, both verbal and non-verbal.¹⁰

The morphological word has more extensive function than the phonological one. The third unit is that which comprises a word and is of three types, verbal stem, nonverbal stem and suffix. A verbal stem is defined by listing¹¹ and the nonverbal by elimination.¹² A suffix is defined by its environment and its distinct function. Both verbal and non-verbal stems are of two types, primary and secondary, the secondary ones are defined in terms of combinations.¹³ The so-called indeclinable in Panini's structural framework is a nonverbal stem which has a zero alternant of an inflectional and a feminine derivative suffix.¹⁴ The fourth unit is the *a-l* (= phoneme) defined in terms of inventory of segmental phonemes and the fifth unit is that which would correspond to a supersegmental¹⁵ phoneme, defined in terms of three-way or two-way contrast or phonetic distinctive quality. These are the viable units of utterance in his analysis.

2. Besides these utterance units Panini defined a set of symbols and terms pertinent to his own description. The purpose was doublefold, (1) to effect maximum economy in mnemotechnical terms (and not in visual terms, as the grammar was intended to be transmitted orally), and (2) to group neatly commonly shared functions and features under a classifier or a single term or a list. These three distinct types of groups intersect each other, so that we automatically get classes and subclasses. The framework of these classifications is so complete and consistent that even when an element is not classified by a particular classifier, it is deemed to be so by the virtue of extended definition of that classifier.¹⁶ Two broad types of zero (variable and invariable¹⁷) and three subtypes of the invariable zero (*LU*) Ś, K and P posited by Panini show his structuralistic ingenuity. The underlying principle is almost invariably binary, so that if one group defined in positive terms the counterpart one is automatically defined in terms of elimination of the first. The positive definition is in terms of units which are an integral part of the structural framework of his grammar, so that meaning or for that matter any extralinguistic criteria, the validity of which is to be sought outside the structure of his linguistic analysis¹⁸ (*anuśāsana*), is not pertinent. Instead of giving such names to suffixes as desiderative, denominative or adjectivalizer or possessive, he uses kernel sentences

⁹ A.VIII.1.16-74.

¹⁰ A.I.4.15.

¹¹ A.I.3.1.

¹² A.I.2.45.

¹³ A.I.2.46 and A.III.1.32.

¹⁴ A. II.4.82.

¹⁵ Three pitch levels, 3 quantity contrasts, nasalization (A.I.2.27-31, I.1.8).

¹⁶ E.g. A.I.2.1-26

¹⁷ A.I.1.60-63.

¹⁸ A.I.2.53-57.

(which are substitutable by these suffixations) as headings of the description. In this respect he is a structuralist par excellence.

3. The arrangement of the description is close to that of a generative grammar. Panini has involved two cycles of rules, the inner and the outer, so that the rules grouped in the inner cycle can rotate within that circle,¹⁹ but the rules grouped in the outer one are not obliged to fall in the inner rotational cycle. Most of the rules of outer circle pertain to external Sandhi and so they are in conformity with the principle that the ultimate realization should be the sentence. Only a few rules pertain to internal Sandhi and those are such which otherwise would have necessitated formation of additional and more complicated rules. Rules operate in succession and limitation, extension and specification of a rule is determined by rules of application. One particular syntactic relationship or type is described in terms of its representation as different types of combinations, a sentence or a compound or a derivation (primary and secondary). Sometimes a combination is obligatory and sometimes as in the case of derivational suffixes it is optional. Then these combinations are joined up in accordance with the principle of immediate constituents (*aṅga*²⁰ in suffixation and *upasarjana*²¹ in compounding are the determining constituents for word-order and morphophonemic presence of the entire combination). Then the external Sandhi rules follow and ultimately the different types of representations (as the ultimate utterance) of the same relationship or sentence are generated so for the procedure matches well with that of the generative grammar, but the description also analyses with great precision all the forms existing in the language so that even proper names which could be source of derivation are enlisted and their structural behavior described in the minutest detail. At the same time with the exception of secondary stem forming suffixes, a kernel sentence has not been used, instead a concept closed to the sememic concept postulated by Sydney Lamb²² has been set up. This in turn is represented by one or more than one morphemes, a morpheme by one or more than one morphophonemes and morphophoneme by one or more than one phonemes. Panini stops at the phonemic level. He did not go in to the allophony, as that had been covered already by the Pratiśākhya. Nevertheless the terms morpheme etc. can not be for certain said to have one to one correspondence with the terms used by Panini, for example, a suffix in Panini is not a morpheme, it is mostly an allomorph and a group of morphemes is sometimes algebraically indicated by a single suffix (LIÑ) and a classifier can not be called a morphophoneme in as much as it stands for morphemic distribution of the constituent with which the element classified by it is combined. In short, we can only say that Panini's structure is a fusion of the two distinct types of grammars.

¹⁹ A.I.VIII.1 inner circle and the rest (VIII.2-4) outer circle.

²⁰ A.I.4.13.

²¹ A.I.2.43.

²² S.M. Lamb, On the mechanization of syntactic analysis (private communication).

4. The English translation of Panini's Aṣṭādhyāyī has led to the misgivings that (i) it is an injunctive grammar (ii) and that it is an item and process grammar. It should be made clear that he never used a finite verb form except in one single instance to describe the relationship and function of his linguistic units. The format allows only for preceding or following environment, the element or elements under description and the linking of the particular rule with the preceding rules. As such it is neither an injunctive nor an imperative statement. As regards the emphasis on process, it is repudiated by the framework of classifiers which to a great extent function as morphophonemes represented by lengthened or full grade vowel alternation or by accented or unaccented vowel alternation. At the same time to designate Panini's grammar as an item and arrangement grammar would not be wholly correct, in as much as a single classifier sometimes has more than one morphophonemic representation. In fact Panini has again shown that a synthesis of these two methods can be as workable as any of the two.

5. The other special features of the structural framework of Panini's grammar are :

- (a) It has 3 component parts:
- (i) Constants in description, lists of verbal stems, lists of nonverbal stems (*dhātupāṭha* and *gaṇapāṭha*) and inventory of phonemes (*pratyāhāra*);
 - (ii) definitions and rules of application (*Sañjñā* and *paribhāṣā*);
 - (iii) main body of the description.

Economy is effected by a chain-technique (*anuvṛtti*) so that in one string of rules, the same component of a description will not be repeated and if it is repeated, it is intended to overrule the following exception.

(b) Panini except for three instances has never given a deductive reasoning for his description.

(c) He did not have the necessity of adding illustrations to his rules, firstly because Sanskrit was a spoken language during his period, at least in his area, and secondly because he did not write the description – he merely prepared a mnemonic text, precise and compact, meant for oral transmission.

(d) Panini's achievement lies in presenting a mathematical model of linguistic description and presenting it in a framework, which is one whole and unless one goes through the entire grammar one cannot generate or analyse a communicable unit of utterance.

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APPENDIX

Illustration of the operation cycle of Panini's grammar: Given *bhū* (=to bc existent), if we have to generate a form which would mean, "that which refers to a thing which had been existent" (in the feminine), the operation will be as follows:

- (1) *Bhū* is a verbal stem from A.I.3.1.

(2) It can take a primary suffix under the heading A.III.1.1, subheading A.III.1.91, suffix defined by III.1.2-4, primary suffix defined by A.III.1.93 – *bhū + Kṛt* (primary suffix).

(3) It takes optionally a past tense action primary suffix after a verbal stem through the force of A.III.2.84, which is one of the two (*Ktā* and *KtavatU*) from A.I.1.26 (particularized by A.III.3.114) viz. *Kta* and makes the form a neuter gender one through A.III.3.114 itself; thus *bhū + niṣṭhā* (*past participle*) (*Kṛt*) *bhū + Kta* (neuter gender stem formative).

(4) The stem thus formed is nonverbal through A.I.2.45 read with A.I.2.46, (*bhū + Kta*) is a nonverbal stem (neuter).

(5) The whole becomes a constituent for a secondary derivational suffix (*taddhita*) through the force of A.IV.1.1 read with A.IV.1.46 and A.IV.1.82 [(*bhū + Kta*) *Prātīpadika* (*nonverbal stem*) (*neut.*) + (*taddhita*)].

(6) This suffix is *ṬHaK* through the force of A.IV.1.1 read with A.IV.1.61 and forms the whole as a nonverbal stem again through the force of (4) above. [(*bhū + Kta*) (*Prāt. neut.*) + (*ṬHaK*) nonverbal stem].

(7) This whole constituent takes a feminine suffix *ṆīP* by the force of A.IV.1.1. read with A.IV.1.3 and A.IV.1.15 [(*bhū + Kta*) (*Prāt. neut.*) + (*ṬHaK*) (nonverbal stem) + (*ṆīP*)]

(8) This whole constituent singular takes sU for the nominative through the force of A.1.4.54 read with II.3.46, 1.4.102-104 [(*bhū + Kta.*) (*Prāt. neut*) + (*ṬHaK*) *Prāt.*) + (*Ṇīp*) + (sU)]. Now the morphophonemic operation will start.

(9) The *K* of *Kta*, the *K* of *ṬHaK*, the *Ṇ* and *P* of *ṆīP* are designated *it* (classifier) and are realized as zero through the force of AI.3.1-2,8-9 and A.I.1.60, but each one has the following functions to perform.

(i) *K* of *Kta* prevents the *guṇa* or *vṛddhi* (full or lengthened grade) of the stem,

(ii) *K* of *ṬHaK* brings *vṛddhi* (lengthening) of the first vowel of the constituent of the secondary suffix through the force of V.4.1, VII.2.114 read with VII.2.117-118, *vṛddhi* replacement of *ū* in this case being *au* through the force of A.I.1.1, I.1.3.49-50.

(iii) the shift of accent is operated between the immediate constituents till it reaches the final utterance level.

SPEECH, LANGUAGE AND INNER FORM (Some Linguistic Remarks on Thought)

JOHN W. M. VERHAAR, S. J.

INTRODUCTION

The problem of the Saussurian distinction between “langue” and “parole” is by now a beaten track in linguistics. On the other hand, to bring the Humboldtian notion of “inner form” to bear upon this problem is presumably less usual. Its relevance, indeed, is not too obvious, unless there is a special viewpoint to justify it. We hope that a few remarks on what thought should be taken to be, linguistically speaking, will shed some new light on the “langue” – “parole” problem and justify the inclusion of the notion of “inner form” as well.

The study of thought is a domain ransacked by many disciplines: philosophy, sociology, psychology. It is also the linguist’s business, though the modern rejection of philosophical categories has regrettably led to an opposite view, if not always outspoken. It is with special attention to this aspect of thought that we now wish to investigate the notions of “langue”, “parole” and “inner form”.

Needless to say that only a few items of the literature on the subject can be discussed and even these no doubt too briefly. But we may, we think, derive some instruction from a consideration of various approaches, especially with regard to the distinction of “langue” and “parole”. Of an interesting type is the Copenhagen view of the “langue” – “parole” relation. Hjelmslev has shown that, if we may word it like that, the semiological viewpoint is so self-contained that no sociological implications need be recognized in order to explain the relation “langue” – “parole”;¹ if de Saussure was a semiologist with strongly sociologist background (as we shall see), semiology is in itself, according to Hjelmslev, amply sufficient. A similar standpoint is represented by Kristen Møller, who also shows how clearly both the sociological and the semiological view are represented in de Saussure’s “Cours”.² Otherwise there is evidence – as Rulon S. Wells and B. Siertsema have shown – that a purely semiological interpretation of the “Cours” is one-sided;³ it is precisely on account of all this

¹ L. Hjelmslev, “Langue et Parole”, *CFS*, 2 (1942), 29-44.

² Kristen Møller, “Contribution to the Discussion concerning ‘Langue’ and ‘Parole’”, *TCLC*, 5 (1949), 87-94. F. de Saussure, *Cours de linguistique générale*, publié par Charles Bally, Albert Sechehaye, Albert Riedlinger (Paris, Payot, 1949-4).

³ Rulon S. Wells, “De Saussure’s System of Linguistics”, *Word*, 3 (1947), 1-31; 13. B. Siertsema, *A Study of Glossematics; Critical Survey of Its Fundamental Concepts* (s-Gravenhage, Martinus Nijhoff, 1954), 3-8.

that we think the Saussurian standpoint more rewarding: the "Cours" leaves various ways open. Other literature is less important for our purpose; thus Bühler's "third axiom" has, as is conclusively shown by Lohmann, sacrificed the "langue" – "parole" distinction in the Saussurian sense, in order to squeeze it into a diagram in which it is to be united with an alleged "ergon" – "energeia" distinction; while in doing so it seems to have escaped Bühler that Von Humboldt mentions the "ergon"-notion, not to use but solely to reject it.⁴ Finally there is Sechehaye's study on thought and "langue", but in spite of the introduction of the thought-element, Sechehaye keeps concentrating on the relation between individual and society.⁵

Considering thought as a legitimate object of linguistic science we are now, for lack of space, compelled to take the notion of meaning (especially word-meaning) for granted. And it is our contention that, in speech, each meaning is a thought on the part of the language-user.

A meaning, then, is not what we may think "with" a word, "à propos of" a word – the old associationist theory –, but what we think "in" a word.⁶ Of course I may think much more "à propos of", "with", a word than what I think "in" a word. The former may either be genuine thought, or just mental imagery, or even any subconscious "echo"; in short it need not, but it can be, thought. What is thought "with" the word "prison", for example, will be different in the case of an architect, a lawyer, a prisoner, a probation-officer or of the man living across the road. But what is thought "in" the word "prison" is identically the same meaning for these persons and for all speakers of English: it is, simply, the knowledge of what a prison is. Thus, to mention another example: the "à propos"-element "with" the word "London" as "the place where I live", always presupposes this word "London" and its (objective) meaning.⁷

If all meanings in speech are also, and identifiably, thoughts, it follows that the relation "parole" – "langue" cannot be studied quite apart from the thought-element. It should be noted that we do not now pronounce upon the time-honoured question whether all thought is necessarily embodied in speech; for this would not be a linguistic problem. Yet there is one distinction here relevant to linguistics: in so far as a meaning is inseparable from the word "in" which it is thought, the meaning – or the thought – is lingual;⁸ in so far, however, as it is distinguishable from the word it can, so to speak, be hypostatized. In that case the meaning is a concept, itself also a meaning, of course, but of a special kind, whence the special name. We have a concept

⁴ Karl Bühler, *Sprachtheorie, Die Darstellungsfunktion der Sprache* (Jena, Fischer, 1934), 48-69; Johannes Lohmann, "Karl Bühlers 'Drittes Axiom'", *Acta Ling.*, 3 (1942/3), 5-16.

⁵ A. Sechehaye, "La Pensée et la Langue, Ou: Comment concevoir le rapport organique de l'individuel et du social dans le langage?", *JPNP*, 30 (1933), 57-81.

⁶ A. Reichling, *Het Woord, Een studie omtrent de grondslag van taal en taalgebruik* (Nijmegen, Berkhout, 1935), 325, 243.

⁷ *O.c.*, 239; we have slightly altered Reichling's example.

⁸ To prevent misunderstandings, we use the term "linguistic" only in the sense of "pertaining to linguistic science", while "lingual" will be taken to mean "pertaining to language".

as soon as we abstract from the lingual character of a certain meaning, which, nevertheless, still remains bound up with the word, now called a term. A word is organically one with its meaning; likewise a term is one with a concept. Conceptualization leaves, as it were, language behind, although the words remain as (scientific or philosophical) terms. Linguistically the difference is important in that terms are much more easily substitutable by other terms than are words by other words: it is easier to replace, say, the term "phonology" by "phonemics" (provided I make it clear what is meant), than to replace everyday words like "table" and "chair" by other words. In short: the unity of term and concept is much more liable to disintegration than the unity of word and meaning. Parallel to this distinction, then, of term/concept and word/meaning there is the distinction of two modes of thought, viz. lingual thought and "free" thought, or approximatively, unreflexive and reflexive thought.⁹

"PAROLE" AND "LANGUE"

Thought is naturally always designative of something, it is – to borrow a term from phenomenology – "intentional". This would immediately seem to constitute an objection against our thesis that all meanings are thoughts, for can this also be said of language as "langue"? In other words: a word does not obtain its designative dimension until it is used in the unit of speech: the sentence. The thesis that meanings are thoughts evidently only obtains for speech-meanings, not for the lexical meanings of "langue". And so it appears that lexical (and grammatical) meanings are not equivalent to thoughts. Dictionaries (and grammars) do not think, only language-users do.

The problem, therefore, now arising is: what is the relation of these lexical (and grammatical) meanings to thought? For, though we never come across "langue" as we come across language-use, the "langue" is nevertheless not some nonentity. It is presupposed by speech, not, of course, temporally speaking, for then the reverse also obtains: if there is to be any such thing as "langue", actual speech must have preceded. Our point is not now this chicken-and-egg problem, but only that "langue" is presupposed for any thought to be embodied in actual speech. It is no doubt in this sense that de Saussure considered "langue" as "exterior" with regard to "parole".¹⁰ But when we then ask HOW "langue" is "exterior" to "parole", it appears that de Saussure places this "exteriority" on the community level; "langue" was, to the Genevan scholar, "essentially social and independent of the individual",¹¹ imposing itself on the individuals as a social institution, which is itself immune to individual initiative.¹² This manner of speaking would seem to suggest that "langue" is viewed as a higher-level reality with regard to the lower-level "parole", the latter "executing" what is a social "institution". On the one hand, this suggestion is corroborated when

⁹ The distinction between lingual thought and "free" thought is Reichling's, see *o.c.*, 415.

¹⁰ F. de Saussure, *Cours*, 98, note 1 (editors' comment), 31.

¹¹ *O.c.*, 37.

¹² *O.c.*, 104, 107-108.

we realize how much de Saussure was, as Doroszewski has shown, indebted to the sociologist Durkheim, who, in the domain of "représentations", distinguished individual and collective representations.¹³ The latter are social facts somehow "transcending" individual facts: collective representations are "exterior" to individual ones, determining these individual representations, in the same way as the latter are "exterior" to the individual's brain-cells. According to Durkheim, "social facts" (a term also used by de Saussure) exist in a "collective consciousness", and the latter notion is a cornerstone to Durkheim's sociological edifice.¹⁴ On the other hand (while leaving sociology to the sociologist), we do not recognize an entirely exact parallel in de Saussure's debt to Durkheim, since, when speaking on the relation thought - "langue", the Genevan scholar only recognizes a two-level structure of "amorphous" sounds and "amorphous" thought, looking upon the "langue" as an intermediary between the two and also, as no doubt we may say, as "exterior" to BOTH levels.¹⁵ For the level-structure this would seem to imply that the "langue" is "higher" with regard to the amorphous phonic material, but "lower" with regard to the amorphous thought, a mid-position which seems to square best with de Saussure's statement that there is in the two-level diagram neither question of materialization of thought, nor of a spiritualization of sounds; what is meant to be represented by the diagram is that the FORMAL elements of the "langue" (indicated by the vertical lines) are common to the TWO levels. The "langue" is considered only "formally", but so is EVEN THOUGHT ITSELF, a principle that has consistently been worked out by the glossematic school.

This seems to us the bridge, in the "Cours", from the sociological to the semi-logical method, the latter taken entirely diacritically. Therefore, while rejecting the semiological method itself and especially its far-going diacritical implications, we are left with the sociological view, which, for our investigations of thought, again raises the question whether the "langue" is something "spiritual" with regard to "parole" or something "material".

We think that, as a phonological, morphological, syntactic system and as a lexical thesaurus, the "langue" is no doubt intersubjective, but at the same time it cannot be more than a "substratum" of individual thought and, therefore, with regard to "parole", a lower-level entity. Not, of course, as a sort of prerequisite, but as part of the thought itself. As a speaker of English I cannot have any knowledge, or thought, of a cow, unless this knowledge or thought is embodied in the word "cow". Diacritically speaking this certainly implies a dispositional command of the whole system of English too, but this is not the only part played by the word "cow" in my knowledge of cows. This English language is, as a whole, and by no means only diacritically, a "determinant" of any thought in English. The "langue" is, therefore, to all intents

¹³ W. Doroszewski, "Quelques remarques sur les rapports de la sociologie et de la linguistique: Durkheim et F. de Saussure", *JPNP*, 30 (1933), 82-91.

¹⁴ *O.c.*, 83-87.

¹⁵ F. de Saussure, *Cours*, 156.

and purposes something "exterior" to individual speech, NOT, HOWEVER, ON THE "SPIRITUAL", BUT ON THE "MATERIAL" SIDE. The "langue" is a well-defined, though surely immense, domain, outside which no speaker can think anything at all. The "langue" is certainly autonomous, "exterior", but not in the Durkheimian-Saussurian sense that it imposes itself on individual speech as something "higher", on the contrary: it binds speech "from below".

Another argument for our standpoint is what happens when lingual thought becomes "free" thought; then it precisely strives to get away from, to rise over and above, the binding substratum, by the introduction of a terminology. TERMS, however, have a much smaller number of "subscribers" in human communication; intelligible though terms are within a small group of the initiated only (and, in this case, lexically speaking) they can hardly be said to belong to the "langue", into whose vocabulary they have been inserted to suit the scientific needs of a few. Again, terms remain in many ways bound, often to the scientist's and philosopher's annoyance, to the systematic, and to a certain extent even to the lexical, potentialities of the "langue" in question.

We are, accordingly, inclined to question the opinion of Spence that the "langue" transcends the individual "in the sense that no individual Frenchman or Englishman employs all the linguistic means of expression which are current within the French and English speech-communities".¹⁶ For first, this is not in principle necessary: every speaker understands a considerably greater number of words and other means of lingual representation than he would be able to use in his own speech. It should here be pointed out that "speech" as a term in linguistics is quite misleadingly apt to make us forget the role of the hearer, who is a language-user too, co-conducting the speaker's lingual thought, which the hearer could only do on the basis of his command of the "langue". But even so, and this is more important, the greater or lesser extensiveness of a "langue" as a system and especially as a lexical thesaurus does not necessarily run parallel with a greater or lesser potentiality for individual (lingual) thought. The Eskimo language boasting a dozen or so words for so many varieties of snow, but not possessing one single word for "snow(-in-general)" may perhaps involve a more detailed adaptation to the phenomenal world, it does not for that reason also necessarily facilitate thought or, what is the same, enhance the possibilities of "parole", in all respects; in the present instance a quite obvious and not even scientific mode of generalizing abstraction cannot be conducted simply because a word for it is lacking.

LANGUAGE AND ITS INNER FORM

If we bring Von Humboldt's notion of "inner form" to bear upon this problem, this is not an attempt to ignore the great differences between de Saussure and Von Hum-

¹⁶ N.C.W. Spence, "A Hardy Perennial, The Problem of La Langue and La Parole", *Arch. Ling.*, 9 (1957), 1-27; 21; the author, however, does not attach too much importance to the distinction of "langue" and "parole"; he thinks it rather artificial.

boldt. But, quite apart from the thought-aspect of our approach in so far as this aspect necessitates some comments on Von Humboldt, it is already with this 19th century German philosopher that we find the distinction between "die Sprache" and "das Sprechen", even in the immediate context of the often-quoted statement that "die Sprache" is not an "ergon", but an "energeia". Strictly speaking, says Von Humboldt, this "dynamism" or "energeia" of language is in actual speech, but then it is only in the "Sprache", in each particular language, that he discovers the "totality of all speech" and this even "truly and essentially".¹⁷ Elsewhere the "Sprache" is even viewed as the principle of the mind's dynamism.¹⁸ In other words, each "Sprache" has a particular "form" (which, as need hardly be said, has nothing to do with a diacritical conception of "langue") and this "form" represents a certain view of the world, comprising as it does the whole texture of concepts and modes of representation of one part of mankind.¹⁹ The "inner form", then, is what is purely intellectual in the "Sprachform"; although not wholly to the exclusion of feeling and phantasy, it is predominantly to be found in grammatical characteristics.²⁰ This, however, also influences word-meaning, as Von Humboldt has pointed out,²¹ so that there is an unambiguous relevance to our subject in its restriction, mainly, to word-meaning. But, moreover, the Saussurian idea of "exteriority" of the "langue" finds a remarkable parallel in Von Humboldt's conception: he looks upon the "Sprache" as somehow strange to actual speech and peculiar to it, dependent upon speech and independent alike, as autonomous and effectuated.²² In view of the earlier remarks on the primacy of "Sprache", "energeia" and "principle" as it is, it is, however, clear that independence and autonomy loom large in Von Humboldt's conception of "Sprache". His solution that both elements (dependence and independence, etc.) find their synthesis in the unity of human nature is linguistically (and even, we think, philosophically) no great help. It would seem clear that Von Humboldt does not leave much room, if any, for the creativity of actual speech, in contradistinction to "Sprache". Each language, it is true, implies a view of the world; but speech, we should like to add, is not just bound to this view. We are here reminded of Sechehaye's remark (in discussing the relation between thought and "langue") that no one need surrender to the limitations of his own – or any – language, in the same way as he has to put up with the shape of his head or the colour of his hair.²³ No one is forced to accept the interpretative substratum, collectively determined, of his language; indeed, if this were true, nobody could learn another language not his own. There is always some escape from the

¹⁷ W. von Humboldt, *Über die Verschiedenheit des menschlichen Sprachbaues und ihren Einfluss auf die geistige Entwicklung des Menschengeschlechts*. Ed. by A.F. Pott, vol. II (Berlin, 1876), 55-56.

¹⁸ *O.c.*, 51.

¹⁹ *O.c.*, 59 ff.

²⁰ *O.c.*, 104 ff.

²¹ *O.c.*, 109.

²² *O.c.*, 76-77.

²³ A. Sechehaye, "La Pensée et La Langue", 59, 63.

binding forces of one particular language (and, we like to add, of its inner form). True, no such "escape" is ever final, yet none need be the last.

"ESCAPE" FROM "LANGUE": DISJUNCTIVE APPLICATION
AND CONCEPTUALIZATION

It is on purpose that we are speaking of an "escape" and not, say, of a "correction", which would too readily suggest laws of logic. A logical correction is only one species of escape from the "langue"-substratum or from the "inner form"-core of "Sprache". There are various other characteristics of thought which, though found in the "parole", cannot be reduced to characteristics of the "langue" in question. Of these modes of thought we can now only mention two: conceptualization and disjunctive application.

"Disjunctive application" is a term coined by Reichling.²⁴ By it he means that, though the meaning of a word is always a whole, a unit, it is possible to select one or more of the meaning-elements making up this unit and to apply only these elements and not others. It should here be pointed out that Reichling characterizes the act of denomination in speech by considering it as the application of the word's meaning. Normally ALL the meaning-elements are applied, as, say, when I apply the meaning of "terrier" to a terrier, in which case the application is conjunctive. When, however, I select, from all the elements making up the one meaning-unit "terrier", only the two of "on the alert" and "aggressive" and apply only these to, say, a particular man, I am using a metaphor. What is stylistically called a metaphor is linguistically termed "disjunctive application". Metaphoric denomination, it should be noted, is a species of genuine thought, however little logical. The illogical thing about it is not only that a man is not a terrier, but also that the other meaning-elements of "terrier", though not applied, refuse to be divorced from the two elements I DID apply and are, as a consequence, unavoidably ACTUATED in my thought. Indeed, if they were not, it would not be a metaphor I was using, but just a misnomer. It is the actuation meant here which lends to metaphors, especially when they are original, their quite peculiar stylistic character.

Disjunctive application is not bound to one special "langue", it would rather seem to be a characteristic of the "langue". It opens up possibilities of (lingual) thought, which, for all they cannot be explained in terms of the "langue", are open to linguistic investigation.

Conceptualization, our second example, is the thought-process that gives rise to what we have called "free" thought. We have already pointed out that the unity of term and concept is much more liable to disintegration than that of word and meaning. This is the price paid for the "freedom" attained in the conceptualization of meanings. A price, we say, for a concept cannot so easily "hold on" to a term so

²⁴ A. Reichling, *Het Woord*, 326-331.

liable to substitution. Concepts, as a consequence, tend to have something "elusive", in spite of the fact that, unlike meanings, concepts can usually be so clearly defined. In other words: although conceptualization may be an important help for thought, freeing us as it does from those words of which it has justly been said by Sapir that they are not only keys but also fetters,²⁵ yet concepts themselves do not wholly get loose from language. As it is worded by Langeveld: "Expressing itself, thought experiences language as a basis, then as a limit which is at last transgressed . . . only to become finally a limit again."²⁶ Our point is, however, that we are here concerned with "langage" rather than with "langue". For one of the reasons why concepts cannot be used so elastically as meanings is in the nature of concepts rather than in this or that "langue" in which the corresponding terms belong. Linguistically speaking this nature consists, among other things, in the inapplicability of separate elements of a concept to the exclusion of other elements: concepts cannot be applied disjunctively. Exclude a single element of a concept and the concept is lost: what is left is a different concept.²⁷ Terms cannot be used for metaphorical denomination as can words, the thought embodied in terms is too much hypostatized to allow of such elastic use. The difference between meanings and concepts is linguistically also interesting because of the possibility of intermediate modes of thought like, for example, numbers.²⁸ It is by no means a coincidence that an old philosophical adage should compare concepts to numbers. On the level of "free" thought there is no changing them; if one changes a number, what one gets is a different number; change a concept and a different concept takes its place.

Like disjunctive application, conceptualization is not bound to any particular "langue", while it is, *par excellence*, a category of thought and, on linguistic grounds, also the linguist's business. We think that the relation of "parole" to "langue" has not yet been sufficiently studied on the level of thought (except, of course, in the diacritical "content"-level in glossematics, aiming at a calculus, which is not what we have aimed at). Such studies, even if they do not belong to linguistics of "parole" and neither to linguistics of "langue", may, for all that, be highly relevant to linguistic science. Perhaps, we like to ask, to a "linguistique du langage"?

Maastricht (The Netherlands)

²⁵ A. Sapir, *Language* (New York, 1921), 14, 17.

²⁶ M. J. Langeveld, *Taal en Denken* (Groningen-Den Haag-Batavia, Wolters, 1934), 41.

²⁷ A. Reichling, *Het Woord*, 245, 261; and, by the same author, "Het Handelingskarakter van het Woord", *Nieuwe Taalgids*, 31 (1937), 315.

²⁸ See also J. Lohmann, "'Wort' und 'Zahl'", *Zeitschr. für slav. Philologie*, 25 (1956), 151-158.

REPORT OF THE SECRETARY OF CIPL

CHRISTINE MOHRMANN

Mr. Chairman, ladies and gentlemen:

According to the tradition of our congresses, the Secretary of CIPL reads a short report of the activities of the *Permanent International Committee of Linguists* in the period between two congresses.

Our Committee was instituted at the first Congress of Linguists at the Hague in 1928, as a group of linguists representing different fields of study not primarily representing countries or organizations. It had to ensure a certain continuity in the work of the successive congresses and on the other hand to carry out particular tasks with which it is entrusted by the Congress, or what CIPL considered as urgent and useful. We still are working according to these lines formulated in our statutes.

Before reporting, however, on the work of these last five years, it is my task to commemorate the members of the committee deceased since the Congress of Oslo: Mr. Vendryès, who for many years has been one of the most active and wise members of our committee, Prof. Debrunner, one of the workers of the first hour, and Prof. Deeters who for more than ten years took part in our work. Two of the subcommittees lost their secretaries: the Committee of Linguistic Inquiry (CEL) lost its dynamic and indefatigable secretary, Prof. S. Pop. The premature death of Mlle Marguerite Durand, elected secretary of the Committee of Terminology at the Congress of Oslo, too soon put an end to the work which she had begun with enthusiasm. We commemorate all these colleagues, remembering with gratitude what they have done for the international cooperation in the field of linguistics.

In the course of these five years there have been elected as new members of CIPL Prof. Emile Benveniste, Paris, Prof. Leumann, Zürich, Prof. Steinitz, Berlin, Prof. Scherer, Heidelberg, and the eminent president of our Congress, Professor Haugen.

In its meeting of August 29 CIPL studied the question whether it would be desirable to introduce a time limit for membership on our committee. According to the statutes, members are elected *ad vitam*. After ample discussion we decided that a change of our statutes is advisable in the sense that in the future members of CIPL will be elected for ten years with a possibility of re-election for one period of 5 years.

This month, August 30th, the Committee of Linguistic Inquiry (CEL) has met. It has elected as secretary Prof. Carnochan from London. This committee was set up

in 1929 at the suggestion of Antoine Meillet in order to study particularly the problem of dying and disappearing languages. This last is nowadays as urgent as ever. Therefore, the committee has instituted an executive committee of five members to study the problem. A resolution will be drawn up which will be submitted to the Conference of UNESCO in September in order to solicit funds which will enable linguists to study and record those languages which are menaced by extinction or are neglected. If we succeed in raising the necessary funds, this promises to be a very important object.

As to the activities of CIPL in the period of five years since the Congress of Oslo we have tried with the financial aid of UNESCO given through the medium of the International Council of Philosophy and Humanistic Studies – to carry out the tasks with which we were entrusted. Besides the part we have taken in the preparation of this Congress, these activities resulted in a number of publications.

By far the most important periodical publication of the Committee is the Linguistic Bibliography which is a source of anxiety for our committee because the funds available for this annual publication are insufficient. The 1960 volume will be out within a few weeks. We are quite aware of the fact that it would be highly desirable to speed up the rhythm of this publication. This is essentially a question of money, and we are trying to find a solution which will accelerate the publication and also make it possible to add certain sections to the bibliography, particularly in the field of mathematical linguistics.

In connection with the preparation of this Congress, CIPL has published a volume, *Trends in European and American Linguistics*. A second volume is in preparation which will be out before the end of this year. It will report on linguistics outside Europe and America, which for some reason or another was not represented in the first volume.

Under the auspices of the Committee for Linguistic Terminology two dictionaries have been published: Prof. Hamp published a *Dictionary of American Linguistic Terminology*, and Prof. Vachek a *Dictionary of the Terminology of the School of Prague*.

As to CEL (Comm. for Linguistic Inquiry) the late Prof. Pop had published the first part of his *List of Linguistic Institutions* when death put an abrupt end to his activities.

What I have summed up here are the simple results of our work. That we have been able to carry out these tasks with the very small financial resources available, we owe above all to the disinterested help and the cooperation of many linguists who are supporting our efforts in many ways. We thank them all for what they have done and are doing to facilitate the work of our committee and we hope that in the future we may count on their help and cooperation and that of many others.

Suggestions from members or groups of the Congress for new projects and tasks are welcome: as I have already pointed out, our Committee was instituted 34 years ago to carry out the tasks entrusted to it by the Congress and, besides the “ongoing” work such as that for the Linguistic Bibliography, there is always place for new activities and new undertakings.

ANALOGY AND ANALOGIC CHANGE AS REFLECTED IN CONTEMPORARY HEBREW

AARON BAR-ADON

Languages, as it has been widely assumed, are initially acquired, generation after generation,¹ by imitation, which seems to play a particularly decisive role, in the acquisition of the phonological system (and to a great extent also in the lexicon). It is complemented by analogy and analogical creation (predominantly in morphology and syntax), as well as by some other processes or mechanisms.²

Even imitation (in opposition to “reproduction”)³ may be, or tends to be, different from the “original”, in various ways, as well as selective – partly because of the ever-changing environment and range of persons “imitated”.⁴ However, analogy is still, by nature, a far more independent activity or mechanism, which forges the subsequent development of the idiolect,⁵ depending on the powers of abstracting, etc. It thus opens the way to constant variation and gradual changes in a language, even within the span of an “idiolect” and turns the optimum “static” state of synchronism or synchronic relationships into a relatively “mobile” one, and makes the so-called equilibrium of the language into a dynamic equilibrium, so to speak.

¹ There is a difference in “length” and density among the “generations”. They are shorter and denser in the younger ages. The fact that children learn from other children carries important linguistic significance. For more details cf. Hockett’s “Age Grading and Linguistic Continuity”, *Language*, 26 (1950), 449-457, and my “Linguistic Continuity and Contemporary Israeli Hebrew” (forthcoming).

² Cf. Leopold (1949), p. 79 and fn. 9 there. (For references see bibliography at the end of my paper.) Cf. his statements regarding imitation (156). Cf. Chomsky 1959, 42 f. There may be a *two-fold* distinction between the roles of *imitation* and *analogy*:

a. A kind of “vertical” distinction between the impact of *imitation* on the initial or basic acquisition of language by the child, and the subsequent development through *analogical* creativity. This is, in a way, a “diachronic” approach to linguistic processes, even throughout the development of an “idiolect”!

b. A “horizontal” distinction, i.e., between the role of *imitation* in certain areas of the language (e.g. phonology, lexicon), and that of *analogy*, say – in grammar (morphology, syntax). Both may operate simultaneously (although in different degrees) in “idiolect” and “dialect”, thus calling for some “synchronic” approach. For Ullmann’s comments cf. fn. 7 below.

³ But like “borrowing”, which for Bloomfield are identical. Cf. *Language*, 1933, 365; also 476 f.

⁴ *Ibid.*, 476. Cf. Leopold (1949), 156 (and his bibliogr. in fn. 69, 71). Cf. Hockett (1958), 321 f.

⁵ Hockett (1958) distinguishes between “linguistic phylogeny” (dialect, language) and “linguistic ontogeny” (idiolect), pp. 353 ff. The term “idiolect” was coined by Bernard Bloch (1948, p. 7). Cf. R. A. Hall (1951) p. 22. Following Herbert Spencer’s definition of life one may say that “idiolect is a continual adjustment of the internal system to the external conditions (this is true, in a way, regarding “language” too).

Linguistic changes are usually classified for analytical purposes, in accordance with the systems, or rather subsystems, categories or other divisions and levels discernible in the language, e.g., phonetic,⁶ phonemic, grammatical and morphophonemic, etc. However, the changes are mostly complex, interrelated, and they often involve several subsystems and processes simultaneously.⁷

As for the factors or mechanisms that are responsible for these changes there have been identified some minor ones (partly sporadic), such as metathesis, contamination, haplology, assimilation and dissimilation, as well as the major processes of sound change,⁸ borrowing (i.e., external interference), and analogic creation.

We are here particularly concerned with the latter, and its implications and significance, especially in contemporary Hebrew.

Generally speaking, an inflected language provides greater challenge, or stimulus and opportunity, to the speakers to create or produce analogically – apparently, mainly to normalize patterns or regularize paradigms.⁹ Thus, morphology with its inflected paradigms will, probably, be most productive¹⁰ in analogy, and certainly – in a highly inflected language as Hebrew.

Modern Hebrew is an eclectic and still somewhat uncrystallized language, and will provide an even greater stimulus and opportunity to its speakers with highest analogic creativity in the domain of the verb, since the (possessive) declension of the noun has been in decline in the general “informal” speech, and has almost disappeared (except for specific cases) from the speech of the younger generation, i.e., speakers up to the age of 20-25 years. (The reasons and circumstances of the changes are beyond the scope of this paper.)

The study of this half-century-old revived Hebrew speech may have a wider linguistic significance. It has been operating under tremendous, extraordinary, pressures of social, cultural and other demands, so that almost all linguistic processes have been intensively accelerated in meeting the new needs. The inherent capabilities and mechanisms of the language have been expanded (with very special and interesting manifestations in the speech of the younger generation, for whom Hebrew became for

⁶ Hockett (1958, 384-5 et al) differentiates between “sound change” and what he calls “sudden phonetic change”. The latter has apparently developed from Bloomfield’s “sudden sound-change” (1926, p. 164).

⁷ Cf. Bloomfield (1933), regarding the replacement of OE *stanas* by the nominative-accusative form *stones*, which is now used for the whole plural, regardless of syntactic position. This “is part of a larger process, the loss of case inflection in the noun, which involved both phonetic and analogic changes” (410). Cf., however, Ullmann (1957) on the roles of sound-laws vs. analogy as to the regularity in morphology: “synchronistically, analogy is a factor of regularity and sound-laws factors of disruption; diachronistically, it is sound-laws that work regularly and analogy that dislocates the pattern” (173).

⁸ Hockett states that it is “one kind of phylogenetic change” as well as it must be recognized as one of the *mechanisms* involved in other kinds of phylogenetic change” (p. 388).

⁹ Cf. Bloomfield (1933), 410. He identifies analogic change with “systematization” (p. 365).

¹⁰ For “productivity” cf. Hoenigswald (1960), 59 ff.; Hockett (1958), 307 f., etc.

the first time again, in almost two millennia, a real *native* language). This process could be easily exposed to any kind of linguistic observation and follow-up, as if it were under laboratory conditions where you can “catch” fast developments with a “slow-motion” apparatus.

Thus the linguist could observe not only “analogic creation” in the making, but also the results: i.e., the analogic change, which usually occurs (seemingly suddenly) as soon as the innovations begin to replace the older forms (involving idiolect and dialect, of language).

Under such a dynamism, which (as mentioned above) scarcely leaves an actual static status to synchronism, and in face of the intrinsic and intricate constant interactions between the language of the younger generation and that of the older, it seems somewhat difficult to adopt a rigid, formal distinction between “synchronic” and “diachronic in both “idiolect” and “language”.

Here we are confronted with what I am tempted to call co-dimensional situations, i.e., where “horizontal” and “vertical” or “space” and “time”, respectively, do come in contact and “cross” each other.

It is very commonly said that analogy, by nature, disfavors irregularity and divergence,¹¹ in that it tends to regularize or normalize forms, patterns, or paradigms according to the commonest examples. Some would even expect it to be exclusively a kind of simplifying or economizing device, that reduces “irregularities”, or the number of alternants or allomorphs, as in the case of CaCáCtem (/šamártem/) in contemporary Hebrew, for historical CəCaCtém (/šəmartém/), by analogy with CaCáCti (/šamárti/), etc. But analogy is in fact an unpredicted process or behavior (not yet fully explained psychologically), and hence it involves analogic patterning according to minor or rarer forms too. Thus it introduces conflicting analogies, “blends”,¹² *new* alternants, etc., e.g., historical /mak(k)ir/ is replaced by /mekir/, which in turn is incompatible with the other non-past forms, like /tak(k)ir, nak(k)ir, ləhak(k)ir/, etc.

To illustrate from the broader analogic creations let us take a glimpse, for instance, into the imperative. It has developed mainly along the following two lines:

1) In “informal” speech, all over Isreal, the normative forms of imperative have been replaced by the corresponding periphrastic forms of the second person of the future, e.g.: /telex və -tašuv/, for /lex va-šuv/ “go and return!”, /titbayeš lxa/, for /hitbayeš lxa/, “be ashamed!”. (One may say that there is now in those forms a semantic co-functioning or hierarchy of functions of future and imperative, but this is immaterial here.)

There may possibly be several approaches or explanations to this phenomenon. (One of my informants, a teenager, “explains” that “it is unpleasant to command someone else” – therefore she uses the future forms for imperative too.) However, I

¹¹ Cf. Bloomfield’s (1926) assumption H 6: “Linguistic change may substitute sames for different” and assumption H7: “Analogic change predominantly disfavors irregular glossemes and those which diverge from their fellows; it tends to disfavor them in inverse ratio to their frequency of occurrence.”

¹² Cf. Hockett (1958), 433 etc.

believe that it can be best explained as an analogic creation connected with, or derived from, the “legitimate” negative forms for the imperative (as an apposition to them):

/al tavo/ > /ken tavo/ → /tavo/ (“come!”)
/al tazuz/ > /ken tazuz/ → /tazuz/ (“move!”), etc.¹³

Needless to say, this is accepted most readily by the younger speakers, possibly because most of the commands to them are in the negative. For example, /al telex/, /al tazuz/ “don’t go!”, “don’t move!”, stimulate the responses /telex/, /tazuz/, with or without the emphatic /ken/, that naturally does not appear at all unless it is preceded by a negative challenge.

Such morphological innovations of course result in semantic changes too, for they alter the previous (morpho-semantic) oppositions.¹⁴

2) Also the formal-historical formation of the imperative has been “productive”, especially in certain conjugations. Thus, for instance, we find practically all speakers use the pattern haCCiC for haCCeC (as in /halbiš/, /hagid/ etc).

This is a product of proportions like tešev : šev, t(ə)kabel : kabel, and certainly of tagídi : (h)agídi. Hence – tagid : (h)agid, talbiš : (h)albiš, as well as ta(‘)azvi : (‘)azvi (for ‘izvi, “leave!”, fem.), tišan : šan (for /yšan/ “sleep!”), tipol i pol (for infol/), and so on.

This process has affected much larger areas in the speech of the younger people, for whom it “generated” an over-all, “simple formula”: *future forms minus prefix = equals imperative* (F – Pref = I).

This “formula” has produced very tangible changes in phonology and morpho-phonemics, as illustrated by such proportions as: *tištok : štok*, hence – *tištiki : štki*, *tištku : štku* (with initial 3-consonantal cluster, whereas historical Hebrew does not allow more than two), or *tispri : spri* (adding to the 3-consonantal cluster a change in the historical complementary distribution of the allophones [p ~ f]).

Such morphological changes do, indeed, very rapidly affect the existing “equilibrium” (relatively “dynamic” as it may be) and lead towards a re-organization of the sets of the “distinctive oppositions” (whether binary or otherwise) in all areas or levels of language.¹⁶ We could go on and on like this, pointing out the various instances of analogical creations and comprehensive levellings or normalizations (with extra productivity by the younger generation), within the verbal system, indeed in

¹³ There is probably a grain of rebellion in this formation: notice the (optional) use of the superfluous emphatic /ken/ in the intermediary construction /ken tavo/ (“(do) not come” > “(do) yes come (= come indeed)” → “come!”) which is very characteristic in contemporary Hebrew.

¹⁴ This may imply changes in the syntactic structures too (thus posing new transformational problems, etc.), but this, again, is beyond our scope.

¹⁵ Creations made by the younger generation deserve special mention because: 1. They “grow” with the speakers, and gradually penetrate, as data show, into higher age-levels, thus anticipating, more or less, what will become general (historical) facts after a certain period. This is also implied by the processes of linguistic continuity (including crystallization of speech-habits in puberty; free analogy; imitation of peers, etc.). 2. These native and semi-native young speakers now constitute the major body of speakers for whom Hebrew is the primary or only native language.

practically every category and paradigm. But, in keeping with our initial remarks, we prefer to classify here the analogical creation within the span of an "idiolect" in terms of *three* interrelated or "hierarchical" phases, connected with their time of appearance and duration:

1) *Primary or initial creations* and replacements which appear at a very early age, soon after the child starts to "work" with the morphological patterns,¹⁶ and which fade away around or before the age of seven.

Thus, analogies like /kibal/ (for /kib(b)el/), following /kibálti/ etc. are very common until age 4-5; or /šatáti, šatáta/, (for šatíti, etc.), "normalizing" according to the predominant patterns of the "sound verb" (katálti, katálta) and others.

The most interesting analogy (which fades away by 6-7) is that of the pattern miCaCeC ("mikatel"), for the present form of the niph'al conjugation, for historical niCCaC (niktal), e.g., mikanes, mikanéset/ etc., for /nixnas/ etc., or /miza(h)er/ for /nizhar/ and so on. Through this the present of niph'al also integrates with the *non-past* forms, in analogy with all the other so-called derived conjugations (excluding pa'al), thus creating a "unified" opposition of *past* vs. *non-past* forms.

The residue of these "temporary" analogical creations and their impact on further developments deserve further investigation.

2) *Secondary creations* take place in later stages. Many of them appear at school-age, when the children are exposed to "grammar" (normative drill) and to correction by the teachers. Hence, part of them are rather hyper-corrections, involving analogies with *minor* patterns, e.g.: /somáxat/ ("relying" – fem.) for soméxet; /mekir – mkirim/, for /mak(k)irim/, etc. Many of these innovations become part of the speech habits of the growing young adults.

3) *Essential or permanent creations*, analogic creations that take place in childhood and last through adulthood, thus becoming part and parcel of the system of the idiolect, and, by multiplication, of the "language" as well. E.g. the second person plural of the past /katáltém/ or CaCáCtém (for kataltém = CaCaCtém), /(h)evántém/ and /(h)ivántém/ for /havantém/ etc., with an obvious morphophonemic implication, i.e., that all the syllabic suffixes with initial consonant are now uniformly enclitic (or, in other words, that all the corresponding forms are now penultimate): CaCáCti, CaCáCta... CaCáCtém (!). Another example is the development in the imperative mentioned above: taCCiC and haCCiC for haCCeC etc. Further, the "regularization" of the past of pa'al conjugation – imposing the "katal" pattern and rejecting historical "katel" and "katol", e.g., /yašan/ for /yašen/ ("he slept"), /katánti/ for /katónti/ (and among most younger speakers also /yaxálti/ for /yaxólti/). Or again, the "normalization" of the present there, by imposing the "kotel" pattern and reject-

¹⁶ Lcopold, for instance, states that the intellectual maturity needed for the first steps of abstraction or analogy in the realm of morphological patterns is normally not reached until the turn of the third year. He, however, mentions quite a few instances of awareness of morphological patterns in Hildegard's speech (p. 80 f.). Our data from Israeli children show that many reach this phase between 18-24 months. This is an interesting topic for a psycho-linguistic study. Roger Brown has recently been conducting very important studies in this field.

ing the minor patterns of *katel*, *katal*, etc., which involves new oppositions between the past and the present in the minor patterns mentioned above). Or the “normalizations” in the *pi’el* conjugation like /*pireš*/ for /*pereš*/ or /*peraš*/, and a whole host of changes and implications, that cannot even be hinted at in this brief presentation.

To sum up, analogy is at work throughout the development of the idiolect, on the background of the dialect. But whereas some analogical creations are confined within the younger age-groups, fading away (or being again replaced by other forms) at a certain age, others acquire “tenure” or permanence, within a primary or a secondary phase.

We have glanced at analogic creation and analogic change-in-the-making with some of the interactions between idiolect and language. The accelerated processes in contemporary Hebrew of successive analogical creation, replacement and change suggest that the formal division between synchronic and diachronic may require some modification, since there is “something” co-dimensional in between, especially in the crucial zone between idiolect and dialect.

It might be also worthwhile to mention that analogy has affected the rules of “generation” in syntax, especially in adverbial clauses (temporal and locative) as well as in interrogative ones, but this constitutes a topic by itself.

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DISCUSSION

GALTON:

I should like to point out the foreshadowing of the use of the future for the imperative in the famous prohibitory formulations in Classical Hebrew, *lo tigənov* "thou shalt not steal" or *lo tigəzol* "thou shalt not rob", in the Ten Commandments.

RÔLE DES FACTEURS MORPHOLOGIQUES DANS L'ÉVOLUTION DES PHONÈMES

ALEXANDRE GRAUR

Abstract

La morphologie peut exercer une influence sur la phonétique et, au moins indirectement, sur la phonologie. *-s-* intervocalique a disparu en grec ancien, mais comme *-s-* était devenu le signe du futur, de l'aoriste, du datif pluriel, il a été réintroduit par analogie entre voyelles (*lysō, elysa, trisi*); *k* suivi d'une voyelle palatale a été palatalisé en slave (bg. *rak*, pl. *ratsi*), mais en russe *k* a été réintroduit par analogie (pl. *raki*). Cela veut dire qu'un phonème qui entre dans un mot à paradigme devient par cela même plus stable.

Il faut reconnaître une différence entre les nouveaux phonèmes résultés de changements internes et ceux empruntés à une langue étrangère. Ceux du premier type se maintiennent dans les outils grammaticaux où ils figurent des leur naissance (puisqu'ils ne sont qu'un nouvel aspect des phonèmes existant antérieurement), tandis que ceux du second type sont pendant longtemps confinés à la périphérie de la langue, étant utilisés par le vocabulaire, mais non par la grammaire, ce qui veut dire qu'ils ont bien moins de stabilité dans la langue. C'est le cas de *f*, emprunté au grec par le slave; c'est le cas de *h*, emprunté par le roumain au slave (différents parlés roumains manifestent la tendance à éliminer *h*).

L'utilisation d'un phonème par les oppositions grammaticales pourrait donc indiquer qu'il a été produit par l'évolution intérieure de la langue (c'est le cas de *ă, î* en roumain).

D'autre part, la présence d'un phonème dans les oppositions grammaticales oblige les sujets parlants à en prendre conscience. Comme l'a montré I. Fischer (*Studii și cercetări lingvistice*, XI, 1960, pp. 889-892), la quantité des voyelles est indiquée par écrit en grec ancien pour *e* et *o*, parce qu'elle joue un rôle en morphologie, tandis qu'elle est négligée pour *a, i, u*, parce que les oppositions entre brève et longue sont ici purement lexicales.

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DISCUSSION

JAKOBSON:

This interesting study of morphophonemic factors leading to the preservation or restitution of phonemes elsewhere lost must be complemented by the outline of those morphophonemic factors which confine phonological innovations to one specific category of morphemes, e.g., to desinences only. In both cases we observe a wider or narrower grammatical limitation imposed upon phonemic changes.

M. COHEN:

L'état morphologique se reflète souvent dans des combinaisons de phonèmes et

dans des faits de prosodie. On peut citer l'exemple de langues éthiopiennes modernes où des palatalisations se font dans certaines formes du verbe et non dans d'autres. En grec ancien l'accentuation est différente dans le verbe et dans le nom.

L'observation doit donc tenir compte des mots, et alors on sera amené à considérer non seulement certains changements phonétiques dans leur qualité mais aussi dans leur rendement.

ON DEFINING THE WORD

HANSJAKOB SEILER

I. INTRODUCTION

How is it possible that a phenomenon of language such as the WORD is at the same time so obvious to the layman – most languages have a word for “word”, not however for “morpheme” – and, on the other hand, so questionable to the linguist? A major obstacle which has prevented some linguists from seeing anything linguistically relevant in the word is the idea that the word should be a unit. We should, rather, give up this idea right from the start; for we feel strongly that the word cannot be called a unit in the same sense that a morpheme is so termed. The term unit is in itself not well defined nor applied consistently.

The problem at stake here is that of the levels of linguistic analysis – which has been extensively discussed in the plenary session. Our view in this context is that the word is not a unit but a constituent of a sentence or clause. It shows distribution but it doesn't show contrast. The contrasts among the meaningful elements can be accounted for on the level of the morpheme. The word is a constituent which contracts relations within the frame or on the level of the sentence, this level being one of relations, not of units.

II. OBJECTIVE

The interest in this paper is focused on the linguistic elements or features that prove to be in a distribution relationship with the word. These are: 1. bound morphemes. 2. various features commonly referred to as sentence accent. 3. other words, or pause: the term “word order” might be chosen as covering both phenomena. 4. particles and clitics.

We think that these features are the most relevant to the problem of the word as a linguistic category. As to their formal aspect, they differ greatly. Some are clearly segmental: bound morphemes, particles, clitics. Some are non-segmental: sentence accent.

Some of the features have received attention by other linguists before. Pause has been chosen for defining the word as a minimal unit which occurs between two pauses. The criterion of bound vs. free is involved when the word is said to be the minimum

free form. Some linguists have even preferred to set up four or five different stocks of units such as WORD, LEXEME, IDIOM, MINIMUM FREE FORM, corresponding to the different criteria which, as these linguists think, do not always yield identical results.

There are two major criticisms I should like to put forward concerning such previous work: 1. The relation between the criterion chosen and the unit defined does not become clear: what plausible connection is there between word and pause? 2. Previous work has failed to see that the above mentioned features related to the word in spite of their difference in physical shape are not essentially different in function. Empirical descriptive work in the various languages will have to bring out that there is an extremely close correlation, say, between particles (or clitics) and sentence accent. Or between order and particles; or between order and accent; also between bound morphemes and order, etc.

This can be shown if we base a classification of such features upon their function instead of upon their physical shape. It precisely amounts to classifying linguistic elements according to the way in which they are put to use.

III. DEMONSTRATION

1. We know that BOUND MORPHEMES basically mark the syntactical constructions which relate some words to some other words. The marking is not complete in any language. How about the function of the other features?

2. SENTENCE ACCENT. A feature I can somehow control is the pitch contour in modern standard German. As native speakers we somewhat intuitively know that such segments as *Apfel*, *mittags*, *Baum*, *Sonne* are four separate words and that *Apfelbaum*, *Mittagssonne* are two separate words in spite of the fact that within these last two we find strings of phonemes exactly comparable to the four words mentioned first. Where do we get this intuitive knowledge from?

Suppose a string of phonemes

/vi:rha:benmitagszone/

which we want to be a complete utterance. The morphemic analysis is assumed to be known, not however the word division. If we decide the string /mitags/ to belong in one form class together with such possible substitutes as /a:bends/, /morgens/, there are four segments which might alternatively be given pitch prominence: /vi:r/, /ha:ben/, /mitags/, /zone/. If, however, in an identical string of phonemes we want the segment /mitags/ to belong in one form class with /morgen/, /a:bend/, we only get three alternative shifts of pitch prominence: there is no */mitagszóne/ as there is no */a:bendzóne/ nor */morgenzóne/. This, then, is one of the decisive points in language structure where our information about the difference between two words and a one-word compound made up of two elements is derived from.

What matters most here is the difference in function as signalled by this alternative

shifting of pitch prominence: It is basically a restructuring according to a different hierarchy of meaningful constituents mentioned or implied in previous discourse or in a context situation. What is meant here by hierarchy? We think that there are more than one subtypes of hierarchy within a sentence and that we are dealing here with a subtype that takes into account the amount of information known beforehand, thus varying according to the special needs of the person speaking. Another subtype, far better known to us is represented by phrase structure as marked by (bound) morphemes. This subtype of hierarchy is independent from the speaker.

3. WORD ORDER seems rather to pertain to the subtype mentioned first. When we try to find out what a certain word order, say, /Sonne haben wir mittags/ as opposed to another order /mittags haben wir Sonne/ does, the closest functional correlates we can find are particles: *nun* in the first order type whose function it is to take up a previous expectation; *und* in the second being coordinative in function. While dealing with ORDER we inevitably come across PARTICLES. I have been dealing with this sort of problems in an article in the current issue of WORD published on the occasion of the Congress and need not go into details here.

4. PARTICLES AND CLITICS. As to some of the older IE languages like Greek or Vedic Sanskrit which are notorious for their great number of particles and clitics, a complete reinterpretation of these features seems in order. We know, specifically through Wackernagel's law that already in the Protolanguage particles and enclitics were quite alike as to distribution inasmuch as their preferred occurrence was right behind the first word of a sentence. Now, a class that is distributionally marked off like this must have a common functional denominator. Wackernagel does not tell us what this function is. To put it very briefly, the essential is, as I think, that WORDS are found to be in a syntactic construction with other WORDS. Whereas CLITICS (and particles) rather than being in a construction with any particular element of the sentence belong to the sentence as a whole. They very much act like sentence accent and intonation: they may be used for stressing a particular word; or they may convey their meaning over the whole sentence which is particularly evident in the case of the modal particles in Greek or Sanskrit.

But also the very peculiar rules determining whether a verb is enclitic or non-clitic in Vedic Sanskrit may at last become understandable in the light of what has just been said. When the rule states that the verb is unaccented, i.e. enclitic in a main clause and that it is accented, i.e. non-clitic in the subordinated clause, I think that in the latter case the verb is presented as standing in a syntactic construction with a particular element of the utterance: the subordinated verb depends on the main verb. Whereas the main verb does not show dependence.

These brief remarks referring to a vast complex of problems in these older Indo-European languages ought to make it clear that the problem of enclisis is intimately tied up with the problem of the word. A comprehensive study of all particles and clitics, especially from their functional point of view, should therefore greatly enhance understanding of our immediate problem which is the word.

IV. CONCLUSION

The decisive fact which will have to be uncovered by many more descriptive studies in the various languages is that the features as reviewed in the preceding sections turn out to be subphenomena of one and the same governing phenomenon. This phenomenon must be in itself a sign (signans). Its function is to bring about hierarchy within an utterance.

The intimate interconnection among the subphenomena thus points to one and the same governing phenomenon. And it is this phenomenon which determines the shape, specifically the length of the segments which we call words. For a definition of the word in any particular language the interconnection among the above mentioned subphenomena is decisive whereas the purely segmental aspects are of secondary importance.

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LA NOTION DE NOÈME

LUIS J. PRIETO

Lorsque deux phonèmes sont différents, chacun comporte toujours au moins un trait que l'autre ne comporte pas. Ainsi le phonème /p/ du français comporte le trait "labial" que le phonème /t/ ne comporte pas, et celui-ci comporte à son tour le trait "apical" que celui-là ne comporte pas. Le phonème /p/ comporte le trait "sourde" que le phonème /b/ ne comporte pas, et celui-ci comporte le trait "sonore" que /p/ ne comporte pas. Et de même dans tous les cas.

Si l'on confronte les signifiés des énoncés d'une langue on constate que les choses se passent de façon bien différente. On trouve certainement des énoncés dont les signifiés diffèrent comme diffèrent deux phonèmes, c'est-à-dire, dont les signifiés diffèrent du fait que chacun comporte au moins un trait que l'autre ne comporte pas. Soient, par exemple, les signifiés des énoncés *Donnez-moi le livre* et *Donnez-moi le crayon*: le signifié de *Donnez-moi le livre* comporte le trait "complément direct livre" que le signifié de *Donnez-moi le crayon* ne comporte pas et, à son tour, ce signifié-ci comporte le trait "complément direct crayon" que celui-là ne comporte pas. Pour le reste, ces signifiés sont identiques. Tout se passe donc dans ce cas comme pour deux phonèmes. Mais on trouve aussi des énoncés dont les signifiés diffèrent seulement du fait que l'un comporte un trait ou plusieurs traits que l'autre ne comporte pas, celui-ci ne comportant donc aucun trait qui ne figure également parmi les composants de l'autre. Ainsi, la seule différence qu'il y a entre les signifiés des énoncés *Donnez-moi le livre* et *Donnez-le moi* consiste à ce que le premier comporte le trait "complément direct livre" que l'autre ne comporte pas. Le signifié de l'énoncé *Donnez-le moi* ne comporte en effet aucun trait que le signifié de l'énoncé *Donnez-moi le livre* ne comporte également: les traits "complément direct singulier", "complément direct déterminé", "complément direct masculin", etc., qu'on trouve dans le signifié de *Donnez-le moi*, se retrouvent tous dans le signifié de *Donnez-moi le livre*. D'une certaine façon on peut donc dire que:

$$\text{signifié de } \textit{Donnez-moi le livre} = \text{signifié de } \textit{Donnez-le moi} + \text{trait "complément direct livre"}$$

Lorsque les signifiés de deux énoncés présentent entre eux ce type de différence, je dirai – ce qui me permettra de simplifier l'exposé – qu'ils sont en rapport de "restric-

tion”. Le signifié qui comporte plus de traits sera dit “plus restreint” par rapport à l’autre, et celui-ci “moins restreint” par rapport à celui-là. Ainsi je dirai que les signifiés des énoncés *Donnez-moi le livre* et *Donnez-le moi* sont en rapport de restriction, le premier étant plus restreint par rapport à l’autre et celui-ci moins restreint par rapport à celui-là.

Chaque trait que comporte le signifié de l’énoncé employé dans un acte de parole constitue une précision que le locuteur fournit à l’auditeur à propos du message concret qu’il veut transmettre. Il s’ensuit que, lorsque les signifiés de deux énoncés sont, chacun par rapport à l’autre, l’un plus restreint et l’autre moins restreint, le message concret qu’on transmet au moyen du premier peut toujours être transmis au moyen de l’autre, mais en le spécifiant moins, c’est-à-dire, en fournissant à l’auditeur moins de précisions à propos de ce message. Supposons, par exemple, que le message concret qu’on transmet au moyen de l’énoncé *Donnez-moi le livre* soit la demande à l’auditeur de donner au locuteur un livre déterminé. Or, l’énoncé *Donnez-le moi*, dont le signifié est moins restreint par rapport au signifié de *Donnez-moi le livre*, peut évidemment servir lui aussi à transmettre cette demande. Mais, tandis qu’en employant l’énoncé *Donnez-moi le livre* on indique à l’auditeur que ce qu’on lui demande de donner au locuteur est “déterminé”, “singulier”, “masculin” et, notamment, “livre”, en employant l’énoncé *Donnez-le moi* on précise également que c’est “déterminé”, “singulier” et “masculin”, mais on ne précise pas que c’est “livre”.

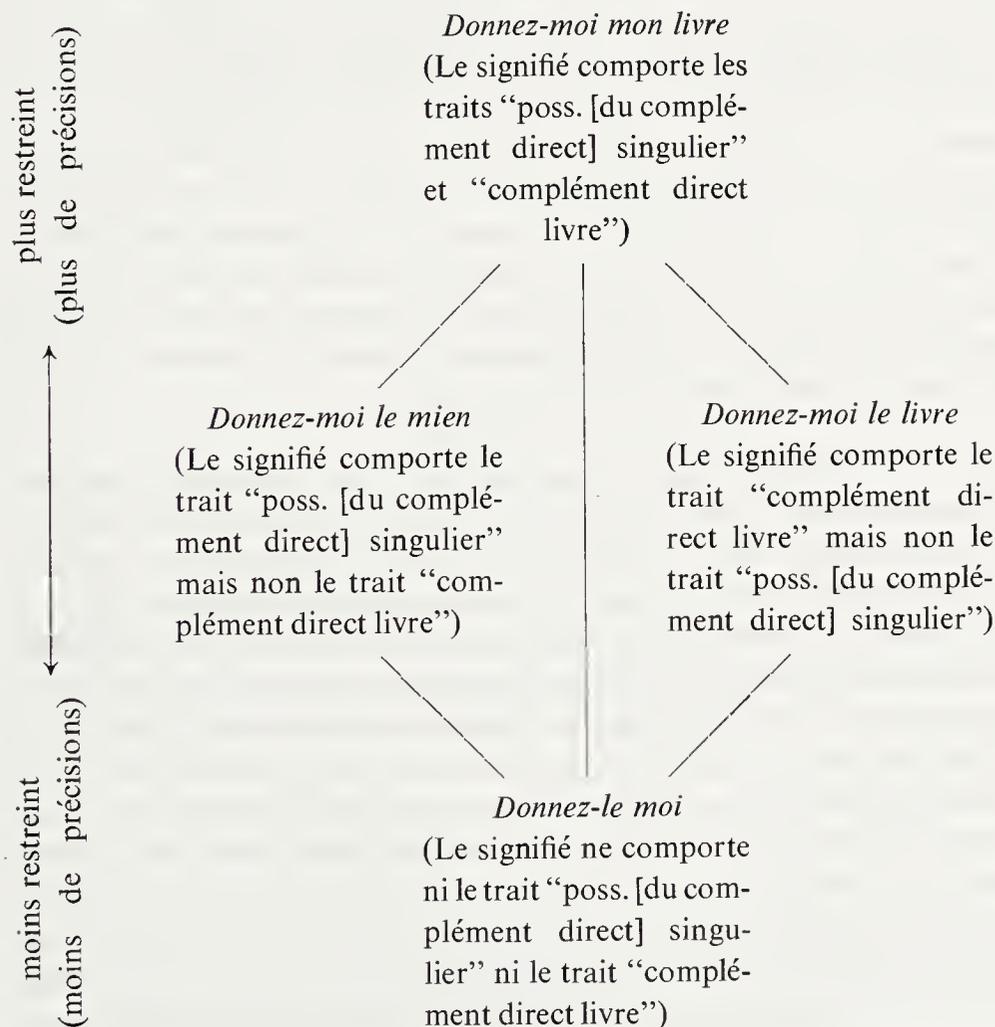
C’est d’une part la situation où l’acte de parole a lieu et de l’autre la tendance à l’économie qui déterminent le choix du locuteur parmi les énoncés servant tous à transmettre son message mais en le spécifiant plus ou moins. La situation, en effet, rend superflues certaines précisions, tandis que d’autres doivent nécessairement être fournies, sans quoi le message risque d’être mal compris ou pas compris du tout. Or, le locuteur choisit de façon à fournir les précisions nécessaires tout en évitant de donner des précisions superflues.

Pourtant, il n’est pas toujours possible de se limiter strictement aux précisions que la situation exige. Il existe en effet des sortes de priorités entre les différentes précisions possibles, de façon que si la situation l’oblige à en fournir certaines, le locuteur est obligé également à en fournir d’autres, même si celles-ci sont, dans la situation, tout à fait superflues. Ce fait, qui est de la plus grande importance car il détermine une organisation spéciale du plan du contenu, devient apparent lorsque l’on confronte le signifié d’un énoncé avec les signifiés des autres énoncés de la même langue qui sont moins restreints par rapport au signifié en question; avec les signifiés donc des autres énoncés de la même langue capables de transmettre le même message qu’on transmet avec l’énoncé en question, mais en le spécifiant moins du fait que leurs signifiés comportent moins de traits. Cette confrontation permet de constater qu’il y a entre les traits qui composent le signifié d’un énoncé trois sortes de rapports. Soit, par exemple, le signifié de l’énoncé *Donnez-moi mon livre*, lequel comporte, parmi d’autres, les traits suivants :

- “possesseur (du complément direct) singulier”
- “complément direct livre”
- “complément direct singulier”
- “possesseur (du complément direct) 1^{re} personne”.

Considérons d'abord les traits “possesseur (du complément direct) singulier” et “complément direct livre”. Il y a dans la langue à laquelle appartient l'énoncé *Donnez-moi mon livre*, c'est-à-dire, en français, des énoncés comme *Donnez-le moi*, dont le signifié est moins restreint par rapport au signifié de *Donnez-moi mon livre* et ne comporte aucun des deux traits en question. Il y a aussi des énoncés comme *Donnez-moi le mien*, dont le signifié est également moins restreint et comporte le trait “possesseur (du complément direct) singulier” mais non le trait “complément direct livre”. Et il y a enfin des énoncés comme *Donnez-moi le livre* dont le signifié est lui aussi moins restreint que le signifié de *Donnez-moi mon livre* mais comporte au contraire le trait “complément direct livre” et non le trait “possesseur (du complément direct) singulier” (voir Tableau I).

TABLEAU I

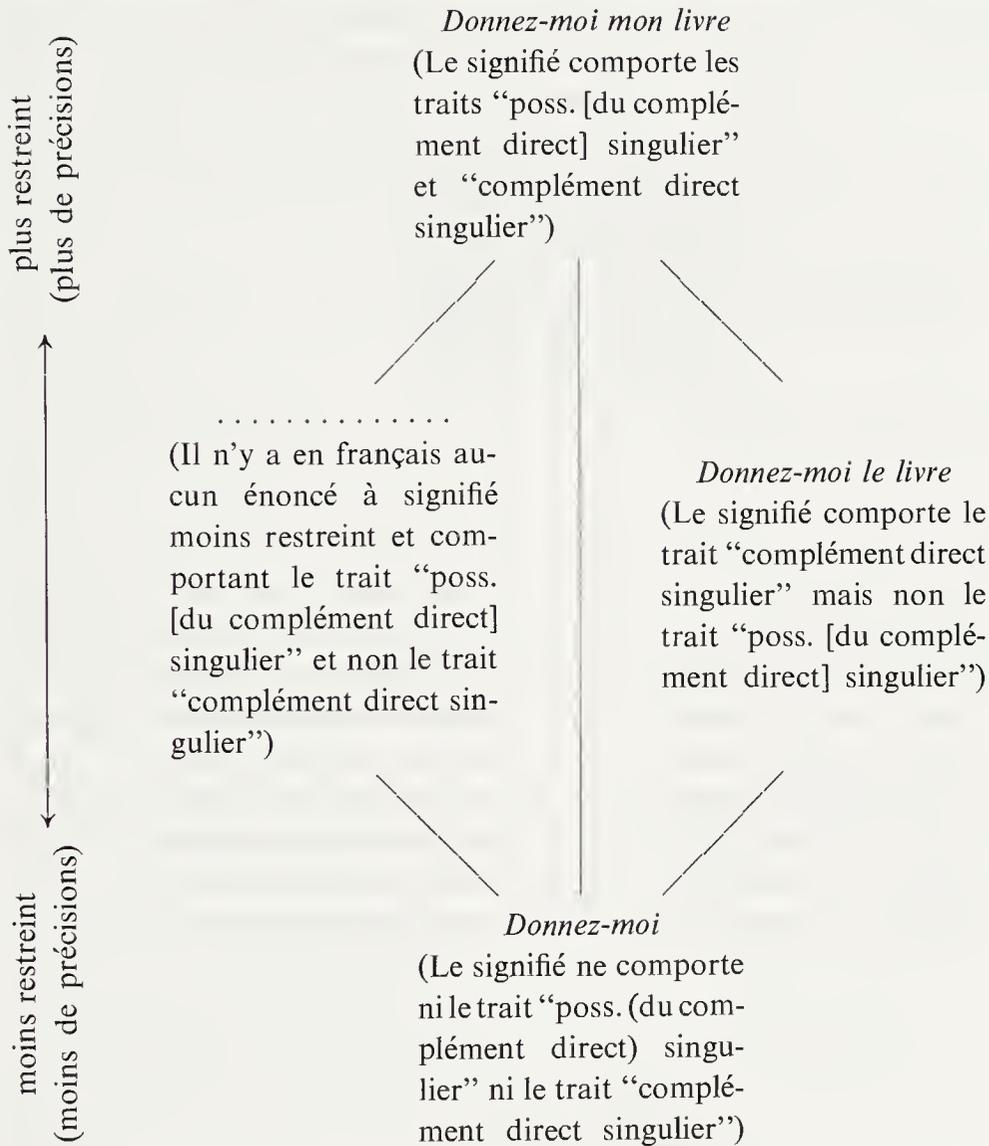


En employant donc l'énoncé *Donnez-moi mon livre* le locuteur indique à l'auditeur, à propos du message concret qu'il veut transmettre, que la chose à donner est un livre ("complément direct livre") et que son possesseur est singulier ("possesseur [du complément direct] singulier"). Mais la langue employée lui offre aussi la possibilité de transmettre le même message en ne fournissant aucune de ces précisions, ou en fournissant la première et non pas la seconde, ou en fournissant au contraire la seconde et non pas la première. Lorsque deux traits composant le signifié d'un énoncé se trouvent dans le cas des traits "possesseur (du complément direct) singulier" et "complément direct livre", je dis qu'ils sont en "rapport a/b " (lire: "rapport a ET b "). Deux traits composant le signifié d'un énoncé et se trouvant en rapport a/b entre eux constituent deux précisions que le locuteur, en employant l'énoncé en question, fournit à l'auditeur, mais dont chacune peut être fournie indépendamment de l'autre.

Considérons maintenant les traits "possesseur (du complément direct) singulier" et "complément direct singulier" qui, comme il a été déjà signalé, figurent parmi les composants du signifié de l'énoncé *Donnez-moi mon livre*. Il y a en français des énoncés, comme *Donnez-moi*, dont le signifié est moins restreint par rapport au signifié de *Donnez-moi mon livre* et ne comporte ni le trait "possesseur (du complément direct) singulier" ni le trait "complément direct singulier". Il y a aussi des énoncés, comme *Donnez-moi le livre*, dont le signifié est moins restreint par rapport au signifié de *Donnez-moi mon livre* et comporte le trait "complément direct singulier" et non pas le trait "possesseur (du complément direct) singulier". Mais il n'y a par contre en français aucun énoncé dont le signifié soit moins restreint par rapport au signifié de *Donnez-moi mon livre* et comporte le trait "possesseur (du complément direct) singulier" et non pas le trait "complément direct singulier": tout énoncé dont le signifié est moins restreint par rapport au signifié de *Donnez-moi mon livre* et comporte le trait "possesseur (du complément direct) singulier", comporte également le trait "complément direct singulier" (voir Tableau II).

En employant donc l'énoncé *Donnez-moi mon livre* le locuteur indique à l'auditeur, à propos du message concret qu'il veut transmettre, que ce qu'on demande de donner est singulier ("complément direct singulier") et que son possesseur est singulier ("possesseur [du complément direct] singulier"). La langue employée lui offre aussi la possibilité de transmettre le même message en fournissant la première de ces précisions, sans la dernière. Mais aucun énoncé ne permettrait au locuteur, tant qu'il parle français, de transmettre le même message en fournissant au contraire la dernière et non la première des précisions mentionnées: du moment qu'il doit indiquer que le possesseur de ce qu'il demande est singulier, le locuteur est obligé d'indiquer aussi que ce qu'il demande est singulier, même si cette dernière précision, dans la situation où l'acte de parole a lieu, est tout à fait superflue. Lorsque deux traits qui composent le signifié d'un énoncé se trouvent dans le cas des traits de l'exemple, je dis qu'ils sont en "rapport $\frac{a}{b}$ " (lire: "rapport a SUR b "). Deux traits composant le signifié d'un énoncé et se trouvant en rapport $\frac{a}{b}$ entre eux constituent deux précisions que le locuteur, en

TABLEAU II

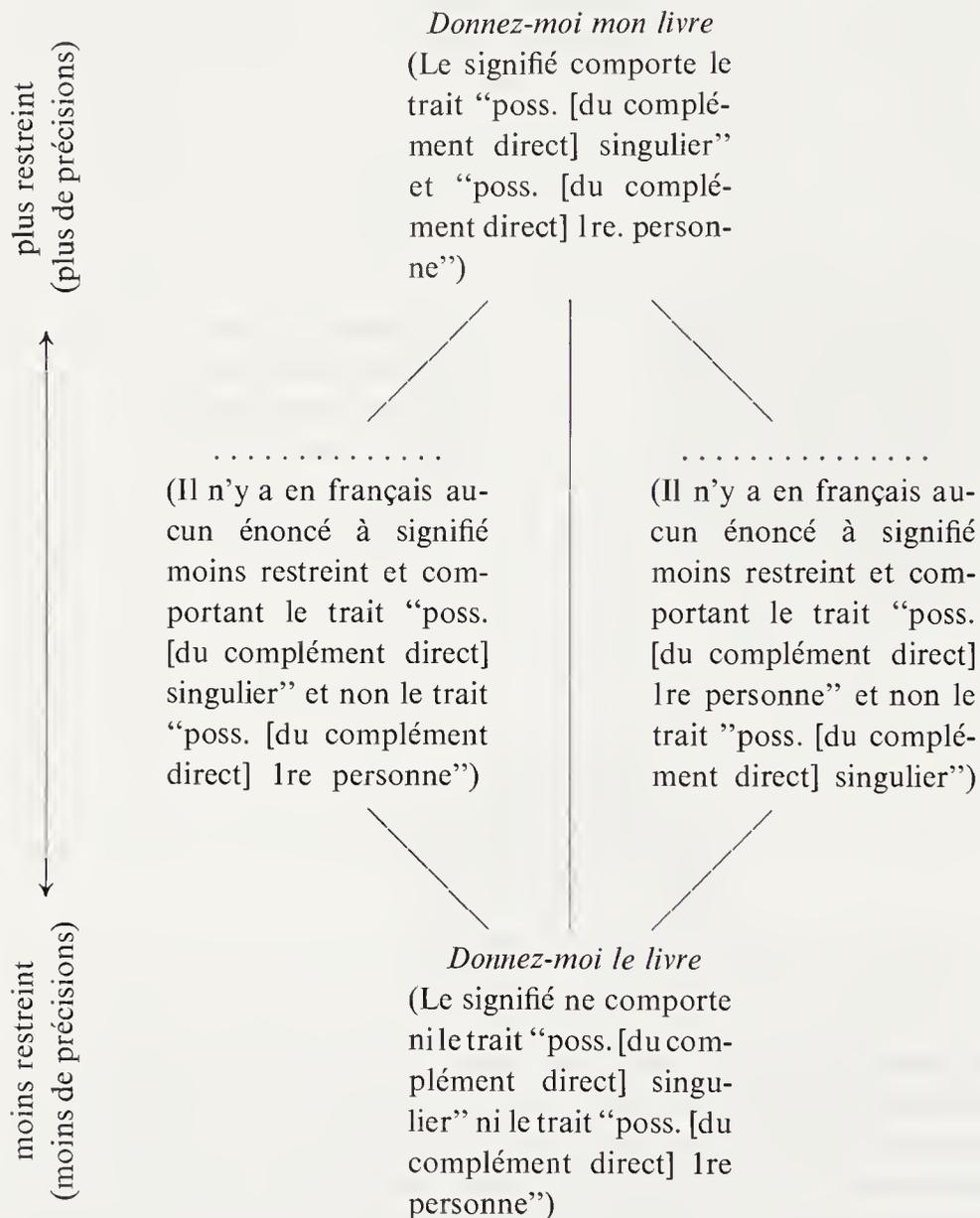


employant l'énoncé en question, fournit à l'auditeur, et dont l'une (a) peut être fournie indépendamment de l'autre (b), mais non vice-versa.

Considérons enfin, toujours dans le signifié de l'énoncé *Donnez-moi mon livre*, les traits "possesseur (du complément direct) singulier" et "possesseur (du complément direct) 1re personne". Il y a certainement en français des énoncés, comme *Donnez-moi le livre*, dont le signifié est moins restreint par rapport au signifié de *Donnez-moi mon livre* et ne comporte aucun des deux traits en question. Mais il n'y a par contre aucun énoncé dont le signifié soit moins restreint par rapport au signifié de *Donnez-moi mon livre* et comporte, soit le trait "possesseur (du complément direct) singulier" et non pas le trait "possesseur (du complément direct) 1re personne", soit, au contraire, le trait "possesseur (du complément direct) 1re. personne" et non pas le trait

“possesseur (du complément direct) singulier”: tout énoncé français dont le signifié est moins restreint par rapport au signifié de *Donnez-moi mon livre* et comporte l’un de ces traits, comporte l’autre aussi, et vice-versa (voir Tableau III):

TABLEAU III



En employant donc l’énoncé *Donnez-moi mon livre* le locuteur indique à l’auditeur, à propos du message concret qu’il veut transmettre, que le possesseur de ce qu’on demande de donner est singulier (“possesseur [du complément direct] singulier”) et que le locuteur y figure (“possesseur [du complément direct] 1re. personne”). Dans la langue employée il y a des énoncés qui permettent au locuteur de transmettre le même

message en ne fournissant à l'auditeur aucune des deux précisions mentionnées. Mais dans cette langue il n'y a par contre aucun énoncé capable de transmettre ce même message en fournissant l'une et non pas l'autre ou vice-versa: du moment où il doit fournir l'une quelconque de ces deux précisions, le locuteur est obligé de fournir l'autre aussi, même si la situation rend celle-ci superflue. Lorsque deux traits composant le signifié d'un énoncé sont dans le cas des traits "possesseur (du complément direct) singulier" et "possesseur (du complément direct) 1^{re} personne", je dis qu'ils sont en "rapport ab ". Deux traits composant le signifié d'un énoncé et se trouvant en rapport ab entre eux constituent deux précisions que le locuteur, en employant cet énoncé, fournit à l'auditeur, et dont aucune ne saurait être fournie indépendamment de l'autre.

En résumant, et en appelant a et b deux traits composant le signifié d'un énoncé, nous dirons donc:

1) que ces traits sont en rapport a/b lorsque la précision à propos du message que constitue a peut être fournie indépendamment de celle que constitue b et réciproquement;

2) que ces traits sont en rapport $\frac{a}{b}$ lorsque la précision à propos du message que constitue a peut être fournie indépendamment de celle que constitue b , mais non réciproquement;

3) que ces traits sont en rapport ab lorsque la précision à propos du message que constitue a ne peut pas être fournie indépendamment de celle que constitue b ni vice-versa.

Ce que j'appelle un "noème"¹ est un ensemble maximum de traits composant le signifié d'un énoncé et se trouvant en rapport ab entre eux. Par "ensemble maximum" je veux signifier un ensemble de traits se trouvant en rapport ab entre eux et ne faisant pas partie d'un autre ensemble plus grand de traits se trouvant également en rapport ab entre eux. Du fait du rapport qu'il y a entre les traits qui composent un noème, celui-ci se comporte comme un tout du point de vue de la spécification du message: un noème constitue en effet un ensemble de précisions que le locuteur fournit à l'auditeur à propos du message concret qu'il veut transmettre, et dont aucune partie ne saurait être fournie indépendamment du reste. Le noème est donc l'*unité de spécification du message*.

Le message concret qu'il veut transmettre constitue pour le locuteur le point de départ de l'acte de parole. Choisir les précisions qu'il fournira à l'auditeur à propos de ce message c'est pour le locuteur choisir l'énoncé, l'entité linguistique abstraite, qu'il emploiera dans cet acte de parole. C'est donc lors de cette opération de choisir les précisions à fournir, opération dans laquelle, comme nous avons vu, le noème se comporte comme l'unité, qu'a lieu pour le locuteur le passage du concret (le message) à l'abstrait (l'énoncé). L'opération mentionnée est analogue à celle qu'accomplit l'auditeur lorsqu'il reconnaît les traits pertinents de l'ensemble de sons concrets qu'il entend: cet ensemble de sons concrets, en effet, constitue pour l'auditeur, tout

¹ Le terme est employé par Bloomfield dans "A Set of Postulates for the Science of Language", définition 50 et dans *Language*, p. 264.

comme le message concret pour le locuteur, le point de départ de l'acte de parole; et en reconnaissant ses traits pertinents l'auditeur identifie l'énoncé employé dans cet acte de parole. C'est-à-dire, que le passage du concret (l'ensemble de sons) à l'abstrait (l'énoncé) a lieu, pour l'auditeur, lors de l'opération de reconnaître les traits pertinents de l'ensemble de sons concrets qu'il entend. Or, dans cette opération, c'est le phonème qui se comporte comme l'unité: du fait que les traits qui le composent sont simultanés, le phonème constitue en effet un ensemble de traits pertinents dont aucune partie ne saurait être reconnue séparément du reste. Comme nous voyons, le passage du concret à l'abstrait se produit pour le locuteur sur le plan du contenu et pour l'auditeur sur le plan de l'expression. Nous pouvons conséquemment en conclure que le noème et le phonème sont, l'un sur le plan du contenu et l'autre sur le plan de l'expression, des unités foncièrement analogues: elles remplissent des fonctions analogues dans les opérations analogues où a lieu le passage du concret à l'abstrait pour le locuteur et pour l'auditeur respectivement.

Les faits que je mentionne dans la présente communication, ainsi que l'unité qu'ils nous ont permis de définir, ont une importance fonctionnelle assez considérable pour qu'on puisse supposer qu'ils se reflètent dans le "sentiment linguistique" des sujets parlants. Aussi me semble-t-il que leur connaissance peut contribuer à obtenir la coïncidence entre ce "sentiment" et la théorie linguistique (coïncidence qui est la pierre de touche de celle-ci, qu'on l'admette explicitement ou non) dans des domaines où elle n'a pas encore été atteinte: je pense surtout au domaine du "mot" et en général de la première articulation. Mais, naturellement, toute tentative dans ce sens se trouve au-delà de ce que je me propose dans cette communication.

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DISCUSSION

KURYŁOWICZ:

Les trois relations traitées dans la communication ($A \parallel B$, $\frac{A}{B}$, et AB) ont été établies par Hjelmslev dans son livre *Grundleggelse: constellation, détermination, et interdépendance*.

SUR LA CATÉGORIE DU NEUTRE

A. ROSETTI

Dans les langues qui possèdent le neutre, la constitution de cette catégorie grammaticale, opposée au masculin et au féminin, est fondée sur la tendance générale des langues d'établir un lien entre forme et substance.

Cette tendance réagit contre l'arbitraire du signe linguistique. L. Hjelmslev a relevé la tendance à la motivation ou manifestation optimum des langues, opposée à la tendance conservatrice. La tendance à la motivation apparaît, reparaît et disparaît, au cours de l'évolution des langues: on lui doit la réintroduction d'anciennes distinctions disparues. C'est à son action qu'est due la création de la catégorie du neutre dans diverses langues indo-européennes.

Nous prendrons comme exemple les faits du tokharien et du roumain, deux langues nettement séparées, attestées à des dates différentes de l'évolution de l'indo-européen et dont l'influence réciproque est exclue.

Dans ces deux langues, le neutre s'est perdu.

Originellement, en indo-européen, les noms étaient répartis en animés et inanimés. Le neutre (inanimé) était caractérisé par une flexion, des désinences propres et le vocalisme de la tranche prédésinentielle. Ensuite, cette distinction s'est perdue et on est passé à une opposition sexuelle, masculin et féminin. La nouvelle situation a eu pour effet l'élimination du neutre.¹

Le vieux système du genre, de l'indo-européen, a été remplacé, en tokharien, par une autre classification, avec opposition du masculin et du féminin et un genre "commun" (ou neutre).

Le tokharien s'est donc créé un nouveau neutre, pour désigner les inanimés. Pour marquer cette catégorie, la langue a recouru au procédé le plus simple, en employant les désinences existantes du masculin et du féminin. Le tokharien a donc donné au singulier du neutre la désinence du masculin, et au pluriel du neutre la désinence du féminin.²

¹ J. Lohmann, *Genus und Sexus* (Göttingen, 1930), p. 80-81; A. Meillet, *Introduction à l'étude comparative des langues indo-europ.*⁷ (Paris, 1934), p. 189-191; L. Hjelmslev, "Animé et inanimé, personnel et non-personnel", *Travaux de l'Institut de linguistique*, I (Paris, 1956), p. 155-199; V. Ivanov, dans *Actes du VIII^e Congrès international des linguistes* (Oslo, 1958), p. 611.

² W. Schulze, E. Sieg, W. Siegling, *Tocharische Grammatik* (Göttingen, 1931), p. 32-33.

Le roumain a procédé d'une manière analogue. Les études consacrées au genre du roumain sont fondées sur la conception traditionnelle, selon laquelle le genre exprime le sexe (masculin ou féminin). D'après cette conception, le neutre n'est donc aucun des deux: ni masculin, ni féminin. La plupart des grammairiens ayant adopté ce critère, ont été amenés à refuser de reconnaître l'existence du neutre en roumain. Selon cette manière de voir, le roumain possède le genre "ambigène" (ou "hétérogène"), à côté du masculin et du féminin.

Cette classification est fondée sur ceci que le roumain, comme le tokharien (v. ci-dessus), emploie pour désigner le neutre la désinence des noms masculins, au singulier, et des noms féminins, au pluriel: sg. m. *scaun* "chaise", pl. f. *scaune*.

C'est en évoquant la tendance commune à plusieurs langues indo-européennes, de marquer la distinction entre "animé" et "inanimé", que les faits du roumain peuvent être expliqués.

Le neutre a été créé, en roumain, pour exprimer l'inanimé;³ car il n'y a pas d'animés, en roumain, qui soient du neutre.⁴

Quant aux désinences, le roumain a recouru, comme le tokharien, aux outils grammaticaux qu'il avait sous la main, à savoir les désinences du masculin, pour le singulier, et du féminin, pour le pluriel.

Par la création du neutre, le roumain s'est montré sensible à la classification des noms en animés et inanimés (en slave aussi, le neutre exclut l'animé, et notamment le "sexué").⁵ Il est allé d'ailleurs encore plus loin, dans cette direction, car il a créé, à l'intérieur de l'animé, un genre personnel (nom de personnes et d'animaux personnifiés), comme en vieux slave, en russe et en polonais. Dans ces langues, l'animé s'oppose à l'inanimé (non-animé) et le personnel au non-personnel.

Le roumain n'a pas hérité le neutre du latin, qui avait déjà disparu dans les premiers siècles de notre ère et n'a été conservé qu'en italien. Il faut repousser l'hypothèse d'une influence du slave méridional, car les faits ne coïncident pas.

Le genre grammatical exprime la forme pure. Mais la catégorie du genre est liée à la substance sémantique des morphèmes et les faits sémantiques sont des faits d'appréciation, donc subjectifs. L'évolution du genre s'explique donc par développement de l'appréciation subjective des faits.⁶

En russe, le genre animé comprend deux catégories corrélatives: le féminin, membre

³ V. nos "Remarques sur la catégorie du genre en roumain", dans *Studia linguistica*, 1959, p. 133-136.

⁴ Souvent, les raisons pour lesquels certains noms font partie du genre animé nous échappent, ce qui ne veut pas dire que ces raisons n'existent pas (sans doute des noms qui ont désigné des forces agissantes, comme en latin *uentus*, qui est du masculin, etc.); L. Bloomfield, *Language* (New York, 1946), p. 271-272: en algonquin, les noms de la "framboise", du "chaudron" et du "genou" sont du genre animé.

⁵ Il y a des cas d'"extension" de l'animation; cf. en russe, вот вино, я его куплю ou en roumain: *am cumpărat cartea pe care m-ai rugat* (G. L. Hall-J. St. Clair Sobell, "Animate Gender in Slavonic and Romance Languages", *Lingua*, IV, 1954, p. 194 s.).

⁶ L. Hjelmslev, *l.c.*

marqué, et le masculin, membre non-marqué, opposés tous les deux à l'inanimé (ou non-animé), qui comprend le neutre.⁷

La tendance à la "motivation", que nous avons déjà mentionnée, explique donc d'une manière satisfaisante la réintroduction du neutre, en tokharien, en roumain et dans d'autres langues indo-européennes.

Le principe de la "motivation", comme l'a montré L. Hjelmslev, "concerne le rapport entre la forme et la substance du mot. Ce rapport peut être plus ou moins "motivé": une catégorie formelle du contenu peut correspondre, dans une mesure plus ou moins grande, à un contenu logique. Mais, au cours de l'évolution des langues, ce rapport peut s'affaiblir ou même disparaître entièrement: ainsi, pour un sujet parlant de nos jours le français, la distinction d'après le genre de divers objets domestiques, tels que la *chaise* (f.) ou le *couteau* (m.), par exemple, n'est pas motivée. Mais au cours de l'évolution des langues, en réaction contre cette tendance à l'effacement, il arrive que la langue cherche à "motiver" ce qui est devenu "immotivé": comme on l'a vu, les langues slaves ont créé ainsi la distinction entre animé – inanimé (personnel – non-personnel), distinction "motivée" par rapport à la distinction masculin – féminin, qui avait perdu sa motivation logique. Et de même le roumain, qui s'est constitué un inanimé (neutre)", opposé à l'animé, qui contient le genre personnel, réservé aux noms de personnes et d'animaux personnifiées.⁸

Nous dirons, pour conclure, qu'il est intéressant de pouvoir suivre, au cours de l'histoire des langues, le jeu des forces internes qui rétablissent parfois des oppositions depuis longtemps disparues, exprimées à l'aide de distinctions formelles existantes dans le système de la langue.

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DISCUSSION

BONFANTE:

Prof. Rosetti claims that Rumanian possesses neutral nouns which are characterized as a definite and independent category by the fact of being masculine in the singular and feminine in the plural (as Graur already had pointed out). While there is no doubt that he is right in attributing a neuter to Rumanian, it is equally true that such a category also exists in ancient and modern Italian: it is even more clearly characterized than in Rumanian by having in the plural a special ending *-a*, which is neither masculine nor feminine, and which Rumanian does not have (both Rumanian and

⁷ R. Jakobson "Zur Struktur des russischen Verbuns", dans *Charisteria G. Mathesio... oblata* (Prague, 1932), p. 79.

⁸ V. notre exposé dans *RLiR*, XXV (1961), p. 369-370.

ancient Italian have another characteristic neutral ending, *-ora*, of Lat. *tempora*, *corpora*, plurals of *tempus*, *corpus*, etc.). We must also remark that the Italian neuter, just like the Romanian neuter, is a *new* neuter, different from the Latin one. For several Italian and Rumanian neuters were masculine in Latin, thus It. *il frutto* : *le frutta* (Lat. *fructus* m.), *il dito* : *le dita* (Lat. *digitus* m.). The Italo-Rumanian neuter follows a new, modern, "scientific" conception of the neuter as an "inanimate". All the Italo-Rumanian neuters are inanimate in the new sense and do not follow the ancient animist Indo-European and Latin conceptions (by which, e.g., the "finger" is a living, animated creature of the masculine sex, Lat. *digitus*, Germ. *der Finger*, Gk. δάκτυλος Rus. *palec*, and so on). I will soon publish an article with more details on the subject.

LOMBARD:

Pour désigner la catégorie grammaticale représentée en roumain par les substantifs du type *scaun*, *val*, pluriel *scaune*, *valurie* (avec *-uri* de *-ora*), et en italien par les substantifs du type *il frutto*, plus *le frutta* (masculins au sing. et féminins au plur.), on a proposé de retenir ou bien (1) la ressemblance que ces substantifs offrent avec les substantifs latins dits neutres, ex. *bellum*, plur. *bella*, ou bien (2) le fait sémantique qu'ils expriment un inanimé ("chaise", "vague", "fruit", etc.). A cela, nous objecterons: (1) La ressemblance avec le neutre latin n'est que partielle, puisque la terminaison *-e* du plur. roum. *scaune* ne remonte pas directement à une terminaison neutre latine, que certains substantifs romans du type indiqué n'étaient pas neutres en latin, et que d'innombrables neutres latins ne font pas partie de ce type roman. (2) Si l'on désigne le type italo-roumain par le terme "inanimé", il faut réserver celui de "animé" aux subst. italiens et roumains appelés généralement "masculins" et "féminins"; mais cela ne saurait convenir, puisqu'un très grand nombre de ceux-ci désignent des choses, des notions abstraites, etc.

Les termes de "neutre" et de "inanimé" ne sont donc, pour désigner la classe des substantifs en question, adéquats qu'en partie. C'est pourquoi nous leur en préférons un troisième, qui est uniquement formel – serait-ce la un inconvénient? –, qui a déjà rendu des services, et qui offre l'incontestable avantage de convenir entièrement et à tous les points de vue: celui de "ambigène".

WEINREICH:

The discussion of new genders might be taken out of its now traditional rut if our horizon were broadened beyond the well known facts of Rumanian and Italian. I believe that the evidence from Yiddish could profitably be added to the standard examples. In the Northeastern dialect of Yiddish, the historical Germanic neuter has disappeared. Sapir, Jakobson, and others tried to attribute this to calquing from the neuterless Lithuanian language or from Belorussian, where the neuter is supposed to have been "weakened". (These are the languages which have been coterritorial with the Yiddish dialect in question.) However, Lithuanian influence is virtually out of the

question, while in Belorussian the neuter gender is safe and sound. In my opinion, there has indeed been Slavic influence, but by a much more indirect and complicated mechanism. The historically feminine words have in NE Yiddish been reclassified into two subgenders, one (mostly) for animate nouns denoting females, the other for inanimates. In this, there may well have been some Slavic influence. But after the feminine had yielded a new inanimate gender, the old neuter lost its distinctive feature and hence its *raison d'être*. The more immediate cause for its disappearance is therefore, in my opinion, an internal development within the Northeastern dialect of the Yiddish language.

PARTITIVE APPOSITION

E. ADELAIDE HAHN

For a considerable time I have been studying¹ the phenomenon of partitive apposition or *σχῆμα καθ' ὅλον καὶ μέρος* and its traces in some of the Indo-European² languages. Here I propose to summarize my findings.

As will be seen, I employ the term "partitive apposition" in a somewhat wider sense than is usual. The commonest type of partitive apposition is that involving a group and one of its members, or an individual and part of his body;³ but I recognize the

¹ I have treated this subject and allied topics in the following articles, all of which appeared in *TAPA*: "Voice of Non-Finite Verb Forms in Latin and English", 74 (1943) 269-306; "Vestiges of Partitive Apposition in Latin Syntax", 84 (1953) 92-123; "Partitive Apposition in Homer and the Greek Accusative", 85 (1954) 197-289; "A Source of Vergilian Hypallage", 87 (1956) 147-89; "The Origin of the Greek Accusative in Latin", 91 (1960) 221-38; "Body and Soul in Vergil", 92 (1961) 193-219. Full bibliographies may be found in these articles. Unless there is indication to the contrary, all references in the present article are to these articles; they are by volume number only, with no designation of author or periodical.

Other bibliographical data are to be interpreted as follows. *AJP* = *American Journal of Philology*. Brugmann, *Gr. Gr.* = Karl Brugmann, *Griechische Grammatik*⁴, rev. by Albert Thumb (Munich, 1913). Brugmann, *Grund.* = *Grundriss der vergleichenden Grammatik der indogermanischen Sprachen* 1-2², (Strassburg, 1897-1916). Chantraine = Pierre Chantraine, *Grammaire homérique* (Paris, 1948). Delbrück, *Grund.* = B. Delbrück, *Grundriss der vergleichenden Grammatik der indogermanischen Sprachen* 3-5 (Strassburg, 1893-1900). Goodwin = William W. Goodwin, *A Greek Grammar* (Boston, 1895). Götze, *Hatt.* = *Hattušiliš*, ed. by Albrecht Götze (Leipzig, 1925). Hofmann = Manu Leumann and Joh. Bapt. Hofmann, *Stolz-Schmalz lateinische Grammatik*⁵ (Munich, 1928). *IF* = *Indogermanische Forschungen*. *JCS* = *Journal of Cuneiform Studies*. *KIF* = *Kleinasiatische Forschungen*. *Lg.* = *Language*. *Monro* = D. B. Monro, *A Grammar of the Homeric Dialect*² (Oxford, 1891). Schwyzer = Eduard Schwyzer, *Griechische Grammatik*, 2 vols., vol. 2 completed and edited by Albert Debrunner (Munich, 1939-50). *TAPA* = *Transactions of the American Philological Association*. Whitney = William Dwight Whitney, *Sanskrit Grammar*², 5th issue (Cambridge, 1923). References are to pages. All citations from Greek, and all from Latin with no indication of authorship, are to be attributed to Homer and Plautus respectively. Quotations are sufficiently full to show sense and syntax, but are not necessarily complete; there is no indication of words omitted. As is obvious, translations aim at being literal, not literary.

² Or Indo-Hittite. I agree with Sturtevant's views on the subject; see especially, among recent publications, Lane, *Lg.* 37.472 fn. 12a, and the reprint of Sturtevant's own chief statement of the case, *Lg.* 38.105-10. But the controversy has no bearing on the present discussion, and I shall in this paper use the term Indo-European as it is frequently used, to include Hittite.

³ See especially 84.93 and 85.199. Two particularly interesting examples from Homer, involving not two members in apposition but three, combine both types: *Il.* 11. 11-2 Ἀχαιοῖσιν δὲ μέγα σθένος ἔμβαλ' ἐκάστω καρδίη "she instilled great strength into the Achaeans into each (of them) into (his) heart", and 7.215 Τρῶας δὲ τρόμος αἰνὸς ὑπήλυθε γυῖα ἕκαστον "terrible trembling seized the Trojans their) knees each (of them)".

phenomenon in any phrase involving the placing in apposition with each other of any two substantives that are not mutually coextensive, such as those denoting respectively a person and an action that he either performs (subjective relation) or receives (objective relation).⁴ I shall refer to the two members of a pair in partitive apposition as respectively the whole-noun⁵ and the part-noun.

There is evidence that in the Ursprache partitive apposition was extremely common and it still continues so in the early form of many derived languages,⁶ although it came gradually to be replaced by a new construction in which the whole-noun is dependent on the part-noun and is in the genitive case. For convenience and brevity, I refer to this new type (with not complete accuracy) as the genitive construction. This is simply one phase of a widespread development, the replacement of parataxis by hypotaxis.

In Hittite, which probably presents the earliest connected written remains of all the languages belonging to our group, partitive apposition is still extremely common. However, the language even in its earliest documents has already arrived at a transitional stage, for parallel passages, and even duplicate versions of the same passage, vary between the appositive and the genitive types without, so far as I can judge, the slightest difference in meaning. Such pairs abound, for instance in the Law-Code. I cite just one set: *KBo* 6.3.1.29 (§ 11) *ták-ku LÚ.GÀL.LU-an QA-AZ-ZU ku-iš-ki tu-wa-ar-ni-iz-zi* "if any one breaks a man his arm" (appositive), and 1.37 (§ 15) *ták-ku LÚ.GÀL.LU-aš iš-ta-ma-na-aš-ša-an ku-iš-ki iš-kal-la-a-ri* "if any one tears a man's ear" (genitive).

Partitive apposition of the ordinary type is apparently extremely rare in Sanskrit,⁷ and is not very common in Latin;⁸ but it abounds in early Greek, especially in the combination of person and body-part. Indeed, there may be more examples of it in Homer than is commonly realized, since these are often ambiguous: in instances of two genitives, the whole-noun may be interpreted as a genitive of possession; in instances of two datives, the whole-noun may be interpreted as a dative of reference, or the part-noun may be interpreted as a dative of specification; and in instances of two accusatives, the part-noun may be interpreted as an accusative of specification.⁹

⁴ Since verbal nouns lack voice, the two relationships are formally indistinguishable. See 74.269-306, especially 273-4, 286-9, 298-300, 303.

⁵ As a matter of convenience, I shall apply this term even to pronouns, though in such instances "whole-substantive" would be more accurate than "whole-noun".

⁶ I am confining my attention to Indo-European languages because they are the only ones of which I have first-hand knowledge. However, I am aware that the construction is by no means restricted to them; cf. e.g. the studies of Arabic scholars, who call it *Badal*.

⁷ See Brugmann, *Grund.* 2.2.633, and Delbrück, *ib.* 3.385.

⁸ See 84.100-7; and, for an earlier, and very useful, treatment of the subject, Hofmann, *IF* 42 (1924) 75-87.

⁹ However, that actually most of these instances are to be interpreted as exemplifying partitive apposition is strongly suggested by the tendency of dative to combine with dative, and accusative with accusative. See 85.219 fn. 67 and the cross-references there given.

So far as possible,¹⁰ I cite one example below, from Hittite as well as from Greek and Latin, for each of the three cases.

*Genitive*¹¹

Greek. *Od.* 22.310 Ὀδυσῆος λάβει γούνων¹² “he took hold of Odysseus (his) knees”.

Latin. *Amph.* 1135-6 Alcumenae usuram corporis cepi “I took the use of Alcmena (her) body”.

*Dative*¹³

Hittite. *Ann.* 1.10-4 A-NA EN SISKUR A-NA GĪR.MEŠ-ŠUŠU.MEŠ-ŠU iš-tar-na SĪG ḥa-ma-an-ki “on the sacrificer on his legs (and) on his arms in the middle(?) she binds wool”.

Greek. *Il.* 3.338 = *Od.* 17.4 ἔγχος, ὃ οἱ παλάμηφιν ἀρήρειν “the spear, which fitted him (his) hands”.

Latin. *Cas.* 337 quis mihi subveniet tergo aut capiti aut cruribus? “who will bring aid to me to (my) back or head or legs?”

*Accusative*¹⁴

Hittite. *KUB* 26.69.7.8-9 nu-wa-za ¹Na-na-ya-an gi-nu-wa e-ip-pu-un¹⁵ “I clasped Nanayas (his) knees”.

Greek. *Il.* 15.433 τὸν ῥ’ ἔβαλεν κεφαλῆν “I wounded him (his) head”.

Latin. *Men.* 858-9 hunc senem dedolabo assulatim viscera “I’ll cut up this old man (his) guts to bits”.

There are no corresponding examples with the nominative,¹⁶ for the appositive construction in this case seems to be confined to the group-individual type.¹⁷ As examples of this type, we may cite the following.

Hittite. *Tel.* 1.9 DUMU-MEŠ-ŠU ku-iš-ša pa-iz-zi “his sons each (of them) goes”.

¹⁰ I know of no instance of the genitive in Hittite. Hofmann, *IF* 42.85, denies the existence of the genitive in Latin, but I have found two possible examples, *Amph.* 1135-6 (the one here cited) and the very similar *Amph.* 108. See 84.102 fn. 48.

¹¹ See 85.212-4 and 84.101-2.

¹² One or the other of the two genitives may be replaced by an accusative: note *Il.* 24.465 λαβέ γούνατα Πηλεΐωνος “clasp the knees of Achilles” (or possibly “take hold of Achilles as to the knees?”); and *Il.* 4.463 τὸν δέ ποσόντα πεδῶν ἔλαβε “he caught the fallen man by the foot”. A double accusative is also possible and rather more likely to occur (cf. fn. 9), as in *Od.* 18.258 ἐλὼν ἐμέ χεῖρα “seizing me (my) hand”.

¹³ See 85.214-9 and 84.105-7.

¹⁴ See 85.219-39 and 84.101-3.

¹⁵ Cf. with this *Od.* 18.258 (cited in fn. 12).

¹⁶ Unless in the Hittite passage *Ull.* 2.2.12 (Güterbock, *JCS* 6.14) [ÍG]I.ḪI.A-wa-ma-wa-ra-aš da-štu-wa-an-za “he (is) blind in (his) eyes”, we are to interpret IGI.ḪI.A-wa (representing the neuter noun *sakuwa*) as a nominative. Ehelolf takes it as an accusative of specification, *KIF* 1.395 fn. 6. – In Homer’s references to parts of the body, when the part-noun is nominative, the whole-noun is genitive (possessive), as in *Od.* 20.348-9, or dative (reference), as in *Il.* 17.695-6; and when the whole-noun is nominative, the part-noun is dative (specification), as in *Il.* 3.192-4, or accusative (specification), as in *Il.* 3.226-7. To this last-named construction, I shall revert later.

¹⁷ See 85.200-11 and 84.100-1.

Greek. *Il.* 1.606 οἱ μὲν ἕβαν οἰκόνδε ἕκαστος "they went home each (of them)".

Latin. *Ep.* 212 filios suos quisque visunt "they go to see their sons, each (his own)".

A certain concomitant feature of the development from appositive to genitive construction seems to have occurred particularly often in Latin.¹⁸ When the original (partitive) appositive construction was in process of being replaced by the more logical genitive construction,¹⁹ it is not surprising that by a sort of hyper-urbanism or over-correction the genitive construction sometimes encroached where it did not belong, i.e. where the apposition was not partial but complete. Thus while the always common *multi homines* may occasionally be replaced by *multi hominum*²⁰ without much difference in meaning, *omnes homines* or *cuncti homines* in strict logic should never be replaced by *omnes hominum* or *cuncti hominum*; yet this type of expression (like the much commoner *all of the men* for *all the men* in English) does exist.²¹ Perhaps we may posit a similar origin for the appositional genitive,²² as seen particularly in phrases involving *nomen*,²³ occasionally in geographical terms,²⁴ and not infrequently in colloquial expressions of the type seen in *Poen.* 273 *monstrum mulieris*,²⁵ a vivid variant for *mulier monstrum* (though here the combination of a noun and adjective, *mulier monstruosa*, might seem more likely).

There was also a genuine type of partitive apposition which developed in a peculiar way. This consisted of a word designating a person and – instead of a body-part – a quality.²⁶ Here we might expect the part-noun rather than, as usual, the whole-

¹⁸ See 84.97-8.

¹⁹ The replacement was not absolutely complete. Thus though *decem milia passus* regularly gave way to *decem milia passuum*, the older form, perhaps under the influence of *mille passus*, continued to occur sporadically, as in Nepos, *Milt.* 4.2 (which some editors unnecessarily emend).

²⁰ Cf. Cato, ap. Gellius 10.3.17 *multi mortales*, vs. Pliny, *Hist. Nat.* 16.40.96 *multi hominum*.

²¹ As in Ovid, *Met.* 4.631. For other examples, see Hofmann 391.

²² This type of expression is so common in English (as in *the city of New York*; cf. French *la ville de Paris*) that we usually do not realize how queer it is.

²³ Cf. Cicero, *Fin.* 2.24.78 *nomen amicitiae*, Livy 40.54.9 *sub nomine Flaminii*.

²⁴ The use is mainly but not exclusively Augustan or later, and poetical; we may note the following examples. Of towns, *Bell. Afr.* 36.2 *ex oppido Thysdrae*. Of rivers, Lucilius 126 (Marx) *Silari ad flumen*; Vergil, *Aen.* 6.659 *Eridani amnis* and 7.714 *flumen Himellae*. Of lakes and springs, Vergil, *Aen.* 1.244 *fontem Timavi*; Livy 24.12.4 *lacum Averni*. Of mountains, Vergil, *Aen.* 8.231 *Aventini montem*.

²⁵ So too Cicero, *Fam.* 5.8.2 *quaedam pestes hominum*. Cf. English "broth of a boy".

²⁶ The effect of thus equating a person with one of his qualities or other outstanding features enhances the importance of the particular quality or other feature. We can say that a man is all courage or all charity, just as we can say that he is all eyes, or all ears, or (to echo Catullus 13.14) all nose, or all heart. The so-called bahuvrihi compounds I believe have their origin in this type of expression; so too many of the personal names in allegories (as Piety, Greatheart, etc., in *Pilgrim's Progress*) and folk-lore or fairy-tales (as Goldy-Locks, Red Riding-Hood, etc.). So too a person may be equated with one of those two essential components of the whole man, his body or his soul; this leads to many periphrases or other special forms of expression in Vergil, which I treat in 92.193-219. A person may also be equated with his name, which in myth and folk-tale is a very important part of his personality; thus arose, I believe, the use of *nāma* in Sanskrit and *ὄνομα* in Greek, accusatives of specification which I think had their origin just as did the accusative of specification denoting a part of the body, in early partitive apposition; but this is a special case needing a special investigation, which I am undertaking in a monograph on *Naming-Constructions* now in process of completion.

noun, to become subordinated. An original **vir scelus* might become (since *vir sceleris* is not idiomatic Latin) *vir scelestus*; instead it becomes, following the usual pattern, *scelus viri*, yet it is used in the sense of the reverse *vir scelestus*, as in *Mil.* 1434-5 *scelus viri Palaestrio*, is me inlexit “(that) sin of a man, Palaestrio, he enticed me”, i.e. “that man of sin Palaestrio” etc. Many beautiful Vergilian instances of hypallage may have originated in just this way.²⁷ Thus we may conceive of *Aen.* 10.496 *rapiens immania pondera baltei* “seizing the huge weight of the belt” as having its starting-point in an expression of partitive apposition *rapiens balteum, immania pondera*, and representing in reverse the genitival substitute *rapiens balteum immanium ponderum*.

Partitive appositional phrases involving the accusative were particularly productive in Greek, and those involving the dative were particularly productive in Latin. To the former I believe that we owe the Greek accusative of specification,²⁸ and to the latter the Latin double dative.²⁹

It is very easy to say, and it has been said by various scholars,³⁰ that in *Il.* 5.188 *μιν βάλων ὄμων*, “I hit him (his) shoulder”, a misinterpretation of the second accusative *ὄμων*, really in partitive apposition with the first accusative *μιν*, gave rise to the construction known as the accusative of specification, a construction which, being extremely common in Greek and almost wholly confined to Greek, well merits the name often given to it of *accusativus graecus*.³¹ But the proponents of this view have never been able to refute their opponents who point out that if they are correct the word that is in the accusative with the active voice, being according to them in partitive apposition with the direct object, should become a nominative with the passive

²⁷ See 84.98-9, 87.161 fn. 37, and 92.199.

²⁸ See 85.239-289.

²⁹ See 84.107-11.

³⁰ Notably by Brugmann, *IF* 27 (1910) 128-34. Cf. earlier Delbrück, *Grund.* 3 (1893) 385-6 and 391, and a little later Brugmann again, *Grund.*² (1911) 2.2.633 and 640-41, and *Gr. Gr.* (1913) 436 and 438.

³¹ It is surely significant that precisely the one language in which partitive apposition in the accusative was extremely common was also the one language in which the accusative of specification became likewise common. In Latin, partitive apposition in the accusative is extremely rare; the example already cited for this language, *Men.* 858-9, is the only sure instance that I know of, and even that is not universally deemed sure, for it has been explained as an intentional anacoluthon to suggest madness. (Other instances that might be cited exemplify distributive rather than partitive apposition; see 84.102 fn. 52.) Vergil revives it in the *Aeneid*, doubtless as a borrowing from Homer; but even he has only two instances of it, both in late books, 10.698-9 and 12.270-6. So if the accusative of specification is, as I believe, an outgrowth from the accusative in partitive apposition, it certainly had no opportunity to develop in Latin. On the contrary, like the accusative in partitive apposition, it was surely a deliberate Hellenism. It too was almost non-existent in early Latin; the only sure example is from Ennius, *Ann.* 400 (Vahlen), and this too may be a Hellenism. It reappears in the Ciceronian Age, but among prose-writers it never occurs in the masters of pure Latinity, Caesar and Cicero, being found only in Sallust, who is noted for Grecisms, and the anonymous writer of the *Bell. Afr.*, who has a tendency to use poetic constructions; among poets it is fairly common in Lucretius, who as a writer in the epic style is much influenced by Homer, whereas in Catullus it is confined to his epyllion (64), his one poem markedly epic in character. Of course it is in Vergil, the Homer of Rome, that the Greek accusative becomes really naturalized in Latin. (For all the above statements, see 91.221-38.)

voice, being in partitive apposition with the subject. And this does not happen.³² For the passive corresponding to *Il.* 5.188, we find *Il.* 17.598 βλήτο γὰρ ὄμων “he was hit in the shoulder”, with the body-part noun still in the accusative.

Various other explanations of the origin of the construction have been offered by other scholars, but I find none of them convincing.³³ Brugmann’s view seems to me far preferable, provided we can solve the problem of why we have the accusative and not the nominative of the body-part noun in the passive. And I think this can be done, if we abandon the attempt to go directly from the active to the passive, but instead use the middle as an intermediary stage.³⁴

Here are the successive steps to be followed.

(1) The middle verb indicates that the subject acts either (a) directly on himself, or (b) indirectly for himself. It is thus equivalent to an active verb with a reflexive pronoun in either (a) the accusative, as direct object, or (b) the dative, as indirect object. When an accusative (usually indicating a part of the body) accompanies the middle verb, it must be thought of as equivalent to an accusative with an active verb either (a) in partitive apposition with the accusative reflexive pronoun used as the direct object, or (b) as itself the direct object. The form is the same in either case.

(2) There is no differences in use between the other personal pronouns (or the demonstrative pronouns) and the reflexive pronoun. Cf. *Od.* 13.230 σώω μὲν ταῦτα, σώω δ’ ἐμέ “save these things and save me”; and *Od.* 17.595 αὐτὸν μὲν σε πρῶτα σώω “first save yourself”.³⁵

(3) There is, so far as I can see, no difference in meaning between the active with a reflexive object and the middle. Cf. the just quoted *Od.* 17.595,³⁶ and *Od.* 21.309

³² I have already pointed out (fn. 16) that Homer does not employ in combination the nominative of the person-noun and the nominative of the body-part noun.

³³ See 85.242-54.

³⁴ This I have tried to do in considerable detail, 85.254-89. In the present paper I must content myself with a most rapid summary.

³⁵ The form σώω is extremely troublesome (and so is its treatment by various scholars). It is explained by Monro (20) as the second singular of the present imperative (and also the third singular of the imperfect indicative) formed by irregular contraction from a non-thematic verb in -οω (σαόω) resembling an Aeolic verb in -μι; similarly Chantraine (307). Goodwin (399) actually assigns it to an Aeolic σώωμι, but he classes the indicative form, and presumably the imperative as well, as second aorist, which I suppose would force us to view the imperative as middle. I think the two examples of the indicative (in *Il.* 16.363 and 21.238) are certainly imperfect (conative), and I do not believe the imperative is aorist either. Schwyzer (1.728 fn. 2) says an aorist σώω is “undenkbar”; he prefers to adopt Nauck’s variant (in 17.595) σώου, which would certainly represent a present active imperative, but I think that is what we must have even if we retain the reading σώω. The Liddell-Scott-Jones Greek Lexicon s.v. σώω (1587) rightly – in my opinion – calls the indicative form a third singular imperfect, but its classification of the imperative as a present middle (*ib.*) is surely impossible. Elsewhere, s.v. σφίζω (1748), it simply calls the imperative form second singular, without indicating its tense or voice. (Incidentally, it assigns the forms to σαόω on 1587, and to Aeolic σώωμι on 1748; but I suppose this amounts to about the same thing.) My belief is definitely that the imperative, whether we read σώω (which I would prefer as the *lectio difficilior*) or σώου, is present active.

³⁶ I have just indicated (fn. 35) that in my opinion σώω is surely active. Quite apart from questions of morphology, which must of course be our first consideration, I wonder why if middle it should take a reflexive object.

οὐ τι σώσεται “you will not in any way save yourself”.³⁷ Also cf. with participles instead of finite verbs, *Il.* 14.162 ἐλθεῖν ἐν ἐντύνασαν ἐ αὐτήν “to come, having arrayed herself well”, and *Od.* 12.18 ἦλθ’ ἐντυναμένη “she came, having arrayed herself”.

(4) Thus there is overlapping between the active with a reflexive object and the middle, and the accusative of the body-part appears in the same construction with both.³⁸ But there is also overlapping between the middle and the passive, which so often³⁹ resemble each other in form. Both types of overlapping are evident if we consider the following set of six examples.

Il. 17.551 νεφέλην πυκάσασα ἐ αὐτήν “having covered herself with a cloud”.

Od. 12.225 ἐντὸς δὲ πυκάζοιεν σφέας αὐτούς “they might cover (or hide) themselves within”.

Od. 22.488 ῥάκεσιν πεπυκασμένος ὤμους “having covered your shoulders with rags”.

Il. 14.289 ὄζοισιν πεπυκασμένος “covered (or hidden) by branches”.

Il. 2.777 ἄρματα εὖ πεπυκασμένα “the chariots well covered”.

Il. 23.503 ἄρματα χρυσῷ πεπυκασμένα “the chariots covered with gold”.

In the first two we have the active voice with a reflexive object (first with a participle and then with a finite verb). In the third and fourth we have participles that in form may be either middle or passive; but in the third the speaker probably means the force to be middle (i.e., just as Athena in the first covered herself with a cloud, so Odysseus covered himself with rags; both acted upon themselves), while the fourth is ambiguous. But in the fifth and sixth, referring to inanimate objects, the chariots, the force is clearly passive. Thus the third agrees in sense, but not in form, with the first; the fifth and sixth agree in form, but not in sense, with the third; and the fourth agrees in form with both the third and the fifth and sixth, and in sense with either the third or the fifth and sixth.

Similarly *Il.* 22.461 παλλομένη καρδίην may mean either “shaking herself (her) heart” (middle) or “shaken as to (her) heart” (passive). Examples of the sort can be multiplied, especially with verbs of emotion (joy, sorrow, anger, etc.). Even a verb of striking like βάλλω can be so used when it is employed metaphorically, as in *Il.* 9.9 ἄχει βεβολημένος ἦτορ “piercing his heart with grief” or “pierced to the heart with grief”; but of a real physical injury, there would be a great difference between “wounding one’s heart” and “being wounded in one’s heart”, and thus the sense can be *only* passive in *Il.* 16.660 βεβλαμμένον ἦτορ “injured in his heart” or 17.535 δεδαῖγμένον ἦτορ “pierced to the heart” (both to be taken literally). Yet the construction here may well be conceived of as influenced by that in the various examples

³⁷ See my discussion in 85.258 fn. 199, where I conclude: “Antinous does not envisage the possibility of the beggar’s being saved by any external agency (passive); if he is to be saved, he must by himself accomplish this for himself (middle).”

³⁸ Cf. πόδας in *Od.* 19.356 ἢ σε πόδας νίψει, where the agent washes some one else, and χεῖρας in *Il.* 16.230 νίφατο δ’ αὐτὸς χεῖρας, where the agent washes himself. In the latter passage the use of the middle, νίφατο, of washing one’s hands, is particularly interesting because it directly follows the active, ἔνιψ’ (in 229) of washing an external object, a cup.

³⁹ But not always. That is why we must recognize them as two distinct categories.

just treated; and so we have finally arrived at the passive with a retained accusative that ultimately developed into the accusative of specification.⁴⁰

The development from partitive apposition of the so-called double dative construction in Latin is much simpler to trace. We have already noted one combination of person and body-part, *Cas.* 337. A less certain instance is *Curc.* 486 *linguae moderandum est mihi* "a check must be placed on (my) tongue so far as I am concerned" (i.e. "by me").⁴¹ The mind may be involved as well as the body: *animo* is fairly close to a part of the body in *Amph.* 1057-8 *vae miserae mihi animo malet* "oh dear, things are going badly with poor me, (my) heart"; more purely incorporeal in its connotations in *Stich.* 524 *si tibi nullast aegritudo animo obviam* "if there is no trouble to upset you, (your) spirits". Again, as we noted above in connection with the development of hypallage, a quality may be involved instead of a body-part, as in *Bacch.* 1083 *nimis nolo desidiaei dare ludum*⁴² "I don't want to give too much indulgence to him, (his) idleness", where a slightly different interpretation is also possible, namely, "I don't want to give too much indulgence to him *for the purpose of idleness*". So too we may find a noun denoting the result of action, as in *Amph.* 989 *sum Iovi dicto audiens*⁴³ "I am obedient to Jupiter, (his) word"; or even actual action, as in *Poen.* 1217 *gaudio ero vobis* "I shall be a source of joy to you", where the relation of the person noun to the action noun is subjective ("you will rejoice"), and *Amph.* 1131 *adsum auxilio tibi* "I am on hand as a source of help to you", where the relation is objective ("I will help you"). Examples involving action nouns are legion.

When the noun of action used with a concrete noun which denotes the agent or the recipient of the action is a part of the verbal system, the descent from a phrase in partitive apposition is less obvious than when we have a combination of two more typical substantives; but none the less I think it can be traced. Here are some examples.

Genitive

Hittite. *KUB.* 2.1.2.28 *ZI-aš ar-nu-um-ma-aš* ^DKAL-ri⁴⁴ "to the tutelary deity of the wish, of fulfilling (it)", i.e. "of wish-fulfilment". The verbal noun here belongs to the type which ends in the nominative in *-war/-mar*, in the genitive in *-was/-mas*.

⁴⁰ The fact that so many words for parts of the body are neuter, and thus indistinguishable in nominative and accusative, may have helped to establish the construction, but could hardly have sufficed to bring it about.

⁴¹ Here the normal and natural order, whole-noun preceding part-noun, is reversed.

⁴² On the order, cf. fn. 41.

⁴³ Contrast with the appositive construction the almost directly following example of the genitive construction, *Amph.* 991 *eius dicto, imperio sum audiens*. Possibly the addition here of a second dative, *imperio*, absolutely parallel to *dicto*, may have been instrumental in bringing about the substitution of a subjective genitive, *eius*, for the whole-noun *ei*, in subjective relationship with the part-noun *dicto*.

⁴⁴ This passage is one of a series of parallel expressions in paratactic form. But one member of the series has achieved the hypotactic form, in which in this type of locution the concrete noun becomes the object of the verbal noun, *ib.* 2.26 *ŠU-an ap-pa-an-na-aš* ^DKAL-ri "to the tutelary deity of shaking hand(s)". Since the verbal noun in Hittite is much closer to a noun than to a verb, it is rare and rather surprising to find one taking an object, as here; see 74.272-3.

Götze (*Hatt.* 140) calls it a gerund and points out that it was associated with an adjective which we may call a gerundive.⁴⁵

Latin. I believe we may find in the genitive gerunds exemplifying this construction the origin of the Latin gerundive. I cite some of the fairly numerous examples from early Latin.

Amph. 638 viri potestas videndi “the opportunity of the man, of seeing (him)”, i.e. “the opportunity of seeing the man”.

Capt. 1008 lucis tuendi copiam “the opportunity of light, of seeing (it)”.

Capt. 748 eius conveniundi copiam (of a man) “the opportunity of him, of meeting (him)”.

Truc. 370 tui videndi copia (of a woman) “the opportunity of you, of seeing (you)”.

Capt. 852 nominandi istorum copia “the opportunity of them, of naming (them)”.

In the second, fourth, and fifth examples, where the concrete substantive is either feminine or plural, the invariable form of the verbal form makes it clear that the latter is an active substantive. There is no compelling reason to treat the similar forms in the first and third examples differently; but since here they appear to agree with the concrete nouns, this is in my opinion the type of expression in which the active substantive which we call the gerund, being misunderstood, generated the passive adjective which we call the gerundive, whether or not that generation has already taken place in these particular examples.⁴⁶

Dative

Hittite. *Hatt.* 4.4 *na-an-kán A-NA ERÍN.MEŠ ni-ni-in-ku-u-an-zi ú-e-ri-ya-at* “he directed him for the troops, for gathering (them)”, i.e. “to gather the troops”. Here we have the dative verbal noun which is called by some the infinitive, by others the supine. The relationship to it of the concrete noun is objective.

Sanskrit. *RV* 10.14.12 *asmabhyaṃ dṛṣāye sūryāya punar dātām asum* “let them give us life again for the sun, for seeing (it)”, i.e. “for seeing the sun”; objective type, exactly parallel to the Hittite passage quoted immediately above. *RV* 1.24.8 *cakāra sūryāya panthām anvetavā u* “he made a path for the sun for following”, i.e. “for the sun to follow”; subjective type, much rarer than the other. Scholars have treated as anomalous this agreement in case with the verbal noun of the concrete noun which they think “ought” to be the object of the verbal noun;⁴⁷ they call it “anticipation”

⁴⁵ I go a step further than Götze, and believe this gerund actually generated the gerundive, much as I think it did in Latin (on which see just below); but, as the Hittite development does not involve participative apposition, I shall not deal with it here.

⁴⁶ See 74.277-86.

⁴⁷ Actually, in *RV* 1.24.8, where, as already pointed out, the relationship of the substantive to the infinitive is subjective not objective, what the noun “ought” to be, according to the view of these grammarians, would be the subject and not the object of the infinitive. But Sanskrit infinitives do not have subjects; this is a peculiarly Greek and Latin development. (I have tried to trace the origin in Latin of the subject-accusative of the infinitive in *TAPA* 81.117-29; and I was pleased to note that my lead was followed and my thesis confirmed, so far as my protest against the traditional explanation was concerned, by Moorhouse in regard to Greek in *AJP* 76.176-83.)

or “attraction”.⁴⁸ But actually this is in my opinion a perfectly normal instance of partitive apposition.⁴⁹

Latin. Here we must take cognizance of two types of verbal noun, the gerund and the supine, which behave quite differently, for the concrete noun with the gerund is always in the objective relation (as we saw in the examples with genitives), and the concrete noun with the supine is always in the subjective relation. This undoubtedly explains why the gerund generated a passive adjective agreeing with the concrete noun, since the latter represents the recipient of the action (as in *Amph.* 1131 *adsum auxilio tibi*); whereas the supine could not generate any adjectival construction of the sort, since the accompanying dative represents the agent (as in *Poen.* 1217 *gaudio ero vobis*).

Example of gerund. *As.* 250 *argento comparando fingere fallaciam* “to contrive a trick for money, for getting (it)”. This is of the same ambiguous type as *Amph.* 638 and *Capt.* 748, cited as examples of genitives.

Examples of supines.⁵⁰ *Mil.* 724 *amicis usui est* “he is to his friends to use” or “for use”, i.e. “he is of use to his friends”. *Men.* 693 *tu me tibi habes despiciatui* “you have me for yourself to despise”, i.e. “you have me as a source of contempt”. *Rud.* 294 *hami atque harundines sunt nobis quaestu et cultu*⁵¹ “hooks and rods are for us to earn and live”, i.e. “are a source of profit and livelihood for us”.

Accusative

Sanskrit. *AV* 6.117.1 *pāśān vicṛtaṃ vettha sarvān* “you know all bonds, (their) loosening” or “you know all bonds, (how) to loosen (them)”, i.e. “you know how to loosen all bonds”. In Vedic Sanskrit, as in Latin, many infinitives – indeed in the case of Vedic most infinitives – were datives. But in classical Sanskrit there survived only the verbal noun in *-tu-* (allied to the Latin supine); this, though there are a few dative forms, is mainly accusative, and perhaps it is significant that this particular form is very frequently combined with the accusative of the concrete noun. Such an accusative is called the object of the infinitive, but perhaps originally constituted the whole-noun with which the part-noun was in apposition.⁵²

Latin. I take up the supine first, because it so closely resembles the Sanskrit “infinitive” in *-tu-* just treated.⁵³ Examples are *Bacch.* 347 *amicos iit salutatum* “he went to (his) friends to salute (them)”; and Terence, *Phorm.* 837-8 *me ire dicam ancillulam emptum* “I’ll say I’m going to a maid to buy (her)”, i.e. “I’m going to

⁴⁸ See especially Brugmann, *Grund.* 2.3.917-8; Delbrück, *ib.* 4.470; Whitney 352. But Whitney differs from the other two in that, with his usual acumen, he recognizes that the concrete noun may be in subjective as well as objective relation with the infinitive; cf. above, fn. 47.

⁴⁹ See 84.112-3, also *Lg.* 29.246-9.

⁵⁰ See 74.299 and 84.118-9.

⁵¹ The supine in *-u* is regularly classed as an ablative, but in this passage at least it seems to me to be a dative.

⁵² See 84.113-4, also *Lg.* 29.250-1.

⁵³ See 74.298-9 and 84.120-1.

buy a maid".⁵⁴ Once more I see significance in the fact that the accusative supine of Latin, like the accusative infinitive of Sanskrit, takes a so-called object; and I see added significance in the fact that the dative and ablative supines of Latin *never* do.

We might expect the accusative gerund like the accusative supine to tend to combine with an accusative concrete noun that came to be regarded as its object; but probably the extensive use of the gerundive construction as a substitute for the genitive or dative of the gerund with an object spread into the accusative case as well through the working of analogy. We of course have examples of the ambiguous type (like *As.* 250) in the accusative, e.g. *Bacch.* 338 *diviti homini id aurum servandum*⁵⁵ *dedit* "he gave a rich man this money, (its) keeping", i.e. "he gave a rich man this money to keep". Pacuvius 51 (Ribbeck) *ad stirpem exquirendum* "for (his) family, for seeking (it) out", i.e. "for seeking out his family", is ambiguous if *stirpem* here is masculine,⁵⁶ but a sure instance if it is feminine. *Poen.* 599 *ad hanc rem agundum* "for this business, for transacting (it)", i.e. "for transacting this business", is a sure instance if we accept the well-attested reading *agundum*, which I think should be preferred as the *lectic difficilior*, though the variant *agundam* is usually adopted.⁵⁷ The usage unquestionably occurs in Varro, whose fondness for archaic types of expression is well-known: *RR* 1.23.6 *alia ad serendum* and *LL* 9.42 *ad discernendum figuras*.⁵⁸

I have now completed my rapid survey of various types of locution which, though of great number and great diversity, I think may all be traced ultimately to the single phenomenon, as I understand it, of partitive apposition.

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⁵⁴ A construction of this sort played a large part in the generation of the (so-called) future passive infinitive. If the speaker instead of saying "I'll say that I am going to buy a maid" had wished to say "I'll say that some one is going to buy a maid", he might have used the so-called impersonal passive and said *iri dicam ancillulam emptum*. Just like this is Terence, *Hec.* 39-40 *rumor venit datum iri gladiatores*, in which I recognize a possible example of original partitive apposition, "a rumor came that some one was going to gladiators, a presentation (of them)", i.e. "a rumor came that there was going to be a presentation of gladiators". Presently *ancillulam* in my fictitious example would seem to be the object of *emptum*, and *gladiatores* in the Terence passage would seem to be the object of *datum*. But eventually *iri emptum* and *iri datum* would be fused into a single periphrastic expression, with *iri* suggesting at once future tense (as the verb "go" so often does, in many languages) and passive voice; such a form could not take an object, and so *ancillulam* and *gladiatores* would be thought to be the subject of the supposed future passive infinitive just as they would be of the past passive infinitive *emptam esse* or *datos esse*. Perhaps had the form been used more often, *emptum* and *datum* might have completely lost their supine origin and have become an inflected participial form like *emptam* and *datos*. Cf. 84.120-1.

⁵⁵ The grammarians deny the existence of an accusative gerund except with a preposition, just as they deny the existence of a nominative gerund; but I endeavor to prove the existence of both, 74.286-98 and 84.114 fn. 112.

⁵⁶ Nonius' express statement (226.32) that *stirpem* is masculine here may be due to a misunderstanding of the construction; but it is a fact that the word is often masculine in early Latin.

⁵⁷ See 84.115 fn. 116.

⁵⁸ See 84.115-6.

TOWARD ANOTHER CONCEPTION OF WORD ORDER

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Abstract

In English, where inversion of the subject is relatively infrequent, the following predications, commonplace in Spanish (and a number of other languages) would be utterly impossible:

- * startled-him a sound of...
- * ... a pencil that lent-him the waiter
- * founded-it John Smith
- * When..., always decreases the density

It is, however, quite possible, while using "normal" word order in English, to put into post-verbal position what would have been the post-verbal subject in Spanish; e.g.

he was startled by a sound of ...
... a pencil which he borrowed (got) from the waiter
the founder was John Smith
When ..., there is always a decrease in density

So far as I know, there has been no attempt to make a thorough study of such parallelism as the examples above afford, in any two languages; Tesnière's very general discussion of "interversion des actants" is not directly concerned with the order of sentence-elements. I believe we may be able to analyze exhaustively the possibilities of this kind of parallelism between two languages, then to examine those cases in which it would be impossible (e.g. **Took John his cane* cannot be paraphrased to match *Tomó Juan su bastón*), and that it may (often) turn out to be true that when such parallelism is precluded, what we call the Inverted Subject in the Spanish (etc.) construction represents, historically, a secondary, an adventitious subject (perhaps a resumptive = "he took, John did, his cane"; cf. my study "Reprise in Disguise", in *MLN*, LXXVI, 239-47).

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DISCUSSION

POTTER:

In those contrastive sentences – *John won: The winner was John* – Miss Hatcher has doubtless touched upon a notable trend not only in Spanish but also in the Romance and Germanic languages in general. It might be described as the western European drift from stronger full-verb to weaker copula-noun predication. Compare also *We think: We are of the opinion* and *They hope to succeed: Their hope is for success*. The Germans have a word for it – Substantivseuche. Students of stylistics sometimes deplore it as a symptom of weakening thought, but no one, so far as I know, has investigated it thoroughly in the field of comparative syntax.

A PROPÓSITO DE LAS ORACIONES SUBORDINADAS

GASTÓN CARRILLO HERRERA

1. En sintaxis, teoría de la estructura de campo simbólico de la oración, presentan interés especial los problemas que se refieren a la complejidad de las oraciones hipotéticas.

En este terreno, tiene importancia fundamental la forma en que está estructurado el campo simbólico de la oración total y su ocupación por los signos lingüísticos, especialmente por la llamada oración subordinada. Además, debe tenerse presente que todo lo que se afirme de este tipo de oraciones (génesis, desarrollo histórico, función de los signos específicos) necesariamente debe ser consistente, no contradictorio y de acuerdo a la descripción general que se haga de ellas, descripción que debe ser válida para todos los niveles en que se opere.

2. Las oraciones hipotéticas han sido miradas como oraciones compuestas, opuestas a las paratéticas. Queremos mostrar que esta concepción carece de fundamentos; que ambos tipos son esencialmente diversos. Entre estas oraciones no cabe establecer ninguna oposición o relación, pues no son especies discordes bajo un mismo género. La oración compuesta por coordinación es compuesta pues su "unidad de sentido (mínima independiente)"¹ se estructura por adición de campos simbólicos sintácticamente independientes, enlazados o no por morfemas nexuales. En la hipotaxis nos encontramos, en cambio, con una unidad de sentido estructurada en un campo simbólico único, en el que se halla inserto otro² llamado proposición subordinada.

I

1. En la tradición gramatical y lingüística se ha entendido que una oración compuesta por subordinación es una oración constituida por la unión de dos proposiciones de modo que una de ellas (la subordinada) es parte de la oración total y la constituye mediante su unión con otra, de la cual depende, llamada subordinante.³ La hipotaxis,

¹ Karl Bühler, *Teoría del lenguaje* (Madrid, 1950), p. 404.

² Que no hay que entender como expresión de una "unidad de sentido (mínima independiente)".

³ Albert Sechehaye, *Essai sur la structure logique de la phrase* (Paris, 1950), pp. 179-186; y otros. Sechehaye agrega otros casos, aquellos con subordinada completiva, en los que la proposición principal es el conjunto proposicional que contiene a la completiva (pp. 184 y s.). Este modo de decir es

así entendida, es mirada como tipo especial de composición opuesto antinómicamente a la parataxis. Así trata Bühler,⁴ entre otros,⁵ el problema.

Este pensamiento llega a su forma extrema de presentarse explícitamente en dos trabajos de Karcevski.⁶ Karcevski se pregunta “Comment sont agencées deux propositions en vue de ne former qu’une seule phrase?”⁷ Responde que la unión se efectúa mediante dos procedimientos diversos: a) Por la mera secuencia; resultan así estructuras que llama paratácticas (yuxtapuestas o asindéticas). b) Por explicitación y especificación de las relaciones entre los dos actos de predicación; resultan así estructuras que son de competencia de la sintaxis (síndeton). Esta explicitación sigue la vía de la coordinación o de la subordinación, expresadas mediante signos lingüísticos distintos (pp. 34-36). La subordinación corresponde a aquellas estructuras (“phrases”) entre las cuales hay relación explícita mediante elementos pronominales; estructuras en las que “le rapport entre deux propositions accouplées” se interpreta “comme celui de déterminé a déterminant (TT’ ou T’T)”, cosa que la sintagmática consigue imperfectamente (p. 37).

Esta concepción carece de fundamentos gramaticales, lógicos, psicológicos, genéticos e históricos e introduce en el análisis de ejemplos concretos de un estado de lengua dificultades innecesarias que llevan al investigador a perderse en una casuística sin sentido (ver aquí I, §§ 3-3.3).

2. Desde un punto de vista gramatical no se ha fundamentado nunca claramente por qué debe entenderse la hipotaxis como composición. En una oración compuesta por coordinación hay unión de campos simbólicos. Esto no ocurre en las llamadas compuestas por subordinación, que presentan un campo simbólico único, que no resulta de la unión de proposiciones, sino que depende exclusivamente del verbo fundamental.

Si nos atenemos al sentido, ocurre algo similar. La unidad de sentido de la oración paratáctica se constituye por adición de los sentidos de las proposiciones componentes. En las compuestas por subordinación la unidad de sentido no se establece a partir de la unión de los sentidos de las proposiciones a cuya integración debería su origen.

La complejión de la oración “*le dije que saliera*” no resulta de la adición de campos simbólicos, sino que es análoga a la de la oración “*le dije esto*”; difiere de ella sólo porque presenta en lugar del signo “*esto*” un campo simbólico nuevo, “*que saliera*”.

La oración “*iré cuando me llames*” no se opone a “*yo canto y tú ríes*”. En lo que

sorprendente. Cómo puede ser posible que un elemento esté subordinado a otro del cual él mismo forma parte?

⁴ *Teoría*, § 27. Bühler habría obtenido mejores resultados si hubiese intentado un análisis fenomenológico de los últimos ejemplos que propone, pero que deja intocados (ver p. 466); o aplicando consecuentemente su concepto de campo simbólico, lo que intentamos hacer aquí; ver III.

⁵ Sechehaye, *Ob. cit.*, l.c.; Charles Bally, *Linguistique générale et linguistique française* (Berne, 1950), § 88 (pero una interpretación distinta en el § 191). Rodolfo Lenz, *La oración y sus partes*³ (Madrid, 1939), § 351; y otros. Ver nota 6.

⁶ Serge Karcevski, “Sur la parataxe et la syntaxe en Russe”, en *CFS*, 7, pp. 33-38; y “Deux propositions dans une seule phrase”, en *CFS*, 14, pp. 36-52.

⁷ Karcevski, *Sur la parataxe*, p. 33.

respecta a la estructura de su campo simbólico se la puede oponer sólo a la oración “*iré entonces*”, puesto que presentan igual conformación de campo y difieren únicamente porque en la primera la determinación temporal es analítica y en la segunda sintética.⁸

3. La falta de fundamentos gramaticales de la teoría que criticamos se patentiza al enfrentarla con el análisis de ciertas estructuras concretas, que, analizadas a partir de esos supuestos, se hacen problemáticas, mostrando así desnudamente la inadecuación a lo real de esta concepción.

3.1. Desde este punto de vista no tienen explicación atinada posibles estructuras del tipo “*L’agua que llega a la puerta, Delgadina que moría*”,⁹ pues no pueden entenderse como constituidas por unión de una proposición subordinante y otra subordinada. Tenemos en la oración¹⁰ “*L’agua que llega a la puerta*” un sujeto, “*L’agua*”, determinado no por una construcción verbal (“*llega a la puerta*”) sino por la subordinada “*que llega a la puerta*”, construcción con la que, por una parte, se coge lo real como acción realizándose y, por otra, se la atribuye como característica del sujeto (Tobler, pp. 314 y s.). Ahí reside su peculiaridad.

Para explicar estas construcciones, el pensamiento tradicional se refugia en el expediente del “*que*” pleonástico. Usa, entonces, una hipótesis accesoria e inconsistente, ya que la fuerza de la expresión no proviene del “*que*” allí introducido, el que no es pleonástico pues no es ni innecesario ni sobrante: nada repite de la construcción. La diferencia, además, entre “*L’agua que llega a la puerta*” y “*L’agua llega a la puerta*” no radica en la presencia o ausencia de una palabra, sino en la especial conformación del campo simbólico (Tobler, pp. 314 y s.).

3.2. También esta concepción introduce una dificultad innecesaria en la racionalización de las oraciones subordinadas sin antecedente o con antecedente implícito. En oraciones del tipo “*Quien canta, sus males espanta*” ve el pensamiento tradicional en el mismo pronombre relativo la presencia del antecedente;¹¹ y entiende que “cuando *quien* lleva envuelto en sí su antecedente ha de desdoblarse, atribuyendo el relativo a una oración y el antecedente a la otra”.¹² Se piensa así salvar dos dificultades: 1) Se le encuentra antecedente al relativo; 2) Se sigue considerando la oración total como constituida por la unión de dos proposiciones: “*aquel sus males espanta*” y “*que canta*”.¹³

⁸ Esta manera de considerar el tipo de oración que nos preocupa nos parece esencial. La opinión corriente ha ocultado el problema fundamental, impidiendo así que se expliciten los rasgos verdaderamente relevantes y de acuerdo con ellos se hagan las oposiciones necesarias. Por otra parte, la 1.ª y la 2.ª oración podrían oponerse atendiendo a que una presenta sus elementos distribuidos en dos campos simbólicos y la otra en uno solo. Pero ésta es la diferencia entre una simple y una compuesta.

⁹ Julio Vicuña Cifuentes, *Romances populares y vulgares* (Santiago de Chile, 1912), p. 34. La complejidad de estas oraciones la analiza acertadamente Adolf Tobler, *Mélanges de grammaire française* (Paris, 1905), pp. 301-316.

¹⁰ Ya que el ejemplo propuesto es de una yuxtapuesta. Ver Karcevski, *Sur la parataxe*, p. 34.

¹¹ H. Sweet, *A New English Grammar* (Oxford, 1898), §§ 112 y 220.

¹² Real Academia Española, *Gramática de la lengua española* (Madrid, 1931), § 367.

¹³ Véase la acertada crítica a este modo de decir en Otto Jespersen, *The Philosophy of Grammar*⁶ (London, 1951), n.p. 104.

3.3. Finalmente, es notable la inconsecuencia que se manifiesta en el hacer de lingüistas y gramáticos que trabajan armados de esta concepción. Luego de indicar que la oración compuesta por subordinación está constituida por la unión de subordinante y subordinada, cuando se trata de analizarla, señalan que se estructura en sujeto y predicado, olvidando la racionalización inicial que propusieran. Claro está que esta última interpretación es la única justa, pues sólo a partir de ella puede hacerse inteligible la estructura total de la oración.

II

1. Ante las dificultades señaladas, podría pensarse que lo erróneo no está en considerar la hipotaxis como tipo de composición, sino en no explicitar adecuadamente en qué plano se conciben así los hechos, que no sería el de las estructuras existentes (sincrónicas), sino que esta teoría haría referencia al proceso de composición, a las vicisitudes que habrían experimentado los compuestos en su evolución histórica (Bühler, 446).

La inconsecuencia, entonces, se debería a la confusión en los planos en que se analizarían los hechos. Se consideraría primero el modo de generarse esas estructuras o el desarrollo histórico de ellas y luego se llevaría el resultado del análisis a los compuestos existentes. Así, el error estaría en el tránsito no esclarecido de un plano a otro. Mostraría inconsistencia no la teoría sino su ulterior aplicación al nivel sincrónico.¹⁴ La concepción misma se mostraría impenetrable a la crítica efectuada.

2. Pero ocurre que la falla no está sólo en este tránsito, aunque en él se haga relevante. Entender la hipotaxis como tipo de composición, aún desde un punto de vista genético o histórico, es inadmisibles pues a lo largo de todo el análisis del problema se opera con los conceptos de coordinación y subordinación que se pretende obtener a posteriori.

3. Además, los fundamentos de esta interpretación son también inconsistentes. Los autores piensan la coordinación como forma de composición más antigua, de la que se originaría la subordinación por una mayor cohesión de las estructuras. Se trata de fundar este pensamiento acudiendo al testimonio de las lenguas primitivas, que mostrarían estructuras más antiguas, a las lenguas de los pueblos de baja cultura o al proceso de adquisición del lenguaje por los niños.¹⁵

3.1. Así se quiere inferir de la inexistencia de relativos en una lengua dada en una época dada, la inexistencia de subordinación. Esta aseveración se hace a partir de un supuesto implícito, el de que un estado sintáctico dado sea solidario de un estado morfológico lexical dado. Tal solidaridad no ha sido demostrada y es impensable.

¹⁴ Sin embargo, aquello que es aplicable, en gramática, al plano universal debe lógicamente tener validez en el nivel histórico.

¹⁵ Wilhelm Wundt, *Völkerpsychologie*, II (Leipzig, 1912), pp. 334-446; Lenz, §§ 339-342; Bühler, p. 459.

Hermann ha querido inferir de la no concordancia de los relativos de las lenguas indoeuropeas la inexistencia de subordinadas en el indoeuropeo primitivo. De aquí concluye que la subordinación es posterior a la coordinación y pertenece a un estado lingüístico más avanzado.¹⁶ Sin embargo, Meillet ha mostrado el valor del factor musical como instrumento de subordinación en el indoeuropeo. Ha indicado, además, la existencia de por lo menos un relativo; y supone la existencia de conjunciones y relativos perdidos, pues, como lo demuestra la lingüística histórica, estos elementos están sujetos a renovación continua.¹⁷

3.2. Tampoco tienen mayor valor las inferencias hechas sobre la base de lenguas de pueblos salvajes pues éstas pueden instruir sobre las relaciones del lenguaje y del pensamiento pero no sobre la forma primitiva del lenguaje.¹⁸

3.3. El proceso de adquisición del lenguaje por los niños nada nos puede enseñar de lo que ha podido ser el lenguaje en el origen de su desarrollo, ya que el niño efectúa un trabajo lingüístico de imitación; no de creación (Vendryes, 13).

III

1. Las dificultades y fallas que acabamos de señalar en esta concepción teórica (y no son las únicas posibles) y especialmente su inadecuación a lo dado, provienen de un supuesto inicial no fundado: concebir coordinación y subordinación como procedimientos sintácticos esencialmente análogos, con diferencia únicamente en el grado de integración de las proposiciones componentes: primero coordinación, luego subordinación. Sin embargo, ambas estructuras así como los procedimientos sintácticos que las originan son de naturaleza diversa.

Esto puede verse aplicando la teoría del campo simbólico de la oración de Bühler (§ 15). Bühler señala que “el mundo de formas de la gramática ha surgido principalmente de la inserción sinsemántica de signos lingüísticos” (412). La oración gramatical, en cuanto forma lingüística, se caracteriza por presentar una “unidad de sentido (mínima independiente)” y estructura de campo simbólico, configuración de campo de coordenadas en el que se insertan los signos lingüísticos, adquiriendo con ello precisión signífica (en lo que a su ubicación en el campo se refiere).¹⁹

2. Según esto, una oración será compuesta, es decir formada por la unión de proposiciones, cuando se den las condiciones siguientes, de las que la segunda es esencial:

a. Cuando se estructure su unidad de sentido mediante la unión de los actos de predicación expresados por las proposiciones que la forman (más la idea establecida por el nexos).

¹⁶ Ed. Hermann, *Gab es im Indogermanischen Nebensätze?* (Jena, 1894), cit. en Bühler, p. 446

¹⁷ A. Meillet, *Linguistique historique et linguistique générale*, I (Paris, 1948), pp. 162 y 159 y s.

¹⁸ J. Vendryes, *El lenguaje* (Barcelona, 1943), p. 12.

¹⁹ Bühler, p. 412, y Gastón Carrillo, *Análisis sintáctico* (Santiago de Chile, 1959), pp. 9 y s.

b. Cuando su campo de coordenadas se constituya como compuesto, es decir por unión de los campos simbólicos de las proposiciones que lo forman, de manera que sea igual a la suma de ellos, expresada por el nexos.

2.1. De esto se infiere que sólo son compuestas las paratácticas, pues están constituidas por unión de proposiciones según el esquema general S-P + S-P, etc. Su sentido es la suma de los sentidos, de los actos de predicación expresados por las proposiciones que las forman. La complejión de su campo simbólico se presenta articulada como mera adición de los campos simbólicos de esas proposiciones. Tanto es así que las proposiciones que las forman presentan autonomía sintáctica. Las conjunciones que pueden encontrarse entre ellas son exponentes de la suma de los campos simbólicos que abren sus verbos respectivos.

2.2. Las llamadas oraciones compuestas por subordinación no presentan ninguna de estas características; mal puede entonces considerárselas compuestas. Su unidad de sentido es expresión de un solo acto de predicación y su campo simbólico es simple: no resulta de la unión de campos simbólicos. Por el contrario, sus coordenadas están determinadas sólo por un verbo: el de la llamada proposición subordinante. Esto se hace explícito en su análisis. Al racionalizar su complejión procedemos en general como con cualquiera otra oración simple. Lo único que distingue a estas oraciones del resto de las simples radica en que en un punto dado establecido por las coordenadas viene a insertarse no una palabra o frase sino un nuevo campo simbólico, que, en cuanto al campo simbólico total, se comporta como mera palabra. De ahí proviene la analogía estructural entre las oraciones “*el niño que estudia, aprende*” y “*el niño estudioso aprende*”.²⁰

3. Tal como las estructuras son esencialmente diversas, también difieren esencialmente los procesos de creación.

3.1. Para las paratácticas podemos aceptar ya la vía de creación indicada por Paul, ya la indicada por Kretschmer (Bühler, 461). Que de dos se haga una, o de una dos, poco importa en lo esencial (es irrelevante desde un punto de vista gramatical). Por descomposición o composición lo que se hace es constituir una unidad compuesta: integrar campos simbólicos en una sola oración.

3.2. En la hipotaxis acontece algo totalmente distinto. No hay integración de oraciones en una. Como lo observara Lenz,²¹ en lugar de una palabra o frase, el hablante inserta un nuevo campo simbólico en el campo único de la oración total (campo determinado en su configuración por su propio verbo, con independencia del verbo de la proposición inserta). Con esto se consigue coger lo real como proceso y atribuirlo como característica de algo.

4. Resumiendo, en la coordinación hay composición, unión de campos simbólicos en una oración. En la hipotaxis, no. Sólo se inserta un campo simbólico en el campo

²⁰ Lenz, § 351; Lucien Tesnière, *Éléments de syntaxe structurale* (Paris, 1959), ch. 164, §§ 14-18. Sin embargo, persiste en hablar de proposición subordinada y principal, en contradicción a lo que señala anteriormente (ver ch. 239, § 1).

²¹ Lenz, § 351; Tesnière, ch. 164, § 16.

simbólico total, como elemento de él. Como ambos tipos de oraciones y los actos de creación que los producen son diversos, es incorrecto y arbitrario agruparlos u oponerlos como especies de una misma clase. Las oraciones subordinadas carecen de autonomía sintáctica, requisito esencial de las proposiciones que forman una oración compuesta. Son miembros o submiembros de oración y no partes autónomas en la estructura total.

IV

1. Como las oraciones subordinadas no son proposiciones que con otras formen un compuesto, no necesitan nexos. Las palabras que generalmente las encabezan no son, entonces, conjunciones ni relativos, pues nada unen. Su papel es otro. Como generalmente estos campos simbólicos insertos tienen conformación de campo (estructura proposicional) y como esta estructura es en nuestras lenguas indoeuropeas el tipo preferido de oración, para indicar claramente su carácter de elementos carentes de autonomía necesitan un morfema que indique que no cumplen función oracional sino sólo la de miembros o submiembros de la oración total. Desempeñan también una función enmarcadora; hacen que los signos que los siguen y configuran con ellos una integridad de entonación, sean entendidos constituyendo una unidad de sentido.

2. Como está claro, estos elementos difieren profundamente de las preposiciones, que son instrumentos de campo.²²

3. Los morfemas de oración subordinada pueden ser de carácter lineal (conjunciones subordinantes, pronombres y adverbios relativos) o supralineal (la entonación).

V

1. Como las oraciones subordinadas son elementos insertos en el campo simbólico de la oración total, tal como palabras o frases, pueden clasificarse, atendiendo al modo de significar, en substantivas, adjetivas y adverbiales.

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²² Bühler, § 15. No es justo señalar que la función de las preposiciones y las conjunciones subordinantes sea la misma, como lo hace Tesnière, pp. 386-387, y, G. Gougenheim, "Prépositions et conjonctions de subordination en français", en *BSLP*, LVI (1961), pp. 86-103. Las llamadas conjunciones subordinantes indican que una proposición funciona como elemento en un campo simbólico mayor; las preposiciones, el lugar que ocupa en el campo simbólico de la oración el signo al cual preceden. Por ello, no es sorprendente que una proposición pueda presentarse precedida de preposición y del morfema de subordinación: "*huyó sin que lo vieran*".

PROPERTIES OF A CLASS OF CATEGORIAL GRAMMARS

ROBERT P. MITCHELL

Abstract

Let σ denote the set of all strings over a two-symbol alphabet $\{0, 1\}$, and σ_m denote the subset of strings of length m . A string $x \in \sigma$ is called a "zero-string" if all elements of x are 0. Let σ^1 and σ_m^1 denote the subsets of non-zero strings of σ and σ_m respectively.

A cancellation function $K^n: \sigma^1 \times \sigma^1 \rightarrow \sigma$ is said to have degree $n < m$ if, for every $x = x_1 x_2 \dots x_m$ and $y = y_1 y_2 \dots y_m$ in σ_m^1

$$K^n(x, y) = \begin{cases} x_1 x_2 \dots x_{m-n} y_{n+1} \dots y_m, & \text{if } x_{m-n+1} \dots x_m = y_1 \dots y_n \\ 0, & \text{if } x_{m-n+1} \dots x_m \neq y_1 \dots y_n \end{cases}$$

A sequence a_1, a_2, \dots, a_k of categories in σ_m^1 is said to cancel to a string in σ^1 if successive left-to-right-applications of K^n for fixed n yields a non-zero string.

A class G of categorial grammars is defined over this system, using σ_m^1 as a set of categories and K^n as a cancellation function with m and n having different values for each grammar of the class. Such properties as associativity of sequences of categories, solvability of sequences, and adequacy relative to a given English corpus are defined and investigated. These properties are taken as explications of intuitive grammatical notions. The last section discusses the adequacy of these explications relative to normal linguistic usage.

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INFORMATION ESTIMATES

HANS KARLGREN

A. A QUANTITATIVE MODEL

Thanks to information theory it is, in principle, possible to quantify many essential aspects of linguistic systems, aspects that earlier could not be expressed quantitatively. That is so in principle, but we are very far from relevant quantitative data. We can reason in terms of information theory, but we cannot easily compute numbers for reference. This is, no doubt, a grave drawback of a statistical model.

B. ONE APPLICATION: SPEECH RATE

My reason for making information computations has been the study of speech rate. The speed of speaking varies in a way that cannot be explained only from the phonetic character of the words pronounced. The number of "phonematic" syllables¹ per second, a number which in natural discourse often varies by a factor of ten within the same utterance, is not only a matter of physiological difficulty. The variations of speed must be seen as an optimization on a higher level: the duration is determined not only by the words that appear but also by those that do not appear but that could have appeared in the context. Information theory takes account of the paradigmatical dimension, and this is why it seemed reasonable to try to correlate duration with information content, statistically defined and computed, and not only with phonetical complexity.

C. NUMERICAL ESTIMATES

In the following I shall report briefly on the attempts to find numerical estimates which could be useful to our purpose.

D. FREQUENCY COUNTS

An obvious approach is to count the occurrences of linguistic symbols – phonemes,²

¹ Cf. H. Karlgren, "Speech Rate and Information Theory", in *Proceedings of the Fourth Intern. Congress of Phonetic Sciences, Helsinki 1961* (The Hague, 1962).

² A report on a new Swedish phoneme count and a comparative presentation of earlier counts is given in *Statistical Methods in Linguistics* (published by Skriptor, Fack, Stockholm 40), No. 1, 1962, by Mary Weiss, Institute of Phonetics, Uppsala.

letters, syllables, words, etc. The relative frequency of short text fragments – that of two or three letters³ and of individual words – can fairly easily be tabulated.

From the statistics of individual letters we get, to cite two examples, the entropy values 4.28 and 3.83 bits per letter for Swedish and Finnish texts respectively. Taking two-letter groups we arrive at the following entropy for the second letter of the group: 3.48 and 3.05 respectively.

From a Swedish word count⁴ we get approximately 10 bits per word, equalling roughly 2 bits per letter, treating space as a letter. For Finnish we have no word count available, but useful tables for other languages have been exploited; especially we have found the word statistics of German, English, Dutch and Russian valuable for comparison.⁵

Generally we know that as we consider longer ranges, the information per letter tends to decrease, possibly towards a limit.⁶ But neither this limit for different text materials nor the variations within the texts are well known.

On the whole, such numbers as can be derived from frequency tables have high reliability but their relevance for the study of human linguistic communication processes may be questioned. We need other measures, based on the conditional probabilities of longer fragments.

E. LENGTH OF TRANSLATIONS

Another way, it seemed, to arrive at least at relative information values would be to compare the length of translations with the original. If the text is well translated and keeps the style of the original, we have reasons to expect the information content – in the statistical-technical sense – to be approximately equal.

It is remarkably difficult to find texts that fill the requirements of being exactly parallel and carefully translated. The Bible will not do; it is a special case. The

³ Data on bigrams, trigrams and tetragrams are given by the cryptanalysis expert Yves Gylden in *Statistical Methods in Linguistics*, No. 2 – frequency tables for the Swedish language in the microfiche appendix, *SMIL M 5* – but for linguistic communication research it is wise not to exceed the length of 3 for mechanically delimited sequences; already tetragrams are a linguistically disconcerting mixture of common word fragments, derivatory or inflexional elements and sheer nonsense. Linguistic units have the maddening habit of being of varying lengths, on all levels.

⁴ P. G. Widegren, *Frekvenser i nysvenskans debattspråk* (Stockholm, 1935). This count covers half a million words of parliamentary speeches and accounts for all typographically different inflexional forms as well as for homonyms; Widegren was shorthand reporter of the Swedish parliament. Thanks to the helpfulness of the Phonetic Institute of Uppsala these data have now been rearranged and statistically exploited, for information computations and other purposes.

⁵ An attempt at an exhaustive bibliography is made in the bibliographical section of *Statistical Methods in Linguistics*. Only few word counts of the many existing, however, are adequate for information computations; in the first line those made for the construction of shorthand “codes” are suitably technical and exact, whereas pedagogical counts often have merged e.g. supposedly connected inflexional forms.

⁶ For a summary of fundamentals of information theory, see e.g. L. Brillouin, *Science and Information Theory*, 2d ed. (New York, 1962), or C. Cherry, *On Human Communication* (London, 1957).

texts I have found useful have been bilingual international treaties and the proceedings of the European Coal and Steel Union. The latter have been published in four languages, so that I had at my disposal four parallel versions of each text and, what is important, I knew the direction of translation. My material thus consisted of samples of translations from 12 directions.⁷

F. ELIMINATING THE TRANSLATION FACTOR

The direction of translation has proven highly important. The ratio of text length for each pair of languages depends to no small degree on which text was the original. Translation as such has a considerable lengthening effect.

Let r_{ij} be the ratio of output and input text length in translations from language i into language j with inherent lengths l_i and l_j , disregarding for the moment stochastic variation,

$$r_{ij} = \frac{l_j}{l_i} t_{ij}$$

where t_{ij} is the translation factor. If we accept the hypothesis, that the translation factor is the same for each pair of languages – i.e. that $t_{ij} = t_{ji}$ – we can compute the relative inherent lengths of the four languages involved and the translation factor of all 6 pairs:

$$\frac{\frac{l_j}{l_i} t_{ij}}{\frac{l_i}{l_j} t_{ji}} = \frac{r_{ij}}{r_{ji}}; \frac{l_j}{l_i} = \sqrt{\frac{r_{ij}}{r_{ji}}}; t_{ij} = t_{ji} = \sqrt{r_{ij} \cdot r_{ji}}.$$

G. INHERENT LENGTHS

With the method of least squares we obtain the following inherent length estimates,⁸ arbitrarily putting the inherent length of French to 100:

German	110	± 1,5
Dutch	106,5	± 2,1
Italian	98,4	± 1,7

⁷ For aid in the time-consuming work to collect these data as well as in other statistical computations involved in this study I thank the Institute of Phonetics in Uppsala and for valuable support I thank the Hierta-Retzius funds.

⁸ Actually, as there was not only one observation for each direction but a set of – mostly 50 – samples, there was computed for each set a mean, m_{ij} , and a standard error, v_{ij} , of the – logarithmized – sample values. When minimizing the squares of differences, these error estimates were used to weight the observations:

$$\sum_{i, j, i \neq j} \frac{\left(\frac{l_i}{l_j} - \sqrt{\frac{m_{ij}}{m_{ji}}} \right)^2}{v_{ij}} = \min$$

These relative numbers may be commented upon from many points of view.⁹ The obvious comment is that more extensive material is called for. My point here is only this: for all their structural diversity on the phonematic, morphematic and syntactical levels, these languages do not vary greatly in inherent length. The average information density of texts can, judging from these data and similar experiments,¹⁰ be assumed to differ only little from one language to another; no more than 10 per cent.¹¹ The striking disagreement between West Europe languages, from the quantitative point of view of information theory, is not in their over-all information content per letter but in the distribution of redundancy over different structural levels. – These results, preliminary in themselves, apply only to translatable text of an international style.

H. PREDICTION TESTS

A third method of obtaining an estimate, and a well-known one, is that of prediction tests.¹² It means that a portion of a text is presented to a subject, who is given the task to tell the immediately following letter. If he succeeds, he is asked to guess the next letter in the text: if he does not succeed at the first attempt, he is required to guess again until he arrives at the correct answer. From the number of guesses at each place is derived an information estimate.

I. MECHANIZED TESTS

These experiments are entertaining to the subjects, but they become rather tedious to the experimenter. The agreement between subjects is in fact a good one, and this from one point of view is gratifying, but it takes the excitement out of the experimenter's work. He can, as a matter of fact, successfully predict what the subject will guess next.

To get useful results in sufficient numbers I have used a simple mechanical gadget,

⁹ Thus, the translation factors for a pair of languages can possibly be used as measures of the distances between the two languages, yielding another measure for linguistic affinity in addition to those already mentioned at this congress. Irrespective of their inherent lengths, two languages with similar syntactic and semantic structure can be expected to have a translation factor close to unity; they willingly permit translation. All this under the assumption of even quality of translators and of even standard of translational precision.

¹⁰ Statistics relating to translations between two languages only at a time; including data on Scandinavian languages and Finnish. In general, the "brut ratio" is small: often below 5 per cent and always – except where special circumstances can be found – below ten per cent.

¹¹ Speech tempo varies for other reasons – within the same speech community and even intra-individually – by far greater amounts than an average of ten per cent. Little meaningful correlation can therefore be established between functional requirements and linguistic behaviour as far as these average estimates go. Another thing, and a more hopeful one, is the speech tempo fluctuations within utterances.

¹² Cf. e.g. Brillouin, *op. cit.*

taking over the experimenter's role and giving the "right" and "wrong" responses automatically. This contrivance, which may have a future as a learning machine, will be described in another connection.¹³

J. COMPLEMENTARY INFORMATION

The results of these prediction tests were used by Shannon to estimate the entropy of the text. Shannon's reasoning was based on the assumption of the ideal guesser, who knows the true conditional probability of all units. Real guessers do not know these probabilities exactly, but they have a large amount of intuitive approximate knowledge about them. And so Shannon takes the real, clever guesser – his co-operators – to approximate the ideal guesser.

One can look at the results in another way. One can, then, say that the string of "corrects" and "wrongs" is a message from the experimenter to the subject. This message contains the *complementary information* needed by the subject to identify the text. This information is clearly less, if the subject's a priori knowledge is great and vice versa. If we look at it this way, we do not have to postulate that the subjects are ideal guessers, or even good ones, to make the argument make sense. In fact, the experiments increase in value to linguistics if we allow ourselves to take into account the difference of skill between subjects.

K. HIGH ESTIMATE THROUGH PREDICTION TESTS

In prediction experiments with Swedish subjects – treating the scores of 10-20 subjects, tested at the same text, as one continuous sample – I have obtained values ranging from 2.5 to 1.4 bits per letter, varying¹⁴ with text and person groups; with students and simple prose the scores averaged 1.8.

We know that the information estimate obtained in this way is necessarily on the upper side. If the persons had been more skilled, it would have been smaller. It is satisfactory to have at least an upper limit for the entropy of the text especially since one is intuitively inclined to expect much greater information. Most people are greatly amazed when they are first confronted by the small values arrived at through prediction tests. It is shocking to learn that the words we have not yet uttered or written down are predictable!

However, when the healthy shock has appeased the prediction test – in its standard

¹³ In *Statistical Methods in Linguistics*.

¹⁴ The discussion of how to evaluate the fluctuation between experiments is still not brought to an end. The stochastic model for the dispersion must be a complex one: first there is the selection of the text, then the selection of the subjects, then the subjects efficiency and luck at different points of the procedure.

form – has played its role, for it gives no estimate of information, that can be used, e.g. if we want to compare texts, text segments or word categories.¹⁵

Even to the purpose of startling people who overrate the information of linguistic texts, prediction tests are not so effective as they were once believed to be. The results of prediction tests, where the subjects have had access to half a page of preceding text do *not* strikingly diverge from estimates based on single word statistics – 1.8 vs. 2 bits! – although the prediction result is affected by the powerful restraints governing words in context whereas the word statistics estimate can only take account of the frequency of isolated words.

This result is likely to surprise. The texts were comparable: the texts underlying the frequency tables were parliamentary debates; the text used in the tests were unpretentious, dullish prose of no more imaginative or complicated character.

The result that the information estimate based on longer range dependency was not much lower than the estimate based on the count of isolated words may be due to the fact that the subjects were not selected statisticians but students. Another explanation of the result may be the fact that the word count used had been compiled for one special kind of text. Putting it in another way: the kind of long range dependency that is accounted for by counting words exclusively in one type of text reduces the information estimate almost as much as the whole prediction procedure does.

This is an argument against taking the trouble of carrying out prediction tests to ensure estimates of long range information. I shall give another argument to prove the futility of the prediction tests I have been conducting.

L. PARALLEL TWO-LANGUAGE EXPERIMENTS

A series of parallel experiments were made with the same texts in Finnish¹⁶ and Swedish, with Finnish and Swedish participants respectively. The outcome was in all cases a lower figure for Finnish. Thus, in one short text tested on twice 20 persons, 1.7 bits per letter was computed for the Swedish version and 0.9 bits for the Finnish counterpart. The Finnish figures for other texts were not quite so low but they were always below the Swedish scores.

The Finnish language can, roughly, be characterized as an agglutinative language with a simple syllable structure and few consonant clusters. Therefore, the inherent length of Finnish should be expected to be greater than that of Swedish and so it is. In fact, the text used in the experiment mentioned was considerably longer if measured

¹⁵ In "Die Tragweite lexikalischer Statistik", *Språkvetenskapliga Sällskapets Förhandlingar*, 1958-1960, I presented an approximation formula for computing "mean information", of different categories, e.g. different lengths. This method has since been programmed on the BESK computer. However, it is restricted to word statistics, since prediction tests – so far! – give no information whatsoever on this issue.

¹⁶ The cultural background of Finnish is sufficiently close to that of Swedish to justify comparison restricted to the "technical" level of communication efficiency.

in number of letters. If we multiply by text length, we get 39 and 51 bits respectively for the portion of Finnish and Swedish text. Similar results were obtained with other tests.

It is not reasonable, from what we know about the two languages otherwise, to accept so great a difference of information density between them; that would amount to accepting the statement that the Finnish language differentiates the texts less than the Swedish does. We are more likely to find the explanation in the experimental conditions.

M. WORD BOUNDARY AFFECT

In fact, there are obvious drawbacks in the experimental arrangements. It appears from the test scores that especially the beginning of words have been difficult. When a subject has identified one letter or two of a word he usually gets on with little hesitation to the end of the word. This is partly due to the restraints within the word but partly also to the psychological difficulty to attack the beginning of a word; in these tests the word boundary presents a greater obstacle to the real guesser than to the ideal one. Now the word length is much longer in Finnish – 80 per cent longer in average in the passage mentioned as an illustration.

Actually, on inspection of how people guess in these tests,¹⁷ one finds that they proceed inefficiently in the beginnings of words. They guess for the most likely word to complement the given text; they cannot intuitively¹⁸ make the summation needed to get the most likely following letter. The information on the immediately following letter is not the information they can use best to decide which word follows.

Now, if we want to measure the amount of information the listener actually needs, in addition to his a priori knowledge, to perceive a text, the test arrangements must in certain relevant aspects be realistic. The information delivered to the subject must be in a form readily digestible, so that the amount of information transferred really tells us something about the subjects' uncertainty before the original text. In these respects, the traditional prediction test failed.

N. THE CHART TEST METHOD

Perception in real life takes place in larger blocks, consisting, say, of a second or two in speech or half a line or more on a printed page. Guessing on the other hand, if the number of alternatives shall not increase beyond practical limits, must be performed with one letter – or one phoneme – at a time. But there is no need for taking the letters in the order they appear in the text: Using a syllable chart, where all the phonemes are represented in accordance with their "rank order", i.e. in the relative order they must

¹⁷ The letters suggested by the subjects were recorded. The path of association is mostly evident; subjects are monotonously similar in this respect.

¹⁸ And it is their intuitive knowledge we are trying to get at in explicit form!

TABLE I

Nr of prevocalic phonemes					Nr of postvocalic phonemes						Endings	Juncture
3	2	1	0		0	1	2	3	4	5		
p				a					f	p		
	m	v		o			m					
f				u								
	b			å					v	b		
	t			e						t	s	#
h			l	r	r	l		n			sk	ts
	d									d	j	—
	g			i					j	g		
s		n	j	y			ng					
	k			ä					s	k		
				ö								

have if they appear together in a syllable, the subjects are permitted to proceed according to their own strategy.

These experiments – which are underway – are promising and mean a radical improvement. They give us another and lower estimate of the upper limit for the information of a text. Also this method allows us to draw conclusions on the relative importance of different features of the syllables: it is revealing to see in which order (esp. the skillful) subjects pick out the letters about which they ask the experimenter for confirmation – or, inversely, to compare their scores if they are compelled to follow a predetermined strategy.

These experiments, like those previously mentioned, yield as immediate result a string of yes's and no's, from which the complementary information requested by the subject can be computed in the conventional way. In these experiments, too, were used self-registrating mechanical devices. Possibly this is a road towards relevant numerical data for quantitative linguistic communication research. Investigations are continued on these lines.

O. SWEDISH SYLLABLE CHART

The syllable chart, constructed for Swedish texts, has the design as given in Table I.¹⁹ With very slight modification of standard Swedish orthography, (practically) any Swedish syllable can be plotted onto this chart in such a manner that reading marked off letters from left to right gives back the text. The letters can, thus, be suggested by the subject in arbitrary order, and the experimenter's – or mechanical device's – responses can nevertheless be restricted to "yes" or "no". In these experiments, three syllables were simultaneously available for guessing; longer segments can, of course, be used.

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¹⁹ The structural analysis underlying the establishment of this chart is in essential taken over from Bengt Sigurd, "Rank order of consonants", *Studia Linguistica*, VI (Lund, 1955), pp. 8-20. Further discussion in the author's "Positional Models and Empty Positions", in Ellegård, Karlgren and Spang-Hanssen, *Structures and Quanta* (Copenhagen, Scand. Summer Univ. for Interdisc. Studies, 1963).

DISCUSSION

GRIMES:

In evaluating translations, we find that some translations do not add extra content items appropriate to the target language and the associated cultural system. Such translations tend to be hard to understand, and presumably would rank relatively low on a predictive test of the type Mr. Karlgren has used. On the other hand, a translation that has appropriate items filled in from sources other than the source document alone tends to be more intelligible and presumably would rank higher on this test. I wonder to what degree the differences in predictability between the parallel Swedish and Finnish texts used by the speaker reflect the quality of the translations, and to what extent they reflect structural differences between the languages?

DE TOLLENAERE:

Mr. Karlgren's approval of the Dutch contributions in word frequency is a little confusing, for in Holland we are less impressed by the work done so far. De la Count's list (Batavia, at the time Dutch East Indies, 1937) has no real statistics, but only approximative frequency radii. At present however the Mathematical Center in Amsterdam is working on a two or three year project to make a frequency count of one million Dutch words from texts representing every sort of style of the modern language.

L'ÉVOLUTION DES COUPLES ASPECTUELS DU GREC

MAURICE LEROY

On sait que par le terme *aspect* – dans ce sens, le français est une mauvaise traduction, due à l'intermédiaire russe *вид*, du latin *species*: il vaudrait mieux dire *catégorie* – on entend la manière dont peut être envisagé le procès exprimé par le verbe et de telles nuances sont extrêmement nombreuses et variées; à la vérité, c'est sous forme de couples oppositionnels que les aspects s'imposent à la conscience linguistique des sujets parlants, une valeur aspectuelle n'étant perçue et reconnue de façon claire que pour autant qu'elle s'oppose à une autre valeur qui en constitue la contre-partie; il est donc fondamentalement plus exact de parler de *couples aspectuels*.

On en observe en grec plusieurs qui ont eu, au cours de l'histoire de la langue, des fortunes diverses.¹

Il y a tout d'abord le couple *inachevé/achevé* qui se traduit dans les faits par l'opposition des thèmes de présent et de parfait; θνήσκει "il est en train de mourir" (franç. "il se meurt") / τέθνηκε "il est mort". Le parfait marque donc l'état achevé lequel, par rapport au système temporel – qui était à l'origine indépendant du système aspectuel – se situe généralement dans le présent; ainsi β 132: ζώει ὄγ' ἢ τέθνηκε; "vit-il ou est-il mort?"; de même en face de μαίνω "je rends fou", μαίνομαι "je deviens fou", on a μέμνηνα dans le sens "j'ai l'esprit égaré, je suis fou"; en face de ὄλλυμι "je détruis", ὄλωλα signifie "je suis mort, je suis perdu" (marquant ainsi l'état où l'action de détruire est accomplie).

Le parfait exprimait donc originellement l'état achevé et était intransitif mais dès l'époque classique on voit se développer un rôle nouveau, celui du parfait résultatif; il y a là une évolution complexe qui a été mise en lumière de façon décisive par P. Chantraine dans son *Histoire du parfait grec* (Paris, 1929). Le fait que des parfaits anciens – ceux signifiant "avoir, posséder, abandonner, faire": ἔκτημαι, λέλογχα, λέλοιπα, (F)έ(F)οργα, . . . – aient pu avoir à côté d'eux un accusatif (nous ne disons pas "se construire avec l'accusatif" car la notion de *rection verbale* et, par conséquent, celle de verbes *transitifs* et *intransitifs* n'ont pas de sens pour l'ancien état de langue indo-européen dont Homère montre encore un bon reflet) a favorisé ce développement; d'après des tours comme ὄσα ἔοργε "tout ce qu'il a à son actif", le parfait a été utilisé pour exprimer un résultat, non pas en considération du sujet mais en con-

¹ Nous avons présenté les faits et une esquisse de ce problème dans notre article "L'aspect verbal en grec ancien", *Revue Belge de Philologie et d'Histoire*, 36 (1958), 128-138.

sidération de l'objet. Thucydide, voulant souligner le résultat, dira par exemple (V,26): γέγραφε καὶ ταῦτα ὁ αὐτὸς Θουκυδίδης Ἀθηναῖος.

L'extension, aux IV^e et III^e siècles, du sens résultatif marque l'apogée du développement du parfait en grec mais la floraison de formes que la statistique décèle dans les textes de cette époque annonce aussi sa disparition prochaine, nous y reviendrons.

De plus, la confusion, dès l'époque classique, du parfait et de l'aoriste – chez Homère, aoriste et parfait ne sont jamais employés l'un pour l'autre² – précipite le déclin du parfait entraîné de plus en plus dans la sphère temporelle du passé.

Un autre couple aspectuel, celui du *duratif* et du *momentané*, s'inscrit en grec dans l'opposition des thèmes verbaux présent/aoriste; d'une part, le procès est considéré comme en train de s'accomplir, d'autre part, c'est l'action momentanée qui est envisagée ou bien encore le procès est considéré en bloc, abstraction faite du développement. On a coutume de citer, comme particulièrement démonstratifs de ces nuances d'aspect, des exemples de verbes employés au participe ou à l'infinitif (comme le fameux τοῖς καλοῖς κάγαθοῖς αἰρετώτερόν ἐστιν καλῶς ἀποθανεῖν ἢ ζῆν αἰσχρῶς d'Isocrate, *Panegyrique*, 95 "pour les hommes de bien, une mort glorieuse [*mourir à un moment donné*] est préférable à une vie honteuse [*l'action de vivre dure*]"). C'est qu'à l'indicatif l'opposition n'apparaît de façon claire que dans la catégorie temporelle du passé: c'est le type ἔφευγον/ἔφυγον l'imparfait signifiant "j'étais en qualité de fuyant, j'étais en exil", l'aoriste "j'ai fui à un moment donné, je me suis échappé".

Il y a lieu de noter en effet que cette opposition *duratif/momentané* qui est une des plus nettes du grec, ne s'est pas développée à l'intérieur de la catégorie temporelle du présent: au duratif φεύγω ne correspond pas une forme de momentané *φύγω; c'est que le présent, loin d'être fixe, est un point toujours en mouvement et le présent momentané ne se conçoit guère que s'il annonce d'avance le moment qui vient: il est alors un futur (type franç. "demain, je vais à Paris"); on sait d'ailleurs que c'est là un procédé qui est devenu la base du système verbal des langues slaves dont tout verbe comprend en principe deux séries parallèles de formes, les unes à valeur imperfective (c-à-d. durative), les autres, dites perfectives, dont le présent sert usuellement à exprimer le futur.

Enfin, il y a encore l'opposition que Meillet, empruntant le terme à la grammaire slave – où les faits sont toutefois légèrement différents – a dénommée *indéterminé/déterminé*; il s'agit de la nuance d'aspect exprimée en grec d'une part (pour l'indéterminé) lorsque le procès est conçu sans qu'on en envisage le terme, d'autre part (pour le déterminé) lorsque le terme de l'action verbale est particulièrement envisagé, par-

² Voyez par ex. Ω 765-766 (plaintes d'Hélène sur la mort d'Hector):

ἤδη γὰρ νῦν μοι τόδ' ἔεικοστόν ἐτος ἐστίν
ἐξ οὗ κείθεν ἔβην καὶ ἐμῆς ἀπελήλυθα πάτρης

"voici la vingtième année que *je suis partie* de là-bas et *suis restée éloignée* de ma patrie." (Traduction J. Humbert, *Syntaxe grecque*, 3^e éd., Paris, 1960, p. 136).

fois d'ailleurs par l'indication du point de départ – les deux notions étant logiquement liées – si bien que le concept est assez lâche (Vendryes parlait dans ce cas de valeur “ponctuelle”). Mais – et ce dès les plus anciens textes – il ne s'agit plus ici d'un système cohérent s'inscrivant dans le cadre de la morphologie verbale; l'aspect en question s'actualise dans la langue grâce à des notations diverses qu'on peut grouper en deux catégories.

a) D'une part, il y a des formations qui ne sont plus productives à l'époque historique: elles apparaissent donc comme des survivances isolées qui n'ont plus droit à figurer dans une étude structurale fondée sur la morphologie; leur étude ressortit en somme à celle des faits de vocabulaire; c'est le type ἔχω “je tiens” / ἴσχω “je retiens”, μένω “je reste” / μίμνω “je reste jusqu'au bout”; voyez par ex. cette opposition très nette chez Euripide, *Médée*, 355:

νῦν δ', εἰ μένειν δεῖ, μίμν' ἐφ' ἡμέραν μίαν

“et maintenant, s'il te faut rester, attends jusqu'à un jour, un seul”.

b) D'autre part, l'aspect que nous avons défini comme déterminé peut trouver son expression dans l'emploi de formes à préverbes, le couple aspectuel étant formé dans ce cas par l'opposition de la forme simple à la forme à préverbe; c'est là un procédé bien connu en indo-européen mais qui apparaît avec une netteté particulière en slave, langue dont l'originalité a été précisément de faire du préverbe – vidé de sa valeur propre – un outil grammatical indiquant la perfectivation. Le grec, comme le latin ou le gotique, a aussi utilisé ce procédé (μανθάνειν “apprendre”/ἐκμανθάνειν “chercher à apprendre”; cf. Sophocle, *Oedipe-Roi*, 574-576) – il faut citer ici le volume de J. Brunel, *L'aspect verbal et l'emploi des préverbes en grec* (Paris, 1939) – mais la grammaticalisation des préverbes et leur emploi pour donner à une forme verbale la valeur déterminée sont loin d'avoir eu dans cette langue la même extension qu'en slave. De plus, le procédé opposant le simple au composé se superposait, non parfois sans une certaine subtilité, aux thèmes exprimant l'une ou l'autre des autres valeurs aspectuelles; en d'autres termes il y a eu fréquemment croisement du couple indéterminé/déterminé avec les autres aspects que nous avons reconnus précédemment.

En résumé, trois couples oppositionnels étaient à la base du jeu des aspects en grec ancien: deux aspects systématiques (*inachevé/achevé* et *duratif/momentané*) qui étaient de nature morphologique et faisaient partie du système du verbe, et un aspect non systématique (*indéterminé/déterminé*) qui était de nature lexicale, étant constitué par des survivances et des faits de vocabulaire.

Quels sont les enseignements que l'on peut tirer de l'étude de l'aspect en grec dans le cadre de la diachronie? Il est bon en effet de ne pas oublier que cette langue a le privilège d'être connue au cours d'une période s'étendant sur plus de trois millénaires; c'est dire que l'helléniste, s'il veut combiner les points de vue historique et fonctionnel, se trouve dans une situation particulièrement favorable pour procéder à l'analyse des données selon les principes du structuralisme historique, suivant ainsi la solution

heureuse qui a résorbé l'antinomie que Saussure avait voulu établir entre synchronie et diachronic.

En grec moderne, un seul des trois couples aspectuels que nous venons de passer en revue a subsisté de façon nette, la structure verbale actuelle reposant essentiellement sur l'opposition *continu/momentané* qui domine l'ensemble de la conjugaison et selon laquelle se construisent les modes et les temps; ainsi pour l'ancien φέρω, on a aujourd'hui deux thèmes sur lesquels sont bâties les formes verbales, d'une part le continu *φερν- (présent φέρνω), d'autre part le momentané *φερ- (aoriste ἔφερα).

Comment ont disparu les deux autres oppositions que nous avons reconnues au grec de l'époque classique?

Dans le premier cas, le parfait devenu résultatif n'indiquait plus l'achevé, ce qui a provoqué la rupture du couple aspectuel ancien *inachevé/achevé* dont les éléments – désormais *inachevé/résultatif* – étaient devenus asymétriques. Étape ultérieure, le parfait s'est confondu avec l'aoriste dans la sphère du passé si bien que, n'ayant plus de place obligée dans la structure verbale, il a été utilisé comme procédé expressif pour noter la passion ou l'ironie ou l'emphase ou l'exclamation indignée. Ainsi Démosthène, *Sur l'Ambassade*, 179, fustigeant la trahison, s'écrie: οὐ γὰρ μόνον Φωκέας, ἀλλὰ καὶ Θράκην προδέδωκε Φιλίππῳ "ce n'est pas seulement les Phocidiens, mais aussi la Thrace qu'il a livrés à Philippe". Ménandre, de son côté, fait dire à un personnage de l'*Arbitrage*, 271-273:

καλὸν πάνυ
καὶ λεπτὸν, ὧ θεοί, ταραντῖνον σφόδρα
ἀπολωλεκυῖα

"sa belle robe en laine de Tarente, si fine, par les Dieux, elle l'avait toute gâtée" (traduction M. Croiset).

Mais cet emploi du parfait avec une valeur affective allait contribuer encore à précipiter son déclin: on sait en effet à quel point les procédés du langage expressif sont sujet à renouvellement. La forme tombe de plus en plus en désuétude; dans le Nouveau Testament, le parfait est rare et tend à se confondre avec l'aoriste; dans les textes byzantins, il ne s'en distingue plus et son emploi, restreint à la langue savante, devient artificiel; dans la langue vulgaire, il a complètement disparu. Si le grec moderne s'est reconstitué un parfait (il s'agit d'une formation périphrastique analogue au perfectum des langues romanes: ἔχω γράψει "j'ai écrit"), c'est avec une valeur uniquement temporelle.

Quant au couple *indéterminé/déterminé*, la disparition même de sa caractérisation tant sur le plan sémantique (confusion de plusieurs valeurs) que sur le plan formel (emploi de procédés divers en partie reliques morphologiques, en partie innovations lexicales) a amené l'effacement de son principe oppositionnel et provoqué en conséquence sa suppression du système de la langue.

Un des mérites de Saussure, qu'ont souligné avec raison les structuralistes, est d'avoir

insisté sur le fait que les éléments d'un système linguistique sont solidaires et, partant, que nous n'avons quelque chance d'expliquer les évolutions particulières qu'en considérant l'évolution du système; ainsi se trouve posée la question de l'équilibre qui règle les rapports entre les différentes tendances évolutives. On a cru pouvoir dire que de telles recherches étaient entachées de téléologie: bien à tort, elles se sont au contraire révélées particulièrement efficaces et fécondes; il suffit de considérer la phonologie diachronique – André Martinet a joué ici un rôle important – qui a “réconcilié” comparatistes et phonologistes. Et on ne peut manquer de citer le beau livre *Estructura del sistema de aspectos y tiempos del verbo griego antiguo* (Salamanque, 1954) où M. Sanchez Ruiperez a, dans cette perspective, étudié l'imbrication constante en grec des structures aspectuelle et temporelle.

Or l'étude diachronique des valeurs aspectuelles des formes verbales grecques que nous venons d'esquisser montre précisément comment une rupture d'équilibre causée par des cas fortuits peut conditionner des évolutions importantes de la langue.

C'est ainsi que l'ancien couple aspectuel *inachevé/achevé* qui se traduisait dans les faits par l'opposition des thèmes de présent et de parfait a perdu sa pertinence lorsque le parfait, pour des raisons qui n'ont rien à voir avec la notion de l'aspect, a pris la valeur nouvelle d'un résultatif. D'autre part, le couple aspectuel *indéterminé/déterminé* n'était pas inscrit de façon nette dans la morphologie et s'actualisait par des moyens divers et des faits de vocabulaire: ce manque de caractérisation a entraîné sa disparition.

On voit ainsi que des raisons d'équilibre sémantique dans le premier cas, d'équilibre formel et sémantique dans le second, sont à l'origine de la simplification du jeu des aspects dans l'histoire du grec.

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LES “DÉRIVÉS” SANSKRITS EN $-\bar{A}C-$, $-\bar{I}C-$, $-\bar{U}C-$ (De la composition à la dérivation)

JACQUELINE MANESSY-GUITTON

INTRODUCTION

Les noms en $-a\tilde{n}c-$, essentiellement adjectifs, constituent en védique une catégorie importante par le nombre (29 adjectifs cités par Macdonell, *Vedic Grammar*, p. 181, § 298) et de sens cohérent. Presque tous les adjectifs servent à marquer la direction :

<i>ádharāñc-</i>	“tourné vers le bas”
<i>údañc-</i>	“tourné vers le haut”
<i>ṛjváñc-</i>	“qui va droit devant”
<i>pratyáñc-</i>	“qui est tourné vers . . .”

et le plus grand nombre est formé à partir de préverbes ou d’adverbes indiquant le lieu ou la direction.

Les difficultés apparaissent lorsqu’on veut préciser la nature de la finale $-a\tilde{n}c-$. Les linguistes se partagent en deux camps : ceux qui, comme Whitney (407), voient dans cette finale la racine même d’un verbe $A\tilde{N}Cati$ (dont la forme réduite $AC-$ concorde avec l’aspect faible de certains cas des adjectifs en $-a\tilde{n}c-$), et ceux qui préfèrent poser que $-a\tilde{n}c-$, $-ac-$ sont les correspondants sanscrits d’un suffixe proprement dit de l’indo-européen : $*-enk^w-$, $*-ñk^w-$, auquel répondraient aussi les suffixes $-απός$ du grec ($\eta\mu\epsilon\delta\alpha\pi\acute{o}\varsigma$, $\pi\omicron\delta\alpha\pi\acute{o}\varsigma$; Schwyzer, *Gr. Gr.*, I, 604 n. 1) et $-inquus$ du latin (*propinquus*, *longinquus*). Sont partisans de cette hypothèse entre autres : Thumb-Hirt, 219; Schwyzer, *l.c.*; Walde-Hofmann, I, 820; Hau., II, 269b; Mayrhofer, 24. Debrunner¹ classe $-a\tilde{n}c-$ parmi les suffixes mais ajoute que les formations qui le comportent se situent à mi-chemin entre la dérivation et la composition et que l’on peut admettre (*l.c.*, § 61, p. 156) qu’il s’agit de la racine $A\tilde{N}C-$ dont le sens est encore sensible dans certains adjectifs comme *sváñc-* “qui se meut aisément”. Dans le tome III (p. 230, § 126 b) Wackernagel-Debrunner avaient opté pour la racine $A\tilde{N}C-$ au sens de “mettre en mouvement en exerçant une pression”.

Si dans les formes en $-a\tilde{n}c-$ et $-ac-$ l’hésitation est permise, les faits semblent être, en un certain sens, plus clairs dans le cas des formes où seule subsiste la palatale c avec allongement de la voyelle finale du thème de base. On sait en effet que les adjectifs en $-a\tilde{n}c-$ constituent leur flexion à partir de trois thèmes :

¹ Wackernagel-Debrunner, II, 2, p. 152 (§ 56 sqq.).

un thème en *-añc-* degré plein aux cas forts
 un thème en *-ac-* degré réduit à SNA nt. et devant désinence à
 initiale consonantique

enfin un autre thème en *-āc-*, *-īc-*, *-ūc-* selon le timbre de la voyelle qui termine le mot de base.

Dans ces derniers cas on s'accorde généralement à considérer que l'on a affaire à d'anciens composés dont le second terme s'est plus ou moins fossilisé. On se propose de voir ici, en laissant de côté le problème des formes en *-añc-*, d'une part, comment s'est produite cette suffixalisation et d'autre part, de regrouper et peut-être d'éclairer quelques mots obscurs de la langue védique comportant un suffixe analogue.

PREMIER GROUPE

Le groupe le plus important de formes en *-āc-*, *-īc-*, *-ūc-* est naturellement constitué par les cas obliques des adjectifs en *-añc-* et par les féminins qui leur sont associés.

Ainsi a-t-on de *ápāñc-* "tourné vers l'arrière": *ápācaḥ* au P.A.m. et *ápācīm* au S.A.f.; de *anv-āñc-* "qui va à la suite": *anūcāḥ* au P.A.m. et un duel f. *anūcī*; de *ný-āñc-* "tourné vers le bas", *nīcā* au S.I. adverbial et un S.N.f. *nīcī-*.

Ainsi que Debrunner l'a clairement rappelé,² il s'agit là d'une flexion à supplétisme. Si un radical tel que *ápāc-* peut être expliqué par la contraction de *ápa* avec la forme faible *-ac-* de *-añc-*, ce n'est pas le cas pour les formations constituées à partir de particules ou d'adverbes dont la voyelle finale est autre que *a*, tels que *ánu* ou *ní*.

Quelle que soit l'origine de *-añc-*, il est probable que, pour les formations à voyelle longue et palatale, nous avons affaire à des composés dont le second terme appartient à la pseudo-racine **ok^w-* (comme le rappelle très nettement Debrunner, *l.c.*) que l'on posera plus précisément dans sa forme trilitère **ǎ₃ek^w-*.

En effet, sémantiquement, la solution est raisonnable. Parmi les racines employées en indo-européen pour exprimer l'idée de "voir", plusieurs servent à désigner la vision comme un "acte" et non comme une "perception", avec différentes nuances ou modalités;³ ainsi **spek-* a valeur durative et s'oppose en ce sens à **der-k-* qui est momentané; **ǎ₃ek^w-* est connue surtout pour son caractère "concret et expressif"; elle a servi à former un présent désidératif gr. ὄψομαι; elle se prête également à fournir d'une part le nom de l'organe de la vision à plusieurs langues indo-européennes (gr. ὄμμα, ὄσσε, sl. *oko*, *oči*, arm. *akn*; toch. A *ak*, lit. *akis*, lat. *oculus*) et d'autre part, au grec, un nom du "visage" ou de l'"aspect": *ὄψ, ὄπα, ὀπός. Alors que **swer-* a une valeur indéterminée, **ǎ₃ek^w-* signifie exactement "diriger son regard sur, se tourner vers" (voir A.

² Wackernagel-Debrunner, II, 2, § 61, p. 156-157.

³ J. Vendryes, "Sur les verbes qui expriment l'idée de voir", *CRAI*, 1932, p. 192-206 (= *Choix d'études linguistiques et celtiques*, Paris 1952, p. 115). – A. Prévot, *Verbes grecs relatifs à la vision et noms de l'œil*, (Paris, 1934).

Prévot, *l.c.*, p. 20 sqq.). On comprend dès lors comment une telle racine a pu être employée dans les composés dont le premier terme indique un lieu ou une direction.

<i>ápāc-</i>	est littéralement “qui regarde vers l’arrière”
<i>anūc-</i>	“qui regarde à la suite de”
<i>nīc-</i>	“qui regarde vers le bas”

Le procédé semble du reste avoir été connu dès l’indo-européen; il n’est en tout cas pas propre au sanscrit puisque, par exemple, le latin *antīquus*, *antīcus* suppose aussi une formation du même ordre (* $\partial_2enti-\partial_3k^w-e/o-$).

D’autre part, sur le plan morphologique, nous avons affaire à des composés de détermination d’un type connu dont le second membre est constitué par une racine verbale au degré réduit comme c’est le cas de

<i>agre-gú-</i>	“qui marche en tête” (* ∂g^wu- double degré zéro de * ∂eg^w-u-)
<i>iṣu-dhī-</i>	“carquois” (* $dh\partial_1$ de * $dhe\partial_1-$)
<i>óṣa-dhī-</i>	“plante (salutaire)”
<i>dvi-já-</i>	“deux fois né”.

Étant donné qu’il n’existe pas en sanscrit de nom du “visage” constitué sur cette racine seule, les noms en *-āc-*, *-īc-*, *-ūc-* n’ont aucune chance d’être des composés *bahuvrīh* alors que les noms grecs en $-\alpha\psi$, $-\omega\psi$ sont de formation ambiguë⁴ du fait de l’existence du nom-racine * $\partial\psi$. Les mots sanscrits, en revanche, sont aisément compréhensibles comme des composés directs qui auraient conservé dans leur second terme, depuis l’époque de communauté, le souvenir d’une racine perdue d’autre part. À l’ $\delta\psi\omicron\mu\alpha\iota$ du grec le sanscrit répond en effet par *īkṣate* “il regarde” qui, moins qu’un désidératif de * ∂_3ek^w- , doit être un présent à redoublement fait sur la racine * ∂_3ek^s- parallèle à * ∂_3ek^w- mais différente d’elle et qui a constitué un des noms indo-iraniens de l’oeil: skr. *ákṣi-*, av. *aṣi-*.

La voyelle longue s’explique phonétiquement par la contraction de la voyelle finale du membre antérieur avec la “laryngale” initiale de * ∂_3k^w- :

* $e/onu-\partial_3k^w-$	>	<i>anūc-</i>
* $nī-\partial_3k^w-$	>	<i>nīc-</i>

Ce même phénomène se retrouve dans nombre de composés indiens où la voyelle longue est due non à un phénomène morphologique mais bien au jeu normal des combinaisons phonétiques: de même *dvīpá-* “île” de * $dwi-\partial p-$ (* $\partial p-$ réduction de * $\partial ep-$ “eau”), *pratīpám* “à contre-courant” (* $proti-\partial p-e/o-$). Avec le même élément * ∂_3k^w- , le sanscrit a formé par exemple:

<i>prātīka-</i>	n.	“qui regarde vers, visage, surface”
<i>ánūka-</i>	m.n.	“qui est tourné vers l’arrière, nuque”.

⁴ Voir le bibliographie dans E. Schwyzer, *Gr. Gr.*, I, 425, n. 4.

A cette première catégorie de mots où la racine du second terme a gardé son sens concret on rattachera probablement le difficile *vīcī-* qui figure dans l’hymne X, 10, dialogue de Yama et de Yamī, à la stance 6d :

X, 10, 6d *kād u brava āhanó vīcyā nṛṇ*

“Lascive, en ta rouerie, qu’oserais-tu dire aux hommes.” (Trad. L. Renou, *Poésie religieuse de l’Inde antique*, p. 105.)

Oldenberg avouait dans les *Noten* (II, p. 206) ne pas savoir ce qu’était le mot *vīcyā*. Pour certains ce serait l’instrumental singulier de *vīci-* “ruse, tromperie” (Uhlenbeck, *Et. Wb.*; Wood, *IF* 22.169); selon d’autres ce serait l’instrumental sg. d’un adjectif féminin *vīcī-* (Weber, *S.B. Berl. Ak.*, 1895, 826) qualifiant un nom sous-entendu de la parole: “avec (une parole) qui s’éloigne (de la vérité)”. Dans cette dernière hypothèse, *vīcī-* serait tout à fait comparable, même pour l’accent, à *nīcī-* “tourné vers le bas”, qui sert de féminin à *nyāñc-*; si *vīci-* est un substantif, nous aurions un dérivé secondaire comparable en quelque sorte aux formations telles que les noms propres Ghṛtāci- ou Dadhīci-. Le point de départ en est la particule *vī* qui peut marquer la dispersion, l’extension, la séparation mais aussi la déviation (*W.D.*, II, 1, p. 285):

vī-loman- “à contre-poil”

vī-pathi- “qui va sur une fausse route”

Même si, en l’absence d’autres formes du mot, il est impossible d’en déterminer la nature exacte, nous pouvons dire toutefois que, sous l’un ou l’autre des deux aspects possibles, il s’insère dans une catégorie connue. La formation est peut-être d’ailleurs d’époque indo-iranienne si l’on rapproche de sanscrit *vīcī-* l’avestique *vičina-* (*θwarə-*). J. Darmesteter proposait (*MSL*, III, p. 302, n. 1) de comprendre ce composé, parallèle à *fračina-* (*θwarə-*), comme signifiant “étalé (?). *Vičina-* serait le correspondant d’un **vīcīna-* sanscrit, parallèle à *prācīna-*, et supposerait un thème **vīc-* dont *vīcī-* serait le dérivé.

Ainsi, dans ce premier groupe, le premier terme à valeur locale ou temporelle marque le point d’aboutissement ou la direction de l’action exprimée par **ə₃ek^w-*. On comprend alors comment a pu tenter de se former une deuxième catégorie de mots où le premier terme est soit un adjectif soit un substantif.

DEUXIÈME GROUPE

La formation la plus claire de ce groupe est l’adjectif féminin *svitīcī-* connu par un seul passage de la R.S. (I, 123, 9); le *ṛṣi* évoque la naissance de l’Aurore, elle qui “la brillante, la blanchissante, est née de l’obscur”. Uṣas, sortie du domaine de la nuit, devient de plus en plus éclatante; c’est cette progression qu’évoque l’adjectif *svitīcī-*, correspondant sémantique du masculin *svityāñc-*:

I, 123, 9b *śukrā kṛṣṇād ajaniṣṭa svitīcī*

Tāmiṣīcī- se présente comme l'antonyme du précédent. L'adjectif est employé une fois dans la R.S. pour désigner ou qualifier les maladies :

VIII, 48, 11 *āpa tyā asthur ānirā āmīvā nīr atrasan tāmiṣīcīr ābhaiṣuḥ*
 "Arrière les débilites! Les maux ont pris la fuite, les forces ténébreuses ont eu peur."⁵

et une fois dans l'Atharva-Saṃhitā pour qualifier les Apsaras: A.S. II, 2, 5: "They that are noisy, dusky, dice-loving, mind-confusing, to those Apsarases, that have the Gandharvas for spouses, have I paid homage."⁶

Dans la formation du mot, le radical **tamiṣi-* joue le même rôle que **śviti-* dans le cas précédent. Si *śvitīcī-* signifie "qui va, tend vers le blanc", *tāmiṣīcī-* signifie "qui tend vers le ténébreux", "qui est comme les ténèbres".⁷ La difficulté surgit si l'on cherche à préciser la nature du premier terme. Que sont ces formes en *-i-* inconnues à l'état libre? Il n'est pas exclu que le *-y-* de *śvityāñc-* soit dû à l'analogie. On explique généralement que le *-y-* de *samyāñc-* est irrégulier (Whitney, § 409b; Wackernagel-Debrunner, II, 2, § 562, p. 153), et qu'il est dû au type *pratyāñc-*. On pourrait invoquer la même influence pour expliquer la présence du *-i-* de **śviti-* et de **tamiṣi-*. Cependant le sens de ces deux derniers radicaux les éloigne suffisamment des particules locales pour qu'on puisse envisager une autre hypothèse, étant donné qu'ils se présentent dans des conditions particulières. **Śviti-* en effet s'oppose à l'adjectif libre *śvitrā-* connu par l'Atharva-Saṃhitā (III, 27, 6-X, 4, 5), **tamiṣi-* s'oppose à *tāmīrā-* adjectif substantivé, désignant, au pluriel, les ténèbres. C'est dire qu'il y aurait entre **śviti-* et *śvitrā-*, entre **tamiṣi-* et *tāmīrā-* l'opposition gouvernée par la loi de Caland-Wackernagel:⁸

ex. κυδρός "glorieux", mais κῦδι-ἀνειρα "qui rend les hommes glorieux"
turā- "puissant", mais *tuvi-rādhas-* "qui apporte de nombreux dons"

A côté de ces formations où le premier terme adjectif indique l'état que tend à prendre le nom désigné par le composé, on rangera des mots tels que *vāñīcī-* et *ghṛtācī-*.

Le premier est un hapax de la R.S.:

V, 75, 4 *suṣṭúbho vām . . . ráthe vāñīcy áhitā . . .*

La stance est extraite d'un hymne aux Aśvin et constitue plus particulièrement une invitation à venir jouir du sacrifice qui leur est offert par les hommes. Le terme *vāñīcī-* (sic Pp.) est de sens incertain (Sāyaṇa le glose par *vāgrūpā stutiḥ*, les Naigh. 1,11 par *vāc-*) mais il est certainement proche du substantif *vāñī-* (f.) qui désigne la "voix", surtout la voix du chanteur qui incite les dieux à descendre visiter les hommes; ces

⁵ Trad. L. Renou, *Poésie Religieuse de l'Inde Antique*, p. 49.

⁶ Whitney-Lanmann, I, p. 40.

⁷ L. Renou proposait (*J.As.*, 1939, p. 227, n. 1) de comprendre *tāmiṣīcī* comme "conduisant à la mort", *tāmas-* étant le symbole de tout ce qui est défavorable.

⁸ Voir bibliographie dans E. Schwyzer, *Gr. Gr.*, I, 447.

vāñī- sont parfois représentées comme des jeunes femmes montées sur le char des dieux.⁹ Donc *vāñīcī-* en V, 75, 4 désignant une entité féminine dont il est dit aussi qu'elle est “placée sur le char (des Aśvin)” a de bonnes chances d'être apparenté au nom *vāñī-*. L. Renou le traduit par “*vāñī* en puissance”; on pourra en effet l'analyser, sur le plan morphologique, comme un composé de *vāñī-*: *vāñīcī-* sera “celle qui tend vers l'état de *vāñī*”, “celle qui est comme une *vāñī*”: “celle qui est comme la voix du (chantre) bien-disant (est) placée sur votre char ...”

Le cas de *ghṛtācī-* est particulièrement instructif. D'après F. B. J. Kuiper,¹⁰ *ghṛtācī-* contiendrait une racine *añc-* signifiant “montrer, manifester”. Mais il ne semble pas que sa brève démonstration et son hypothèse – présentée d'ailleurs comme une tentative – soient absolument convaincantes. Si l'on reprend en effet les passages où figure le mot dans la Ṛk-Saṃhitā, on s'aperçoit qu'ils peuvent être répartis en deux groupes, d'importance inégale.

En V, 43, 11 *ghṛtācī-* qualifie la Sarasvatī. Or, pour emprunter l'expression à A. Bergaigne (R.V., I, 326 et 327), Sarasvatī “est la personnification la plus achevée des eaux divines” et “le prototype de toute offrande”. L'adjectif qui lui est appliqué peut donc être interprété très simplement à la lumière des exemples précédents comme signifiant: “qui tend vers le *ghī*”, “qui est comme le *ghī*”, “qui ressemble au *ghī*”, et non avec Geldner (*Üb.*, II, p. 46) “qui va vers le *ghī*”. Geldner lui-même suggère ailleurs une explication de *ghṛtācī-* semblable à celle que l'on propose ici. En I, 2, 7 *ghṛtācī-* est l'épithète de *dhī-*: *dhīyaṃ ghṛtācīm sādhanā(u)* “(Mitra et Varuṇa) qui font réussir la prière ...”. Or Geldner dans le commentaire du pāda en question hésite entre deux traductions:

soit “von eurer Schmalzspende begleitet”

soit “so flüssig wie Schmalz”

Cette dernière s'appliquerait bien à Sarasvatī et peut-être également aux chants, *gīrah*, de VII, 5, 5. Elle s'appliquerait aussi à Ródasī (I, 167, 3) qui est comparée à la parole de l'assemblée, à “l'arme inférieure” (c'est-à-dire la parole terrestre), donc à un élément du sacrifice. Peut-être faut-il voir dans le nom de *ghṛtācī-* donné à la nuit (A.S., XIX, 48, 6) ou à un serpent (A.S., X, 4, 24) une allusion à leur éclat, à leur brillance qui serait comparée à celle du *ghī* (on dit du *ghṛtā-* qu'il est “brillant”, *sūci-* en IV, 1, 6 et VI, 10, 2).

Mais dans les autres passages *ghṛtācī-* a un sens plus affaibli; l'adjectif s'applique en particulier à une cuiller sacrificielle qu'il suffit parfois à dénommer: III, 6, 1; IV, 6, 3; V, 28, 1; VI, 63, 4; VII, 84, 1; VII, 43, 2 au pluriel. Il est évident que, dans ces passages, le mot ne saurait plus avoir le même sens que celui qui a été dégagé précédemment. *Ghṛtācī-* ne signifie plus alors que “qui touche ou *ghī*”, “qui concerne le *ghī*” (cf. *ghṛtāvātī srūc* en VI, 11, 5).

⁹ Voir L. Renou, *E.V.P.*, I, p. 8, citant R.S. I, 119, 5.

¹⁰ *Vāk*, II, déc. 1952, p. 63-64.

Morphologiquement la finale *-āc-* n'est plus ici sentie comme contenant une racine indépendante, mais comme un simple suffixe servant à tirer un adjectif d'un substantif avec un sens large d'appartenance. On ne peut dissocier de l'adjectif *ghṛtācī-* le nom propre *Ghṛtāci* (nom d'un ṛṣi, Ārsh. Br.) désignant en somme "celui qui s'occupe du ghi". Ainsi n'est-il point besoin de considérer *Dadhyañc-*, *Dadhīca-* comme des formations différentes de *pratyāñc-*, *pratīc-* (comme le veut F. B. J. Kuiper, *l.c.*). Si *Dadhyañc-* désigne "celui qui est tourné vers le lait-sûr" comme *asmatrāñc-* signifie "tourné vers nous", *madryāñc-* "tourné vers moi", *Dadhīci-*, *Dadhīca-*, parallèlement à *Ghṛtāci-*, désigne "celui qui s'occupe du lait-sûr".

Nous avons donc là des formes où ce qui était à l'origine un second terme de composé à valeur pleine est devenu déjà un suffixe de sens assez lâche.

TROISIÈME GROUPE

Dans une troisième catégorie de mots le sens paraît à ce point affaibli que le suffixe tend à devenir purement explétif. C'est peut-être le cas du nom *mārīci-*.

Le mot, toujours employé au pluriel, est difficile. Sur le plan sémantique, il semble bien que, quoiqu'on n'en puisse préciser le sens premier, ce mot doive désigner des particules lumineuses conçues comme des éléments de l'atmosphère. Les *māricayaḥ* figurent T.B., 2, 2, 9, 2 dans une énumération entre la lumière, l'éclat (*arcīṣ-*), et les nuées (*udārāḥ*); on les trouve mentionnées avec la brume (V.S., 25, 9; Kath., 28, 4), le vent (Ś.B., XI, 6, 2, 6) ou les oiseaux (Ś.B., XI, 8, 1, 2). Ainsi que le disait Weber¹¹ il y aura bientôt un siècle, le mot doit appartenir à une terminologie atomistique (cf. L. Renou, "Connexion entre le rituel et la grammaire en skr.", *JAs*, 42 (1941), p. 159, qui traduit ŚB IX, 4, 1, 8: "des atomes lumineux qui nagent (dans l'air)").

Sur le plan morphologique, l'explication du mot est rendue difficile par le fait que nous ne connaissons pas en sanscrit de nom, substantif ou adjectif, qui ait pu servir de point de départ à *mārīci-*. Sur l'étymologie de **mar(i)-*, on ne peut formuler que des hypothèses que l'on trouvera résumées dans A. Minard (*Trois Enigmes sur les Cent-Chemins*, II, § 97a). Si l'on admet avec la plupart des auteurs que nous avons à l'origine de *mārīci-* une forme relevant de la racine **mer-* "briller" (lat. *merus* "pur, sans mélange", gr. μαρμαίρω "je brille", μάρμαρα·λαμπρά Hes., cf. Ernout-Meillet, *s.v.*), *mārīci-* serait fait à partir d'un radical **mari-* (comparable soit à **sviti-* adjectif, soit à *vāñi-* substantif) signifiant soit "lumineux", soit "lumière". Si **mari-* était un substantif, les *māricayaḥ* seraient ainsi désignées comme les "(éléments, particules) relatifs à la lumière"; la différence d'accent que l'on observe entre *mārīci-* et un adjectif tel que *svitīci-* (R.S., X, 46, 7) peut tenir à la substantivation du mot marquée par le recul de l'accent. Si **mari-* devait être tenu pour un adjectif, la finale de *mārīci-* serait tout à fait explétive. Dans ce dernier cas, *mārīci-* se rapprocherait singulièrement de *urūcī-* et de *purūcī-*.

¹¹ Weber, *Ind. Stud.*, IX (1865), p. 9, n.1.

Urūcī- sert à qualifier la vache, la langue, la *dhénā* d'Indra ou d'Agni; il peut être aussi l'épithète de Aditi ou des deux Rodasī; il sert à lui seul à désigner la Terre (VII, 35, 3). On voit dès lors combien il est proche du simple adjectif *urvī-*: comme on dit *urvī rōdasī* (nombreux exemples, R.S.), on dit *urūcī rōdasī* (IV, 56, 4; VI, 11, 4), *urvīm amātim* (V, 62, 5; VII, 38, 2) comme *urūcīm amātim* (VII, 45, 3); *urvī-* est lui-aussi, et plus fréquemment, une dénomination de la Terre. Tout se passe donc comme si *urūcī-* n'était sémantiquement qu'une variante de *urvī-*.

D'autre part, *urūcī-* est généralement rattaché à l'adjectif *uruvyāñc-* "qui s'étend largement", ce qui pour le sens est satisfaisant. Mais pour la forme les deux mots ne sont pas immédiatement comparables. On a supposé que le point de départ était un adjectif **urv-añc-*¹² devenu **urvy-añc-* sous l'influence du type *pratyāñc-*, *samyāñc-*, puis passé à *uruvy-āñc-* d'après *uru-vyācas-*. Peut-être est-il plus simple d'admettre avec Wackernagel-Debrunner¹³ que *uruvyāñc-* (dont on n'a qu'une seule forme en *-añc-*, R.S., V, 1, 12d) est une modification passagère de *uru-vyāc-* (de la racine *VYAC-* "embrasser, contenir" cf. *uru-vyācas-*) sous l'influence des noms en *-yāñc-*. *Urūcī-* apparaît donc comme morphologiquement indépendant de *uruvyāñc-* et comme un simple doublet, pouvant servir de variante métrique, du féminin de *urū-*. L'allongement de la voyelle et la finale *-cī-* constituent ici, pour reprendre les mots de L. Renou, "un type de dérivation purement explétif".

Point n'est besoin dès lors de supposer l'existence d'un adjectif **purv-anc-* attesté nulle part et sous aucune forme pour rendre compte du féminin *purūcī-*; celui-ci n'apparaît que dans la R.S. et dans l'A.S. Dans les deux passages de la R.S., il est placé dans la clause trochaïque de la *triṣṭubh* dont il fournit les trois derniers pieds et qualifie *īṣaḥ* en III, 58, 8 (*ásvinā pári vām īṣaḥ purūcīḥ*) et *śarādaḥ* en X, 18, 4 (*śatām jīvan tu śarādaḥ purūcīḥ*). Or nous trouvons dans d'autres stances rigvédiques ces mêmes substantifs qualifiés de *pūrvīḥ*. Il ne paraît donc pas trop hardi de considérer *purūcī-* comme un synonyme de *pūrvī-* dont il pouvait constituer au besoin une variante métrique. La formation d'un tel mot n'a été rendue possible que grâce à l'affaiblissement progressif du second terme de composé passé au rang de suffixe explétif de dérivation.

Les noms en *-āc-*, *-īc-*, *-ūc-* nous ont donc permis de mettre en lumière un phénomène assez intéressant. La langue sanscrite a hérité d'un procédé indo-européen consistant à créer des adjectifs composés dont le second terme est emprunté à la racine **ǵ₃ek^w-* "voir". Mais elle l'a surtout employé dans des adjectifs de direction aux cas faibles et pour en constituer le féminin. Dans cette catégorie, particulièrement bien représentée dans la langue du rituel, le sens concret et actif de la racine est préservé. Dans un autre groupe, nous voyons ce sens se transformer, devenir plus abstrait: *śvitīcī-* signifie "qui vire au blanc". Le composé indique simplement qu'une transformation se produit. Le

¹² L. Renou, *E.V.P.*, IV, p. 56.

¹³ Wackernagel-Debrunner, III, p. 230, § 126 b.

sanskrit avait là l'amorce d'une catégorie possible d'adjectifs de comparaison (*vāñīcī-* "qui ressemble à une *vāñī-*, à une voix", *ghṛtācī-* "qui ressemble au ghī") dont il n'a pas tiré parti. On voit au contraire le sentiment de la composition et de la valeur de la formation s'estomper et la finale devenir un simple suffixe indiquant qu'un être, un objet concerne ou appartient au terme de base: *ghṛtācī-* peut signifier "qui concerne le ghī", *mārīcī-* "qui concerne la lumière".

L'affaiblissement du sens de **a₃ek^w-* en composition ne s'est pas produit seulement en sanscrit; on le retrouve aussi en latin où l'adjectif *antīquus*, *antīcus* signifie "ancien, qui concerne le passé"; mais le comparatif *antīquior* au sens de "préférable" indique que *antīcus* avait à l'origine une valeur locale et devait signifier "qui est tourné vers l'avant" (Ernout-Meillet, *s.v.*)

Enfin le suffixe s'affaiblit au point d'être tout à fait explétif dans *urūcī-* et *purūcī-*, puis de sortir de l'usage en dehors des adjectifs de direction. Sans doute cette extinction est-elle due au fait que, au sein même de la langue, il n'existait aucune famille de mots issue de la racine **a₃ek^w-* qui ait pu maintenir vivant le sentiment de la composition, en dehors du verbe *īkṣate* dont la forme était obscure.

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SOME POSSIBLE SYSTEMIC CHANGES IN A SEMITIC SYSTEM OF LANGUAGE

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It has often been said that a juxtaposition of Israeli and "Classical" Hebrew yields a host of striking illustrations of features of linguistic diachrony; yet, for several reasons, the "changes" observed between these two layers of Hebrew have to be scrutinized carefully before they can be admitted as legitimate examples and "case histories" of linguistic change.

The reasons that call for restraint are, in brief, the following:

1. It is sometimes difficult to establish, what precisely the *état de langue* is from which I(sraeli) H(ebrew) has developed. The "base" for the history of IH is a *koiné* of at least two or three spelling pronunciations of a written tradition¹; consequently, we are, e.g., not entitled to state that a loss of the so-called "emphatic" consonant quality is a diachronic process in the transition from "Classical" to Israeli Hebrew, because it seems that this quality was already absent from the "outset" of IH, i.e. the *koiné* of spelling pronunciations.

2. The "base" itself of IH is not an *état de langue* within the history of Hebrew, but an incongruous mixture of historically preceding *états de langue*.²

3. The undoubtedly present "substrate" or "contact" features that have arisen out of the status of IH as the language of an immigrant community render many changes significant only for contact studies, but not for studies in diachrony.

Therefore, when we examine the differences between Israeli and "Classical" Hebrew in order to study diachronic processes that may be typically inherent in a Semitic system of language, we have to be careful not to include any differences to which one of the mentioned *caveats* applies.

We shall be on comparatively safe ground if we study cases of diachronic change in which the stimulating factor is what we might term "catalytic",³ i.e., if the feature or element that constitutes the stimulator involves a fargoing systemic reorganization without itself taking part in it. In these cases it is not material for us to ascertain as a consequence of what linguistic event the stimulating factor has undergone the

¹ *Revue des Études Juives*, N.S. 17 (1958), 61-63.

² *Bull. Soc. Ling.*, 53 (1957-58), 316-317.

³ Hesitatingly, though, since in so adapting the term from its use in chemistry, we deviate somewhat from the very specialized notion which Martinet (*Économie des changements phonétiques*, 90-91) has clad in this term.

process observed; even if this is due, say, to “contact”, whatever is “catalysed” by the fate, presence or absence of the stimulator, is already an internal diachronic process, whatever the causes of the stimulator’s fate may have been. Moreover, it is highly probable that, in the cases we shall offer here for discussion, even the fate of the stimulating factor is not due to any of the facts we have enumerated as reasons for caution.

Our cases, from various levels of analysis, have one thing in common: in all of them it is the loss of the “catalytic” factor that has caused the systemic change, and the elements affected by the systemic change remain themselves formally unchanged.

A. Loss of phonemic (vowel and consonant) quantity results in an increase in the functional load of the stress opposition.

In B(iblical) H(ebrew)⁴ there exist pertinent oppositions in the system of syllable peaks, that may be aptly described as involving phonemic quantity; e.g. the contrast of the phonemes /a/ and /ā/ in the contextual forms *zākār* “remembered (m. sg.)” vs. *zākār* “male” (pausally both represented by *zākār* through neutralization of the opposition of quantity), the contextual forms *nāḥal* “acquired (m. sg.)” vs. *naḥal* “creek” or their pausal counterparts **nāḥāl* “acquired” vs. *nāḥal* “creek”.⁵ On the other hand, vocalic length, while present in IH, is there automatically conditioned by stress, syllabic structure and intonation. In BH, stress, while phonemic (*rāšū* “wanted [3rd pl.]” vs. *rāšu* “ran [3rd pl.]”), is of extremely low functional load. In most of the minimal pairs that show a distinction of vowel quantity, a difference of stress is concomitant: the above-mentioned examples have the stress patterns [nāḥál] vs. [náḥal] (contextually), [nāḥál] vs. [náḥal] (pausally), the stress being here unaffected by pausal or contextual position. In such cases, the stress is predictable in terms of the pattern of syllabic peaks and of position (contextual, pausal).⁶

A similar situation prevails in respect to consonantal quantity in BH. However, there is no pair of consonant phonemes that are distinguished by length; with most consonants, length (so-called “gemination”) may appear as an additional phonemic entity: *limdu* “learn (imptve. m. pl.)” vs. *lim·du*⁷ “taught (3rd pl.)”. Here, too, stress is concomitant with certain form patterns in which consonantal length is pertinent, and predictable therefrom; e.g. *’āṭā* “came (m. sg.)” – pausal *['āṭā] vs. pausal ['āt·ā]⁸ “thou (m.)”.

Normally, word stress stays in IH where it was in BH; however, all “pausal vs. contextual” distinctions are lost and in the overwhelming majority of cases it is the

⁴ By BH we mean the Tiberian text.

⁵ Strictly phonemically, since *naḥal* is /naḥl/, the stress is automatically on the final syllable (*Revue Biblique*, 60 1953, 31-36).

⁶ For details see my *Textbook of Israeli Hebrew* (Chicago, 1962), 308, and an article (in Hebrew) in *The Jerusalem Hebrew Gymnasia Jubilee Volume* (1962), pp. xciii-xciv.

⁷ The raised point marks consonantal length.

⁸ The difference between *t* and *ṭ* is non-phonemic (*Journal of Near Eastern Studies* 20, 1961, 124-127).

contextual form that applies in both positions. Since word stress is unpredictable in BH only in a minority of cases, but mostly predictable in terms of phonemic features that have been lost as such in the development of IH (vocalic and consonantal quantity⁹), it results that the functional load of the stress opposition is incomparably higher in IH than it was in BH. Examples: /naxàl/¹⁰ “creek” vs. /naxal/ “inherited”; /yelèd/ “little boy” vs. /yeled/ “will create (m. sg.)”; in some cases, homophony may be the result; e.g. “learn (imptve. pl.)” and “taught (pl.)” (both /limdu/) are now distinguishable only on the syntactic level.

B. Loss of phonemic distinctions (such as those mentioned in A) results in the shift of form relations from the phonemic to the morphemic or even syntactic level.

This development may be expected and is evident, e.g., from the IH examples mentioned at the end of A. /limdu/ I “learn” and /limdu/ II “taught” are distinguishable by simple (I) and double (II) transitivity, respectively. – The word pattern *CeCèC* indicates the character of a noun, while *CeCeC* is characteristic of a verb (example *yelèd* vs. *yeled*); likewise, while there are nouns of the patterns *Ca’àC*, *CoCa’*, the patterns *Ca’aC*, *CoCa’* are reserved for verbs; even stress shift may occur as a consequence, e.g. BH [kōbáʕ] (but isolatedly also [kōb̄aʕ]) “cap” > IH *kovà* “hat”.¹¹

C. While these cases, in which the “catalytic” factor of loss is phonemic, are of lesser interest for the analysis of IH and of less considerable impact on its structure and have also to some extent been dealt with previously, the most interesting developments are observable where what is lost is a part of a morphological paradigm, a loss which results in a complete reorganization of the syntactic status of the remaining forms of the paradigm.

The loss of morphophonemically rare-type tenses of some BH qal (radical) verbs results in the emergence, in post-biblical Hebrew, of a new morphosyntactic class (“part of speech”) – adjectives.

These “rare” types are essentially:

a. those in which the participle and the perfect are homophonous in the 3rd m. sg. (e.g.: *ḥāšēr* “caret, caruit”, *zāqēn* “senet, senuit”) and in all likelihood historically represent one and the same form, comparable to the Akkadian “stative”;

b. those in which the final syllable of the m. sg. participle has a vowel other than *ē* or (in monosyllabic forms) *ā* (e.g. *ḥāzāq* “ualens”, *’ādōm* “rubens”, *ṭāhōr* “liquens”, *gāḇō^{ah}* “eminens”, *ṭōb* “being good”).

This verb class as such is now extinct in IH.¹² Even in BH it is rare not only by

⁹ It should be recalled that, as a result of the loss of consonantic length, also contrasts like *p : f* (< *p̄ : p̄*) have become phonemic.

¹⁰ The grave accent marks morphologically meaningful absence of stress from the final syllable in IH; if no accent is marked in a word it has final stress.

¹¹ An extensive sampling will be found in my *Ha-’ivrit šelanù* (1955, in Hebrew), 230-231.

¹² On its fate see *Textbook of Israeli Hebrew*, 331. Some of the verbs are re-interpreted as “incohatives” and have been morphologically changed; the only remainder of the class seems to be IH

virtue of the number of verbs belonging to it, but also by virtue of its paradigmatic use: by far the most frequent form is the participle, while the finite tenses are of comparative scarcity.

The feature common to these BH verbs is that they are syntactically intransitive and semantically “stative” (or “qualitative”); we are wont to translate their participles by adjectives (“high”, “strong”, “red”, “pure”, etc.) and their finite forms by adjectival phrases (“was high”, etc.), precisely as we do with the representatives of the typically Latin verb class with which we have exemplified their translation (*rubet* “is red”, etc.).

BH has no morphological class of adjectives, as has been correctly stated by others.¹³ What we translate by adjectives are either

I. participles of verbs (as those shown above), or

II. nouns, numerals, adverbs or names, extended by the so-called *nisbe*-suffix of appurtenance *-i* “(one) of, (one) who is, (one) who makes”, which serves to make a form other than a participle appositive to an appellative noun or taking its place; e.g. *taḥti* “(one) of below (/taḥt/), *šiš'i* “(one) who makes six (/šiš'/), sixth”, etc.

Since the *nisbe*-suffix forms nouns, and nouns may be appositive to each other, class II does not represent a “part of speech” “adjectives” any more than class I.

While class II has not changed its syntactic status in IH, but though it has become abundantly fertile, most of its representatives are almost exclusively used appositively or predicatively (as of “adjective” status), class I has changed its status radically. The finite tenses of the “qualitative” verbs lost, the remaining former participle (that, like all participles, can be used both attributively and predicatively) now forces us to establish the new morphological class of adjectives.¹⁴

This state of affairs compels us to give recognition to a bipartition of the class of adjectives in IH. Mainly for morphological reasons (but also for syntactical ones, see below) we have to distinguish “strong” (i.e. radical) adjectives (the old *qal* participles) from “weak” ones (some adjectivized participles of formations other than *qal* or adjectives of an identical formation,¹⁵ but mainly the derived adjectives of the *nisbe* type). Their most striking morphological distinction is that while weak adjectives

met “dies (m.), has died (m.), is dead (m.)” – Still the possibility must not be excluded that a distinction between notions “suitably expressible by verbs” and those “suitably expressible by adjectives” is, in fact, of extra-hebraic origin.

¹³ Thus, e.g., Gesenius-Kautzsch-Cowley², § 79a. However, *ibid.*, § 116b, he endeavors to establish a difference between “the participle proper” and “the verbal adjective”. This difference is, I believe, chimerical. The “stative” verbs do not have more forms than the other verbs and we have to distribute the attested forms correctly into their paradigmatic “pigeonholes”. E.g. for the root *g-d-l*: *gāḏēl* is only the perfect (a separate homophonous participle is not establishable!), *gāḏōl* (with the collateral cstr. pl. *gidlē!*) is the participle; *gāḏal* is a collateral perfect leveled according to the common type of verbs, as happens frequently with ‘stative’ verbs.

¹⁴ Occasionally, the complete biblical paradigm is preserved in highly literary style; e.g. the poetical expression *tahartī* “I was pure” has a standard-language equivalent *hayitī tahor*.

¹⁵ See, e.g., *Textbook of Israeli Hebrew*, 357.

almost invariably form their f. sg. in *-t* and do not undergo basic vowel alternations (*yami* (m.) i *yamit* (f.) “marine”; *kavi* (m.) i *kavit* (f.) “linear”¹⁶), strong ones do and have fem. sg. forms in *-a* (*naki(y)* (m.) i *nkiya* (f.) “clean”).

D. As a consequence of the process described under C (which has arisen out of the partial loss of a verbal paradigm), former conjunct participles acquire, in an identical syntagmatic frame, the status of adverbs.

Here we best state first what the situation in IH is. There are two (incorrect, as we shall see) statements currently made about the relations of adjectives and adverbial complements in IH:

1. In colloquial IH, m. sg. (i.e. endingless) adjective forms are used adverbially.¹⁷
2. Attributive phrases consisting of *be-'ofèn* (“in a ... way”) and a m. sg. adjective (e.g. *naxon* “correct”) represent adverbial complements (*be-'ofèn naxon* “in a correct way, correctly”).

Both statements are too widely formulated.¹⁸ Statement 1 is correct only for strong adjectives (e.g. *Hu kotev tov [raa] m'od.* “He writes very well [badly].” – *'al tešev kol-kax gavoah!* “Don't sit so high!” – *Tixtov yoter xazak!* “Write stronger!”). It is, however, not true for derived (*nisbe*) adjectives (e.g. *xodši* “monthly”, from *xodèš* “month”, is not appropriate as complement of the sentence *hu mešalem...* “he pays...”). The opposite is true of statement 2 (*Hu mešalem be-'ofèn xodši.* “He pays monthly”), and only exceptionally do strong adjectives take *be-'ofèn* (e.g. *'efšar laasot 'et-ze [be-'ofèn] yoter pašut.* “This can be done a simpler way”). Adjectives of participial pattern (other than radical, i.e. *kal* and *nif'al* ones) occupy an intermediate position. Some may be used both ways (e.g. [*be-'ofèn*] *lo-naxon* “incorrectly”), some only without *be-'ofèn* (e.g. *Yašanti maspik.* “I slept enough”), some only with *be-'ofèn* (e.g. *Hu diber be-'ofèn me'urpal.* “He talked nebulously”).

This syntactic feature, which deepens the dichotomy between weak and strong adjectives in IH, obviously goes back to the non-adjectival, but participial, nature of the forms from which the IH strong adjectives originate. This becomes at once evident if we examine sentences that, by their structure, can be Biblical as well as Israeli Hebrew, e.g. *'et hak'ol šāšā yāpē.* (*Eccl 3 : 11*) “*S'yn tà pánta epóíēsen kalá'*” (cf. IH *'et-ha-kol 'asa yafe.* “He did everything nicely”). – *Špāṭay bārur mil'elu.* “Clearly spoke my lips” (*Job 33 : 3*); cf. IH *Daber yoter barur bevakaša!* “Speak more clearly, please!”

Within the framework of BH, these forms (*yāpē*, *bārur*) are commonly analyzed as adverbials originating from “accusatives” the inflectional endings of which are

¹⁶ The juxtaposition of these two examples shows that the fact that the first hails from a *mediae geminatae* root is irrelevant. – For more details concerning the morphology of weak and strong adjectives see *Textbook*, 199-202, 241-242.

¹⁷ Which is deplored by purists not only as being allegedly “ungrammatical”, but also as an alleged yiddishism or germanism.

¹⁸ For full statements see *Textbook*, 215, 263.

lost.¹⁹ This explanation, hailing from extra-hebraic (e.g. Arabic) syntax, is valid for participles at the same time as for nouns and is corroborated by passages in which the “adverbial” is coordinated to frozen noun-forms with still preserved (mimaged) accusative ending: *’ani mlē’ā hālaḳti wrēyqām hešibāni YHWH*. “Full had I gone and empty has the Lord made me come back” (*Ruth* 1 : 21).

But within the synchronic framework of BH morphosyntactic structure these forms are conjunct participles. Very frequently they show concord with either the agent or the object of the verb (also the Septuagint translation of *Eccl* 3 : 11, quoted above, may be thus analyzed), but – what is much more important – they do not include derived (*nisbe*) nominals.²⁰

There is a neat distinction in IH between predicative complements (adjectives or “conjunct” participles) that are in concord with either the agent or the object of the verb (*Hu’asa’otam yafim*. “He made them nice”) and endingless adjective forms used adverbially (*Hu’asa’otam yafe*. “He made them nicely”). That the latter are limited to strong adjectives, i.e. original participles, shows that in spite of syntactic and morphological reorganization strong trends of formal conservatism are still inherent in current Hebrew.

We believe that processes like those outlined above are of a purely internal nature (i.e. features of “diachrony” and not of “contact”) and may be illustrative for possibilities of systemic change in languages of similar structure; they should therefore be considered possible in any Semitic language. It may be worth while, in this context, to re-examine, for example, the “adverbialization” of uninflected adjective forms in modern Arabic dialects, in order to ascertain whether a parallelism of development does or does not exist between their history and that of their Hebrew counterparts.

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¹⁹ Gesenius-Kautzsch-Cowley², § 118; Joüon, *Grammaire de l’hébreu biblique*, § 126 a-b; Brockelmann, *Hebräische Syntax*, §§ 81 a, 103 a; S. R. Driver, *Use of the Tenses*, § 161.2.

²⁰ In the often-quoted *way’ēšē’ hāri’sōn ’admōni* (*Gen* 25 : 25), *’admōni* is not an adverbial complement to *way’ēšē’*, but the predicate of the sentence; the verb *yāšā’* is a verb of incomplete predication (cp. *bhiš’āp̄to yēšē’ rāšā’*, *Ps* 109 : 7).

INTONATION AS A UNIVERSAL

DWIGHT L. BOLINGER

Is intonation a universal trait of language? The question divides into two: Do all languages use pitch with some kind of uniform meaningfulness? Are the meaningful uses we find so widely shared that we must assume a common origin? "Common origin" can be taken either in the sense of a single primordial language or a common denominator in human nature that could give rise to similarities.

Needless to say, all languages use pitch; we know of none that are more often whispered than spoken. Conceivably, in some language yet to be analyzed, the fluctuations of pitch might be entirely random; but such a language would be so exceptional as hardly to count against universality in the broader sense. The inquiry is therefore worth pursuing only in the narrower sense of the sharing of particular uses.

Also we must refine it one step further, to set aside syllabic tone. Tone languages are numerous, but I suppose not more so than the other kind.

The remaining uses of pitch, most linguists seem to agree, are to be found everywhere. Gleason advises students to "take it as a working hypothesis that every language, tonal or other, has some sort of intonation system".¹ Pike had earlier been more cautious when he wrote that "probably in every nontonal language the pitches of the utterances tend to be 'frozen' into formalized patterns, or INTONATIONS".² Later he included a "running down" intonation pattern among those pervasive things such as long pauses, change of speaker, sharp change of voice quality, and the like, that must be looked for in the preliminary stage of analysis as an aid to segmentation,³ and Paul Schachter tells us that such a gradual lowering of pitch is very common even in tone languages.⁴ E.M. Uhlenbeck was evidently thinking similarly of pitch as an utterance-binder when he said that "every language has words, i.e. signs having a phonematic structure and capable of being used as a sentence in combination with an element of intonation".⁵

With this said, we come to the end of the agreement. Further generalizations are hard to make because authorities differ in what they mean by intonation. For some,

¹ *An Introduction to Descriptive Linguistics* (New York, 1961), 298.

² *Tone Languages* (Ann Arbor, 1948), 15.

³ *Language in Relation to a Unified Theory of the Structure of Human Behavior* (Glendale, Cal., 1954), 66.

⁴ "Phonetic Similarity in Tonemic Analysis", *Language*, 37 (1961), 231-238.

⁵ *Lingua*, 2 (1950), 247.

fluctuations due to emotion must be ruled out. In his description of Amahuaca, Robert Russell says "there are two levels of tone, but, up to now, no system of intonation has been found. Though when aroused, frightened, or angry an Amahuaca can raise his voice higher than ordinarily", the two phonemic tones stay on an even keel.⁶ Pike generalizes the same point: "All tone languages", he says, "have intonation of the emotional type, with the general height of the voice affected, and so on, but I have not seen reported for them a highly organized contrastive system with a limited number of relative levels controlling the formation of intonations that carry shades of meaning."⁷ He perhaps would modify this now, in view of recent descriptions of some tone languages; but I suspect that he still holds to the view that an intonation SYSTEM presupposes contrastive levels or some similar device.

Trager and Smith in their analysis of English make similar distinctions along phonemic lines. "On the level of phonemics", they say, "there are no such things as 'intonations'."⁸ Pitch enters into pitch-level phonemes and into the transition phonemes that they originally called terminal junctures. For them, as for Pike, a comparison of non-tonemic pitch phenomena would require us to look for similarities in these phonemes and their organization. For example, Trager has recently stated that "ALL LANGUAGES have . . . terminal transitions, by definition", and terminal transitions are a function of suprasegmental phonemes.⁹ If it should turn out that in all or most languages these transitions involve similar uses of pitch, we would have evidence for some kind of universal.

Still another use of pitch appears to require separate treatment. In Oto as outlined by Pike,¹⁰ in Comanche as described by Smalley,¹¹ and in Zuni, according to Newman¹², lexical stress is conveyed by a rise in pitch and there are no other phonemic pitch contrasts. If this is correct, we are obliged to refine out pitch as a signal of stress in the same way we refined out pitch as a toneme. The same probably holds, at the level of syntax, for pitch as a signal of sentence stress, or accent as I prefer to call it, in languages that have this use of pitch.

These are the reservations and qualifications that we must keep in mind when we try to interpret the occasional claim that a given language has no intonation. Take Comanche. Pitch is described as an incidental feature of stress,¹³ which defines that

⁶ "Algunos morfemas de amahuaca (pano) que equivalen a la entonación del castellano", *Perú Indígena*, 7 (1958), 29-33.

⁷ *Tone Languages*, 16-17.

⁸ *Outline of English Structure* (Norman, Okla., 1951), 52.

⁹ "A TERMINAL TRANSITION OCCURS at the end of a sequence of suprasegmental phonemes and affects the whole sequence back to a defined point." *Studies in Linguistics*, 16 (1962), 18.

¹⁰ *The Intonation of American English* (Ann Arbor, 1945), 21.

¹¹ "Phonemic Rhythm in Comanche", *IJAL*, 19 (1953), 297-301.

¹² John M. Roberts and Watson Smith, *Zuni Law: a Field of Values* (= *Papers of the Peabody Museum of American Archaeology and Ethnology*, Vol. 3, No. 1) (Harvard University, 1954). Appendix B by Stanley Newman.

¹³ The reasoning is curious. "Lexical stress", we are told, may be suppressed by certain arrangements of rhythm, but the high pitch associated with it is retained. In what sense then is stress

aspect out. An emphatic stress that raises pitch is identified, but this is optional, hence not part of a system — analogous, I suppose, to an over-all rise due to excitement. Comanche also has a non-anticipatory pause. But again the accompanying drop in pitch is optional, and pitch as a transition feature is ruled out.¹⁴

Obviously by plucking a petal at a time and declaring that it is not a rose, we will end up with something that is not a rose either, and can rule the rose out of existence, assuming there was one to begin with. To make sense out of the question of universality we must get back to more general terms. So let me re-phrase it: Are there, in the non-tonemic uses of pitch, coincidences so striking and so widespread that they cannot be attributed to accident or diffusion?

Take first the most general one, the running-down pattern that seems to be found practically everywhere.¹⁵ As tone languages are the harder case, I'll mention some examples: (1) Zulu, which according to Gleason has the typical downdrift found in many African languages and, in addition, a recitation form which maintains a relative level "until the very end, where there is a sudden and decisive fall".¹⁶ (2) Efik, in which high tones get progressively lower toward the end of the intonation span.¹⁷ (3) Ocaina, where it is the low tone that gets "optionally progressively lower . . . from the beginning to the end of the utterance".¹⁸ (4) Northern Tepehuan, where again the low tone is extra-low at the end of rhythm groups.¹⁹ (5) The Mixtec of Santo Tomás Ocotepe, where both of the two tones have potential downward drift toward the end

"suppressed"? This can only mean that some other phonetic feature, no doubt intensity, is lost. If stress is phonemic, it is not "lost" as long as there is anything there to signal it. Under the circumstances, pitch is hardly "non-phonemic".

¹⁴ In what sense is terminal fall "optional"? And if it is optional is it therefore non-contrastive? Does "optional" mean "freely variable"? The author does not enlighten us. In English a falling pitch associated with terminal transition is also optional — one may end a statement, for example, on any pitch whatever; but no one so far as I know claims that the pitch feature of terminal transitions is irrelevant. The "option" is at a higher level. Once we have decided on our message there is no longer a choice. It is hardly likely that falling pitch in Comanche is purely a matter of accident.

A recent description of Kaiwá Guaraní reaches similar conclusions (Lorraine Bridgeman, "Kaiwá (Guaraní) phonology", *IJAL*, 27, 1961, 329-334). "No contrastive pitch patterns which correlated with the categories postulated" — contours of pitch or of pitch and stress conveying finality, incompleteness, interrogation, etc. — "were found" (329). But junctures are described (332) in which a non-terminal with high sustained pitch contrasts with a terminal with falling pitch. The question here is whether in a language like English one can have a "contour of finality" without a terminal transition of finality — in other words, are the determinants of one not the same as the determinants of the other? If Kaiwá shows non-finality versus finality by the same kind of high versus low contrast found in English, surely this points to a close similarity with Indo-European languages. Perhaps "finality" as the author uses the term refers to 'end of utterance' rather than to the "mood" of finality with which the drop in pitch in English is associated. But then we must inquire whether the Kaiwá speaker is not free to fake it and to conventionalize the fake. The description suggests that he can do this — internalize the fall, which then contrasts with the sustentation.

¹⁵ Yokuts, according to a private communication from Stanley Newman, appears to use a slight rise on the last syllable of an utterance.

¹⁶ Gleason, p. 296.

¹⁷ Gleason, p. 294.

¹⁸ Arlene Agnew and Evelyn G. Pike, "Phonemes of Ocaina (Huitoto)", *IJAL*, 23 (1957), 24-27, p. 26.

¹⁹ Burt Bascom, "Tonomechanics of Northern Tepehuan", *Phonetica*, 4 (1959), 71-88.

of the utterance.²⁰ (6) Saramaccan, with two tones, in which the final word has its tones modified so as always to end on the low tone.²¹ This is not a large sample, but it shows a number of ways in which the tendency toward low pitch at the end overcomes the handicap of phonemic tone. As for non-tonal languages, there is little point in enumerating. I quote one instance as a sample: “the /:/ final terminal signal involves features of primary stress, falling pitch glides, decrease in loudness and intensity, and the initiation of a long pause” —this is not a description of English but of Pocomchi.²²

A second broad zone of possible coincidence is the pitch of questions. The survey of some 175 languages by Eduard Hermann turned up high interrogative pitch in some form or other everywhere. Hermann observed that “Such a unanimous result has not yet been realized in any other phenomenon of speech”. He was careful to point out that by high pitch he did not necessarily mean a rising terminal — he would not regard as an exception a language like Fanti, for example, where all yes-no questions end on a low-pitched interrogative particle,²³ so long as it turned out that the over-all pitch level was relatively high. An example in English would be the interrogative-word question which, like the statement, ordinarily ends in a fall, but nevertheless attains a higher average register than the statement.²⁴ One of the ways in which the elevation of pitch can work in a tone language is illustrated by Maya, where at least some informants add a short high pitch to an interrogative phrase, manifested differently for the different phonemic tones but always involving some kind of extra-high; the same kind of extra-high seems to be used also “before pauses in the middle of sentences”.²⁵ Another example is Lugisu, which makes the high tone extra-high.²⁶

How regular the feature of high pitch will be of course varies from language to language. In Tetelcingo Nahuatl we are told that interrogative sentences are “frequently” characterized by an extra-high pitch on the next-to-last syllable.²⁷ In Campa, “Interrogation is generally indicated merely by . . . the raising of the voice at the end of the utterance”.²⁸ In Tagalog, rises and falls for questions are used substantially as in English.²⁹ In Pampanggo we have the extreme of a required terminal rise for all otherwise unmarked yes-no questions even when they come at the end of a series of

²⁰ Cornelia Mak, “The Tonal System of a Third Mixtec Dialect”, *IJAL*, 24 (1958), 61-70, p. 70.

²¹ J. Voorhoeve, “An Orthography for Saramaccan”, *Word*, 15 (1959), 436-445, pp. 437-438.

²² Marvin K. Mayers, *The Pocomchi: a Sociolinguistic Study* (Chicago, 1960), 15.

²³ William E. Welmers, *A Descriptive Grammar of Fanti* (Baltimore, 1946), 58. (Language Dissertation No. 39, Linguistic Society of America).

²⁴ Eduard Hermann, *Probleme der Frage* (Goettingen, 1942), 363-369. (Translation furnished by Henri Wittmann.)

²⁵ K. L. Pike, “Phonemic Pitch in Maya”, *IJAL*, 12 (1946), 82-88, esp. pp. 87-88.

²⁶ B. Siertsema in *Lingua*, 11 (1962), 393. She generalizes: the intonational function in tone languages is manifested in the SCALE of the contour more than in its SHAPE.

²⁷ Richard Saunders Pittman, *A Grammar of Teletcingo (Morelos) Nahuatl* (Baltimore, 1954). (Language Dissertation No. 50, Linguistic Society of America).

²⁸ Sylvester Dirks, “Campa (Arawak) Phonemes”, *IJAL*, 19 (1953), 302-304.

²⁹ According to informant, García Gilberto.

questions. Of the generalizations about interrogative pitch that I have encountered only one has been completely negative — Zuni is reported not to have even so much as a loose association of higher pitch with questions.³⁰

A third general zone of possible coincidence is pitch accent. Here we have to read between the lines since accent is generally attributed to stress; but a number of descriptions leave little doubt that the controlling factor of prominence is pitch. In Cocama, for example, we are told that "Primary stress . . . is accompanied by high pitch and is followed by a falling off of both pitch and stress". To make the resemblance to the better-known pitch-accent languages even closer, we learn that "When primary stress occurs on the penultima, the fall-off of pitch and stress may begin either immediately following the syllabic or delay to the following syllable".³¹ In Cayuvava we find an account of three "pitch patterns" that are virtual duplicates of the accents that I have labeled A and B,³² and in addition we learn that "Certain imperative verb forms have strong stress on the final syllable, such strong stress overriding any intonation or pitch pattern"³³ — a shift of stress exactly duplicating what occurs sporadically in English and Spanish.

So much for general resemblances. Now a look at schemes embodying resemblances on a more elaborate scale.

A fairly uncomplicated example is Kutenai, which is described as having three "contour-final intonations", one a declarative with fall, one an interrogative with sustained high, and one an emphatic with additional rise.³⁴ In Korean, the contrast between yes-no and interrogative-word questions is similar to that in Indo-European languages, high or rising pitch versus falling pitch.³⁵ In Thai there are intonation contours consisting of one of two voice registers plus one of three terminal transitions; the terminal transitions are the familiar ones — rise, sustention, and fall, and the rise "signals surprise, doubt, or a question". In addition, there is a contrastive stress using the common devices of "extra loudness, a rise in pitch or lengthening of the vowel".³⁶ Resemblances on a larger scale take shape with those systems which have been analyzed into pitch levels after the fashion in English. I shall mention four examples. The first is Egyptian Arabic, which has been analyzed into four levels with three stresses.³⁷ The analytical resemblances to English are accompanied by semantic ones, e.g. rising intonation for short questions, high-to-low for longer questions (differing

³⁰ Private communication from Stanley Newman.

³¹ Norma Faust and Evelyn G. Pike, "The Cocama Sound System", in *Série Lingüística Especial, Publicações do Museu Nacional Rio de Janeiro*, No. 1 (1959), pp. 10-54; see esp. pp. 20-21.

³² "A Theory of Pitch Accent in English", *Word*, 14 (1958), 109-149; esp. pp. 142-143.

³³ Harold Key, "Phonotactics of Cayuvava", *IJAL*, 27 (1961), 143-150.

³⁴ Paul Garvin, "A Descriptive Technique for the Treatment of Meaning", *Language*, 34 (1958), 1-32, p. 2.

³⁵ According to informant, John Yung Sohn.

³⁶ Arthur S. Abramson, *The Vowels and Tones of Standard Thai: Acoustical Measurements and Experiments*, 32-38 (Columbia University dissertation, 1960).

³⁷ Albert George Abdalla, *An Instrumental Study of the Intonation of Egyptian Colloquial Arabic*, (University of Michigan dissertation, 1961). *Dissertation Abstracts*, 21, 2283-2284.

from English only in frequency), mid-to-low for statements, etc.³⁸ The second example is Khmu?, which we are told has four levels and three terminal transitions; the analysis mentions the phonetic behavior of the levels and transitions, which is closely similar to that of English, but does not elaborate on the semantic functions.³⁹ The third example is Pampangó, likewise with four pitch levels and with transitions like the familiar ones.⁴⁰ We may judge the semantic resemblance from this instance of a question: [ʔáíŋ,íŋ | láróʔ | ʔíŋpárol ||] 'Is there oil in the lamp?' The last example is Sierra Popoluca, which, according to Hockett, "has an intonational system which at first appears remarkably — one might even say suspiciously — similar to that of English". He goes on to describe striking differences; but there are equally striking similarities. In fact, one of the supposed differences turns out to be a similarity: "A one-PL [pitch level] intonation accompanies a remainder in which there is only one occurrence of the phoneme of stress, and the PL occurs distinctively on the syllable which bears that stress. A two-PL intonation accompanies a remainder in which there are two or more occurrences of stress: the first PL occurs on the first stressed syllable, and the second PL occurs on the last."⁴¹ With minor changes this becomes a description of the over-all tonal shape of a number of Western languages from the standpoint of pitch accent. The moral has been pointed by Hockett himself: "The most recent studies show a variety of technique which clearly reflects differences among the investigators as much as, or more than, differences among the languages studied."⁴²

The most revealing comparisons thus far made are one between English and Chinese and one between English and Japanese. The first is Chao's, using intonational criteria largely based on Palmer's analysis of English. The large number and great detail of the analogies suggest that if an equally searching comparison were made for other languages, some of those which have been declared markedly different might turn out to be less so. Among the similarities cited are the following (we must of course make allowances for the adjustment to syllabic tone): First, expressive intonation. Chao gives five examples each of rises and falls. The rises are all questions or exclamatory protests; the falls are all statements or non-protesting exclamations. Second, questions. The normal yes-no ends at middle pitch — the significant thing here is that it does not drop. A surprised echo question rises at the end. A reclamatory question (one calling for a repetition) has high pitch. Third, dogmatic statements, which always have a falling ending. Fourth, the confident assertion, of the /á-\ah type

³⁸ This is for Cairene Arabic, as reported to me by Richard Beym.

³⁹ William A. Smalley, "Outline of Khmu? Structure", *American Oriental Series* (American Oriental Society), Essay 2 (1961).

⁴⁰ Catherine Clardy, "Pampangó phonology", *Phonetica*, 3 (1959), 118-144, pp. 120-129. This analysis is unusual in that it treats the four levels as mere phonetic manifestations of pitch phonemes whose significant shape depends only on what happens to the pitch at the end of the contour, e.g. high-fall — the pitch allophones preceding the end are supposed to be predictable on the basis of what that terminal pitch event happens to be, conditioned by certain types of pause phonemes. The odd feature is that it recognizes a SUCCESSION of allophones as the materialization of a single phoneme.

⁴¹ Charles Hockett, *A Manual of Phonology* (Baltimore, 1955), 49 (= *IJAL Memoir* 11).

⁴² *Ibid.*, p. 51.

intonation of Western Desert in Australia is like that of English: "These data from widely separated parts of the world raise the suspicion that we are dealing with a fairly common group of typologically similar systems."⁴⁸ I take this to refer to typology along formal lines only. Certainly it must be true, but it is probably an understatement.

Another view was expressed by Ferguson and Chowdhury after noting the remarkable similarity between English and Bengali in the treatment of high-pitched interrogative words; they feel that the presence of this trait in many branches of Indo-European may indicate that it was inherited from the proto-language.⁴⁹ This too is probably true. We know something of the persistence of intonation — "the more automatic and subconscious a pattern is, the harder it is to change".⁵⁰ In northwest Argentina the transition from Cacana through two hundred years of Quechua to Spanish did not suffice to eradicate the Cacana intonation.⁵¹ It is likely that we must look to lineal descent for many, perhaps most, of the minute details of intonation. To take a trivial example, in English

more left-handed com
 I get low . . .
 pliments from that fel

it is the intonation that tells us that *more* means "a lot of". This, or the association of words and music in *Oh, yeah?*, is not apt to be duplicated elsewhere.

But how far are we willing to go in crediting lineal descent with resemblances on the grand scale? Hermann was convinced that the universality of high pitch in questions proved a genetic kinship of all languages. This view imposes two conditions, first that we consider the similarities to be sufficient and second that genealogy is the best explanation. I am inclined to say yes to the first and no to the second. The similarities among languages without apparent genealogical ties can hardly be the result of chance; but the differences that we find argue against heredity as the all-pervasive influence, powerful as it may be in the short run. On the one hand intonation seems to cling even when a community adopts a new language; on the other, it does not cling tenaciously enough to prevent dialects of the same language from diverging, as is nowhere more apparent than in the contrast between the American and Southern British varieties of English. The paradox is resolved if we see intonation as tied with other forms of behavior such that it will change with a change in community manners as much as with a change in language. If Leonard Bloomfield's simile is valid — that intonation is like gesture — we can imagine a community switching languages but retaining the old gestures, and then modifying the gestures independently.

This leads me to propose that in addition to heredity, certain underlying physiological or psychological traits are at work. Edward Erazmus notes that children learn

⁴⁸ *Language*, 35 (1959), 383-384.

⁴⁹ *Language*, 36 (1960), 25-28.

⁵⁰ Einar Haugen in *Lingua*, 8 (1950), 288.

⁵¹ M. A. Morínigo in *Hispania*, 35 (1952), 95.

their suprasegmentals at an age when motor and sensory faculties are not yet well developed, and concludes that "the functional contrastive features of the suprasegmental area are probably of some primitive type which has not . . . been adequately described and observed".⁵² "The first genuine speech experiences consist in the child's response to . . . the 'melody' . . . These responses occur at an early age (4-5 months or before) . . ." ⁵³ If a child responds to intonation not much later than to petting and stroking, it would seem that there might be a common denominator, a physiological one.

Manfred Sandmann takes a more psychological view. He notes that "one feature most [languages] seem to have in common is that the end is marked by a falling note and the beginning by a rising note . . . We think this reflects the original tension-detension movement which accompanies the cognitional act in its progress from S[ubject] to P[redicate] and has then been used artificially to denote the beginning and end of statements".⁵⁴

Suppose we assume that fundamental pitch as it turns up in most languages reflects a natural condition of human speakers. What are some of the objections that will be raised and how can they be answered?

First, the claim that some languages have no intonation. This is mostly a matter of definition. But if some language were to be found altogether lacking in meaningful non-tonemic uses of fundamental pitch, I would suspect some kind of contrary conditioning, as if to say that a language with intonation does not represent something put in so much as a language without intonation represents something taken out. The blanking out of intonations would not be the first natural tendency countered by a convention. For example, when an organism is stimulated it will normally respond without delay; but, as Olmsted points out, latencies "can be learned as such, i.e., an organism differentially reinforced for delaying its response . . . will learn to do so".⁵⁵ Another example is what Whitely calls "diachronic sentences", in which the sequence of the report matches the sequence of the events.⁵⁶ It probably comes naturally to any speaker to use these, yet we soon learn to manipulate particles so as to reverse the natural order — *He came in and sat down* is changed, part of the time, to *He sat down after he came in*. Interrogative particles may take over for interrogative intonations. Languages ARE sophisticated systems; but this does not prohibit us from determining here and there a natural priority.

The second objection is the existence of tone languages, proving that fundamental pitch can be significant and yet have no direct ties with meaning. But tone languages do use intonation, and the interesting thing is that intonation usually carries the day if any conflict arises. In Lhasa Tibetan, intonation takes over everywhere except in

⁵² *Language Learning*, 9, 3,4 (1959), 76.

⁵³ Paul Garvin, review of Leon Kaczmarek, *Kształtowanie się mowy dziecka*. — *Language*, 31 (1955), 104.

⁵⁴ *Subject and predicate* (Edinburgh, 1954), 132.

⁵⁵ Review of *Psycholinguistics*. — *Language*, 31 (1955), 51.

⁵⁶ *Lingua*, 10 (1961), 169.

citation forms.⁵⁷ In Huichol the tonemes are found only in the intonational precontour and are lost in the nuclear contour.⁵⁸ Burmese loses tonemic contrasts under some conditions.⁵⁹ The other extreme is Otomí, where only the last syllable of the word carries intonational pitches while preceding syllables carry tonemes.⁶⁰ The complete loss of a tonemic system, as has happened with the Yoruba of Cuba and appears to have happened with Adamawa Fulani,⁶¹ is probably not an argument against the objection, since if intonation is truly primitive the opposite must also have occurred — the overlaying of phonemic tone on a language already having intonation. But in this connection there is a line of inquiry that might be worth pursuing, suggested by something that Siertsema observes about Yoruba. She notes that while tone in that language is definitely phonemic, certain facts point to a residue of something expressive. For one thing, numerous word pairs exist in which the morphemes abstracted from tone are related in meaning. For another, there seems to be “a definite tendency to connect *stress* with higher *tone*”.⁶² I do not mean to suggest that intonation is necessarily the basis for phonemic tone, but the possibility exists, and weakens one argument against the universality of intonation.

The toughest objection is how it will be possible to find criteria of meaning that will transcend individual languages. Take a foreign speaker who misuses an intonation and is misunderstood. The naive cultural relativist looks at this and takes it for proof of an accidental similarity in form only. But this is giving up too easily. It fails to make a distinction between meanings and values. For example, a low-pitched fall in two languages may mean finality in both, but finality may be frowned upon sometimes in one community but approved in the other. “Societies . . . have characteristic average value preferences. Using the variable of formality, it is quite possible that one society would show a tendency, at least in some situations, to show a preference for adoption of formal forms of speech, and another in analogous situations to show a preference for informal forms.”⁶³ An example is a certain contour that both English and Spanish use for yes-no questions, but in different proportions. It is formal in both languages, but American English limits it to highly formal situations, like asking directions of a stranger, while Spanish prefers it in all situations that are not highly

⁵⁷ Gleason, p. 300.

⁵⁸ Joseph E. Grimes, “Huichol Tone and Intonation”, *IJAL*, 25 (1959), 221-232.

⁵⁹ William Cornyn, reported by Pike, *Tone Languages*, 17, footnote 34.

⁶⁰ Hockett, *Manual of Phonology*, 50.

⁶¹ “It is one of the very few Niger-Congo languages which is not tonal, and this fact alone would lead one to suspect that it may have been tonal and lost this feature. At present there is a well-marked intonation system, involving in most instances lowering of pitch at the end of intonation spans. There are, however, a limited number of items which are almost always associated with lowered pitches, even when not in clause-final position. These may well be the last vestiges of a tonal system. If so, perhaps the history has been one of morphophonemic leveling and intonational overriding . . . carried forward almost to the complete destruction of the tone system.” I have lost the source of this quotation.

⁶² “Stress and Tone in Yoruba Word Composition”, *Lingua*, 8 (1959), 385-402, esp. pp. 388, 391.

⁶³ John L. Fischer, “Social Influences on the Choice of a Linguistic Variant”, *Word*, 14 (1958), 47-56, p. 56.

informal. This suggests that there is no difference between the two languages at this point, but that there is a difference between the two communities in their preference for formality and informality — a fact that is borne out in many instances of non-linguistic behavior as well. When Sylvester Dirks says of Campa that “A very pronounced intonational feature is the elevation of the voice for a whole sentence or two to express disapproval or surprise”,⁶⁴ he describes something that is no stranger to English, but would certainly be frowned on in polite circles:

What did you have to go and do that
for?

Finding comparable meanings in intonation therefore requires us to put them in the most general terms, away from the polarity of likes and dislikes. And generalizing forces us back to emotion. The very thing that was ruled out of the system comes back at the heart of it; for the only indefectibly universal aspect of fundamental pitch is its association with the muscular tension of the whole organism. Emotional tension is reflected in tension on the vocal cords, which automatically raises pitch. The mere effort of speaking increases subglottal pressure which also raises pitch, and provides a purely physiological explanation for higher pitch in the early part of an utterance and lower pitch toward the end, even when there is no pitch rise due to excitement. A tension-relaxation dichotomy lies back of fluctuations in fundamental pitch, and its universality rests on our psychophysical makeup. It is extended by outreaching metaphors, likewise shared among languages to the extent that they are obvious but are less shared, and differ from language to language, as they become more occult. The primary, transparent metaphor is the simulation of tension, still part of the physiologically given: on the one hand nervous excitement, on the other, unfinished business. At a first remove, excitement, besides pitching high the entire length of an agitated utterance, gives us the rudiments of an accent system in which the pitch goes up only on the items that are exciting. Unfinished business, besides telling us that we are in the middle of an utterance, next transfers the high pitch of the middle to the end, enabling us to leave things like questions deliberately unfinished for the interlocutor to finish them. A language that uses high terminal pitch for unfinished business is like English whether or not it does so for questions; questions are secondary. Metaphors are overlaid on metaphors — a speaker reins himself in and holds down a high-pitched accent the way he controls his temper; this too is simulated and we get the reversed accents that are so common in Indo-European languages to signal restraint. At yet another remove a language using accents for the exciting or important items of discourse may exploit differences in order to show degrees of importance — a scheme of relative heights among accentual peaks. Meanwhile much of this gets partially grammaticized. An accent language employing relative heights may distinguish old from new or topic from comment, with intonation getting a foothold in the syntax.

⁶⁴ Dirks, p. 303.

But the foothold is with one foot; the other one is back there doing its primitive dance. A lower pitch may be used for theme than for rheme; but then themes are generally less interesting than rhemes. It is impossible to separate the linguistically arbitrary from the psychologically expressive. Even so simple a thing as a terminal fall shows by its gradience that what counts is how positively through we are — it is possible not just to be finished but to be finished finished, with an extra-low pitch at the end of a series of utterances that are individually “complete” grammatically:

On_e tw_o thre_e fou_r fi
ve si_x sev_{en} ei
ght.

It is the universality of the tension-relaxation dichotomy, plus the obviousness of the initial metaphors, that makes for the startling similarities between two such languages as English and Western Desert. The more the metaphors are elaborated, the more divergence, of course. To set up an opposition to a fall, one might use a terminal sustention or a terminal rise. A given language might use one, or the other; or it might use both, with gradient differences, such as one finds in English with a question like

Is there any ^{use?}

with terminal sustention, versus

Is there any ^{use?}

with terminal rise, in which the sustention is more likely to be used for rhetorical questions and the rise for questions asked for information; or the language might formalize this into a category of rhetorical questions, with the terminal level never used for anything else. There can be many differentiations, but they are variations on a central theme. Intonational typology, as I see it, should start at the central theme and trace its metamorphoses. In this sense, again, intonation is like gesture, where we find a similar parallel between central themes — actions that are responses to concrete states or circumstances — and differentiated ones. The latter, according to Giovanni Meo-Zilio, carry traces of their origin with them despite a greater or lesser degree of abstraction, of refinement into pure symbol.⁶⁵

The universality of intonation in the wider sense is hardly to be doubted. In fact, it is the universal traits that make it so difficult to deal with using the conventional tools of linguistics.

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⁶⁵ “El lenguaje de los gestos en el Uruguay”, *Boletín de Filología* (Chile), 13 (1961), 75-163, esp. pp. 90-97.

DISCUSSION

STRANG:

The evidence presented in this paper is most impressive, and points to a measure of agreement in the use of certain intonation patterns that cannot be accounted for by either chance or genetic factors. There is, however, evidence which supports Professor Bolinger's rejection of a genetic explanation, but also indicates that the uses of intonation he has shown to be in a sense 'normal' may be radically changed even in an environment that genetically favours them. He has spoken of English as conforming to the normal usage, but it is striking that this patterning, which seems to be of considerable antiquity, is actually reversed in some dialects. For instance, on Tyneside a final °4-2 (I am using Pike's labels) is used for positive affirmative statements, for non-yes-or-no questions, in short, for finality. This usage is absolutely regular and does not have, in Professor Bolinger's terms, a special "value". I wonder whether he would be prepared to consider what factors could account for this counter-normal development in an environment where the norm was, apparently, genetically realised.

DANEŠ:

1. I fully agree with Prof. Bolinger that statements such as "having intonation is a language universal" are of little value. Nevertheless I am afraid that even a more explicit statement, such as "yes-or-no questions have a rising pitch", is not sufficiently explicit, because the term "rising pitch" is still too vague and does not represent a systematic property (feature) of languages.

As we take for granted that linguistic units may be ascertained only in the systematic frame of the given language (their relevant properties being dependent on the system of oppositions), it is very difficult to establish a relation of sameness or identity between resembling items of different languages. We are allowed to say only *cum grano salis* that a sound [a] shared by the languages L_1 and L_2 is the "same phoneme" /a/ in the two languages as well. The same difficulty obtains, of course, on the level of intonation (with so-called intonation patterns as systematic units), too.

Thus I am afraid that linguistic universals can be hardly found among systematic linguistic units and consequently we cannot expect of the statements about universals that they be fully explicit at all.

And I would prefer to be more cautious and not to speak of universals, but of near-universals, in any case. Thus with the rising pitch of the yes-or-no questions, it is not difficult to find languages where the question terminates (or may terminate) in a pitch other than the rising one. And it is well known that for a foreign hearer it is sometimes difficult to distinguish a question from a statement.

While speaking about universals I would suggest always to distinguish (1) linguistic function, e.g. question, (2) means of expression, e.g. intonation, and (3) particular linguistic units of expression, e.g. different intonation patterns. In cases (1) and (2)

the universality may be based on the notion of sameness (identity), while in case (3) only the notion of similarity (resemblance) is pertinent.

2. I would emphasize that sometimes the intonational differences between dialects of a single language may be greater than between different languages (in their standard forms). E.g. between some standard Middle-European languages (Czech, Polish, German) we find very many similarities in their intonational forms, while in different dialects (e.g. of Czech) several quite different intonation patterns are employed.

CARNOCHAN:

It may well be that some of the intonational generalizations pointed out by Professor Bolinger apply to a number of dialects of a particular language, or to a group of languages, but they are surely not universals, as may be seen by reference to languages as far apart as English and tone-languages of Nigeria. I recently had the opportunity of listening to some of my Yoruba material by means of what was for me a new technique, developed at the Charles University in Prague by Dr. P. Janota. It combines the use of a tape repeater and a segmentator, which, acting as a "gate", permits one to hear at normal speed and amplitude a freely chosen segment of the example on each repeat, while suppressing the rest of the utterance, or reducing it to a very low amplitude. Where under normal listening conditions I had heard a final high tone syllable as spoken on a high and level pitch of the voice, under the new technique it became clear that towards the end, as the voice was dying away, the pitch fell slightly; it was also clear that an initial vowel sound frequently began with a slight rise in the pitch, as the human "feed-back" mechanism of ear control was enabling the opening and closing of the vocal cords to settle down to a rather more regular performance. The results of this new listening technique may offer some support to Professor Bolinger's thesis, yet I do not think that he is referring to such phenomena.

In addition, in support of what he says, it is true that in Yoruba, Igbo and Hausa, languages about which I can speak from some personal acquaintance, a high tone syllable after a low tone syllable is frequently, and regularly in the same style of discourse, on a lower pitch of the voice than an initial high tone syllable. If there are a number of high tone syllables interspersed with low tone syllables in the same sentence, then the pitch of the voice is lower for each high tone syllable after a low tone in succession; Professor Ida C. Ward has referred to this as "down drift". There are, however, many sentences where there is a succession of high-tone syllables with what is perceived as level-pitch relationship, and in such sentences, the pitch remains level to the ear, and there is no "down drift" at all; e.g. Hausa *Ya zo gida*, "he came home". In Igbo, successive high-tone syllables are either level, or in step-down relationship, and there are regular arrangements in particular grammatical structure; e.g. *Ụmụ mọ̀dù àjaa ya n'ìhì mma ọ̀ m̀rà̀*, "People admired her on account of her beauty". (The vertical mark indicates the step down from one high tone syllable to another and the grave accent marks a low-tone syllable. The sentence is in the recently

approved official orthography.) There is a step down from the first syllable to the second and from the second to the third, then a change of pitch to the bottom of the voice. The *-jaa* is a high-tone syllable, and its pitch is not higher than that for *ma-*, the last previous high-tone syllable; and so on.

In all these three Nigerian tone languages, and in English and French among others, there are many sentences where the final syllable is on the highest pitch in the sentence. While it is often attractive to look for near universals, it is important to bear in mind, as I am sure Professor Bolinger does, that for every piece of supporting evidence, there is other evidence which throws the problem back once more into the realm of speculation.

MORGAN:

The hypothesis of Professor Bolinger is that typologically similar intonation contours in genetically unrelated languages is probably due to underlying physiological and/or emotional reasons. (Cf. his "Intonation: Levels versus Configurations", *Word*, 7, 1951, 199-210, and Rigault's remarks in the next paper in this volume.)

In my preliminary studies of French intonation, I was impressed by what seemed to be the role of "stretching" along a "continuum" horizontally and, especially, vertically (see Trager and Smith, *Outline of English Structure*, Norman, Okla., 1951, p. 51, and Trager on paralanguage in *SIL*, 13, 1958, 1-12 and *Anthrop. Ling.*, 2, 1960, 24-30). At some "intermediate" level a pitch sequence may convey distinctive meaning, e.g. interrogative in contrast with a statement or sustentation. Each pitch sequence is expandible up or down the *scale* (cf. scales on the piano) without changing in any way its distinctive meaning. These expansions simply add or superimpose *overtones*, indicative of various kinds of emotions such as joy, sorrow, anger, sarcasm, and the like. Moreover, it is of interest to note that these expressive pitch changes are characterized by definite changes and modifications of the physiological conditions characteristic of the articulation of the segmental phonemes co-occurring with pitch or other accentual phenomena. Such modification is usually an increase in muscular intensity, involving increased vocalic or consonantal quantity, syncope or other junctural disturbances at points of transition from one segment to another, or faucal constricture.

One notes in French such contrastive statements as *je ne l'ai pas vu* (neutral): *je ne l'ai pas vu* (marked elevation of all pitch levels throughout sequence with meaning "don't expect me to get involved in it"); *vous le vendez?* (neutral meaning "what price are you asking?"): *vous le vendez?* (*accent d'insistance* plus lowering of pitch on first syllable of *vendez* with simultaneous tendency toward unvoicing of nasal vowel and with meaning "you mean to say people actually buy it"). Accent displacement may be accompanied by other things. When the *accent d'insistance* occurs on the syllable immediately preceding the final, the vowel (usually high front) becomes unvoiced and is followed by faucal constricture, e.g. *à Paris* [appariʔ]. Elsewhere the result may be consonantal lengthening or increase in vocalic quantity and/or faucal

constriction plus postvocalic disjuncture, e.g. “c’est un *mmi-sérable*”; “c’est *im-possible*”; “c’est *imppossible*”.

There seems to be contrast between what this writer labels pitch level 3 (interrogative) and pitch level 4 (expressive) in *vous l’avez vu?* (neutral) and *vous l’avez vu?* (rise on *vu* putting that syllable in bold relief compared with preceding syllables and meaning “you mean to say you really did see him [the President]”). While French cannot properly distinguish between, say, compounds and nominal phrases by accentual means (White Hòuse versus a white hôte or a white hóuse), a French version of this could be *partially* accomplished by marked pitch elevation (e.g. 1 to 4 or 3 to 4) on *blanche* in *maison blanche*.

RÉFLEXIONS SUR LE STATUT PHONOLOGIQUE DE L'INTONATION

ANDRÉ RIGAULT

Tout le monde — ou presque — s'accorde à reconnaître à l'intonation une importance primordiale sur le plan de la communication.

Cependant, si l'unanimité se fait aisément sur l'importance de l'intonation sur le plan de la *parole* — ou du *message* — il n'en est plus de même quand on aborde le plan de la *langue* — ou du *code*. En effet, la question primordiale — et c'est l'objet de cet exposé — est la suivante: les signes prosodiques forment-ils un système? Peut-on leur appliquer les méthodes de la linguistique structurale dont je rappelle la définition donnée par Hjelmslev: "on comprend par linguistique structurale un ensemble de recherches reposant sur une hypothèse selon laquelle il est scientifiquement légitime de décrire le langage comme étant essentiellement *une entité autonome de dépendances internes*, ou en un mot, une *structure*".¹

Autrement dit, les signes prosodiques, et en particulier l'intonation, forment-ils un système structuré de signes conventionnels? Auquel cas ils appartiennent au plan de la langue. Ou bien ne sont-ils qu'un ensemble de manifestations spontanées, naturelles? Auquel cas ils ne sauraient être considérés comme un système linguistique. Car, ainsi que le dit Séchehaye d'après de Saussure, "la langue, phénomène sociologique et sémiologique, système de signes arbitraires, est une chose sui generis qu'il importe de ne pas confondre avec toutes les formes de l'expression qui ne seraient que psychologiques".² On pourrait dire dans le cas qui nous occupe: qui ne seraient que psycho-physiologiques.

Or, les attitudes des linguistes sont ici bien partagées. Pour bon nombre d'entre eux, l'intonation n'appartient pas au domaine linguistique car elle est entièrement conditionnée. C'est ainsi qu'Arisaka³ pense que l'intonation est d'ordre purement physiologique. Pour cette raison, et même si elle a un nombre de qualités exprimées en commun par les usagers de la langue, elle ne forme pas un système social, elle ne forme pas un système phonologique qui est une partie du système social. C'est également l'avis de Abe, Bloomfield, et Bolinger.⁴ Quant à Martinet, il est d'avis

¹ L. Hjelmslev, "Editorial", *Acta Linguistica*, 4 (1949), p. V.

² A. Séchehaye, "Les trois linguistiques saussuriennes", *Vox Romanica*, 5 (1940), 10.

³ H. Arisaka, *The Theory of Phonology* (Tokyo, 1940), p. 128-131.

⁴ I. Abe, "Intonational Patterns in English and Japanese", *Word*, 11 (1955), 386-398; L. Bloomfield, *Language* (New York, 1933), p. 104-114; D. L. Bolinger, "Intonation and Analysis", *Word*, 5 (1949), 249.

que “les variations de la courbe d’intonation exercent, en fait, des fonctions mal différenciées” et il précise: “ce sont le plus souvent des fonctions du type de celle que nous avons appelée expressive.”⁵

Au contraire, pour un assez grand nombre de linguistes — la plupart américains — les faits d’intonation appartiennent sans doute aucun à la structure linguistique, c’est-à-dire à la langue. Ces faits présentent donc des contrastes systématiques — ou des oppositions, tout comme les phonèmes. C’est le cas de Pike⁶ pour qui les éléments suprasegmentaux — ou selon ses termes “surimposés à la structure linéaire” — forment un système linguistique. En ce qui concerne l’intonation, il réduit les profils (“contours”) à quatre niveaux pertinents. D’autres américains vont plus loin et font des différents niveaux de hauteur pertinents (4 en général) des phonèmes. C’est le cas, par exemple, de Trager et Smith.⁷

Les avis étant ainsi bien partagés, tâchons de cerner le problème de plus près afin d’y voir clair. Penchons-nous tout d’abord sur l’arbitrarité ou la non-arbitrarité du signe prosodique. Les éléments prosodiques du langage ne sont pas entièrement arbitraires comme les phonèmes. Il est incontestable que le sujet parlant s’appuie, pour la réalisation de l’intonation, sur des éléments physiques et psycho-physiologiques bien déterminés. Ainsi que le fait remarquer Daneš, “du point de vue génétique, les schémas d’intonation peuvent en effet, être nés de réactions instinctives, d’où la ressemblance de certains types d’intonation dans de nombreuses langues”.⁸

En effet, les intonations transmettant des émotions particulièrement fortes semblent être communes à toute l’humanité. Ces intonations que Troubetzkoy appelle “extra-linguistiques” ont — selon ce même linguiste — “la même signification dans les langues du monde les plus éloignées”.⁹ D’autre part, dans ses cadres très larges, l’intonation serait, comme on l’a dit, “internationale”. C’est ainsi que Abe, se livrant à la comparaison des questions en anglais et en japonais, aboutit à la conclusion que les procédés mélodiques des deux langues sont, dans ce cas, identiques. De fait, on peut dire que dans les questions à mot interrogatif, prononcés sans autre désir que d’obtenir un renseignement, on trouve une intonation identique dans de nombreuses langues (sommet sur le mot interrogatif, puis chute graduelle): *When are you going? Itsu yukuno? ¿Quién ha venido? Wann soll ich kommen? Quel âge avez-vous?*¹⁰

Cependant, en dehors des deux grandes catégories suivantes: d’une part la montée donnant la notion de l’inachèvement, et d’autre part la descente donnant la notion de l’achèvement, il semble que chaque groupe linguistique ait spécialisé certaines variations mélodiques dans un emploi commun seulement aux membres de la même

⁵ A. Martinet, *Éléments de linguistique générale* (Paris, 1960), p. 79.

⁶ K. L. Pike, *The Intonation of American English*, 6th ed. (Ann Arbor, 1956).

⁷ G. L. Trager and H. L. Smith Jr., *An Outline of English Structure* (=SIL, *Occasional Papers*, 3) (Norman, Okla., 1951).

⁸ F. Daneš, “Sentence Intonation from a functional point of view”, *Word*, 16 (1960), 44.

⁹ N. S. Troubetzkoy, *Principes de Phonologie*. Trad. J. Cantineau (Paris, 1949).

¹⁰ I. Abe, *Word*, 11, 396.

communauté linguistique. Donc, si l'intonation est pour une large part conditionnée et offre un aspect international, il n'en reste pas moins qu'elle est nationale et particulière dans ses fixations. La preuve en est qu'un étudiant apprenant une langue étrangère doit maîtriser l'intonation de cette langue, suffisamment s'il veut se faire comprendre, parfaitement s'il désire se faire passer pour un autochtone.

En effet, comme le signale Daniel Jones: "les langues n'emploient pas toutes le même type d'intonation pour évoquer une implication donnée, et réciproquement, une intonation donnée peut évoquer une implication dans une langue et une autre dans une autre langue."¹¹ Il risque quelquefois d'en résulter des erreurs d'interprétation. Ainsi, Elizabeth Anderson-Uldall signale dans sa thèse sur l'intonation de l'anglo-américain qu'une intonation très caractéristique d'Edimbourg et des environs sonne à ses oreilles comme étant très nettement plaintive car elle se termine par un accent de hauteur moyen et tenu (mid level), mais, ajoute-t-elle, "on m'a affirmé que cette intonation n'est pas le moins du monde plaintive quand elle est employée à Edimbourg."¹²

Come and get your tea
 ↑ ↑ ↑ ↑
 (mid low high mid level)

Voilà donc un cas typique d'intonation nettement localisée, ayant subi dans le parler d'une région bien déterminée une fixation ignorée d'autres régions.

En dehors de ses fixations régionales et de ses fonctions émotionnelles, il semble également fort probable que l'intonation se soit spécialisée dans certains emplois d'ordre distinctif. Ainsi que le signale le saussurien Bally "les intonations engendrées par l'émotion ne restent pas l'apanage du langage instinctif. Elles pénètrent sous une forme schématisée dans la langue-même."¹³ De son côté, Daneš, après avoir admis que c'est dans ses usages non-arbitraires que l'intonation fonctionne le plus souvent reconnaît cependant à l'intonation des emplois arbitraires: "Le degré d'arbitraire", dit-il, "est proportionnel au degré d'intellectualité de la fonction intonatoire."¹⁴

Une thèse récente vient d'ailleurs renforcer le sentiment que nous avons que l'intonation peut être arbitraire, au moins dans quelques-unes de ses manifestations. En effet, d'après la théorie neuro-chronaxique — soutenue par Raoul Husson¹⁵ — le jeu des cordes vocales est placé sous le contrôle direct du cerveau. Les vibrations des cordes vocales seraient ainsi assurées directement par l'influx nerveux venant des centres moteurs du langage et non pas par la poussée de l'air expiré, comme le

¹¹ D. Jones, *The Phoneme, its Nature and Use* (Cambridge, 1950).

¹² E. T. Anderson, *The Intonation of American English*, M.A. Thesis (London, 1939), dactylographié.

¹³ Ch. Bally, *Le Langage et la Vie*, 3ème éd. (Genève, 1952), p. 126.

¹⁴ F. Daneš, *Word*, 16, 35.

¹⁵ R. Husson, *Etudes des phénomènes physiologiques et acoustiques de la voix chantée* (Paris, 1950).

veut la théorie myoélastique traditionnelle. Ainsi la fréquence du fondamental serait susceptible de varier, non plus automatiquement, mais selon des ordres envoyés par le cerveau. Puisqu'un contrôle est exercé par l'organe central, il peut donc y avoir utilisation consciente, volontaire, par conséquent arbitraire, des ressources offertes par les variations de hauteur.

Outre son caractère souvent non arbitraire, d'autres arguments ont été invoqués contre l'appartenance de l'intonation au système linguistique. Ainsi, pour Martinet,¹⁶ les faits d'intonation ne sauraient entrer dans le cadre de la phonologie tout d'abord parce qu'ils présentent un caractère non discret et ensuite parce qu'ils se situent en marge de la double articulation du langage.

Essayons donc de repartir de la nature même de l'intonation avant d'essayer de définir les différents rôles qu'elle peut jouer — à notre sens — dans la communication. Toute manifestation phonique émise par un individu donné nous donne les renseignements du tableau I.

Ainsi, placé en face d'un sujet parlant — et pour nous en tenir à l'intonation — l'auditeur doit décoder un ensemble intonatoire complexe et le diviser en sous-systèmes distincts formés d'éléments appartenant à des domaines différents. Autrement dit, nous distinguons plusieurs plans sur lesquels se présentent les diverses manifestations de la parole humaine :

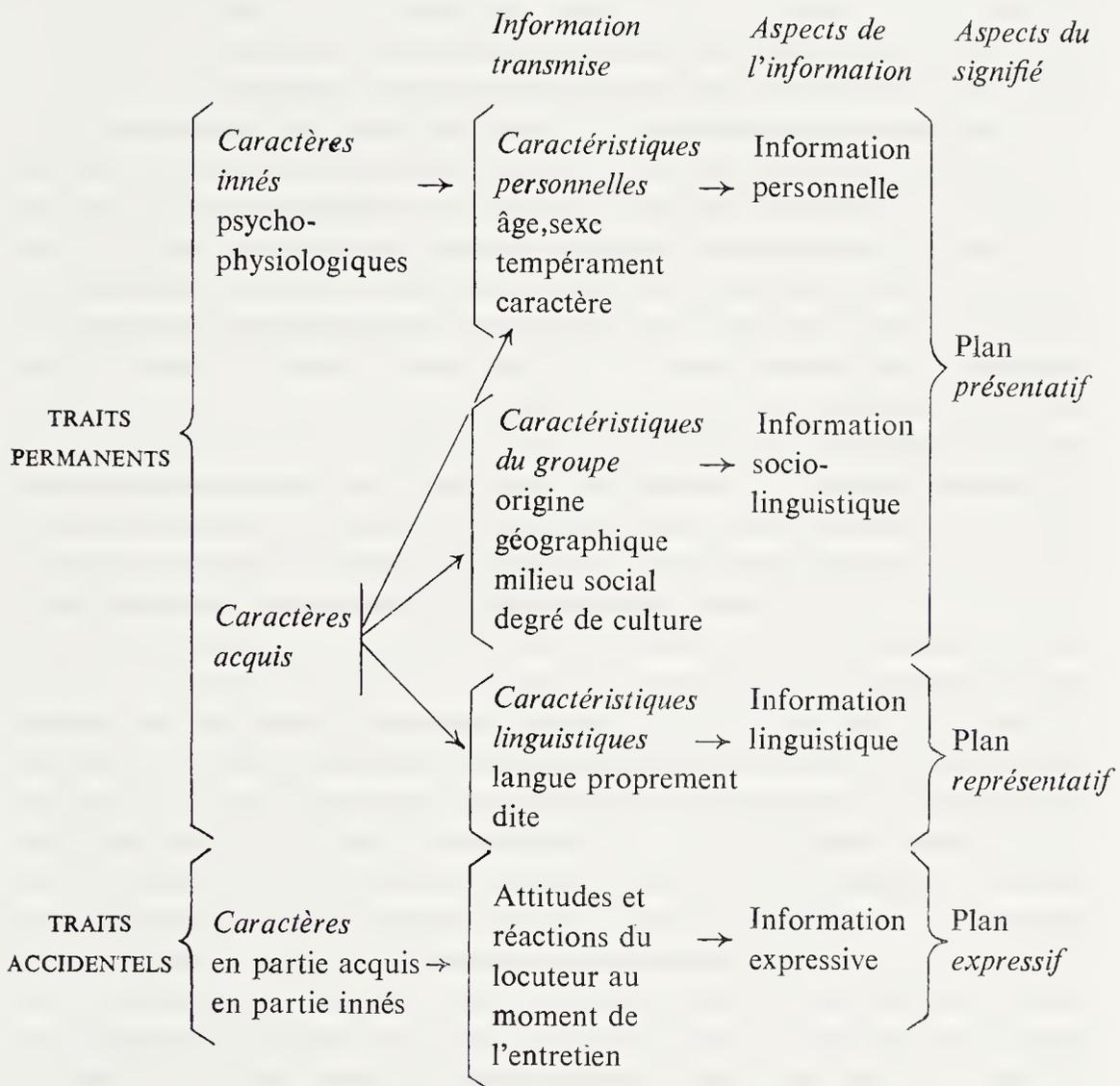
1. *Le plan représentatif*: représentation de l'état de choses, objet de l'entretien.
2. *Le plan présentatif*: présentation des traits caractéristiques du locuteur.
3. *Le plan expressif*: celui où se situent les attitudes et les réactions personnelles du locuteur au moment de l'entretien.

Cette classification correspond, en gros, à celle proposée par K. Bühler et reprise par Troubetzkoy. Cependant, nous préférons appeler "présentatif" ce que ce dernier nomme "expressif" et utiliser "expressif" là où il emploie "appellatif". Nous considérons en effet que l'âge, le sexe, l'origine géographique, etc. n'expriment pas un individu mais le situent. D'autre part, il nous paraît pratiquement impossible de faire le départ entre des procédés visant à provoquer certains sentiments chez l'auditeur (appel) et ceux qui tendent à exprimer ce que le locuteur ressent.

Quelles fonctions l'intonation occupe-t-elle sur chacun de ces plans? La fonction distinctive de l'intonation sur le *plan représentatif* est assez réduite. Il nous semble qu'il ne faille retenir comme traits pertinents de l'intonation que l'opposition entre la montée et la chute qui, on l'a noté depuis longtemps, permet de distinguer entre la continuation et la conclusion. Encore faut-il préciser et bien se rendre compte que la fonction distinctive ne joue que dans quelques cas. Ainsi, en français, il semble bien que le seul cas où l'intonation ait une fonction nettement distinctive, soit celui des oppositions du type: *il vient / il vient?* Ici, incontestablement, seule l'intonation permet de différencier la déclaration de l'interrogation. Nous saisissons, du reste, dans ce cas précis, le fonctionnement d'un phénomène de nature psycho-physiolo-

¹⁶ A. Martinet, *Eléments*, p. 97.

TABLEAU I

Les différents plans de l'acte de parole

gique — donc conditionné — dans une utilisation nettement distinctive. Ici, nous avons un signifié (la montée de la voix) et un signifiant (une question, correspondant à la formule interrogative *est-ce que*).

Dans les autres cas, l'intonation ne joue pas de fonction distinctive sur le plan représentatif. En effet, dans la forme *est-ce qu'il vient?* qui est nettement interrogative (de même que dans *vient-il?*) l'intonation n'est pas seule à transmettre la notion de question. En fait, si l'on utilise une intonation montante, il en résulte une redondance, car le sens du message est parfaitement clair quelle que soit l'intonation utilisée. De même, le rendement fonctionnel distinctif de l'intonation paraît être

nul dans les questions à mot interrogatif. Dans une question telle que *où allez-vous?*, ou *où est-ce que vous allez?* la forme du texte (inversion ou *est-ce que*) indique suffisamment qu'il s'agit d'une question. Ici aussi une intonation montante est redondante. En d'autres termes, si l'intention d'une phrase est indiquée suffisamment clairement par le texte (mots et structure grammaticale) l'intonation joue un rôle pratiquement nul sur le plan représentatif. Si, par contre, l'intention n'est pas clairement indiquée par le texte, l'intonation fonctionne à plein rendement.

Une deuxième fonction exercée par l'intonation sur le plan représentatif est la fonction *démarcative* (ou délimitative). En effet, l'intonation joue dans ce domaine, un double rôle. D'une part, elle intègre les mots en groupes de sens, et d'autre part, elle segmente la chaîne parlée en délimitant les groupes. Certes, elle n'est pas le seul procédé phonique à remplir ce rôle. Cependant, le rendement de l'intonation à ce poste semble élevé, particulièrement en français où la plupart des groupes de mots présentent un mouvement intonatoire — généralement montant — sur les dernières syllabes du groupe.

Enfin, toujours sur le plan représentatif, l'intonation assure une fonction *accidentuelle* (ou contrastive, ou culminative). Mais elle est ici tellement imbriquée aux faits d'accentuation qu'il est extrêmement difficile de dire ce qui revient à l'un et à l'autre de ces faits prosodiques. Toutefois, des recherches récentes ont prouvé clairement que l'intensité (ou énergie articulatoire) ne joue qu'un rôle très négligeable dans la perception de l'accent, alors que la hauteur musicale seule assure sans aucune erreur la perception de l'accent.

Sur le *plan présentatif*, où la plupart des phénomènes phoniques ont un aspect nettement naturel, les fonctions linguistiques de l'intonation paraissent très limitées. Il ne semble pas que les hommes d'une part, les femmes d'autre part, les jeunes d'un côté, les adultes de l'autre, utilisent des intonations différenciées. Il faut noter cependant que l'intonation, jointe à d'autres traits, aide puissamment à préciser l'origine géographique ou sociale d'un individu, ou tout au moins, si l'on ne peut l'identifier à coup sûr, à le faire identifier comme étranger par les membres d'une autre communauté linguistique. C'est ainsi qu'il existe une intonation typique de la Savoie, signalée par Martinet,¹⁷ une intonation typiquement canadienne-française, etc. A ce propos, il faut noter que ce qui est "normal" et passe donc inaperçu dans un groupe linguistique déterminé, devient "anormal", donc chargé d'information sur le plan présentatif vis-à-vis d'un autre groupe.

Nous savons qu'il existe également des intonations d'origine sociale. Ainsi, il ne fait aucun doute qu'il existe une intonation propre en particulier au 16^{ème} arrondissement de Paris, et qui sonne en français comme mondaine et distinguée, ou bien affectée et snob (selon l'origine et l'état d'esprit de l'auditeur). On pourrait également faire un classement de l'intonation selon le caractère. Les sanguins, les nerveux, les flegmatiques ayant sans aucun doute un comportement intonatoire différent. Il y aurait intérêt à préciser les variations de l'intonation dans le domaine

¹⁷ A. Martinet, *La description phonologique* (Paris, 1956), p. 100.

caractérologique. L'étude de tous ces éléments reste à faire. Du reste, ils n'appartiennent pas, à notre avis, au domaine de la langue. Ce sont ou bien des phénomènes individuels, largement conditionnés, ou bien des phénomènes caractéristiques d'un groupe et donc porteurs d'information sociologique, mais sans effet sur le sens du message.

Restent les fonctions exercées par l'intonation sur le *plan expressif*. C'est là qu'elles sont, et de loin, les plus nombreuses et les plus frappantes. En effet, "l'intonation est l'un des plus importants véhicules de l'expression affective du discours".¹⁸ C'est là aussi que surgit la controverse: appartiennent-elles ou non au domaine purement linguistique? Cette intonation expressive forme-t-elle un système de signes arbitraires (condition sine qua non pour lui accorder un statut linguistique) ou bien n'est-elle qu'un ensemble de phénomènes conditionnés par le psycho-physiologique? Nous touchons du doigt le problème central qui se pose au linguiste: la nature des rapports entre les entités phonologiques et les manifestations vocales. Problème identique à celui qui se pose en anthropologie culturelle: celui des rapports entre la culture et la nature. En effet, si, ainsi que l'admettent de nombreux linguistes nous reconnaissons tous immédiatement et sans effort chacune des attitudes associées aux tons est-ce parce que nous appartenons à une communauté linguistique dont nous avons assimilé les structures intonatoires, ou bien tout simplement parce que nous sommes membres de la grande famille des hommes?

Nous devons ici faire preuve d'une grande prudence et nous méfier des conclusions hâtives. Ainsi nous considérons la plupart des descriptions de l'intonation anglaise comme peu satisfaisantes, au point de vue linguistique. Kingdon,¹⁹ par exemple, a tendance à noter dans le détail les moindres variations d'intonation et à leur assigner un sens. C'est aussi l'attitude de M. Schubiger.²⁰ Or, les nuances de la pensée et du sentiment étant virtuellement infinies, on arrive à un tableau extrêmement complexe de l'intonation anglaise. Il serait sans doute possible de grouper de nombreuses intonations sous une même rubrique, comme variantes d'un même type. Autrement dit, les travaux de Kingdon ont sans doute une grande utilité sur le plan pédagogique, mais son classement des faits d'intonation est plus phonétique que phonologique.

A l'opposé, nous trouvons la description résolument phonologique de Trager et Smith, suivis par d'autres linguistes américains. Cependant dans leur volonté de réduire l'intonation à un système simple et cohérent de quatre tons fondamentaux, ils sont amenés à simplifier à l'extrême et de façon qui nous semble arbitraire une réalité fort complexe. D'autre part, nous ignorons tout de la façon dont ces "tons" ("pitch levels") ont été isolés. En fait, Bolinger, Lehiste et Peterson²¹ ont refuté de

¹⁸ E. Zwirner, "A Contribution to the Theory of Pitch Curves", *Archives Néerlandaises de Phonétique Expérimentale*, 7 (1932), 39.

¹⁹ R. Kingdon, *The Groundwork of English Intonation* (Londres, 1958).

²⁰ M. Schubiger, *English Intonation: Its Form and Function* (Tübingen, 1958).

²¹ D. L. Bolinger, "Levels versus Configurations", *Word*, 7 (1951), 199-210; I. Lehiste and G. Peterson, "Some basic Considerations in the Analysis of Intonation", *Journal of the Acoustical Society of America*, 33 (1961), 419-425.

façon décisive, à notre sens, cette théorie et montré que seuls les "contours" d'intonation ont une valeur significative en anglais. L'étude de W. Jassem,²² par contre, va tout à fait dans le sens que nous souhaitons.

En réalité, les études de l'intonation sont terriblement gênées par l'absence de matériaux. Pour résoudre le difficile problème de l'appartenance ou de la non appartenance de l'intonation à la linguistique, il faudra sans aucun doute consacrer de nombreuses et longues études aux faits d'intonation. Il faudra en particulier, décrire soigneusement l'intonation d'autant de groupes linguistiques qu'il sera possible de le faire, en éliminant ce qui paraît personnel, accidentel, conditionné. L'idéal nous paraît être de faire de nombreuses monographies portant sur l'intonation d'un individu ou d'une famille ou d'une petite communauté (village). Ceci permettrait de dégager le système d'intonation commun au groupe étudié. Ensuite, et ensuite seulement, il sera possible de comparer entre eux les systèmes d'intonation des différents groupes linguistiques, sur le plan régional, à l'intérieur d'une même langue (cf. l'étude de K. Hadding-Koch sur le suédois méridional²³). Enfin, on pourra comparer les diverses langues entre elles.

De cette façon, il serait possible de déterminer avec quelque chance de précision les traits différents de chaque langue dans le domaine de l'intonation, l'hypothèse étant la suivante: les traits communs à toutes les langues sont, selon toute probabilité, ceux qui relèvent de la *nature* (conditionnement psycho-physiologique), ceux qui constituent véritablement l'intonation internationale. Par contre, ceux qui seraient spécifiques à telle ou telle communauté linguistique auraient de grande chance de faire partie d'un système, arbitraire et proprement linguistique, de formes et de sens pour lequel l'aspect psycho-physiologique ne joue pas.

Ainsi pourrait être créée une *stylistique phonologique*, selon les mots de Troubetzkoy, ou mieux une *prosodématique* qui serait une branche de la phonologie. En effet, si les profils d'intonation et les phonèmes sont deux phénomènes d'ordre différents, tous deux appartiennent au plan phonique du langage. Si nous considérons la phonologie comme la partie de la linguistique traitant des phénomènes phoniques au point de vue de leurs fonctions dans la langue (et non pas au sens étroit du mot, comme la théorie des phonèmes seulement — qu'il faut appeler la *phonématique*), il est clair que les principes méthodologiques de base de la phonologie peuvent s'appliquer également à l'intonation.

C'est donc une tâche de la linguistique de décrire le système phonologique des faits d'intonation, dans chaque langue, et d'en préciser les fonctions. Tâche ardue, tâche nouvelle, car ainsi que le signale Martinet: "personne n'a osé jusqu'ici faire, de la fonction, le principe de base de la classification des faits prosodiques".²⁴

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²² W. Jassem, *Intonation of Conversational English* (Wrocław, 1952).

²³ K. Hadding-Koch, *Acoustico-Phonetic Studies in the Intonation of Southern Swedish* (= *Travaux de l'Institut de Phonétique de Lund*, III) (1961).

²⁴ A. Martinet, *Accents et Tons* (= *Miscellanea Phonetica*, 2) (1954), 13.

DISCUSSION

CARNOCHAN:

It is easy to be misled by the similarity or identity of terms into thinking that we are talking about the same concepts, and perhaps I am wrong in linking Rigault's use of *la phonématique*, *la prosodématique* and the use of phonematic and prosodic by Professor J. R. Firth and his followers. After reading the synopsis of this paper, and listening to our speaker today, however, I conclude that he is thinking along lines similar to those indicated by Firth in his "Sounds and Prosodies", *TPS*, 1948. Since then a number of phoneticians and linguists at the SOAS have been treating intonation in certain African and Oriental languages as a prosodic element of structure of the grammatical "piece", be it phrase, group, clause, or sentence.

Listing sentences, or parts of sentences that have the same tune is not totally without value, as it provides a sort of dictionary of tunes, and may be useful for teaching, but it is my experience that it is more profitable to consider intonation within the grammatical framework of the language under analysis. In the case of three Nigerian tone-languages with which I have some acquaintance, certain intonational features regularly correspond to the grammatical structures, and certain other intonational features to the subclassification of the items, e.g. to the subclassification of verbs and of nouns into tonal groups; it is in this respect that I disagree with Professor Martinet's view of intonation as put forward by Rigault this morning.

May I give two examples from Yoruba. I recognize three types of sentence in Yoruba in order to deal with such intonational differences as are found in *O dé*, (Statement) 'He arrived'; *O dé?* (Question) 'Did he arrive?'; *O dé*, (Linked Answer) 'Yes'. Here the three different intonations regularly go with the three different types of sentence. Parallel with these are *O ló*, 'He went away'; *O ló?* 'Did he go away'; *O ló*, 'Yes'; and *O bọ*, 'He returned'; *O bọ?* 'Did he return?'; *O bọ*, 'Yes'. There are three different sets of intonations, one set for *dé*, another for *ló*, and a third for *bọ*, and the differences between the sets correspond to the setting up of three different tonal classes of verbs in Yoruba, *dé* being a member of Class I, *ló* of Class II, and *bọ* of Class III. It is by the use of phonological criteria, and not grammatical criteria, that the verbs are differentiated into these three groups.

My second example is illustrated by two sentences, *Ige ba mi gbe e*, Ige helped me lift it; *Mo ba Ige gbe e*, I helped Ige lift it. In the first, there is a sequence of noun and verb *Ige ba...* of the order Nominal Phrase and Verbal Phrase in subject-verb relation. In this sentence the syllable *-ge* rises sharply from a low to a high pitch of the voice, and I recognize a High Junction phonologically as typical in such structures of the subject-verb relation (NP)^{HJ}(VP), nominal phrase-verbal phrase in subject-verb relation. In the second sentence there is also a noun verb sequence. *Ige gbe...* but this is not of the order NP VP in subject-verb relation. There is no High Junction, and the pitch of the voice for *-ge* is heard as low and level. If other sentences similar grammatically to the first are considered, where instead of *Ige*, one has *Pope*, *Ojo* (proper names) or *omọ*, child, items which are all nouns but are members of different

tonal classes, one finds that all the sentences have a High Junction, but that this is achieved phonetically in different ways; the last syllable of *Popo* is on a high level pitch; of *Ojo* is high rising; of *omø* is rising from a mid pitch of the voice. These sentences of the same grammatical structure have a common intonational feature, the High Junction; they have also intonational features that differ, and these differences correspond to the different classes of the nouns. These classes are arrived at by taking into account phonological criteria, the exponents of which in the stream of speech are the changing pitches of the voice, the intonation.

THE THEORY OF THE FUNCTION OF THE PREVERB *GA*-

PHILIP SCHERER

The study on which I am reporting considers inadequate the previous theories of the function of the preverb *ga*-, all of which explain the formal shift from simplex to *ga*-compound as a function of some sort of connotational shift. In the classical aspectual theory this is a shift from an imperfective simplex to a perfective *ga*-compound; in the theory proposed by M. M. Makovskij ("K probleme vida v gotskom jazyke", *Učebnye Zapiski*, XIX, 1959, 41-98), the perfective simplex is in free variation with the perfective *ga*-compound, at a time when the originally exclusive perfective function of *ga*- has become vestigial; in the one proposed by Ju. C. Maslov ("Kategorija predel'nosti/nepredel'nosti glagol'nogo dejstvija v gotskom jazyde", *Voprosy jazykoznanija*, 1959, No. 5, 69-80), the terminant simplex is in free variation with the terminant *ga*-compound, at a time, indeed, when the originally exclusive terminant function of *ga*-has become vestigial; lastly, in the theory proposed by Maurice Marache ("Die gotischen verbalen *ga*-Komposita", *Zeitschrift für deutsches Altertum und deutsche Literatur*, 90 [Sept. 1960-April 1961], 1-35), the simplex corresponds to the subject-centered intent while the *ga*-compound is in response to the event-centered intent of the speaker: thus, a differentiation of intent which may be arrived at only after a careful analysis in each instance of the verb in relation to its context.

All these theories are clearly inadequate linguistically since they proceed from meaning to form and not conversely; and they are also unworkable, even on their own terms, since they fail to explain instance after instance of formal shift.

I shall illustrate the latter difficulty on two pairs of forms (out of a very large corpus of such forms) in which a simplex and a *ga*-compound are apparently in minimal contrast: *ḡahaidedi* vs. *gaḡahaidedi* 'siōpēsēi' (*andbitum* ina ei *ḡahaidedi* [L 18.39] vs. *hivotidedun* imma managai ei *gaḡahaidedi* [Mk 10.48]); *habaida* vs. *gahabaida* 'ekrātēsen' (*atgaggands inn habaida* handu izos [M 9.25] vs. *insandjands gahabaida* Iohannen [Mk 6.17]). How do these theories meet the difficulty?

The aspectualist explains the simplex as an imperfective (despite the aorist) by interpreting *andbitum ina ei ḡahaidedi* as 'ordered him silence' (a state of silence). Marache believes this interpretation of Streitberg artificial and suggests a contrast between the subject-centered *ḡahaidedi* in terms of the meaning of *andbitum* and the event-centered *gaḡahaidedi* in terms of the meaning of *hivotidedun*. Makovskij and Maslov suggest free variation which is in fact no explanation but a diachronically and ad hoc based postulation.

The present theory studies the formal conditions of occurrence of the simplex and those of the *ga*-compound. It distinguishes between verb forms which occur in isolate structures and those which occur in combined structures (paratactic and hypotactic sequences), thus, ISOLATIVE and COMBINATIVE verb forms, and notes (a) that all ISOLATIVE verb forms (regardless of tense and meaning) are simplicia and (b) that COMBINATIVE verb forms alone may occur as *ga*-compounds; it also notes (c) that COMBINATIVE verb forms may also occur as simplicia. It then studies the *ga*-compounds in relation to COMBINATIVE simplicia and discovers that *ga*- functions as a marker of temporal differentiation of the tense incidence of the COMBINATIVE in relation to that of the governing verb (in parataxis and hypotaxis), a differentiation which under certain conditions was PROGRESSIVE, that is, posterior to the tense incidence of the governing verb, and under other conditions REGRESSIVE or anterior to that of the governing verb, while the absence of *ga*- indicates lack of temporal differentiation, thus, simultaneity or merely congruence of temporal incidence as in narrative sequence. It illustrates these fundamental distinctions (in the study itself, these distinctions and their elaboration give rise to an essentially rewritten Gothic syntax) in the following examples:

1. Iesus *stob* ('éstē') faura kindina (M 27.11) vs. is *urreisands gastob* ('éstē') (L 6.8): the ISOLATIVE simplex, *stob*, contrasts with the PROGRESSIVELY differentiated COMBINATIVE, *gastob* ("he rose and (then) he stood").

2. *weihaida ist* ('hēgíastai') qens so ungalaubjanda in abin, jah *gaweihaidis ist* ('hēgíastai') aba sa ungalaubjands in qenai (K 7.14): the isolative simplex, *weihaida ist* (ISOLATIVE because it occurs in the first or governing member of the paratactic sequence) contrasts with the REGRESSIVELY differentiated, *gaweihaidis ist* ("the unbelieving wife has been sanctified by her husband, and the unbelieving husband had been sanctified by his wife"; the Greek text has the order of clauses reversed: *hēgíastai gar ho anēr . . . kai hēgíastai he guné . . .*).

3. *þahaidedi* vs. *gaþahaidedi* (cf. above): the combinative simplex (*andbitun . . . ei þahaidedi*) is not temporally differentiated from the governing *andbitun* and thus indicates simultaneity or immediacy of temporal incidence; while the COMBINATIVE *gaþahaidedi* (*hivotidedun . . . ei gaþahaidedi*) is PROGRESSIVELY differentiated from the governing *hivotidedun* ("they rebuked (threatened) that he should be silent").

4. *habaida* vs. *gahabaida* (cf. above): the COMBINATIVE simplex (*atgaggands inn habaida*) is not temporally differentiated from the governing *atgaggands inn* and thus indicates simultaneity or immediacy in temporal incidence ("he went in and (immediately) took her by her hand"); while the COMBINATIVE *gahabaida* (*insandjands gahabaida*) is PROGRESSIVELY differentiated from the governing *insandjands* ("(Herod) had sent and (then) seized John").

There is no question but that *ga*- functions as a temporal differentiator between the COMBINATIVE and the governing verb. However it must be noted that this function of *ga*- as one of temporal differentiation applies only to verbs which I name BI-PHASE, that is verbs which may function as either temporally (durationally) DETERMINATE

or as NON-DETERMINATE; it does not apply to the very much smaller group of verbs which I name SINGLE-PHASE or verbs which function only as DETERMINATE or only as NON-DETERMINATE: in this case, *ga-* functions as a meaning differentiator of the verb in relation to its context.

I shall indicate this distinction in function by contrasting the BI-PHASE *bairan* "be in travail (NON-DETERMINATE); give birth to (DETERMINATE)" with the SINGLE-PHASE *bairan* "carry":

1. *qino, þan bairid* ('tíktēi'), *saurga habaid* "When a woman is in travail she has sorrow" (J 16.21): *bairid* "is in travail" is a temporally NON-DETERMINATE verb because it represents process apart from termination (a fuller treatment in the study), and it is also an ISOLATIVE simplex as it occurs in the first or governing member of the hypotactic conditional sequence.

2. *ip Aileisabaiþ usfullnoda mel du bairan jah gabar* ('egénnēsen') *sunu* "Now the time came for Elisabeth to be delivered, and she gave birth to a son" (L 1.57): *gabar* "gave birth to" is a temporally DETERMINATE verb because it is one whose meaning is coextensive with a point of termination, and it is also PROGRESSIVELY differentiated from the governing *usfullnoda mel du bairan*.

The BI-PHASE *bairan* differs essentially from the SINGLE-PHASE *bairan*:

3. *saei ni bairiþ galgan . . .* "Whoever does not bear his own cross . ." (L 14.27): *bairiþ* "bears" is a temporally NON-DETERMINATE verb which has no DETERMINATE complement; only *atbairan* "bring" or *briggan* "bring" may be considered as being its functionally DETERMINATE complements. However, *gabairan* does occur with the differentiated meaning "conferre":

4. . . *hve galeikom* ('homoiósōmen') *þiudangardja gudis, aiþþau in hvileikai gajukon gabairam* ('parabálōmen') *þo?* "With what can we compare the kingdom of God, or what parable shall we use for it?": *gabairam* "shall carry together" (Mk 4.30) occurs in temporally non-differentiated parataxis, thus in a situation which among BI-PHASE verbs calls for a COMBINATIVE simplex whose meaning remains unaltered, yet here *ga-* differentiates the centextually determined "carry together; set alongside" from the contextually unmodified "carry".

In summary, *ga-* functions as a differentiator in relation to context: as a temporal differentiator in relation to the governing verb in the case of BI-PHASE verbs, as a meaning differentiator in relation to the context in the case of SINGLE-PHASE verbs.

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DIE ALTGERMANISCHEN ELEMENTE IN DEN BALKANSPRACHEN UND DIE FRAGE DES SGN. BALKANGERMANISCHEN

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Nach bisherigen geschichtlichen Forschungen sind die ersten grösseren organisierten Gruppeneinfälle der Germanen auf die Balkanhalbinsel schon in der zweiten Hälfte des 3. nachchristlichen Jahrhunderts nachgewiesen (die Schlacht bei Naissus [Niš] i.J. 269, Amm. Marcellinus, XXX, 5, 7; cf. C. Patsch, *Btr.*, III, 6 ff.). Die ostgermanischen Stämme, die schwer voneinander zu sondern sind, behaupteten in den verschiedenen Provinzen des röm. Reiches ihre Sitze fast ein halbes Jahrtausend hindurch, denn die letzten geschichtlichen Erwähnungen und sonstige Anklänge an die Germanen auf dieser Halbinsel fallen noch ins 9. Jahrhundert (W. Strabo, *Libellus de exordiis et incrementis rerum ecclesiasticarum*; *Die Chronik des Pfaffen von Doclea* [*Letopis Popa Dukljanina*]). Es ist also normalerweise zu erwarten, dass ein mindestens fünfhundertjähriger Aufenthalt und die Symbiose dieser Germanen mit anderen schon damals sprachlich verschiedenen Balkanvölkern, trotz grösserer Völkerbewegungen, doch manche sprachliche Reste hinter sich gelassen hatte.

Die Westgoten hatten bereits im ersten Viertel des 4. Jahrhunderts ihr eigenes Bistum, wahrscheinlich in Tomi (Dobrudscha, Bulgarien) oder auf der Krim, deren Bischof Theofilos Gotthias an dem Konzil von Nicea teilgenommen hatte. Der Belgrader Kameo, mit der Darstellung eines hoch zu Ross über tote, gefangene und um Gnade flehende Barbaren triumphierenden Kaisers, gehört der ersten Hälfte des 4. Jahrhunderts (C. Patsch, *Btr.*, III, 27). Konstantin II. brachte den Goten im Jahre 332 eine vernichtende Niederlage bei: centum prope millia fame et frigore extincta sunt (Orosius, VII, 28, 29; Exc. Vales., 6, 31). Die Goten wurden Förderaten und stellten 40.000 Mann als Hilfstruppen (Jordanes, *Getica*, XXI, 112).

Im Jahre 348 nahm Konstantius die Getreuen des Wulfila über die Donau herüber, in Mösia inferior (Goti ~~minores~~), deren Hauptsitz in zweisprachigem Gebiet in Donaubulgarien bei der Stadt Nicopolis (Stari Nićip), nordwestlich von Trnovo war. Noch 200 Jahre nachher waren sie eine gens paupera (Kleinviehzucht; Jordanes, *Getica*, LI, 267). Ihr so langes Erhalten in diesem Gebiete setzt eine beträchtliche Zahl der Bevölkerung voraus.

Um dem Ansturm der Hunnen zu weichen, setzten auch die heidnischen Goten i.J. 376 unter Fritigern die römische Grenze über. Die Zahl der waffenfähigen Männer allein betrug 200.000, was natürlich auf einem übertreibenden Gerüchte beruht, wohl

aber für eine grosse Zahl der Germanen spricht (Eunapius, *Frg.*, 42 [Müller, *FHG*, IV, 31]; C. Patsch, *Btr.*, III, 62, Anm. 3).

Obwohl die Hauptmasse der Germanen ihre Migrationen aus dem Balkan nach Südwesten setzte und sich nach Italien, Spanien, Südfrankreich und sogar nach Afrika ergoss, wo sie christianisiert und zugleich romanisiert wurden, wäre es doch irreführend nicht anzunehmen, dass eine beträchtliche Zahl von ihnen auch auf dem Balkan weitergeblieben ist, wo sie ebenso romanisiert und als dünnere Schicht von anderen Balkanvölkern assimiliert worden sind.

Die fast ein Jahrhundert alte Hypothese von den altgermanischen Sprachresten in den Balkansprachen – von den ersten Untersuchungen über die balkangotischen Elemente im Altslavischen (J. Kopitar) bis zu den kritischen Forschungen von R. Loewe, St. Mladenov, A. Stender-Petersen, C. Diculescu, V. Kiparsky, M. Vasmer, H. Barić, I. Popović (um nur die bekanntesten zu erwähnen) über die altgermanischen Bestandteile in den einzelnen Balkansprachen – wurde sehr oft bezweifelt, aufgegeben, und wieder ins Leben zurückgerufen. Dass dieses Problem so viel und zugleich erfolglos diskutiert worden ist, geht oft, m. E., auf die Rechnung der falsch angewendeten Methoden und der Unterschätzung, oder vielmehr Ausserachtlassung der verwickelten balkanischen Mixoglotte. Man vernachlässigte, ausserdem, einerseits, die Lautsysteme der sgn. ostgermanischen Sprachen festzustellen, und andererseits die der einzelnen Balkansprachen, um sie erst dann zum Vergleich heranzuziehen.

Es ist nicht zu leugnen, dass die Erforschung des Balkangermanischen eher alles als einfach, weit komplizierter als die Sonderung der altgermanischen Elemente in anderen ausserbalkanischen Sprachen ist. Ausser Wulfilas westgotischer Bibelsprache verfügen wir über keine anderen sicheren direkten Sprachquellen. Um die phonetischen und morphologischen Sonderzüge des Balkangermanischen – das schon in sich selbst nicht einig ist – vom Bibelgotischen zu sondern, müssen wir diese Sonderzüge durch die Analyse der sehr dürftigen unmittelbar bezeugten germanischen Sprachreste auf dem Balkan rekonstruieren. Bei der Behandlung der germanischen Elemente auf dem Balkan handelt es sich weiter sehr oft um die sgn. Balkanismen, d.h. um die Sprachelemente die ein Gemeingut mehrerer Balkansprachen bilden.

Es entsteht dann die Frage, ob ein als sicher germanisch angesehenes Relikt direkt in die betreffende Balkansprache oder indirekt aus einer anderen Balkansprache Eingang gefunden hat. Wenn es sich um die altgermanischen Elemente in den südslavischen Sprachen handelt die aus diesen Sprachen in die anderen Balkansprachen eingedrungen sind, dann stellt sich die Aufgabe der Sonderung dieser Elemente, die auf dem Balkan übernommen wurden von derjenigen, die aus der vorbalkanischen Epoche der germanisch-slavischen and eventuell germanisch-südslavischen Beziehungen stammen, d.h. die Sonderung der älteren Schichte der gemeinslavischen (südslavischen) Lehnwörter von denen der balkanischen Schicht.

Und zuletzt liegt eine nicht minder wichtige Aufgabe in der Erforschung und Sonderung der altgermanischen Elemente, die aus dem Westgermanischen einige Jahrhunderte früher ins Balkanlatein, in die römische Soldatensprache auf dem Balkan ein-

gedrungen sind; diese Lehnwörter könnten dann vor der Einwanderung der Ostgermanen aus dem Balkanlatein in die Balkansprachen (ausser Südslavischem) Eingang gefunden haben. Vor allem aber ist Vorsicht geboten, die vermeintlichen Germanismen, die das altererbte indogermanische Gemeingut des Germanischen und der Balkansprachen bilden, die sog. Pseudogermanismen von echten Germanismen zu sondern.

Nach den obenerwähnten Erwägungen, auf Grund vieler Vorarbeiten (deren Ergebnisse die oben angeführten Literaturangaben darstellen), gestützt durch eigene, zuerst hier vorgetragene Berichtigungen, führe ich die sichersten balkangermanischen Relikte in den einzelnen Balkansprachen an. Auf Grund dieses Wortmaterials werde ich dann die Frage des sgn. Balkangermanischen beantworten.

1. IM AUSGESTORBENEN ROMANISCHEN DER ADRIAKÜSTE

1. Vergl. *bertáin* "jung verheiratete Frau, Schwiegertochter", vulglat. *brūtes* (vulglat. -ēs < -īs), bezeugt in drei Inschriften aus dem 3/4 Jahrhundert (Kärnten, Serbien, Bulgarien) (Domaszewski, *Neue Heidelb. Jahrb.* III, 193; *Rhein. Mus.*, LV, 318; H. Barić, *Lingv. st.*, 92; J. Brüch, *Der Einfluss der germ. Sprachen auf das Vulgärlatein*, 15, 48 u.a.; R. Loewe, 278 ff.).

Mgr. βρούτις, -δος lat. *brūta*; afrz. *bru(t)*; rätorom. *brütt*; friaul. *brüt* "id."; dt. *brüten*, *Braut*, *Bräutigam* (Meyer-Lübke, *REW*³, 121; P. Skok, *ZfrPh.*, 43, 187; 49, 383; cf. S. Feist, *GEW*³, s. v. *bruþ-faþs*). M. Bartoli (*Das Dalmatische*, I, 144; II, 448) stellt mit Recht vegl. *bertáin* zu germ.-lat. **brūtis*, welches mit dem Suffix *-ēnis* gebildet zu vegl. *bert-áin* führte (declinatio semigraeca). Da das Wort auch in anderen romanischen Sprachen (ausser im Italienischen) bezeugt ist, ist es als ein Relikt der vulgärlateinischen Soldatensprache, und zwar der auf dem Balkan (fehlt im Italienischen!) anzusehen.

Die romanische Form beruht auf germ. **brūdiz* "Seherin, weissagende Frau, Frau bei den Germanen" (R. Loewe, 279), bestätigt durch got. *bruþ* (M 10,35 Akk. Sing.) "neuvermählte junge Frau", *bruþ-faþs* m. *Bräuti-gam*, ahd. *brut* "id.", mhd. *briuten* "concupere cum aliqua". Es ist demnach ein *vorbalkanisch-germanisches* Wort.

2. Dalm. *rango* (Ragusa) "hinkend, humplig" (H. Barić, *Lingv. st.*, 92), ital.-tolos. *ranco* "id."

Zu got. *wraiqs* (Adj.) "schräg, krumm"; schwed. dial. *wrek* "verdrüssliche Person". Vielleicht kontam. mit got. *hrunga* (Mc 6,8) "Stab" zu altind. *kruncati* "krümmt sich" (bei H. Barić, *Lingv. st.*, 92 angeführtes got. *wranks* ist nicht bestätigt, cf. S. Feist, *GEW*³).

3. Vegl. *rek* "reich"; lat. *ricus*, *riccus* "id."

Zu got. **reik-s*, *reiki* "Reich", *reikists* (Superl.) Dat. Sing. (Mc 3, 22), aus dem Keltischen entlehnt, vgl. gall. *Dumuo-rix*, woher germ.-lat. *ricus* (rich) (H. Barić, *Lingv. st.*, 92).

4. Vegl. *stal* "stallo". Zu langob. *stollo* "id." (H. Barić, *o.c.*, 93).
 5. Dalm-ragus. *škljet* "rein, verdünnt", *škljeta rakija* "verdünnter Schnaps" (P. Skok, *ZfrPh.*, L 532; E. Gamillscheg, II, 236; H. Barić, *Lingv. st.*, 94).
 Rom. *selettus* "id.". Zu got. *slaihts* (Dat. Pl. L 3,5) "schlicht, glatt"

2. IM RUMÄNISCHEN

1. *burtă* "Leib, Bauch, Mutterleib"; istrorom. *burta* "id." (Puşcariu, *St. istroromane*, II, s.v.). Zu got. *baurþei* "Bürde, Last" (Akk. Sing. *baurþein* G 6,5), *gabaurþs* f. "Geburt, Geschlecht" (Diculescu, *ZfrPh.*, XLIX, 431; H. Barić, *Lingv. st.*, 95). Got. þ > rom. t, wie ital. *biotto* got. **blauþs*, dokum. got. *blauþjan*; got. o (aú) > rom. u, wie tosc. *rucciare*, germ. *wrotjan* "wühlen" (nach H. Barić, *o.c.* 95 = ostgotisch!).
 2. *brusture* "Klette" (Scriban, *Archiva*, XXXIX, 93; E. Gamillscheg, III, 36; I. Popović, *Geschichte der serbokroatischen Sprache*, 93; H. Barić, *Lingv. st.*, 95). Zu ostgerm. **brustilô*, dokum. in ags. *brystl*.
 3. *a căină* "weinen, jammern" (Giuglea, *Dacorom.*, IV, 15; Diculescu, *o.c.*, bei Kisch, *Festschrift Teutsch*, 41; E. Gamillscheg, II, 251; H. Barić, *Lingv. st.*, 96). Zu ostgerm. **quainon*, bezeugt in got. *qainon* "id."; aisl. *kveina*, ae. *cwanian* "id.". Got. q > rum. k ist in Ordnung (cf. rum. *care* < lat. *quare*; got. ai > rum. ai macht Schwierigkeiten, falls es nicht um got. Dipht. ai handelt!
 4. *gard* "Zaun, Umzäunung". Nach Kisch, *o.c.*, 45, E. Gamillscheg, II, 252, sollte es slavisches Lehnwort sein, zu asl. **gordъ*. Aus semantischen Gründen aber kann man es nicht zu asl. **gordъ*, ksl. *gradъ*, s-k., bulg., slov. *grad* stellen, weil dieses Wort im Slavischen *Stadt, Festung, Burg* bedeutet, *nicht Zaun, Umzäunung* (cf. H. Barić, *o.c.*, 98). Rum. *gard* ist auch nicht aus alb. *gardhi*, weil alb. *dh* rum. z ergeben hätte (alb. *bardhe* "die Weisse" -rum. *barza* "der weisse Storch"). Es ist demnach zu germ. *gards*, got. *gards* "das Haus als umzäunter Besitz", aisl. *gardr* m. "Zaun", ae. *geard* m., as. *gard* f. "Umzäunung", ahd. *gart* m. "Kreis" zu stellen (cf. S. Feist, *GEW*³, s.v.)
 5. *năsture* "Knopf, Haftel"; südital. *νάσῖουλον* "Knopf" (Rohls, *EW der unterital. Gräzität*, 1442); olympovalach., arum. *nastur*, *nasture* "Knopf, Knotte". Nach R. Loewe 304 zu ostgerm. **nastilô*. ahd. *nestila* "Schnur"; dt. *Nestel* (cf. H. Barić, *Lingv.st.*, 98; I. Popović, *o.c.*, 93); ital. *nastro* "id.". Es steht nichts auf dem Wege, dass das germ. (ostgerm.) Wort **nastilô* als der Name eines typisch germanischen Trachtartikels von den Rumänen übernommen worden ist (H. Barić, *Lingv. st.*, 98).
 6. *pungă* "Köcher, Pfeilköcher, Beutel"; ital. dial. (venez.) *ponga* "Kropf der Vögel"; mlat. *punga*, mgr. *πογγίον*; ksl. *pogy* "Köcher, Beutel". Obwohl im ganzen Germanischen die Bedeutung Beutel ist, kann es doch, wegen der sehr leichten Bedeutungswandel, zu got. *puggs* m. "Geldbeutel" gestellt werden (cf. F. Miklosich *EW*, 117; Brückner, *SEW*, 402; A. Stender-Petersen, *o.c.*, 396).
 7. *strănut* (H. Barić, *o.c.*, 102 = *stärnut!*) "mit weissgefleckter Stirn" (vom Pferd); rum. *străin*, *strein* "mit dem Flecke auf der Stirn". Zu germ. **sternô*, bestätigt in got. *stairno* (Nom. Pl. *stairnons*, Mc 13,25); krimgot. *stern* "stella" usw. Der Bedeutungs-

wandel von *Stern* zu *die Flecke auf der Stirn* ist ohne weiteres annehmbar, vgl. grödn. (rom.) *stairna* "eine Kuh mit weissgefleckter Stirn" (aus dem Ahd.). Rum. *stărnut* wurde mit dem Suffix-*ut* gebildet, wie *corn-ut*, *limb-ut* usw. Vgl. weiter schwed. *stjerna* "die Flecke beim Pferde, bei der Kuh", norw. *stjerne* "Stern (am Vieh), Blesse". kärnt. *stearna* "eine Kuh mit weissgefleckter Stirn". Für das hohe Alter des rum. Lehnwortes spricht das Suffix-*ut*, weil das lat. Suffix-*ut* im Rumänischen nicht mehr produktiv ist (H. Barić, *Lingv. st.*, 103).

8. Arum. *balu* "mit dem Flecke auf der Stirn"; alb. *balos* "id."; fränk. *bali* "id."; istrorom. *balius* "id.". Zu germ. **baleis* (Capidan, *Dacorom.*, II, 519; H. Barić, *Lingv. st.*, 103).

3. IM SÜDSLAVISCHEN

1. Bulg., s-k., slov. *bradva*, asl. **brady* ἀξίμη, ascia, securis πέλεκυς; rus. (kirchrus.) *bradъvъ*, altčech. *bradaticě*, oberlaus. *brodácica* "id." sind Lehnwörter aus dem Südslavischen (Kirchenslavischen). Da dieses Wort aus dem Südslavischen stammt, muss es aus dem Balkangermanischen sein (Loewe, 318; H. Barić, *Lingv. st.*, 113). Balkangerm. **bardô* ist nicht dokumentiert; im Krimgot. *bars* haben wir ein anderes Wurzel-determinativ, das gleich dem lit. *barzdo*, let. *barsda* "id." ist.

2. *gobino* (slov) "ubertas, copia"; asl. *gobъzъ* "reichlich"; s-k. (ksl.) *gobiznъ* "reich". Rus. *gobzenie*, *gobzitzъ*, *gobzovatъ* sind dem kirchsl. entlehnt; čech. *obih* ist aus < (h)*obih*, < *gobigъ* unter dem Einfluss von čech. *obilí* "Überfluss" entstanden (Kiparsky *o.c.*, 198; H. Barić, *Lingv. st.*, 115). Lit. *gabana*, *gebeti* "pflegen, reich sein, imstande sein", *gobus* "begierig" sind genuine Wörter.

Zu got. *gabei* f. "Reichtum", *gabeigs*, *gabigs* (Adj.) "reich" (Fr. Kluge, *Neuphil. Mitt.*, 123 f. (1921); T. E. Karsten, *Fragen*, 86 ff.; A. Stender-Petersen, *o.c.*, 315 f.).

3. Asl. *šlěmъ* "cassis, περικεφαλία, galea" (ursl. **šelmъ*); bulg. *šlem* "galea", s-k. *šljem*, slov. *šlem* "id.".

Zu urgerm. **χelmaz*, germ. *helm* (Janko, *Slavia*, IX, 346; H. Barić, *Lingv. st.*, 115). Dazu s-k. (ksl.) *hlěmъ* "galea" zu got. *hilms* "Helm" (cf. noch die ksl.-bg. Form *hilemъ*, H. Barić, *o.c.*, 115).

4. *skut*: asl. *skutъ* "περύγιον, extrema vestis, fimbria, amictus"; bulg. *skutъ* "Schoss, Saum, Schürze", s-k. *skut* "Saum, ora, limbus". Rus. *skutъ* ist dem Südsl. (kirchsl.) entlehnt.

Zu got. *skaut* "Schoss, Saum" (A. Stender-Petersen, *o.c.*, 394 ff.; H. Barić, *Lingv. st.*, 116; I. Popović, *o.c.*, 93).

5. Asl. (Gen. Sing. *smoky*) "Feige"; bulg., s-k. *smokva*, slov. *smokev* "id.". Da es ein ausschliesslich südslavisches Wort ist, kann es nicht ein Gemeingut des Germanisch-slavischen sein.

Zu got. *smakka* m. "Feige", *smakka-bagms* "Feigenbaum". Got. *smakka* ist nicht dem Südslavischen entlehnt, da sl. *k* nicht got. *kk* ergeben konnte (cf. auch H. Barić, *Lingv. st.*, 117). Es handelt sich um ein germanisches Lehnwort aus einer vorindoger-

manischen (kaukas.) Sprache, und zwar durch das Gotische (oder Krimgermanische) dem Südslavischen entlehnt. Cf. auch gr. *σῦκον*, lat. *ficus* und arm. *t'uz*. (K. Oštir, *Alarod. Sprachwis.*, 12 ff.; H. Hirt, *Indogerm.*, 568; A. Meillet, *Mém. soc. ling.*, XV 162; H. Barić, *o.c.*, 117).

6. Asl. *vinogradъ* "ἄμπελος, vitis"; s-k *vinograd* "vinea", slov. *vinograd* "id.".

Rus. *vinogradъ*, čech. *vinohrad* sind dem Südsl. (ksl.) entlehnt. Zu got. *weina-gards* "Weinberg", krimgot. *wingart* "vitis" (R. Loewe, 317, 332 ff.; R. Kiparsky, *o.c.*, 226; A. Stender-Petersen, *o.c.*, 364 ff.; H. Barić, *o.c.*, 118). Phonetisch wie auch semantisch stimmen die beiden Wörter vollkommen überein.

4. IM ALBANISCHEN

1. *fang* m. "wüster Boden, terreno incolto, sterile".

Zu got. *waggs* "Weise" (Akk. Sing. 2K 12,4) (Jokl, *Germ.*, 120; I. Popović, *o.c.*, 93). Got. *w* für alb. *f* macht Schwierigkeiten, die aber leicht zu überwinden sind.

2. *fat* "Ehemann". Dieses Wort muss von alb. *fat* "Los, Schicksal", das lat. Herkunft ist (lat. *fatum*), getrennt werden.

Zu got. *-faþs*, in Zusammensetzungen wie *hunda-faþs* "centurio", *þusundi-faþs* "hiliarchos, Anführer von Tausend", *bruþ-faþs* "Bräutigam". Wegen der Bedeutung des alb. *fat* "Ehemann" entlehnt dem got. *bruþ-faþs*, und zwar aus dem casus obliquus (Jokl, *Stud.*, 107 u. anderswo; I. Popović, *o.c.*, 93; dagegen H. Barić, *Lingv.st.*, 88).

5. ORTS- UND EIGENNAMEN

1. Got. *Fuþark* auf einer Halbsäule der gotischen Kirche, gefunden in Breza (Kreis Sarajevo), aufbewahrt im Landesmuseum in Sarajevo (Bosnien) (Čremošnik-Sergejevski, "Gothisches und Römisches aus Breza bei Sarajevo", *Novitates Musaeii Sarajevoensis*, 1, 1933).

2. Die Ruinen einer gotischen Kirche in ehem. municipium Novae Cheutae (heute ON Runovići bei Imotski, Herzegowina).

3. ON *Bredenъcъ* (Slovenien) von *Brenti* (lat. Praedenecenti), mit dem germ. Suffix *-ing* (sl. *-ędz*) (J. Kelemina, "Goti na Balkanu", *Časopis za zgod. in narodop.* XXVII 121 ff.; H. Barić, *Lingv. st.*, 75).

4. ON *Onogošt* (heute Ruinen bei Nikšić in Montenegro).

Zu got. PN *Anagasts* (K. Jireček, *Gesch. der Serben*, I, 36; H. Barić, *Lingv. st.*, 76; I. Popović, *o.c.*, 93). *Onogošt* ist sl. Bildung mit dem slav. Suffix *-j*.

5. FeldN *Gacko* (Metohija), ON *Gacko*, Adj. *gatački* (Herzegowina), FN *Gacka* (dh. reka-Fluss) Kreis Otočac, südwestlich von Otočac, DorfN *Gačani* (2 mal) Kreis Manja Luka (Bosnien).

Zu altsl. (altsüdsl.) **gъt-ъskъ* aus **Gъt* "Gote". (J. Kelemina, *o.c.*; H. Barić, *Lingv. st.*, 76; I. Popović, *o.c.*, 93).

6. BN *Hrgul* (Herzegowina). Zu germ. **Harjagauta* (anord. PN *Hergautr*), ahd. *Herigoz* (M. Vasmer, *ZfslPh*, XVIII, 58; H. Barić, *Lingv. st.*, 77; I. Popović, *o.c.*, 93).

7. ON *Otolež* (*Otolenž*) (Herzegowina).

Zu got. (ostgerm.) **aþaling*, altgerm. *aþala* "Adel, Adeltum" (M. Vasmer, *ZfslPh*, XVIII, 366; H. Barić, *Lingv. st.*, 77). ON *Otilovići* "id." (Herzegowina), oder zu *Atila*?

8. DorfN *Kotezi* (Herzegowina, zwischen Stolac und Trebinja, nach meiner Notiz i.J. 1962).

Zu got. **Gotenegg*, mit germ. Suffis-*ing*, wie *könęzъ* (kuning-as)

9. FN *Vipava* (Slovenien).

Zu germ. **up-ahwa*, Dt. *Wippach*, ital. *Vipacco* (J. Kelemina, *o.c.*: aus slov. **v-yp-ava*, dann aus dem Slov. ins Deutsche zurückentlehnt).

10. ONN Γότισσα, Γουδοβάσδα (Griechenland).

Zu germ. (got.) **Gud-badwo*; *badwo* "Streit", *bod* "Kampf" (I. Popović, *o.c.*, 93).

11. ON *Gossensass* (Südösterreich). Zu **Goten-sass* (I. Popović, *o.c.*, 93).

12. BN *Muntele Gotului* (Rumänien) "gotische Gebirge" (I. Popović, *o.c.*, 93).

13. BachN *Pîrăul Gotului* (Rumänien) "gotischer Bach" (I. Popović, *o.c.*, 93).

14. ON *Ika* (Ungarn). Zu germ. **ika(o)*, *eiko*, dt. *Eiche* (I. Popović, *o.c.*, 93).

15. Festungsnamen: *Gessila-fossatum* in Haemus mons, nach Procopius).

Zu PN *Gessila* (H. Barić, *Lingv. st.*, 80). *Bastarnas* (Name eines Kastell in Thracia). Zu *Bastarni* (H. Barić, *Lingv. st.*, 80).

Mare-burgo, *Stili-burgo*, *Skulko-burgo* (Ostserbien, Westbulgarien), *Alicani-burgo* (am Schwarzen Meer). Der zweite Bestandteil ist germ-*burgs*, von den Römern als *burgus* übernommen; die ersten Bestandteile stellen die germ. (ostgerm.) PN dar, wie auch got. Appellativ *skulka*, weil norw. *skulka* "lauern". Dokum. auch in lat. *sculcare*, *exsculcare* (5 Jhrh.), altport, *escolca*, altlogod. *iscalca* (H. Barić, *Lingv. st.*, 80; Wieser, *ZfrPh*, XXXV, 443).

16. Ich führe hier noch eine Zahl von ONN die meiner eigenen Sammlung entstammen, die alle dem Balkangermanischen entlehnt sind, die aber noch nicht als solche anerkannt und behandelt worden sind:

Delegošta (Srebrenica, Bosnien), *Logovardi* (Kreis Bitola, Makedonien) *Vardište* (Bosnien) = Langobard-ište, *Obre* (Kreis Visko, Bosnien), BN *Obrovica* (bei Zara, Dalmatien), *Orgosta* (Herzegowina) = **argosto* (Superl., oder -*got-st*), *Obrež* (Bosnien), *Obrovo* (Kreis Fojnica, Bosnien), *Otigošća* (Kreis Fojnica, Bosnien) = zu *Got-*, *Obrinje*, *Otalež* (Idrija, Istrien), *Obršje* (Herzegowina) u.m.a.

Auf Grund dieses Wortmaterials, besonders zahlreicher Orts- und PN, die immer an den ursprünglichen Formen festhalten und am längsten aufbewahrt werden, geht es klar hervor, dass wir es auf dem Balkan mit einem Sondergermanischen, dem Balkangermanischen zu tun haben. Die meisten Sprachzüge des Balkangermanischen sind die des Gotischen und anderer ostgermanischen Sprachzweige, einige auch des Westgermanischen, die sog. vorbalkangermanischen Elemente.

DISCUSSION

BIRNBAUM:

Den Hauptfehler des Beitrages von Herrn Prof. Pudić sehe ich darin, dass er glaubt, das Balkangermanische, d. h. praktisch das Bibelgotische sowie germanisches Namentgut auf dem Balkan, mehr oder weniger als einen Bestandteil des balkanischen Sprachbundes behandeln zu dürfen, obgleich dieser Sprachbund, wenn es ihn überhaupt je als solchen gegeben hat, erst viel später zustande kam.

DIACHRONIC DEVELOPMENT OF STRUCTURAL PATTERNS IN THE GERMANIC CONJUGATION SYSTEM

EDGARD C. G. POLOMÉ

The structure of the Germanic verbal system, as it is evidenced from the earliest attestations of its various dialects, appears to be organized around the same basic contrast of time references – *present* versus *past* – as J. Kuryłowicz¹ assumed for the Indo-European system from which it ultimately developed. However, within the preterit system only the functional distinction of mode between the “indicative” and the “optative” is marked by morphologically integrated inflectional forms, whereas the present system also possesses a “medio-passive” paradigm for those two modes, besides preserving a fairly complete set of “imperative” forms. Of the two fundamental types of preterit stems, traditionally designated as “strong” and “weak”, the latter is undoubtedly a Germanic innovation characterized by the expanded use of a “dental suffix” as tense morpheme, whereas the former is considered to be fundamentally a continuation of the Indo-European perfect, in combination with definite aorist elements. To assume this syncretism in the Germanic “strong” preterit, special stress is usually laid on the Southern Germanic form of the second person singular indicative in *-i*, with the root-vowel grade of the plural, e.g. OHG *buti*, OS *budi*, OE *bude* : Goth. *baust*. This form is interpreted as a reflex – without augment – of an Indo-European thematic aorist of the type of Greek ἔφουγες. A similar explanation is offered for the Germanic ending *-un* of the third person plural indicative preterit, which continues the IE “secondary” ending **-nt*, occurring in the same type of aorist (cf. Gk. ἔφουγον, Skt. *ābhujan*), whereas the corresponding perfect ending is *-ur* in Sanskrit, e.g. *dadūḥ* : OHG *tātun*. Moreover, the long-vowel preterit-forms of the type of Goth. *qēmum* (: Lat. *uēnimus*) have also been compared with the Vedic third person singular “passive” aorist-forms of the type of *āgāmi*.²

The purpose of this paper is to show that, upon closer examination, the evidence adduced to assume the continuation of IE aorist-forms in the Germanic preterit system appears to lack cogency and that a satisfactory explanation of the latter can be given on the mere basis of the IE “perfect”.

¹ *L'apophonie en indo-européen* (= *Prace Językoznawcze P.A.N.*, 9) (Wrocław, 1956) (abbreviated: *Apophonie*), p. 26.

² Cf. J. Sverdrup, “Der Aorist im germanischen Verbalsystem und die Bildung des starken Präteritums”, in *Festskrift til Hjalmar Falk* (Oslo, 1927), pp. 296-330; E. Prokosch, *A Comparative Germanic Grammar* (= *W. D. Whitney Linguistic Series*) (Philadelphia, 1939), pp. 160-164.

It seems indeed fairly well established that the IE "perfect" is essentially characterized by vowel alternation in the root and special inflexional endings. Reduplication was originally an accessory feature,³ which became an essential part of the perfect formation in Greek and Indo-Iranian⁴ and developed secondarily, in some dialects, into the main characteristic of a perfect without vowel-alternation to present stems with *a* or *o* + resonant,⁵ e.g. Lat. *caedō* : *cecīdī*, Goth. *haldan* : *haihald*. At its earliest stage of diachronic development the conjugation system of Proto-Germanic appears to have included the following types of preterits:

a. Ablaut with internal vowel-alternation:

1. /a/ + resonant + obstruent in the singular: Germanic reflex of the IE vocalic allophone of the resonant⁶ + obstruent in the plural, e.g.
 - (class 1) sg. /aiC/ : pl. /iC/, e.g. Goth. *bait* : *bitun*;
 - (class 2) sg. /auC/ : pl. /uC/, e.g. Goth. *bauþ* : *budun*;
 - (class 3) sg. /alC/ : pl. /(u)lC/, e.g. Goth. *halp* : *hulpun*;
 - sg. /anC/ : pl. /-(u)nC/, e.g. Goth. *band* : *bundun*; etc.
2. /a/ + resonant⁷ or obstruent in the singular: /e/ + resonant or obstruent in plural, e.g.
 - (class 4) sg. /al/ : pl. /e'l/, e.g. Goth. *stal* : *stēlun*;
 - sg. /am/ : pl. /e'm/, e.g. Goth. *nam* : *nēmun*; etc.
 - (class 5) sg. /aC/ : pl. /e'C/, e.g. Goth. *gaf* : *gēbun*;

b. Ablaut without internal vowel-alternation:

1. without reduplication, e.g.
 - (class 6) /o/ + resonant or obstruent in the singular and in the plural, e.g. Goth. *sōk*, *sōkun* (: infin. *sakan*);
2. with reduplication, e.g.
 - (class 7) /o/, e.g. Goth. *lailōt*, *lailōtun* (: infin. *lētan*);

- c. Reduplication without Ablaut: mostly with verbal stem ending in /a/ + resonant + obstruent, e.g. Goth. *haihait*, *haihaitun* (: infin. *haitan*);
 Goth. *aiauk*, *aiaukun* (: infin. *aukan*);
 Goth. *faiþalþ*, *faiþalþun* (: infin. *falþan*); etc.

Type (a. 1) reflects the IE alternation of the *o*-grade in the singular with the *zero* grade in the dual and plural forms of the perfect indicative active, e.g. Gk. πέποιθα : πέπασθε (2d pl. < *-παθ-τε).

³ Presumably with iterative-intensive connotations; cf. B. Rosenkranz, *KZ*, 75 (1958), 218-221.

⁴ Cf. R. Birwé, *Griechisch-Arische Sprachbeziehungen im Verbalsystem* (= *Beiträge zur Sprach- und Kulturgeschichte des Orients*, ed. O. Spies, 8) (Walldorf-Hessen, 1956), p. 33.

⁵ Cf. H. Hirt, *Indogermanische Grammatik*, IV: *Doppelung, Zusammensetzung, Verbum* (Heidelberg, 1928), p. 276; Fr. Van Coetsem, "Das System der starken Verba und die Periodisierung im älteren Germanischen", in *Mededelingen der Koninklijke Nederlandse Akademie van Wetenschappen, Afd. Letterkunde*, N.R., vol. 19, No. 1 (Amsterdam, 1956) (abbreviated: *System*), p. 54.

⁶ Cf. W. Lehmann, in *JEGPh.*, 52 (1953), 150, but note also footnote 23 below.

⁷ Except /y/ and /w/; cf. W. Lehmann, *Language*, 31 (1955), 359.

Types (a. 2) and (b. 1) presumably represent the first major innovation of Proto-Germanic in the structural pattern of gradation in the verbal stem: in spite of the apparent similarity between Goth. *sētum*, *qēmum* and Lat. *sēdimus*, *uēnimus*, the quantitative vowel-alternation within the Germanic paradigm is plainly a substitute for the IE functional gradation /o/ : /Ø/ as a characteristic of number, whereas the Latin long-vowel perfect is due to a reinterpretation of an Indo-European morphophonemic alternation as “tense-marker” (e.g. **yeH₁-k-* : **yH₁-k-ye-* > *iēc-ī* : *iaciō*).⁸ The parallel of Goth. *sētum* and Vedic *sādi* (with augment: *āsādi*) is deceptive as well, as this so-called “passive” aorist whose occurrence is restricted to the third person singular, is presumably an Indo-Iranian innovation due to the incorporation of neuter action nouns in *-i*, accented on the root, into the verbal system,⁹ with the original function of agentless “passives”.¹⁰ Actually, the direct continuation of the IE gradation /o/ : /Ø/ is clearly evidenced in the 4th class by Goth. *man* : *munum* as compared to Gk. μέμωνα : μέμαμεν. Whereas in Goth. *munum* and *skulum* /un/, /ul/ represent the normal Germanic reflexes of IE [ŋ], [ʃ] in /C-C/ in the athematic paradigm of the perfect (e.g. 1st pl. **mŋ-mé*, **skʃ-mé*), a few plural forms of the 5th class give evidence of the difficulties encountered by Proto-Germanic to provide a phonologically adequate pattern of alternation to reflect the IE functional gradation in verbal stems with a final single obstruent: in OE *ġeneah* : *ġenugon* it has followed the pattern of the 4th class; in Goth. *mag* : *magum* phonological leveling within the paradigm has blurred out the IE gradation contrast between singular and plural.¹¹ The pattern of alternation which was finally adopted in the 4th and 5th class is based on the reshuffling of gradation which occurred in class 6, when IE /o/ became Gmc. /a/. This class included a number of verbs with the root-vowel /a/, from PIE **H₂é*, in their present stem, e.g. Goth. *skaban*; the perfect stem of these verbs originally showed /o/, from PIE **H₂ò*, in the singular and presumably /a/, from PIE **H₂*, in the plural.¹² In order to preserve the functional distinction marked by the gradation /o/ : /a/ when the contrast between the reflexes of those pre-Germanic short vowel phonemes got blurred in Germanic, a *quantitative Ablaut* was substituted for the lost *qualitative Ablaut*. The lengthening of /o/, which can be compared to the process by which Indo-Iranian *ā* developed in the 3rd person singular perfect, e.g. Skt. *jajāna* : Gk. γέγονε,¹³ was actually backed up by a pattern of alternation which developed in Proto-Germanic when *-*ye/o*-presents with short root-vowel were associated with inherited long perfect-stems with /o/ from PIE **òH*, e.g. Goth. *skapjan* : *skōþ*

⁸ Cf. E. Benveniste, *Archivum Linguisticum*, 1 (1949), 18-19.

⁹ Cf. Hirt, *Indogermanische Grammatik*, IV, 109, 134-135; T. Burrow, *The Sanskrit Language* (London, 1955), pp. 176-177, 340.

¹⁰ Cf. J. Gonda, *Remarks on the Sanskrit Passive* (Leiden, 1951), pp. 100-101.

¹¹ Unless Goth. *mag* : *magum* reflects PIE **mHòghé* : **mHghmé*!

¹² The notation **H* merely represents the functional zero grade of the PIE syllable nuclei containing a vowel and a “laryngeal” without implying that the involved PIE “laryngeal” phoneme must be phonetically represented by a syllabic allophone in such a case. – It is assumed that the dialectal reflex [a] of PIE **H₂* was already part of the /a/-phoneme (from PIE **H₂e*) in pre-Germanic.

¹³ Cf. Kuryłowicz, *Apophonie*, 336-337.

(: Gk. ἀ-σκηθής “unscathed”), OHG *heffen* : *huob* (parallel to Lat. *capiō* : *cēpī*).¹⁴ However, the aberrant number contrast: singular /o·/ : plural /a/ could so much the less be maintained in the Germanic preterit paradigm as it also included inherited plural forms with /o·/ as a result of the coalescence of the reflexes of IE /a·/ and /o·/ in Proto-Germanic /o·/ practically simultaneously with the blurring of the /o/ : /a/ contrast,¹⁵ e.g. ON *ólu*, *óku* which reflect PIE reduplicated perfect forms **H₂e-H₂l-*, **H₂e-H₂g-* to present stems **H₂él-*, **H₂ég-* (ON *ala*, *aka* = Lat. *alō*, *agō*).¹⁶ To level out these discrepancies, Germanic /o·/ was ultimately spread to the whole paradigm of the preterit of the 6th class. This led to the parallel leveling of the singular and plural stems in type (b. 2) in Goth. *lailōt*, *lailōtum* instead of *lailōt* : **lailatum*, reflecting PIE **le-loHd-* : **le-lHd-*. On the other hand, it supplied a new pattern of gradation for the reshaping of the plural forms of the 5th class: as Kuryłowicz accurately points it out, “le rapport **skaban* : **skōbum*, **skōbuþ*, **skōbun* a engendré **geban* : **gēbum*, **gēbuþ*, **gēbun*, tandis que la quantité vocalique du singulier (**gab*, **gaft*, **gab*), qui différait de **geban* par le timbre, est restée intacte”.¹⁷ Moreover, this development was facilitated by the existence of at least one inherited preterit plural form in /e·/ to a root in /e/ + obstruent, namely *ētum* which reflects PIE **H₁e-H₁d-* like Lat. *ēdimus*. Further on, “sous l’influence conjointe des types **faran* : **fōrum*, **fōruþ*, **fōrun*, et **geban* : *gēbum*, **gēbuþ*, **gēbun*, on a formé **bērum*, **bēruþ*, **bērun* remplaçant l’ancien pluriel **burum*, **buruþ*, **burun*. Ici encore, à cause de la différence de timbre, le singulier a maintenu l’ancienne quantité brève (**bar*, **bart*, **bar* en face de **beran*).”¹⁸

Accordingly, the diachronic development of new vowel alternation patterns in the Germanic preterit system appears to be motivated by the necessity of reshaping the inherited *Ablaut* pattern of the IE perfect on account of the redistribution of the reflexes of IE vowel phonemes in Proto-Germanic.

As for the inflexional endings of the preterit, they clearly reflect the IE “perfect” endings in the singular:

1st person: PIE **-H₂e* > Pre-Gmc. *-a* (cf. Gk. *oĩδa*) > Gmc. *Ø*;

2nd person: PIE **-tH₂e* > Pre-Gmc. *-tha* (cf. Gk. *oĩσθα*) > Gmc. *-t*¹⁹;

¹⁴ Though the morphological process involved in the Germanic alternation shows a definite similarity with the Latin type, it implies the expected IE **o*-grade versus the **e*-grade reflected by Latin *cēpī*. Besides, some apparently obvious Germanic-Latin parallels are deceptive, e.g. Lat. *scabō* : *scābi* as compared to Goth. *-skapjan* : *-skōþ*, in which Gmc. /o·/ is due to secondary lengthening of IE /o/ (cf. Lat. *scaber* : *scobis*), whereas the Latin perfect has been made at a recent date on *scabō* (maybe by Lucilius?).

¹⁵ Cf. Van Coetsem, *System*, 76.

¹⁶ Lat. *ēgi* is analogical for **āgi*, cf. Benveniste, *Archivum Linguisticum*, 1 (1949), 17.

¹⁷ *Apophonie*, 311.

¹⁸ *Ibidem*.

¹⁹ Hardly due to the presence of the “laryngeal” as claimed by Chr. Stang in *Norsk Tidsskrift for Sprogvidenskap*, 15 (1949), 336-340 (cf. however I. Dal, in the same journal, 16, 1952, 328 ff.), but rather ascribable to analogical spread according to the pattern of the verbal paradigms where the voiceless stop remained unshifted as second element of clusters like /st/.

3rd person: PIE **-e* > Pre-Gmc. *-e* (cf. Gk. *οἴδε*) > Gmc. \emptyset .

In the dual which is only preserved in Gothic, the 1st person ending *-u*, e.g. Goth. *witu*, continues the PIE “perfect” ending **-wé* (e.g. Skt. *vidvá*); in the second person, the ending *-ts* (ultimately reflecting PIE **tH₁es* as evidenced by the thematic present form Goth. *bairats* corresponding to Skt. *bhárathah*)²⁰ has been added to the first person form in *-u*, presumably on the analogy of the Germanic contrast: 1st person $\{\emptyset\}$: 2nd person $\{t\}$ in the preterit singular paradigm. As for the plural, the 1st person also continues the PIE “perfect” ending **-mé*, e.g. Goth. *witum*, corresponding to Skt. *vidmá*. The 2nd person form preserves a reflex of the PIE “secondary” ending **-te*,²¹ which also occurs in Greek, e.g. *πέπασθε* (< **-παθ-τε*), *ἴστε* (< **F₁δ-τε*), with an analogical transfer of *-u-* in the historically attested forms, e.g. Goth. *witub*. This connecting vowel which apparently functioned as a secondary number-marker throughout the dual and plural paradigm²² after the Germanic development of $[m]$, $[ŋ]$ into $/um/$, $/un(b)/$, was obviously introduced with a view of eliminating the morphophonemic changes occurring in the Germanic verbal stems ending in fricatives, the more so as these changes were blurring out the contrast between singular and plural in the preterits without internal vowel alternation, e.g. Goth. (2d sg.) *slōht* : (2d pl.) *slōhub*, instead of **slōht* with preserved *-t* from IE **-te* in the cluster $/kt/$ > PGmc. $/xt/$.²³ As for the third person plural, the Germanic forms in **-un(b)* clearly reflect the athematic “secondary” IE ending **-nt*, e.g. in Goth. *bundun* < early PGmc. $*[bŋ^1d̥ŋθ]$ < Pre-Gmc. $*[b^hŋ^1d^hŋt]$. Whereas Greek shows the corresponding “primary” IE ending **-nti* in a few perfect forms, e.g. Homeric *λελόγχασιν* [λ 304],

²⁰ Cf. W. Krause, *Handbuch des Gotischen* (Munich, 1953), pp. 246-247.

²¹ It is hardly possible to decide whether this reflects the original PIE situation; anyhow, the ending *-a* of Skt. *vidá* is not found outside Indo-Iranian.

²² Cf. the Gothic forms:

dual 1st <i>wit-u</i> ,	plural 1st <i>wit-u-m</i> ,	versus singular 1st <i>wait</i> ,
2d <i>wit-u-ts</i> ;	2d <i>wit-u-b</i> ,	2d <i>waist</i> ,
	3d <i>wit-u-n</i> ,	3d <i>wait</i> .

²³ The repatterning of the pre-Germanic plural paradigm: 1st* *wid-mé*
2d **wid-té*
3d **wid-ŋt*,

must have taken place at a rather early date since no trace of the expected development of the pre-Germanic cluster $/dt/$ has been preserved in Germanic. This assumption is apparently contradicted by Lehmann’s tentative chronological delimitation of Proto-Germanic (*Language*, 37, 1961, 67-74), as he defines the loss of final short low vowels without primary stress after the accent shift as the structural change marking the final stage of Proto-Germanic. However, if – in spite of J. Marchand’s objections (*Language*, 32, 1956, 287) – Lehmann is right in assuming (*Language*, 31, 1955, 360) that PIE $[m]$, $[ŋ]$ were still reflected by vocalic resonants until a relatively late period in Germanic, “late” Proto-Germanic $[^1witme]$ > $[^1witm]$ must have developed into $[^1witum]$ almost immediately after the loss of final $/e/$, since no factual evidence points to any further preservation of $[m]$ as a mere allophone of the resonant $/m/$ at that stage of the development of Germanic. This, then, would situate the re-shuffling of the “preterit” paradigm in the transition period between Proto-Germanic and the historically attested dialects, presumably at the end of the so-called “*e-a*-period”, during which the structure of an important part of the preterit-system was so fundamentally modified (cf. Fr. Van Coetsem, *System*, esp. chapter IV).

Ætolian (inscr.) γεγόνεατι, etc. – presumably through analogical transfer from reduplicated presents of the type *τῆ(θ)ατι (: Skt. *dádhati*), other Indo-European languages suggest an original ending in *-r* in the “perfect”. This is evidenced by Vedic *vidur*, *cakrur*, *sedur*, *dadhur*, etc. and Old Latin *dederi* > class. Lat. *dedēre*, *uīdēre*, *dīxēre*, *cecidēre*, as well as by Tocharian “preterit”-forms in (A) *-ār*, *-ar*, (B) *-āre*, and (A) *-ār*, (B) *-ar*, e.g. (A) *sasātkār*, *kātkar*, *campār*, (B) *lyakāre*, *šerpār*, and the Hittite preterits of both conjugations, e.g. *ḫi-in-kir*, *e-ku-ir*, *zi-na-ar*, *pí-i-e-ir*, *da-a-ir*, etc. Upon closer examination it appears however impossible to reconstruct a common Indo-European prototype for this third person ending:

a. Tocharian B *-āre* cannot be readily equated to Lat. *-ēre* since the *-ē-* of Lat. *uīdēre* can hardly be interpreted as a tense-marker like Toch. *-ā*²⁴; if W. Couvreur²⁵ is right in assuming that Tocharian B *-re* reflects the IE middle ending **-ro*, it may even be quite irrelevant as evidence for the assumption of an original IE 3d person plural perfect indicative active ending in *-r-*.

b. In colloquial speech Latin seldom used *-ēre*; the usual ending *-ērunt*, which is already attested in inscriptions from Pisaurum by DEDRO(T) for **dēdērunt* and has survived in the Romance languages, does not contain the assumed IE perfect ending in *-r-*, but an allomorph of the morpheme {*is*}, also attested in the 2d person singular and plural and throughout the paradigm of all the other tenses derived from the perfect stem.²⁶

c. Two endings correspond to the Indo-Aryan 3d person plural perfect indicative ending *-ur* in Avestan: *-arəš* and *-arə*; whereas the former can be readily equated with Old Indic *-ur* as a reflex of Proto-Indo-Iranian **r-s*, it appears much more difficult to reconstruct a common prototype for Avestan *-arə* : Vedic *-ur*.²⁷

d. The Hittite ending appears consistently as *-er* ~ *-ir* but for two cases where *-ar* is attested: E. H. Sturtevant²⁸ has put a high value on these aberrant forms to posit a vowel gradation in the “Indo-Hittite” prototype of the ending: **-e'r* : **-b'r* > Hitt. *-er*, *-ir* (cf. Lat. *-ēre*) : *-ar* (cf. Skt. *-ur*); it is however more plausible to consider these forms merely as aberrant spellings, eventually due to analogy with the pattern /*irC*/ ~ /*arC*/, e.g. *arḫa-* ~ *irḫa-* “boundary”.²⁹ Accordingly, we have to posit a prototype **-er* for the 3rd person plural ending of the Hittite preterit, whose endings

²⁴ Cf. W. Krause, *Westtocharische Grammatik*, I: *Das Verbum* (Heidelberg, 1952), p. 154.

²⁵ *Hoofdzaken van de Tochaarse Klank- en Vormleer* (Louvain, 1947), p. 57. The same assumption may be made for Latin *-re*, since OLat. *-i* in *dederi* may be merely graphic for *-e* (cf. R. Kent, *The Forms of Latin*, Baltimore, 1946, p. 125, § 402 II).

²⁶ Cf. A. Meillet, in *BSL*, 34, 2 (1933), 127-130.

²⁷ Cf. M. Leumann, “Morphologische Neuerungen im altindischen Verbalsystem”, *Mededelingen der Koninklijke Nederlandse Akademie van Wetenschappen, Afd. Letterkunde*, N. R., vol. 15, No. 3 (Amsterdam, 1952), p. 112; cf. also Burrow, *The Sanskrit Language*, 309; A. Thumb-R. Hauschild, *Handbuch des Sanskrit*, II: *Formenlehre* (Heidelberg, 1959), p. 205.

²⁸ *Language*, 16 (1940), 180-181.

²⁹ Cf. H. Kronasser, *Etymologie der hethitischen Sprache*, I: *Zur Schreibung und Lautung des Hethitischen* (Wiesbaden, 1962), p. 25, § 18.3.

do not show any other immediate correspondence to the IE “perfect” paradigm.³⁰

Besides, even if we accept all the discussed forms as cogent evidence for the assumption of a PIE {r}-morpheme as a characteristic feature of the 3d person plural perfect indicative, it does not rule out the possibility of its being a secondary development, whether we consider it, with Meillet,³¹ as an agentless “passive” or, with Hirt,³² as an originally “nominal” form. Actually, as the latter pointed out, the further assumption³³ of two morphemes {r} : {nt} as characteristic of the 3d person plural perfect at a definite predialectal stage of development of Proto-Indo-European appears as a quite plausible solution, as it would account – on the morphological level – for the semantic contrast between the “personal” and “impersonal” use of the form! This duality is indeed reflected by various forms in the languages which apparently generalized the use of the {r}-ending, e.g. in Tocharian B, where the “thematic” preterit of *käm-* “come” and *länt-* “go out” shows the ending *-ṃ* < IE *-nt (*kameṃ* : *śemare* – analogically reshaped on the 3d sg. *śem(o)*; *lateṃ* : (A) *lcär*).

³⁰ Though the disputed question of the correspondences between the Hittite *-ḫi-* conjugation and the IE “perfect” lies outside the scope of this paper, it may be briefly pointed out, with E. Sturtevant, *A Comparative Grammar of the Hittite Language*, I, 2d edition (New Haven, 1951), p. 132, that:

(1) the distribution of the singular endings of the *-ḫi-* conjugation corresponds to the pattern of the IE perfect paradigm;

(2) Hittite, as contrasted with Luwian, may reflect a secondary development into a complete two-tense conjugation of a Proto-Anatolian paradigm:

singular 1st	*-Ha	< PIE *-H ₂ e,
2nd	*-tHa	< PIE *-tH ₂ e,
3rd	*-e	< PIE *-e,

in which *-e > -i spread all through the present in Hittite, in compliance with the pattern of the contrast between the present, with “deictic” -i, and the preterit, without -i, of the *-mi-* conjugation. In that case the Luwian 1st sg. pret. *-ḫa* would be the direct continuation of the PIE 1st sg. perf. ending *-H₂e, whereas the extensive reshuffling of the paradigms may have blurred the original situation elsewhere (cf. e.g. the use of the middle ending *-to in the 3d sg. preterit in Luwian *aḫta* “came”, like in Venetic *doto* “gave”). In the Hittite 1st sg. pret. *-ḫun* the ending *-un* of the *-mi-* conjugation has been substituted to the inherited *-a.

³¹ *Introduction à l'Étude comparative des Langues Indo-Européennes* (Paris, 1937^s), p. 235; cf. also V. Pisani, in *KZ*, 60 (1933), 222.

³² *Indogermanische Gram.*, IV, 123-127, 270; cf. also H. Hartmann, *Das Passiv* (Heidelberg, 1954), p. 200. – Actually, both views may merely reflect two chronologically distinct stages in the functional use of the *-r-* form, along the lines suggestively sketched by V. Pisani, in *Miscellanea G. Galbati*, vol. III (= *Fontes Ambrosiani*, XXVII) (Milan, 1951), pp. 30-31, where he identified it with the nominative-accusative neuter of the *-r/n-* stems, with “il valore di elemento indicativo dello stato, della inerzia” — which “stative” meaning led to its intransitive and impersonal use in the perfect and the medio-passive. As W. Belardi stresses it in his remarkable analysis of the formation of the IE “perfect” (*Ricerche Linguistiche*, I, 1 (1950), 103), this probable origin of *-r-*, as well as its basically impersonal meaning, suggest that “la sua specializzazione nella terza persona plurale del perfetto sia relativamente tarda nell'indoeuropeo”!

³³ Cf. *Indogerm. Gram.*, IV, 156, where Hirt emphasizes the parallelism with the alternation *-r- : *-nt- in nominal forms. If the derivation of *-nt in the 3rd person plural from the nominal suffix *-nt- of the “collectives” (cf. A. Erhart, quoted in J. Perrot, *Les dérivés latins en -MEN et -MENTUM*, Paris, 1961, p. 335, fn. 1) is correct, the differentiated use of both endings could possibly be linked up with the use of *-nt- as a nominal plural-marker (cf. J. Perrot, 312-315).

Though this ending is usually ascribed to repatterning of the paradigm on the model of the present,³⁴ the Tocharian aberrant forms may be compared to the Faliscan reduplicated perfect with thematic vowel and "secondary" personal ending F(IF)IQOD.finxerunt for **fifigond* and the Oscan and Paelignian 3d person plural perfect in *-ns* < **-nt*, e.g. Osc. *uupsens.operauerunt*, *fecerunt*,³⁵ *prúfattens.probaeuerunt*, Pael. *coisatens.curauerunt*. It can however be objected that these "thematic" forms are irrelevant to the discussion of the basically "athematic" paradigm of the perfect. Therefore, the Luwian athematic 3d person plural preterit *aüenta* to *aüi* "come" (< *au* "hither" (: Hitt. *u-*) + *i* "go")³⁶ provides valuable evidence of the coexistence of *-nt-* and *-r-* endings in the Proto-Anatolian paradigm.³⁷ Luwian *-nta* has however been explained as reflecting the IE middle ending **-nto*, but *-a* may as well be due to analogical extension, as the repatterning of the paradigm led to the contrastive use of *-i* : *-a* as tense-markers for the present versus the preterit, e.g. Luwian *aüiti* "he comes" : *aüita* "he came"; Palaic *lukit* "he lighted" plainly shows indeed that the Luwian situation reflects a secondary development. Therefore, if the Anatolian preterit can be – partly, at least – considered as a continuation of the PIE "perfect", the Luwian athematic 3d person plural in *-nt(a)* may be equated with the Germanic ending **-un(b)* < IE **-nt*. The latter cannot indeed have been taken over from the thematic aorist, where IE **-ont* would yield Gmc. **an(b)*, and even if it should be interpreted as a recent innovation, the pattern on which it developed could be supplied by the Germanic prototype of the Old Saxon reduplicated 3d person plural preterit indicative *dedun* "they did". This form is indeed a direct continuation of PIE **dhe-dh(H₁)-nt*, which also survives in the Gāθā-Avestan athematic imperfect *dadaṭ*. The corresponding Vedic form *adadhur* is plainly an innovation: it occurs 32 times without augment versus 17 times with augment, which shows that it is actually a perfect-form "vom Sprachgefühl als augmentlose Vergangenheitsform (...) gefasst und demgemäss gelegentlich durch das fakultative Augment verdeutlicht".³⁸ Formally, the *-e-* vowel of the reduplication syllable indeed rather points to a perfect-stem, which suggests that the Avestan form *dadaṭ* is an original perfect, commonly used with preterital meaning³⁹ and reinterpreted as an imperfect on account of the exclusive

³⁴ Cf. W. Krause-W. Thomas, *Tocharisches Elementarbuch*, I: *Grammatik* (Heidelberg, 1960), p. 254; the 3rd sg. (B) *šem*, *lac* may however reflect original forms with the IE perfect ending *-e* (cf. *ibidem*, p. 253).

³⁵ Presumably a long-vowel perfect of the type of Lat. *ōdi* (cf. Benveniste, in *Archivum Linguisticum*, 1, 1949, 17), reflecting a PIE reduplicated form **H₃e-H₃ps-* from a "theme I" **H₃ep-s-* > Proto-Italic **ōps-* > Osc. *úpsannum*. operandum, faciendum.

³⁶ Cf. also Palaic *lukinta* "they lighted", in which *-i-* is presumably a part of the verbal stem (cf. A. Kammenhuber, in *RHA*, 17, 64, 1959, pp. 13-14, 80-81).

³⁷ However, in a lecture given in Austin (11 July 1962), R. Crossland suggested that the 3d person plural ending **-nt* of the proto-Anatolian paradigm was replaced by **-er* in Hittite because its reflex **-un* in the athematic preterit would have blurred out the distinction between the 3d person plural and the 1st person singular in *-un* < **-m*.

³⁸ M. Leumann, *Morphologische Neuerungen im altindischen Verbalsystem* (Amsterdam, 1952), p. 100.

³⁹ Cf. H. Reichelt, *Avestisches Elementarbuch* (Heidelberg, 1909), pp. 311-312.

use of the *-r-* forms in the third person plural perfect in Indo-Iranian.⁴⁰ When verbal paradigms were reorganized in the Indo-European dialect from which Proto-Germanic developed, the morpheme {nt} functioned everywhere as third person plural marker. This entailed the complete ousting of the discrepant **-r-* ending in the perfect, the more so as the latter assumed definitely preterital functions: the generalization of the originally merely alternative **-nt-* ending may indeed have been favored by its use in the eliminated imperfect which the perfect replaced.⁴¹

The early Proto-Germanic “preterit” system whose organization we have tried to reconstruct, was however partly disrupted by the use of some “resultative” Indo-European “perfects” like **woida* “he has seen” > “he knows” as presents. New preterits had indeed to be formed on these “presents”, which had preserved their morphological features of IE perfects. As it clearly appears from the historically attested evidence, the new preterit paradigm was formed on the stem of the inherited perfect participle in IE **-t-*,⁴² presumably by expanding it with the “preterit” suffix **-ē-* or **-ō-*, followed by the “secondary” personal endings.⁴³ It then provided a model for the derivation of a Germanic preterit from the stem of verbs without IE apophonic perfects, like the causatives in **-eio-/-eie-* or younger denominative formations. But while the “dental” preterit thus developed into a tightly knit subsystem of the Germanic verbal system, reduplication was progressively discontinued as a characteristic of the preterits continuing IE perfect forms. The process leading to its almost complete elimination from the conjugation system, after the Goths had left their original Scandinavian homeland, has been accurately described by F. Van Coetsem⁴⁴ as a consequence of the Proto-Germanic change of IE **ō* to *a*, which provided a new Ablaut pattern as a substitute for reduplication. In the preterits with internal vowel-alternation, the contrast between singular and plural (and dual) forms in the indicative, originally reinforced by the position of the IE accent, led to the isolation of the second person singular with {t} after the late Proto-Germanic loss of final short vowels,⁴⁵ since the first and third person singular with preterit-stem I and

⁴⁰ Actually the only 3d person plural perfect form from the PIE root **dhéH₁-* occurring otherwise in Avestan is the younger form *dādarə* “they have put”, with long reduplication vowel (: 3d sg. *dada* “he has created”).

⁴¹ Even if, in spite of the *e*-vocalism of the reduplication syllable, Avest. *dadaŋ*:OS *dedun* have to be interpreted as reflecting the original imperfect, to a reduplicated present (3d pl.) **dhé-dh-nti* (> Skt. *dādhati*), this inherited imperfect form without augment can as plausibly be assumed to have provided the model for the replacement of {r} as characteristic marker of the 3d person plural perfect active by {nt} in the new Proto-Germanic preterit system!

⁴² In view of the consistent correspondence in vowel grade with the plural forms, e.g. in (class 3) *parf*:*haurbum*:(pret.) *haurfta*:(part.) *haurfts*, G. Must (*Language*, 27, 1951, 125-128) can hardly be right in deriving the dental suffix from the 2d person singular form in perfects without vowel gradation, like OHG (1st/3d sg.) *mag*:(2d sg.) *maht*. On the other hand, the connection of these early Proto-Germanic “dental” preterits with the IE **-to-* participle does not necessarily result from morphophonemic changes in a periphrastic formation without “connecting vowel”, as J. Sverdrup contends in *Norsk Tidsskrift for Sprogvidenskap*, 2 (1929), 94-96.

⁴³ Cf. Must, *Language*, 28 (1952), 104-106.

⁴⁴ *System*, esp. chapter IV on these innovations no longer shared by Gothic.

⁴⁵ Cf. Lehmann, *Language*, 37 (1961), 70.

zero ending then contrasted everywhere else with preterit-stem II and endings containing a vowel, e.g. in Gothic:

	Singular	Dual	Plural
1st	<i>nam</i>	<i>nēmu</i>	<i>nēmum</i>
2nd	<i>namt</i>	<i>nēmuts</i>	<i>nēmup</i>
3rd	<i>nam</i>		<i>nēmun</i>

This new pattern of the preterit indicative paradigm, outside the 2d person singular: *Stem I + {Ø} : Stem II + {V(C)}* led to the replacement of the *stem I + {t}*-form of the second person singular of the indicative preterit by a *stem II + {V}*-form in Southern Germanic. This dialectal development must have taken place long after the diachronic development of the Germanic preterit system had been completed. The IE "resultative perfects" functioning as presents were indeed in no way affected by this innovation, as their paradigm was, by that time, completely integrated in the present system as an aberrant conjugation pattern within the latter. Owing to its relatively late date of introduction the new form of the 2d person singular preterit can hardly have been taken over from a pre-existent thematic aorist paradigm of whose survival there is obviously no trace otherwise in Proto-Germanic. Besides, if a Proto-Germanic form **-iz* were inherited from IE **-es*, e.g. in Gk. ἔφουγες, it would only be reflected by *-i* after the "light" stems in the first two classes of "strong" preterits in Southern Germanic, e.g. in OHG *bugi*, whereas it would have to be ascribed to analogical restoration or spread on the pattern of these forms in the other classes. In this case, the absence of Umlaut in Old English forms like *buġe* (= OHG *bugi*) would be particularly striking as it cannot be accounted for by referring to its non-occurrence in the optative where "the identity of the endings with those of the present optative led to the disappearance of the Umlaut".⁴⁶ It therefore appears much more plausible that the Southern Germanic *-i-* form of the 2d singular preterit indicative is a recent innovation: it may have been taken over from the optative whose 2d person singular ending **-īz* should normally appear as *-i* in the Southern Germanic dialects, as evidenced by the OHG optative preterite functioning as a negative imperative *ni curi. noli*, and paralleled by the original optative functioning as a present OHG *wili* : Goth. *wileis*. The introduction of the inherited second person singular form of the optative preterit into the indicative paradigm to fit into the pattern *stem I + {Ø} : stem II + {V(C)}* would then have entailed the restoration of *-s* as distinctive feature of the 2d person singular in the optative paradigm of some dialects, e.g. OHG *-īs*,⁴⁷ OS *-is* : OE *-e*, Ofri. *-e*.

It appears accordingly possible to account for all the innovations in the Germanic preterit system in the course of its diachronic development from the earliest stage of

⁴⁶ Prokosch, *A Comparative Germanic Grammar*, p. 218 (§ 73b).

⁴⁷ With analogical *-ī-* before *-s* according to the distributional patterns of the allomorphs of the optative marker: [i] in /-#/ (before {θ} as personal ending): [i·] before consonantal personal ending.

the Proto-Germanic conjugation system by deriving it exclusively from the IE perfect, without having to resort to the problematic survival of otherwise unattested aorist-forms in definite dialects or in apparently discrepant forms within the paradigm or the inherited vowel alternation pattern. This conclusion is in keeping with the structure of the Proto-Germanic verbal system which is strictly based on a "present": "past"-contrast excluding a morphological denotation of the aoristic aspect.

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THE BASIC GRAMMATICAL CATEGORIES OF BENGALI

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0. The writing of a very brief description of a language, a "structural sketch", is a familiar custom. Such brief descriptions have had various aims and purposes and have been written from various points of view. One useful purpose is essentially typological – to provide linguists who are not specialists in the language with an understanding of how the language works, how it compares with other languages, what characteristics join it with sets of other languages and what characteristics set it apart from all other languages.

It seems quite possible to provide a summary of the sound system of a language in a few pages which will accomplish this purpose satisfactorily,¹ but it seems very difficult to do justice to the grammatical and lexical complexities of a language within a correspondingly small space. It is the author's feeling, however, that a competent linguistic analyst who has specialized in the study of a certain language should be able to describe it in summary form within several lectures or several short chapters of a "sketch". The present paper is intended to provide one of such chapters for Standard Colloquial Bengali (SCB).

The paper makes a number of assumptions about the characteristics of all languages, and in particular about the characteristics of Bengali, which should be made explicit at least in a general way. It assumes for example, that every language has WORDS, i.e. elements in the grammatical structure of the language which (a) are in at least some instances phonologically and grammatically more complex than morphemes and less complex than sentences, (b) show a maximum freedom of occurrence of other elements on either side of them as well as a maximum of bondage of elements within them, and (c) serve as a useful point of departure for describing major features of the whole grammatical structure of the language.

It assumes also that the grammatically definable WORD-CLASSES of every language in terms of which the constructions of the language may be identified will always include certain classes which are roughly equivalent across languages in syntactic and semantic function. Specifically such classes include nouns, verbs, qualifiers of some sort, noun-substitutes, demonstratives, and other classes depending on the particular languages. The word-classes of SCB posited here are nouns, verbs,

¹ An attempt to provide this kind of summary for the sound system of Standard Colloquial Bengali was made in C. A. Ferguson and M. Chowdhury, "The Phonemes of Bengali", *Lg.*, 36 (1960), 22-59.

adjectives, numbers, pronouns (including demonstratives), correlatives, definitives (which are generally suffixes but have a marginal status as words), and particles.²

Finally, the paper assumes that the major word-classes of any language exhibit certain grammatico-semantic categories which are obligatory for that language and are identifiable in terms of patterns of affixation, concord, and special types of selection. These may be called the basic grammatical categories of the language. The basic grammatical categories of SCB examined here are as follows: tense (verbs); "correlation"; person and its sub-categories of deixis and respect-grading (verbs, pronouns); case (nouns, pronouns); definiteness and its sub-categories of noun-classing and number (nouns, adjectives, pronouns, numbers). The categories will be described in that order in the paper. It may be noted that the order reflects in part ease of description and in part the relative "importance" of the categories judged on the basis of their pervasiveness in the grammar, their semantic distinctiveness, and the degree to which other categories presuppose them.

1. *Tense*. The Bengali verb³ has ten TENSES i.e. sets of forms identified by special suffixes ("tense formants") and personal endings, and having basically temporal semantic values. In addition to these finite forms, SCB verbs have four non-finite forms: a verbal noun, infinitive, and two "conjunctives" which show some correlation grammatically and semantically with the finite tenses. The ten finite tenses may be classified into five "systems" such as "present", "past", etc. each of which has one to three tenses, all summarized in table I, which gives tense labels and third person examples of the verb *bol-* ~ *bol-* "say" with sample English equivalents.

One interesting formal feature of the SCB verb system is the use of three different sets of personal endings, reminiscent, in distribution and degree of difference, of the primary and secondary endings of older Indo-European. The "primary" endings (e.g. first person *-i*) are used with tenses 1, 4, and 6; the "secondary" endings (e.g. first person *-um*) are used with tenses of past reference 3, 5, 7, and 8; the "tertiary" endings (e.g. first person *-o*) are used with tense 2, the future. The endings of the imperatives are a mixture of these sets with several unique endings.

The semantic value of the tenses is sufficiently suggested by the English equivalents for the purposes of this paper, but several comments may be useful. The past tense, 3, is used in narration, i.e. where the action is represented as one of a series in a story, and as such is frequently used in conversation, but the perfect tense, 4, is the general-

² The classes listed here are substantially those of the author's doctoral thesis, *The Phonology and Morphology of Standard Colloquial Bengali* (unpubl. Univ. of Pennsylvania, 1945).

³ The forms of the verb in SCB have been described several times. Cf. S. K. Chatterji, *Bāñlā bhāṣār vyākaraṇ*, 3rd ed. (Calcutta, 1945), 289-357; W. Sutton Page, *Introduction to Colloquial Bengali* (Cambridge, 1934), 137-164; C. A. Ferguson, *Phonology and Morphology . . .*, Ch. 6: Verbs; id., "Chart of the Bengali Verb", *JAOS*, 65 (1945), 54-5; E. A. Dimock, "Notes on Stem Vowel Alternation in the Bengali Verb", *Indian Linguistics*, 17 (1957, for 1955-56), 173-7; M. Abdul Hai, *Nasals and Nasalization in Bengali* (Dacca, 1960), 15-159. Of these, Ferguson's thesis gives the fullest listing of forms; Dimock's and Hai's pay special attention to the stem alternants.

TABLE I

Tenses of the Bengali verb

Systems	"Unmarked"	"Marked"	Imperative	Non-finite
Present	1 PRESENT <i>bole</i> "says"	6 PRES. CONTINUOUS <i>bolce</i> "is saying"	9 PRES. IMPERATIVE <i>boluk</i> "let...say"	VERBAL NOUN <i>bola</i> "saying" INFINITIVE <i>bolte</i> "to say"
Future	2 FUTURE <i>bolbe</i> "will say"		10 FUT. IMPERATIVE <i>bolbe</i> "let...say"	
Past	3 PAST <i>bollo, bolle</i> "said"	7 PAST CONTINUOUS <i>bolchilo</i> "was saying"		
Perfect	4 PERFECT <i>bolece</i> "said, has said"	8 PERFECT PAST <i>bolechilo</i> "said, had said"		PERF. CONJUNCTIVE <i>bole</i> "having said"
Conditional	5 CONDITIONAL <i>bolto</i> "used to say, [if]...had said, would have said"			COND. CONJUNCTIVE <i>bolle</i> "if...says, said, had said"

purpose past tense, corresponding very often to the English simple past, but also to some uses of the English perfect ("have said"). The perfect past tense, 8, simply emphasizes the remoteness of the past without the reference to another past action which typifies uses of the English past perfect ("had said"). Thus Bengali tenses 3, 4, and 8 all have as their normal equivalent the English simple past ("said").⁴

The infinitive, in addition to its use with verbs like "be able", "want", and so on (e.g. *bolte cai* "I want"), occurs in a repetition construction with the meaning "while ...ing" (e.g. *bolte bolte* "while saying").

Finally, the negative of verb forms in SCB is normally made by adding the suffix *-na* (e.g. *bolena* "doesn't say"). There are two interesting exceptions, each involving the neutralization of a tense distinction. (1) The negative of the perfect tenses 4 and 8 is made by suffixing *-ni* to the present tense. Thus the negative of both *bolece* and *bolechilo* is *boleni*. (2) The addition of *-na* to the present imperative forms merely softens the command to a request or entreaty. The future imperative forms with *-na* serve as the negatives of both present and future imperatives.

2. "Correlation". In Bengali there are sets of words which show a five-term category: interrogative, relative, and three-fold demonstrative ("unmarked", near, far). These

⁴ The lack of congruence between the Bengali perfect past and the English past perfect is shown by the over-use of the past perfect on the part of Bengali speakers who use it in their English in sentences where the usual English form would be the simple past.

words may be regarded as constituting the word-class of correlatives, which crosses other word-classes in the language, each correlative being also a member of another word-class (pronoun, adjective, number, particle).

The forms, samples of which are given in table II, are in most cases clearly composed of two morphemes, the stem and the correlative prefix. In several instances, however, there is some question about the appropriate morphemic segmentation, and it seems preferable to list them here simply as a paradigmatic array.⁵

TABLE II

Sample Correlatives of Bengali

Interrogative	Relative	Demonstrative		
		Unmarked	Near	Far
<i>ke</i> "who?"	<i>je</i>	<i>e</i>	<i>o</i>	<i>se</i>
<i>ki</i> "what?"	<i>ja</i>	—	—	<i>ta</i>
<i>kəkhon</i> "when?"	<i>jəkhon</i>	<i>əkʰon</i>	<i>əkʰon</i>	<i>təkʰon</i>
<i>kəto</i> "how much?"	<i>jəto</i>	<i>ətʰo</i>	<i>ətʰo</i>	<i>tətʰo</i>
<i>kəmon</i> "how?"	<i>jəmon</i>	<i>əmon</i>	<i>əmon</i>	<i>təmon</i>

The semantic value of the interrogative forms is clear enough from the label and the English glosses in the table. The relative and unmarked demonstrative co-occur in sentences corresponding to English sentences with relative clauses, with the unmarked demonstrative serving as a resumptive rather than an antecedent. Thus the normal Bengali equivalent of an English sentence like *The man who came here yesterday has come again* would be something like "Yesterday which man came, that-one again came" (*kal je lok esechilo se abar esece*). Frequently also, however, the unmarked demonstrative is used simply as a weak demonstrative which could be regarded as in some sense parallel in semantic value to the English definite article in comparison to the demonstrative (the : this : that).

Some members of the *ke, je* series serve both as pronouns and adjectives, but in their pronominal function they refer only to animates. The incomplete *ki, ja* series fills the pronominal function for inanimates, with the demonstrative gaps being filled by the *e o se* forms with appropriate definitive suffixes (e.g. *e-ṭa, o-khana*, etc. Cf. § 5.1 below).

3. *Person*. Bengali finite verb forms and personal pronouns mark a category of person, the verb by the use of personal endings, the pronoun by the use of different stems. In both verbs and pronouns an "honorific" morpheme (~ ∞ -n-) occurs in the system, yielding additional terms in the category. Also in SCB, as was suggested

⁵ A fuller listing of correlatives which includes alternate forms and marginal examples is given in the author's thesis previously cited, Ch. 9.

in the preceding section, the correlatives cross the personal pronouns, providing a kind of sub-category of deixis.

The verb marks four persons by different endings, which may be illustrated by present tense forms:

1st	boli	"I, we say"
2nd "inferior"	bolis	"you say"
2nd "ordinary"	bəlo	
3rd	bəle	"he, she says; they say"

In addition, *-n* may be added to the 3rd person form, yielding the "honorific" form (e.g. *bəlen*).

The personal pronoun in SCB distinguishes all the persons of the verb, with the addition of a separate stem for the 2nd person "honorific" and the three-way deictic distinction in the 3rd person ordinary and honorific, amounting to ten forms in all:

1	ami	"I"
2 i	tuī	"you"
2 o	tumi	
2 h	apni	
3 o	e o se	"he, she"
3 h	ini uni tini	

The full correspondence of the verb forms and pronouns form may be illustrated as follows:

1	ami boli
2 i	tuī bolis
2 o	tumi bəlo
3 o	e, o, se bəle
2 h	apni
3 h	ini, uni, tini } bəlen

The range of use of the various persons is shown fairly well by the traditional person labels and the English glosses, but the use of the different grades of the 2nd persons and 3rd persons deserves comment. There is considerable variation in usage reflecting in part individual and group usages and in part changes which are taking place in the whole system. The following summary of 2nd person usage on the part of many will give some indication of the possibilities:

- 2 i To junior siblings; to junior cousins of the same generation; between close friends when the friendship was established in childhood; by an old-fashioned high-caste person to persons of certain lowly occupations; to animals; sometimes in songs, poems, and prayers to lover, deity, or personification.
- 2 o To senior relatives; between friends; between husband and wife; to children; educated men to certain tradespeople.
- 2 h Between educated people not close friends; to people of high social status.

4. *Case*. SCB nouns and pronouns generally show four case forms: the direct case, which consists of the stem without case ending; the genitive and oblique cases, with the endings *-er* and *-e* respectively; and the objective, which is marked by a loosely attached suffix *-ke*. For example, the case forms of *ghor* "house" are: *ghor*; *ghorer*, *ghore*; *ghorke*. The direct case is the citation form of the noun and is used both as subject and object. The genitive occurs in a variety of adnominal uses (possession, material, subjective, objective, etc.), with postpositions and in other constructions. The oblique case has locative and instrumental uses and occurs in a double construction to mean "between" (e.g. *mae jhie* "between mother and daughter").

Interestingly, the locative and instrumental uses are not completely integrated into a single case, although a single oblique case was listed above for convenience. The oblique case ending is usually *-e* after consonants and monosyllables ending in a vowel and *-ě* after vowels otherwise. After vowels the *-e* or *-ě* varies freely with *-te*; after consonants *-ete* also occurs in place of *-e*, though less commonly than the *-te* after vowels. The point of interest is that the variants in *-te* and *-ete* generally have only locative value while the forms in *-e* or *-ě* have either locative or instrumental value.

The objective in *-ke* is a less clearcut case than the others, both because many nouns may be used in the direct case in objective constructions and because the *-ke* is more loosely attached than the other case endings, being added, for example, only to the final noun of a series. Except where *-ke* must be used, as with the first of a double object construction (e.g. "They call this *city* Calcutta"), its use tends to be limited to nouns and pronouns referring to persons and tends to imply a certain definiteness (see below § 5.2).

5. *Definiteness*. There is in SCB a set of morphemes, called "definitives" by S. K. Chatterji,⁶ which are roughly equivalent translationally – at least in some of their uses – to the English definite article *the*. When a noun, adjective, or demonstrative has one of the definitives suffixed to it, it is "definite", i.e. it is pointed out as having a referent "presumably identifiable by the hearer" in much the same way as a noun or adjective in English accompanied by *the*.⁷

Definiteness in SCB as shown by the presence of a definitive has one particularly striking characteristic: the signalling of definiteness entails simultaneously the signalling of class membership in a system of six singular and two plural classes which are shown by no other mechanism in the language. Whether the noun, adjective, or demonstrative is singular or plural and whether it is a *khan* type noun (e.g. table, book) or a *gach* type noun (e.g. rope, cane) or some other type is shown only by

⁶ Cf. S. K. Chatterji, *Origin and Development of the Bengali Language* (London, 1926), 777-81. See also his *Bāñlā bhāṣār vyākaraṇ*³, 215-9, and Page, *Introduction*, 115-17, Ferguson's thesis Ch. 7 § 3. None of these descriptions gives a fully satisfactory account of the definitive system of Bengali.

⁷ The phrase "presumably identifiable by the hearer" is taken from R. A. Close, *English as a Foreign Language* (London, 1962), Chapter "The Articles".

the definitives. There is no trace of a number category at all in verbs and there are no patterns of concord which otherwise signal this class membership of nouns.⁸

The Bengali definitives occur in other constructions, however, in which there is no semantic value of definiteness. Also, two other mechanisms are used to convey this value in constructions where the definitives either do not occur or appear without the value of definiteness. In this section (§ 5) the definitives and the constructions, definite or not, in which they occur will be identified and the other two mechanisms will be described.

5.1 SCB has eight definitive morphemes. These may occur in four different constructions. Only two of the eight, however, occur regularly in all four constructions. Five of the definitives have a distinction between augmentative and diminutive forms. The augmentatives are either neutral in tone or are used with reference to largish objects; the diminutives are used with reference to smallish objects or to objects (or persons) toward which the speaker has affectionate feelings.

The four constructions are listed here without details but enough is given to make the essentials of the system clear. The morphemes used in the examples are: *boi* "book(s)", *khan* a singular definitive, *khana* its augmentative form, *gulo* an augmentative plural definitive, *pāc* "five", *ek* ~ *æk* "one". Abbreviations are: N = noun, Df = definitive, No = number,⁹ - = suffixation.

A	The classifier construction.		
	No-Df N	<i>pāc-khana boi</i>	"five books"
B	The article construction.		
	N-Df	<i>boi-khana</i>	"the book"
		<i>boi-gulo</i>	"the books"
C	The approximative construction.		
	Df No- <i>ek</i> N	<i>khan pāc-ek boi</i>	"about five books"
D	The definite classifier construction.		
	N No-Df	<i>boi pāc-khana</i>	"the five books"

Of these four constructions it may be noted that only construction B admits of a plural definitive, and constructions A and B are much more frequent in occurrence than C and D. Also, A is the normal way of counting things in SCB: there is no other construction available to say "five books" while there are other constructions which could be translated about the same way as C (e.g. *praē pāc-khana boi* "about five books").

⁸ Number is obligatory in the pronouns of the first and second person, and in this instance plural is shown by the (obligatory) presence of *-ra*, the plural definitive referring to persons: *ami* "I" : *amra* "we"; *tui*, *tumi*, *apni* "you (sg)" : *tora*, *tomra*, *apnara* their respective plurals. The demonstrative pronouns (not adjectives) of the 3rd person also show plural by *-ra* (*era*, *ora*, *tara*; *ēra*, *ōra*, *tāra*).

⁹ "No" here refers to all cardinal numbers and several other items such as the interrogative *kō* "how many?", *kōto* "how much?".

TABLE III
Bengali Definitives

	Definitive		Class value	Construction				
	Unspec.	Augm.		Dim.	A	B	C	D
1. —		<i>ṭa</i>	<i>ṭuku (ṭu)</i>	“mass nouns”	(+)	+	-	-
2. —		<i>ṭa (ṭe, ṭo)</i>	<i>ṭi</i>	“count nouns”	+	+	-	+
3. <i>khan</i>		<i>khana</i>	<i>khani</i>	flat objects; objects made for human occupancy	+	+	+	+
4. <i>gach</i>		<i>gacha</i>	<i>gachi</i>	long, thin objects; bangles	+	+	+	+
5. <i>goṭa</i>		—	—	round objects	-	-	+	-
6. <i>jon</i>		—	—	persons	+	-	+	+
7. —		<i>gulo</i>	<i>guli</i>	things (pl.)	-	+	-	-
8. <i>ra (de)</i>		—	—	persons (pl.)	-	+	-	-

The eight definitives are listed in table III, which gives also some indication of the semantic value of the classes and shows the constructions in which each definitive occurs (+) or does not occur (-). Alternants are given in parentheses without any indication of the conditions of their occurrence.

The primary marker of definiteness is the use of definitive suffix in construction B. It should be noted that the definitive may occur with adjective plus noun e.g. *bhalo boi-khana* “the good book”, demonstrative plus noun e.g. *e boi-khana* “this book”, *e boi-gulo* “these books” or with a genitive *cheler boi-khana* “the boy’s book”, *cheler boi-gulo* “the boy’s books”. Further, the noun may be omitted in these subvarieties of construction B:

<i>bhalo-khana</i>	“the good one [= book]”
<i>e-khana</i>	“this one [= book]”
<i>e-gulo</i>	“these [= books]”
<i>cheler-khana</i>	“the boy’s [book]”
<i>cheler-gulo</i>	“the boy’s [books]”

It is worth noting that this construction offers a special kind of conciseness: *e-gulo* and *e-ra* both mean “these” but the former refers to things, the latter to people, and similarly with the other class differences in the definitives.

From the semantic value of the class “mass nouns” it would not be expected that *ṭa*, *ṭu(ku)* would occur in construction A. Combinations with *ek* “one”, however, do occur. For example, *ek-ṭu* is the regular equivalent of “some” (e.g. *ek-ṭu jāl* “some water”), and *ṇ-ek* (lit. “not-one”) means “much, a great deal”. The aug-

mentative *æk-ṭa* does not occur in this construction, but probably the expression *bṛo æk-ṭa* “quite a bit, often” is related to it.

5.2 The secondary mechanisms for indicating definiteness in SCB are (a) the postposing of the number with definitive, i.e. construction D, the definite classifier construction, and (b) the definite objective use of *-ke* mentioned in § 4.

The use of construction D may represent an avoidance of the repetition of definitives which would result from adding a definitive to the noun when there is already one present on the number, i.e. *{*pāc-khana boĩ-gulo*}; there would also be a conflict of singular and plural definitives.

It is also worth noting that informants report that the “definiteness” of the D construction is not quite so strong as that of construction B. On the other hand, D seems to parallel B quite exactly, being used for example with the definite antecedent to a relative clause, e.g.

<i>je boĩ-khana ami dekheci</i>	“the book which I saw”
<i>je boĩ pāc-khana ami dekheci</i>	“the five books which I saw” ¹⁰

Finally, the objective ending *-ke* may indicate definiteness with nouns referring to persons. It was noted above that *-jon* is not suffixed to nouns as a definitive (B), occurring only in constructions ACD. In the non-objective, “nominative” uses of a *jon*-type noun there is no way to indicate definiteness, but as a direct object a *jon* noun commonly has no objective ending when indefinite and *-ke* when definite. For those nouns referring to person which may take *ṭa* or *ṭi*, the singular definite objective may have either *-ṭake/-ṭike* or simply *-ke*. In the plural with *ra* the simple genitive in *der* (= *-ra + -(e)r*) is at least as common as the form in *-derke*. Thus the possibilities for *chele* “boy” are:

indefinite (sg/pl)	<i>chele</i>
def sg direct	<i>chele-ṭi</i>
def sg objective	<i>chele-ke</i> or <i>chele-ṭi-ke</i>
def pl direct	<i>chele-ra</i>
def pl objective	<i>chele-der</i> or <i>chele-der-ke</i>

One important feature of the SCB system of definitives not made clear in the listing of morphemes and constructions in § 5.1 is the overlapping of the noun classes marked by the definitives. To be sure, the singular suffixes (1-6) and the plural ones (7, 8) are mutually exclusive. Also, the line between mass nouns (1) and the various count nouns (2-6) is fairly firm. But there is a surprising freedom of selection among the count noun suffixes. For example, “five” may be translated as *pāc-ṭa*, *pāc-ṭi*, *pāc-khana*, or *pāc-khani* with *boĩ* and *pāc-ṭa*, *pāc-ṭi* or *pāc-jon* with *chele*, and so on.

Generally, *-ṭa* is the most widely used of these and may occur with almost any

¹⁰ It seems likely that the same category of definiteness is involved in B and D, and that D developed to round out the pattern after A and B were already in existence.

noun even though one of the others is actually more common with a particular noun. Also it may be noted that *-ʔi* is frequently used in construction B with nouns referring to persons; this is probably correlatable with the fact that *-jon* does not occur in this construction and otherwise there is no way of indicating definiteness apart from the objective.

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THE STANDARDIZATION OF WRITING

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Abstract

Standardization is a complex concept analyzable into three components – efficiency, rationality and commonality. Only the first component can be studied in abstraction from specific intellectual or historical situations and by methods of formal reasoning and experimental observation. This paper presents a point of view which is purely theoretic and only marginally relevant for practical application. It extends as well as summarizes what is on record about the limits and scopes of different types of writing, inclusive of questions concerning direction of arrangement, design of items, expansion of inventory, correlation to sound and to meaning in codification, spelling and punctuation. A set of six basic concepts is shown to suffice for describing and evaluating a representative selection of different empirically attested types. This is not graphemics in the sense of a technique for identification of unit signs, but may be described as an analog of structural phonology, with the difference that while questions of relative efficiency have been (except for Martinet's recent beginnings) rather unfruitful in phonology, they build a mainstay of analysis here. As an incidental result, many questions that have been considered within phonemics are taken out of it and found of easier approach from this new angle.

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WRITING, SPEECH, AND SOCIETY: SOME CHANGING INTERRELATIONSHIPS

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Historical linguists, linguists who have sought to reduce to writing the speech of preliterate groups, and those who have attempted to simplify and standardize the writing systems of developing nations all make certain assumptions concerning the interrelationships between writing and speech that are nowhere explicated. We briefly consider, without any claim for completeness, some of the significant patterns that linguists have overlooked.

Our hypothesis is that the relationships between writing and speech have, in an overall sense, varied historically according to the general socio-economic basis of the society. We distinguish three main types of society: preliterate, preindustrial civilized, and industrial.¹ Preliterate societies have no writing. Preindustrial civilized societies – e.g. traditional India, those in the Middle East, China, Japan, medieval Europe – have had writing systems but knowledge of these has until very recently been the province of a small, typically urban upper class. Industrial societies on the other hand are characterized by mass literacy.

We treat first the main facets of the tie between writing and speech in preindustrial civilized societies, after that some striking patterns in industrial orders that differ from those in preindustrial civilized societies, and finally the situation in transitional societies that are in the process of shifting from a preindustrial to an industrial base.

PREINDUSTRIAL CIVILIZED SOCIETIES

In the nonindustrial civilized society, various systems of writing have been employed in different cultures and in different eras. Nevertheless, some generalizations can be made. In this type of society writing has generally been restricted to a small, educated group. With the relative paucity of printing and duplicating devices the bulk of writings has been small. Much of the literature has consisted of the society's sacred writings: the scriptures and commentaries on these. For in the traditional society the body of writings, and indeed the script itself, are perpetuated by a priestly group which makes up a large portion of the literati. And because writing is the medium of the sacred literature,

¹ Gideon Sjoberg, *The Preindustrial City: Past and Present* (Glencoe, Ill., 1960).

the very script, typically complex, acquires a measure of sanctity; it is not to be shaped to utilitarian ends. The result is that it has been quite resistant to formal change.

The traditional writings have provided the basis for standardizing thought and action for the literati – who are the leaders in the society – over time and space. In China with its diverse dialects and several different languages, the elite in all parts of the country have long been united by their knowledge of the written language. Yuen Ren Chao remarks that even scholars from areas that employed different pitch patterns could understand one another if reference were made to the dictionary form.² In India with its many diverse languages and several language families it was the Sanskrit writings that kept north and south more or less culturally united over many centuries. And in the Arab world educated persons from a variety of nations have, through the written language and the particular spoken form that corresponds to it, been able to communicate with one another, whereas uneducated persons from different parts of the area usually could not, the various nonstandard speech varieties having evolved each in its own way.

Given the reverence attached to the traditional writings, it is not surprising that the speech form with the highest societal prestige has been that most closely approximating the written form. But generally only a small upper class has had the means by which to attain this ideal. Thus it is the educated elite that, in formal situations of various kinds, speaks in the traditionally high-status manner. In other situations, especially to communicate effectively with less educated or uneducated persons, they must use a more informal speech style, one that is more akin to the colloquials of the area. The result is that the upper status, educated group typically employs at least two speech styles, in some cases more. And all of these differ from the speech of the common man – in the lexicon and often the phonology and grammar. Numerous examples could be cited, including Telugu,³ Tamil, Bengali, Japanese, Korean, Persian, Arabic, Uzbek, German, and Greek.

The formal speech style, moreover, tends to be perpetuated over centuries with relatively little change, a phenomenon that results from the high prestige accorded it and its close tie with the written language.

INDUSTRIAL SOCIETIES

Turning to the industrial society, we discover that the relationship between writing and speech diverges fundamentally from that in the traditional civilized order. Why should this be so? The spread of industrialization with its scientifically based technology has been related to efforts to maximize communication within and among the modern

² Yuen Ren Chao, "What is Correct Chinese?", *Journal of the American Oriental Society*, 81 (1961), pp. 171-77.

³ Andrée F. Sjoberg, "Coexistent Phonemic Systems in Telugu: A Socio-cultural Perspective", *Word*, 18, no. 3 (1962).

society's complex organizations. For science can not progress without rapid and efficient communication, nor can technological data readily be diffused if the ordinary man is unable to read and write.

In turn the rise of mass literacy, and the proliferation of science and technology, have also brought about a democratization of the class structure. No longer are there well-defined social strata; in particular the educated elite tends to be ill-defined. And no longer is it clear who sets the standards for either the written or the spoken forms of the language. Associated with all of these changes has been a "secularization" of traditional religious systems. In most industrial societies very few writings are still considered sacred. And it is likely, for example, that the recent translation of the Bible in a form that more closely approximates colloquial English will accentuate this process.

So too, in other literature – novels, for instance – writers are increasingly employing the vernacular. Modern novelists, in contrast to those of the past, are generally more apt to use grammatical forms that a few generations ago would have been identified with a particular region or with lower status groups. Certain traditionally substandard forms have as a result been dignified by this usage.

All of these trends have forged firmer bonds between the spoken and written forms of the language. It is becoming increasingly difficult to separate educated from non-educated speech (although this distinction has not entirely disappeared).

The falling together of speech and writing in industrialized societies has of course engendered controversies. One that is raging at the present time in the United States concerns the recently published third edition of Webster's dictionary.⁴ This may be an extreme instance; yet it highlights the patterns occurring in one way or another in most industrial societies.

The traditionalists see this dictionary as leading to a "corruption" or dissolution of standards, for it includes many words that other dictionaries have rejected, yet does not clearly mark what is standard, colloquial, substandard, etc. English teachers, particularly, find increasingly fewer ideal standards to which to refer students in the matter of acceptable usage, especially for writing.⁵

The structural linguists who propound the view that "language is as it is spoken", that nothing in language is intrinsically right or wrong, are viewed by some as the instigators of this process. Yet these linguists are simply reflecting the ideology and meeting the demands of an industrial, democratic society. For in a democracy who has the ultimate right to impose his will on others in the matter of language? Although we can sympathize with persons who argue that certain kinds of words in the new edition of Webster's should have been labeled substandard, what really are the standards today? The second edition of this dictionary, which appeared in 1934, is hardly a valid guide. So many changes have occurred in the interim. Only when a small intellectual group (usually coterminous with the upper class) dominates the social order – as was the case

⁴ *Webster's Third New International Dictionary* (Springfield, Mass., 1961).

⁵ Mario Pei, "The Dictionary as a Battlefield", *Saturday Review*, July 21, 1962, pp. 44-46, 55ff.; "Letters to the Editor", *Saturday Review*, August 18, 1962, pp. 61-62.

in traditional societies – can there be consensus as to what is right or wrong in speech and in writing.

Implied in the above is the fact that highly innovating industrial orders seem to require a language whose vocabulary, and perhaps even grammar, can adjust to the continuing social, political, and technological changes. At the same time, contrary pressures are at work; science, for example, must have a standardized vocabulary that the bulk of its practitioners can agree upon. Then too, some aesthetic norms attach to writing and speech; probably there will always be the more desirable and the less desirable varieties of each.

TRANSITIONAL SOCIETIES

Having sketched some of the relationships between speech and writing in industrial societies, we can turn to transitional ones – those now seeking to industrialize. These include what were until recently either preliterate or preindustrial civilized orders.

In preliterate orders more and more languages are being reduced to writing, and existing systems introduced in the past century or so improved, directly or indirectly through the efforts of scholars trained in industrial societies.⁶ The pattern in these transitional societies differs from that in the past when preliterate social orders were incorporated into preindustrial civilized ones – as was the case, for example, with the Turkic peoples of Central Asia, who came to be culturally and politically dominated by Arabs and Persians. The ruling group made little or no effort to devise new writing systems; they tended to impose their own on the languages of the subject peoples, frequently with few adaptations to meet the special problems involved.

But we focus attention primarily on transitional societies that are emerging out of preindustrial civilized traditions. One of the major changes taking place is reform or revision of traditionally cumbersome writing systems. Whereas in the past the society's leaders had little interest in making the written language easier for the common man to learn and use, today the emphasis is quite different. For industrialization demands mass education, and to achieve this with some degree of efficiency, the older writing systems must be simplified and, moreover, made the medium of the prevailing colloquial speech. Also, to make effective use of typewriters and to speed typesetting, and in general to increase the flow of technical and other vital information, the writing system often must be overhauled. The logographic system for Chinese, the vast number of symbols employed in the many scripts of India, the Arabic writing used over a wide area, and so on, are significant barriers to industrialization and the spread of mass literacy.

Granted the existence of significant cultural differences among these transitional societies, the various efforts to cope with the problems of language and script display certain recurring patterns.

⁶ E.g. see: J. Jacobs, "Principes généraux de la nouvelle orthographe otetela-kikusu", *Kongo-Overzee*, 25 (1959), pp. 145-60.

In some societies attempts have been made to introduce an entirely new writing system. Although in China the officials have blown hot and cold on the subject, the trend seems to be toward some kind of alphabetic writing system for general use. In Turkey some decades ago the Arabic script was replaced by the Roman. And in Soviet Central Asia the traditional Arabic writing system, used for the Turkic languages until the late 1920's, was first replaced by a latinized alphabet and then by the Cyrillic. In all these instances political factors, as well as the demands of modern technology, have been crucial in the introduction of new kinds of scripts.

The second pattern involves modification of the often complex traditional writing systems in an attempt to adjust them to the need for efficiency and broader communication. In both Japan and China many of the most common characters have been simplified.⁷ In the Arab world the proposals are mostly still in the talking stage.⁸ One important proposal is the "unified" system for Arabic suggested by Nasri Khattar: here each letter maintains a given shape regardless of its position in a word. At the present time some Arabic letters have as many as four different variants, depending upon whether the letter stands alone or at the beginning, the end, or the middle of a word. Another proposal, recently adopted in Morocco, adds special vowel signs to the ordinarily consonantal script. But both of these systems would reduce the number of symbols required to about one-tenth of the present number.

In India the need for simplification of the numerous semi-syllabic scripts has been urgent.⁹ Each of these writing systems employs several hundred ligatures – and these combinations of letters are often quite different from each of the components occurring separately. In the Telugu area the proposals call not only for greater ease in printing but also for bringing the writing system more in line with the spoken word.¹⁰ The idea is to use similar signs for similar sounds. To cite one example: *g* and *gh* would be written with the same basic sign, with an additional mark for the aspirated member of the pair (గ గి), instead of with two fundamentally different signs as is now the case (గ ఘ). There are suggestions also to raise the numerous subscribed letters up to the main line of writing, for these subscripts break off easily and generally slow the printing process as well as readability.

In most societies, however, the efforts to revise the scripts have met with firm resistance from many quarters. A common fear is that the new nation will be cut off from its cultural heritage. Often the political leaders recognize that such would reduce the

⁷ Florence Sakade *et al.* (eds.), *A Guide to Reading & Writing Japanese* (Rutland, Vt., 1959), p. 8; *New York Times*, July 16, 1959, p. 3, col. 8.

⁸ Salih J. Al-Toma, "The Arabic Writing System and Proposals for its Reform", *The Middle East Journal*, Autumn, 1961, pp. 409-11.

⁹ E.g. see: Norman Brown, "Script Reform in Modern India, Pakistan, and Ceylon", *Journal of the American Oriental Society*, 73 (1953), pp. 1-6.

¹⁰ Government of Andhra Pradesh, India, Education Department, *Final Report of Telugu Script Reforms Committee*, Hyderabad, *The Andhra Pradesh Gazette*, Part 1 – *Extraordinary*, no. 194 (December 11, 1961) (in Telugu); Veturi Prabhakara Sastri, *Telugu Merugulu* (Hyderabad, 1960), Chapter 15 (in Telugu).

nationalistic fervor that seems to be so necessary if unity is to be achieved and industrialization advanced. The Pakistan Ministry of Education recently observed that any radical change in the Perso-Arabic script now in use would “sever our links with the past”. And it gives as one of the reasons for retaining this script, rather than replacing it by the Roman, the aesthetic one: it is far more pleasing to the eye.¹¹ Similar arguments have been adduced against those who want a changeover to a new kind of script for Chinese; abandonment of the traditional characters in favor of a radically different system like an alphabet would in just a few generations render the Chinese Classics unintelligible – unless they were soon translated into the new medium. This, of course, fits in with the goals of the present-day leadership, though national unity could be undermined thereby.

The third main type of linguistic reform in modernizing societies, as suggested by our discussion of industrial societies, involves elevating the status of the average person’s mode of speaking and bringing the writing system into closer accord with this. Here the conflict between the traditionalists and more democratically oriented writers is more acute than any in the industrial order. Yet, as the class barriers crumble and the traditional upper class relinquishes much of its former influence in matters of language, the speech of the common man will concomitantly gain in prestige.

CONCLUSION

Although we do not deny the effects of cultural differences among societies, we argue that the relationships between writing and speech vary, in a general sense, according to the basic type of socio-economic system, broadly conceived. This perspective has important implications for historical linguistics (including glottochronology) and to some extent for other areas of linguistics as well.

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DISCUSSION

SJOBERG:

I strongly disagree with Gleason’s statement just now that the Dēvanāgarī script used in writing Sanskrit is or was “perfectly phonemic”. For one thing, there is a symbol transliterated *ī* that, as far as we know, did not stand for a sound or group of sounds, but was included merely to fill in a gap in the pattern. Some other letters do not represent phonemes at all, but rather phones or morphophonemes. Furthermore, I cannot agree with Gleason’s assumption that the language described by Pāṇini was

¹¹ Government of Pakistan, Ministry of Education, *Report of the Commission on National Education* (Karachi, 1959), pp. 301-11.

actually the prevailing spoken language of that time. At most, it was the spoken language of a small educated elite. Although we cannot know absolutely, we can assume that like most grammarians Pāṇini was probably concerned much more with describing “what should be” (or perhaps “what once was”) than with demonstrating “what is”. And to judge from all other ancient societies, and especially from what we know about the history of India, it is doubtful that the ancient Sanskrit grammarians’ methods were really “scientific”. Even today, Indian scholars have an aversion to studying the actual spoken forms of a language.

SEGMENTATION OF NATURAL SPEECH INTO SYLLABLES BY ACOUSTIC-PHONETIC MEANS

ARTHUR N. STOWE

Abstract

Digital computers have been used to simulate a system for segmenting natural speech into syllable-like units. Two gross measurements, one of the time vs. amplitude waveform and one of the power spectrum, are the primary features of the speech signal used to make segmentation decisions. The former is the amplitude envelope of the waveform. It is used to make approximate speech vs. silence detections, and to mark negative to positive amplitude slope change points. The latter measurement is the amplitude envelope of the 1,000 cps low-pass filtered waveform, used to make approximate voiced-unvoiced detections. The occurrence of these three features in succession and combination determines the times in the speech signal at which syllable boundary points occur.

Finer spectrum measurements are useful in two ways. First, they provide more accurate voiced-unvoiced and speech-silence detections. Second, they can provide information, such as manner of articulation, which can correct wrong decisions made with the gross measurements alone.

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DISCUSSION

ENKVIST:

If the results of a search for acoustic syllable cues are to have linguistic validity, the search should start out from a linguistic definition of syllables. Only when segments have been defined linguistically can we proceed to match acoustic cues with the segments. The use of the term "syllable" both for an acoustic and for a linguistic segment may lead to confusion.

PHONETICS, PHONEMICS, AND METAPHONEMICS

HERBERT PILCH

In linguistic writings we often read statements to the effect that some element is “phonetically *p*, but phonemically *q*”, or that it is there phonetically, but not phonemically.¹ One is inclined to wonder strongly at the precise meaning of such statements. Apparently the terms *phonetic* and *phonemic* refer to two different kinds of descriptive framework. These are commonly distinguished by such labels as factual vs. functional, comprehensive vs. distinctive, physical vs. linguistic, or individual performance vs. social norm. Beyond this, the dichotomy is today largely taken for granted. Earlier it was articulated by the Prague School to the Saussurean distinction of *langue* and *parole* and even to the (very dubious) opposition of science (Naturwissenschaft) vs. the humanities (Geisteswissenschaft). Phonetics and phonemics were to be two different sciences, each having its own methods and objects of investigation.

The contradictions implicit in this version of the dichotomy were exposed long ago by E. Zwirner.² But Zwirner’s impact was small, and the fashion was already well established for splitting up phonetics into a whole host of different so-called “phonetic sciences” – each with its own objects and methods of study. Some of the better known specimens are phonometry, experimental phonetics, and laletics. A recent author³ even sets up three different phonemic disciplines: general phonemics, theoretical phonemics, and descriptive phonemics.

Another breakdown is

phonetics as the study of speech sounds, and

phonemics as the linguistic (functional) classification of speech sounds (into phonemes).

The assumption is that speech sounds are immediately accessible to observation (either by ear or by measuring instruments), while phonemes are abstractions such as classes of sounds, bundles of distinctive features, terms of an opposition, or hypothetical constructs. This boils down to the traditional positivistic distinction between FACTS (speech sounds) on the one hand and the INTERPRETATION of those facts (phonemic analysis) on the other. The facts remain, even though the interpretations may

¹ This paper owes much to preliminary discussion with Göran Hammarström and H. M. Truby.

² “Phonologie und Phonetik”, *Acta linguistica*, 1 (Copenhagen, 1939), 29-47.

³ S. K. Šaumjan, *Problemy teoretičeskoj fonologii* (Moscow, 1962), 3 f.

vary or, as we say technically, be non-unique. Charles Hockett's suggestive metaphor about "hugging the phonetic ground closely" is a remonstrance not to let the (phonemic) interpretation wander too far away from the (phonetic) facts.

This point of view has also served as a justification for some more conservative linguists to hug the phonetic ground even more closely and reject phonemics altogether as basically superfluous: "La tâche essentielle du philologue", writes Félix Lecoy,⁴ "est d'établir les faits . . . l'interprétation de ces faits n'est pour lui qu'une tâche, sinon secondaire, du moins seconde."

The view that there are, on the one hand, "the phonetic facts" (existing independently of any interpretation) and, on the other hand, "phonemic solutions" (which add theories to these facts) seems, however, to be based on a fallacy. Following a current fashion of literary criticism, we may dub it "the phonetic fallacy". The speech which we hear or measure in the laboratory does NOT "naturally" present itself to our observation as a sequence of discrete segments (speech sounds), but as a physical continuum. Interpreting an X-ray film or a spectrogram of speech means assigning parts of this continuum to (previously known) linguistic segments. The linguistic segments or speech sounds themselves are not "physically given", but are established through prior LINGUISTIC analysis.

In order to account for speech both as a physical continuum and as a sequence of discrete elements, we choose different types of descriptive framework. One type of descriptive model has reference to events of either indefinite or very brief duration. In mathematical terms, we can treat them as events of the duration Δt . The speech continuum, then, consists of an infinite number of such events in succession. These models we call "phonetic models" in the narrow sense.

Like all other audible phenomena, speech may be investigated as to the way it is generated, perceived, and transmitted. We accordingly work with three different kinds of phonetic model: articulatory, auditory, and acoustic phonetics. It is important to remember that in these models we specify NOT speech sounds, but shapes of the vocal tract, auditory responses, and compositions of sound waves – all of them of the duration Δt .

In the "phonemic models" we operate on DISCRETE elements, each of them lasting a finite length of time. Any utterance is said to consist of a limited number of discrete units (phonemes) in a given order. Phonemic models are necessary in order to account for the observation that people COMMUNICATE through speech. This could not be done, if utterances were analyzed solely as an infinite rather than a finite series of events.

The fundamental assumptions of phonemic theory are:

- 1) that each utterance may be specified as a finite set of discrete elements (such as phonemes) occurring in succession and, in some cases, simultaneously, and
- 2) that each language comprises only a limited stock of DIFFERENT phonemes.

⁴ *Romania*, 81 (1961), 398-402, quoted by G. Hammarström, *Romania*, 82 (1961), 131.

Phonemic sameness and difference are defined by special conditions. Same phonemes or, more precisely perhaps, specimens of the same phoneme must

- 1) occur in the same language or dialect,
- 2) they must be in complementary distribution or in free variation,
- 3) they must be phonetically similar. This means that, within the language concerned, they have a unique phonetic specification (a unique set of distinctive features).

It is implicit in this last condition that phonemic analysis, for its concept of identity, depends on data from the phonetic models. The task of the phonetic models is precisely to supply such data. We do not design them JUST for the fun of classifying all possible vocal tract formations or sound waves.

I should therefore suggest that the phonetics/phonemics dichotomy should not be looked on as a dichotomy at all, but that there is one single phonetic science – phonetics – which deals with speech as an audible phenomenon in its different aspects. In its framework the speech sound – the central unit of classical phonetics – belongs, if anywhere, to the PHONEMIC models. The dichotomy is probably to be understood historically as an attempt on the part of early phonemicists to explain how their work went beyond contemporary phonetics. Therefore they claimed only the phoneme to themselves, leaving the speech sound with the established part of the science.

Now there is a basic antinomy between the phonetic and phonemic models. On the one hand, we assume that each phoneme may be uniquely specified through a certain set of phonetic features (the distinctive features). The phonetic models, on the other hand, do not yield any discrete entities such as sounds or phonemes at all. For them any stretch of speech extending over a measurable length of time consists of an infinite series of identifiable events with as many different phonetic specifications. In phonemic theory, some of these stretches are said to possess constant and common phonetic features.

In order to overcome this antinomy, we propose to recognize in our theory a unit to be called the METAPHONEME. The metaphoneme is, first, a noise by which we name a phoneme or, more precisely, by which we re-produce it. Many speakers, especially linguists, are able to pronounce any given phoneme in isolation. We talk of such things as “the English phonemes /a/ or /i/ or /t/ or /d/”. These linguistic signs /a, i, t, d/ may appropriately be called “metaphonemes” as they belong to a meta-language and designate phonemes of the object language English.

A metaphoneme should fulfil two specifications:

- 1) It is an audible element of measurable length and with definite phonetic characteristics.
- 2) There is a one-to-one relationship between each metaphoneme and some phoneme of the object language concerned.

Two further assumptions seem to be called for, if we wish to incorporate this concept into phonemic theory:

- 1) The constant physical features of each phoneme are the same as of the corresponding metaphoneme.

2) When phonemes occur in connected speech, their physical characteristics are MODIFIED under the influence of the surrounding phonemes. This modification is a function of time. It varies at each single instance of the speech chain, thereby making it into a constantly changing physical continuum.

These assumptions underlie, as far as I can see, the usual interpretation of sound spectrograms. Reading a sonagram (say) of the English word *peep* /pip/, we start, for instance, from the metaphoneme /i/. We then find that the second formant of the vowel in *peep* is lower than in the metaphoneme /i/, and that its frequency varies in time. We attribute the lowering and the formant bendings to the low-pitched loci of the preceding and following /p/'s which, we say, affect the constant physical characteristics of /i/ to various degrees at different instances.

In an X-ray film of the English word *plotch* recently produced and shown at this Congress by H. M. Truby we may observe that the tongue is already in a lateral position during the occlusion of the initial stop. In this instance, the lateral feature of the following /l/ is superimposed on the constant physical features of /p/. We say that the two phonemes are CO-ARTICULATED. The co-articulation extends even beyond the /l/ to the following /a/. While the /p/ is released, the pharynx is already in the contracted position characteristic of /a/.

The modification of phonemes by their environments and the co-articulation of successive phonemes must be taken for granted as normal in any utterance. This means that although phonemes do occur in a time sequence, there are no fixed boundaries between ADJACENT phonemes.

Modification by surrounding phonemes yields different ALLOPHONES of a phoneme. It follows that each phoneme has at least as many allophones as it has different environments (it has usually many more). Co-articulation is connected with what Charles Hockett has called "the re-drawing of boundaries".⁵ It implies that some of the peculiarities of a given allophone belong to an adjacent phoneme. As M. Joos has shown,⁶ the modification of phonemes by their neighbours in the spoken chain is NOT due simply to mechanical inertia. We should therefore not anticipate a single mathematical formula covering all such modifications. In fact, they vary from language to language just as much as allophones do.

One of the correctives to mechanical inertia must be in the fact that communication requires (at least two) distinct signals and that the number of such signals in the code is, on principle, inversely proportional to the length of the messages. The Finnish language, for instance – with a relatively small stock of phonemes and phoneme clusters – has longer words of more syllables than English or German. This is a point where synchronic analysis has an immediate bearing on diachronic studies.⁷

With this approach to phonetic analysis we hope to fit phonemic theory to the re-

⁵ *A Manual of Phonology* (= *IJAL Memoirs*, 11) (Baltimore, 1955), 156 f.

⁶ *Acoustic Phonetics* (Baltimore, 1948), 106 f.

⁷ Cf. the analysis of the causes of sound change given by A. Martinet, *Eléments de linguistique générale* (Paris, 1960).

sults of current experimental phonetics. Phonemics is still largely based on the conventional "sound-and-glide" theory, that is, on the view that speech physically and/or perceptually consists of a sequence of discrete sounds linked by glides. The inadequacy of this "classical" phonetic model has been demonstrated by such eminent phoneticians as Menzerath and Lacerda, Eberhard Zwirner, Martin Joos, and H. M. Truby. The metaphoneme is, in a sense, a revised version of the steady-state sound and of H. M. Truby's PHYSICAL PHONEME.⁸ Some remoter analogues would be the Zwirners' *linguistic norm*,⁹ M. Joos's *neureme*,¹⁰ and S. K. Šaumjan's *abstract phoneme*.¹¹ The metaphoneme is, however, physically present in a metalanguage and hypothetically present in an object language. As such it is used to interpret experimental data in linguistic terms.

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DISCUSSION

FRANCESCATO:

In Mr. Pilch's theory, phonemes seem to be identified with metaphonemes.

PILCH:

Each one of us *uses* metaphonemes, I think, for instance in teaching students and making statements of the form: "Language L comprises the vowel phonemes /a, i, u/, *a* is an allophone of the phoneme *x*" etc. Given the appropriate definitions, the term *x* could perhaps, even in this context, be said to be an allophone of the phoneme /*x*/. It would, however, be a very special allophone, namely the *metalinguistic* form used to designate the phoneme /*x*/. There is, also an analogy between metaphonemes (used to designate phonemes) and city names used metalinguistically, for instance, in discussing place names: "The name *Boston* occurs both in England and in the United States". The analogy disappears as soon as we discuss not the *linguistic sign Boston* as occurring in the object language English, but the City of Boston, for instance: "Boston is in Massachusetts."

⁸ "Acoustico-cineradiographic analysis considerations", *Acta radiologica*, supplementum 182 (Stockholm, 1959), 128.

⁹ E. & K. Zwirner, *Grundfragen der Phonometrie* (Berlin, 1936).

¹⁰ *Acoustic Phonetics*, Ch. 6.

¹¹ *Problemy teoretičeskoj fonologii*, 38-47.

LINGUISTIC STATEMENT AND LANGUAGE TEACHING

ALBERT VALDMAN

For the last two decades structural linguists have been increasingly concerned with problems of language instruction. The totality of the linguist's language teaching activities has come to be known as *applied linguistics* but since the participation of linguists in foreign language (FL) teaching has been very diverse indeed, it is useful to differentiate clearly the role of the linguist qua linguist, role in which he speaks as it were ex cathedra, and his role as FL teacher in which he merely offers scientifically unproven assertions about the process of FL learning. Applied linguistics, then, deals neither with the fabrication of consumer products for the FL classroom or language laboratory nor the contrastive analysis of two languages outside of the context of FL pedagogy: it is concerned more properly with the evaluation of linguistic analyses and their conversion to statements suitable for the ultimate preparation of teaching material or for direct pedagogical application. The pedagogical grammar that the applied linguist produces must be incorporated subsequently as unobtrusively as possible in teaching material according to the dictates of specific teaching situations.

The present paper examines some problems encountered in the conversion of linguistic analyses to pedagogically useful statements and attempts to provide evaluating criteria for the determination of pedagogical simplicity and economy within the frame of reference of the teaching of French to English speaking adolescents and young adults.

Let us first consider the teaching of the oral vowels of Standard French and the corollary problem of the elaboration of a respelling. The raw material of the applied linguist is the most exhaustive description available of the relevant facts. Fig. 1 illustrates the distinctive feature relations that obtain among the oral vowels: observe that they constitute a 3 + 3 + 3 + 2 four-tongue height subsystem; in addition, high and high-mid vowels are tense and high-mid rounded vowels are long particularly in final syllables of CVC structure. There is considerable instability at the three lowest tongue heights: as shown by Table I high-mid and low-mid vowels are in near complementary distribution in utterance-final position, their opposition is well nigh neutralized in other positions, and Standard speakers exhibit extreme diversity in the distribution of the front unrounded pair /é/ versus /è/ in individual morphemes, for example, "milk" may be /lé/ or /lè/ and "I'll give" may be /donré/ or /donrè/; the two low vowels are in free variation even in final position and putative minimal pairs like

TABLE I
Distribution of French Mid-vowels in Phrase-Final Position

Environment		Vowels					
		é	è	ò	ó	œ	õe
-#	-#	poignée	poignet		peau		peu
ž			aurais-je	loge	auge		(Maubeuge)*
t			sept	hotte	hôte		meute
z			pèse		pause		creuse
d			raide	rode	rôde		(Eudes)
l			sel	sol	saule	veulent	(veule)
n			benne	bonne	Beaune	jeune	(jeune)
f			chef	étouffe	sauf	bœuf	
v			lève	love	mauve	peuvent	
r			serre	sort		sœur	
j			oreille			feuille	
p			guêpe	choppe	taupe		
b			plèbe	robe	aube		
k			sec	roc	rauque		
š			pêche	poche	embauche		
m			aime	homme	heaume		
g			bègue	vogue			
ñ			règne	grogne			

* Items in parentheses are marginal.

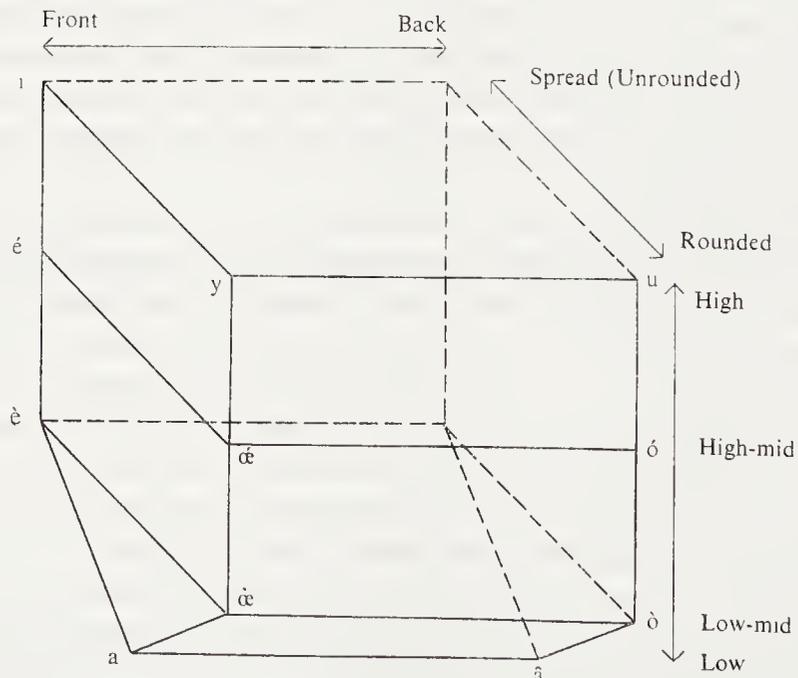


Fig. 1. French Oral Vowel System

pâte "dough" versus *patte* "paw" are maintained consistently by most speakers in formal style only.¹

The traditional analysis of the French oral vowels, first established by IPA-oriented articulatory phoneticians and adhered to by structuralists who follow literally the canon "once a phoneme, always a phoneme", assumes eleven oral vowels plus the problematic schwa.² This system, best represented by the vowel quadrilateral (see Fig. 2), is generally defined in terms of articulatory features and does not take into

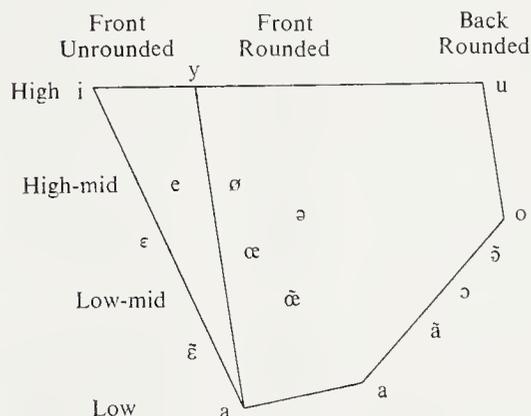


Fig. 2. Traditional French Vowel Quadrilateral

account explicitly distributional and functional load facts. The simplest yet exhaustive and non-contradictory analysis of the French oral vowel system recognizes contrastive pairs of mid and low vowel correlates (*/é/* versus */è/*, */cé/* versus */œ/*, */ó/* versus */ò/*, */a/* versus */â/*) only in positions of latent contrast and a single *archiphoneme* in environments where the contrast is suspended (Martinet). A reduction of the IPA inventory may be achieved also by considering one of the members of each pair as *marked* and decomposing it into a *hypophoneme* plus an *additive component* of tension and/or lengthening or backing; the non-marked member is then considered to manifest the hypophoneme but the additive component must be noted whenever the phonetic features it subsumes are realized even in environments where there is no opposition between the correlate pairs.³ The traditional, archiphoneme, and hypophoneme solutions are compared in Table II.

While a pedagogically oriented analysis must be complete and non-contradictory in the sense that it should account for all oppositions which normally keep linguistic signs apart and should reflect realistically native speakers' phonic habits, it need not

¹ Compare statements in distribution of contrast */a/* vs. */ɑ/* in A. Martinet, *La prononciation du français contemporain* (Paris, Droz, 1945), p. 22, and R. Reichstein, "Études des variations sociales et géographiques des faits linguistiques", *Word*, 16 (1960), 55-99.

² See, for example, P. Passy, *Les sons du français* (Paris, Didier, 1932); R. A. Hall, Jr, *French* (*Language Monographs* No. 24) (Baltimore, Linguistic Society of America, 1949).

³ The concept of the hypophoneme has been introduced by R. A. Hall, Jr. "Italian [z] and the Converse of the Archiphoneme", *Lingua*, 9 (1960), 194-197. The application of his system, as I understand it, to French is mine.

TABLE II

Conventional Spelling	IPA	"Pure" Archiphoneme	Hypophoneme	Modified Archiphoneme
saute	so:t	sót	so [^] t	sót
sotte	sət	sòt	sot	sòt
sort	sɔ:r	sor	sor	sòr
auto	oto	oto	oto [^]	otó
jeûne	ʒø:n	žæ̃n	žæ [^] n	žæ̃n
jeune	ʒœn	žæ̃n	žœn	žæ̃n
jeu	ʒø	žæ	žæ [^]	žæ̃
heureuse	œrø:z	œrœz	œrœ [^] z	œrœz
salé	sale	salé	sale [^]	salé
salait	salɛ	salè	sale	salè
c'est laid	sɛlɛ	sele	sele	selè
éclair	eklɛ:r	ekler	ekler	eklèr
pâte	pa:t	pât	pa [^] t	pât
patte	pat	pat	pat	pat
pâtissier	patisje	patisjé	patisje [^]	patisjé

meet the linguist's canons of simplicity, elegance, or economy. Indeed, the latter may and should be overruled by principles of pedagogical efficiency and utility which require, among other things, that drill material:

- (1) proceed from the simple and the known to the more complex and the unknown;
- (2) illustrate frequent and analogy-inducing patterns first;
- (3) be organized in a graduated series of cumulative minimal steps.

In handbooks of French pronunciation and many present-day textbooks which profess a structural orientation, the French oral vowels are presented in terms of vertical series: first the front unrounded vowels /i é è a/, then the back rounded series /u ó ò â/, and finally the front rounded series /y é œ/. This sequence of presentation violates all three pedagogical principles listed above and fails to distinguish between primary contrasts – those that obtain in all environments, in all styles, and for all speakers – and contrasts subject to neutralization, distributional limitations, and stylistic or "free" variation. In addition, the IPA or "once a phoneme, always a phoneme" transcriptions which rest on the traditional analysis are both clumsy and arbitrary since they cannot be "read", i.e., converted to sound, without an intricate system of phonetic rules nor "written", i.e., encoded from acoustic information, without lists of normalizations.

Both the archiphoneme and hypophoneme analyses are more amenable to pedagogical restatement. Since they presuppose a hierarchy of phonemic oppositions they suggest the abstraction of a *minimum* system of oral vowels for initial presentation consisting of the primary contrasts – the three high vowels /i y u/ and the four archiphonemes /e œ o a/ – and then of a *maximum* system consisting of the three high

vowels and eight mid and low vowels. In the elaboration of a pedagogically oriented re-spelling it would be most practical to modify the archiphonemic transcription illustrated in Table II in the direction of redundancy: high-mid versus low-mid and front versus back and/or long distinctions would be indicated in all final syllables including positions where contrast is not possible. Note that this modified archiphonemic transcription is more easily convertible to the conventional spelling than IPA notation since *é* often represents /*é*/, *è* often represents /*è*/, *o* often represents the archiphoneme /*o*/ and *eu* generally represents the archiphoneme /*œ*/ as well as the phonemes /*œ̃*/ and /*œ̂*/.

But the choice of the modified archiphonemic analysis is motivated by more important and higher-level considerations. It has become a hallowed tradition in the initial stages of a FL course to present first the entire phonology of the language without reference to grammatical patterns although connected discourse, usually in the form of dialogues, is specifically included. This procedure, much as it appeals to the analytical prejudices of linguists, cannot lead effectively the learner to a near-native control of the sound perception and production habits of the FL; nor is the presentation of such phonological stretches as *Je suis heureux de faire votre connaissance, Mademoiselle* /*ʒæsyzærcédfèr votrœkonesãs madmwazèl*/ compatible with a gradual, minimal-step procedure.⁴ For some researchers, including the present speaker, involved in the elaboration of self-instructional programmed material, the order of introduction of phonological features is determined by their relative importance in the realization of grammatical signals. For example, the sequence of presentation; (1) /*a*/, (2) /*e*/, (3) /*œ*/, though it is completely unmotivated from a phonological point of view is pedagogically efficient since it permits the opposition of gender and number markers in nominal phrases, for example:

la file	“the line”	vs.	le fil	“the thread”
la file	“the line”	vs.	les files	“the lines”
le fil	“the thread”	vs.	les fils	“the threads”

etc.; and the commutation of contrastive sets of noun markers:

la fille	ma fille	ta fille	sa fille
les filles	mes filles	tes filles	ses filles

French verb morphology presents English speaking students with many learning problems since the simplest French verb paradigm contains more than four times the number of forms found in weak English verbs. In linguistic descriptions most available to the classroom teacher,⁵ French verbs are classified according to two intersecting sets of criteria: (1) the number and distribution of themes (present stems) and

⁴ *Basic French* (Foreign Service Institute, Department of State) (Washington, U.S. Government Printing Office, 1960), p. 1.

⁵ Particularly, R. L. Politzer, *Teaching French: An Introduction to Applied Linguistics* (New York, Ginn, 1960) and A. Valdman, *Applied Linguistics – French: A Guide for Teachers*, S. Belasco, ed. Boston, D. C. Heath, 1961).

TABLE III
Traditional Structural Classification of French Verbs

Class	Theme	Past Participle	Infinitive/ Future Stem
Monophematic I	dòn-	doné	doné/donEr-
Double Theme I Vowel Change	mÈn-	mEné	mEné/ mènEr-
Consonant Loss	âplwaj-	âplwajé	âplwajé/ âplwaEr-
Double Theme Consonant Loss II	part-	parti	partir
„ „ III	vâd-	vâdy	vâdr
„ „ IV	vâ- finis- fini-	fini	finir

Note: Classes numbered I to IV are determined by thematic extension or modification in the derivation of "principal parts".

(2) the shape of the thematic extensions one must assume in the derivation of the past and future stems from the theme; for the purposes of this paper, except for Class I verbs, the past stem is manifested by the past participle and the future stem by the infinitive. This classification which yields six major groups is presented in Table III.

This classification is unsatisfactory from the pedagogical point of view since it fails to take into account an important cuing device for the identification of person reference: Class I verbs are characterized also by the fact that the opposition third person singular present versus third person plural present is not overtly marked unless the verb happens to begin with a vowel; in Class II verbs this distinction is always marked by the absence or presence respectively of the final consonant of the theme and is redundantly cued in verbs of that class that begin with a vowel. Compare:

	Plural	Singular
Class I	/ildòn/ /ilzèm/	/ildòn/ /ilèm/
Class II	/ilfniʒ/ /ilzɛlarʒis/	/ilfni/ /ilelarʒi/

Double theme verbs of the type /âplwaj-/ ~ /âplwa-/ and /apɛl-/ ~ /apèl-/ must be grouped with monothematic verbs like /dòn-/ since for them the opposition third plural present versus third singular present is not marked by stem alternation, viz., /ilzâplwa/ – singular ~ /ilâplwa/ – plural and /ilzapèl/ – singular ~ /ilapèl/ – plural.

For the former type there remains the problem of the derivation of first and second person plural present forms that contain a /-j-/ between the theme and the person reference suffixes *-ons* and *-ez* e.g., *employons*, *employez*. The infixation of /-j-/ is easily predicted by a rule which prohibits VV sequences at morphological boundaries and which necessitates only a short list of exceptions, e.g., /kre/ "create, imperative singular" but /kree/ "create, imperative plural".

In addition to the present indicative forms and their imperative transforms, the primary forms of French verbs from a pedagogical point of view are the past participle used in the composition of various perfect phrases including the *passé composé* and the infinitive' which enters into many modal phrases including those expressing immediate future and near past, e.g., *il va partir*, "he's going to go" *il vient de partir* "he has just left". In the derivation of the past participle and the infinitive it is useful to distinguish between productive or analogy-inducing classes and residual patterns. Members of the former class can be identified easily from comparison of the third singular and third plural present forms and the phonological shape of the theme and their past participle, infinitive, and future stem are easily derived from the full theme. For Class I verbs (monothematic verbs that show no contrast third singular versus third plural present), the past participle and the infinitive are derived by the suffixation of /-é/ to the theme, e.g., *donn-*/*donné*/*donner*; there remains a small list of anomalous verbs like *ouvr-*, *offr-*, *cueill-* which are considered residual. The future stem of Class I verbs is formed by the suffixation of /-Er-/ to the theme, e.g., *donn-*/*donnerons*/*donnerions*. Class II verbs (double theme verbs that show the opposition third singular versus third plural in the present) whose full theme consists of at least two syllables and ends in /-is-/ have identical past participle and reduced theme derived by the removal of the final /-s-/ of the full theme, e.g., *finiss-*/*fini*/*fini*; their infinitive and future stem are also identical and formed by the suffixation of /-r/ to the reduced theme, e.g., *fini*/*finir*/*finir*-. The past participle and infinitive of other so-called regular verbs whose infinitive and future stem are identical cannot be predicted neatly from the phonological shape of the theme; for instance, many verbs with past participle formed by suffixation of /-y/ and infinitive formed by suffixation of /-r/ have full themes ending in /-Ṽd-/ but cannot be distinguished from Class I verbs of similar phonological shape: compare *vend-*/*vendu*/*vendr*- and *mand-*/*mandé*/*mander*. In addition, Class II verbs whose full themes have similar phonological shape show divergent derivation of past participle and infinitive, e.g., *batt-*/*battu*/*battre* but *mett-*/*mise*/*mettre* and *sent-*/*senti*/*sentir*; *écriv-*/*écrite*/*écrire* but *suiv-*/*suiwi*/*suivre*. With regard to Class II verbs other than the polysyllabic /is-/ type, it is preferable to merely list under separate rubrics past participle and infinitive formation types rather than to attempt the formulation of complex rules which derive the past and future stems from the full theme and which relate them to each other; this classification appears in Table IV. While the linguist qua linguist is interested primarily in the isolation of all recurring partials, the applied linguist seeks to discover overt grammatical signals that must be internalized by the learner and to offer the FL pedagogue

TABLE IV
Pedagogic Classification of French Verbs

Class	Theme	Past Participle	Infinitive/ Future Stem
Productive			
I (No. 3 Sg. vs. 3 Pl in present)	dòn- âplwa- mÈn/mèn	doné âplwajé mÈné	doné/donEr- âplwajé/âplwar- mÈné/menEr-
II (3 Sg. vs. 3 Pl. in present)	finis- fini-	fini	finir
Residual A	part-/par- dòrm-/dòr-	parti dormi	partir dormir
„ B	rõp-/rõ- vâd-/vâ-	rõpy vâdy	rõpr vâdr
„ C	kõdyiz- kõdyi-	kõdyit	kõdyir
„ D etc.	liz- li-	ly	lir

information directly convertible to drills which will lead the learner to produce grammatical sentences with near-native accuracy and automaticity. For the applied linguist, the most economical and elegant rule often turns out to be the simple-minded list or the redundant statement replete with unreduced morphological data.

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DISCUSSION

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I am not quite clear about the distinction to be drawn between a descriptive grammar and a pedagogical grammar, unless the former is to be regarded as embodying a complete or fairly complete description of the language and the latter as embodying a *selection* from this complete description of what should be taught to particular learners. But if this is so, one can visualise a whole range of pedagogical grammars, which would contain varying amounts of the total description, since not all learners are required (at least at a particular stage in their learning careers) to master the use of the language to the same extent.

Even with a pedagogical grammar to hand, however, the average foreign-language teacher (untrained or partly trained) is still unlikely to know what to do. He needs

to be told this in a teachers' course-book, which would include not only the grammatical, phonological and other material on which the course is based but detailed instructions on how to teach a command of it effectively to the age of pupils concerned in the mother-tongue area concerned. If such a course-book is provided, there seems to be no need for the intermediary pedagogical grammar: an appropriate choice from the total description can be embodied in course-books. True, it is a little harder to make these, because appropriate teaching experience as well as linguistic knowledge is called for; but it is relevant also to stress that neither can useful 'pedagogical' grammars be made by nonteachers.

As a tailpiece, I would like to enter an objection to the use of the terms "linguist" and "teacher" as if they referred to mutually exclusive categories of person – not that Mr. Valdman *quite* did this. We must of course, distinguish linguistics from language-teaching theory, but a linguist may be a language-teacher and a language-teacher a linguist – and more of this crossbreeding is urgently needed.

"Applied *linguistics*" – yes; but what is an "applied *linguist*"? Is the term necessary?

THE LOGICAL BASIS OF LINGUISTIC THEORY

NOAM CHOMSKY

1. GOALS OF LINGUISTIC THEORY

1.1. In this paper,¹ I will restrict the term “linguistic theory” to systems of hypotheses concerning the general features of human language put forth in an attempt to account for a certain range of linguistic phenomena. I will not be concerned with systems of terminology or methods of investigation (analytic procedures).

The central fact to which any significant linguistic theory must address itself is this: a mature speaker can produce a new sentence of his language on the appropriate occasion, and other speakers can understand it immediately, though it is equally new to them. Most of our linguistic experience, both as speakers and hearers, is with new sentences; once we have mastered a language, the class of sentences with which we can operate fluently and without difficulty or hesitation is so vast that for all practical purposes (and, obviously, for all theoretical purposes), we can regard it as infinite. Normal mastery of a language involves not only the ability to understand immediately an indefinite number of entirely new sentences, but also the ability to identify deviant sentences and, on occasion, to impose an interpretation on them.² It is evident that rote recall is a factor of minute importance in ordinary use of language, that “a minimum of the *sentences* which we utter is learnt by heart as

¹ This work was supported in part by the U.S. Army Signal Corps, the Air Force Office of Scientific Research, and the Office of Naval Research; and in part by the National Science Foundation.

The account of linguistic structure sketched below in part incorporates, and in part developed in response to many stimulating ideas of Zellig Harris and Roman Jakobson. Its present form is to a large extent a product of collaboration over many years with Morris Halle, to whom (along with Paul Postal and John Viertel) I am indebted for much helpful criticism of this paper. For references, see bibliography at the end of the paper.

² Cf. Chomsky (1955, chapter 4, 1961b), Ziff (1960a, b, 1961), Putnam (1961), Miller and Chomsky (1963). Apparently many linguists hold that if a context can be constructed in which an interpretation can be imposed on an utterance, then it follows that this utterance is not to be distinguished, for the purposes of study of grammar, from perfectly normal sentences. Thus, e.g., “colorless green ideas sleep furiously”, “remorse felt John”, “the dog looks barking”, etc., are not to be distinguished, in this view, from “revolutionary new ideas appear infrequently”, “John felt remorse”, “the dog looks frightening”, though the distinction can clearly be both stated and motivated on syntactic grounds. Thus grammar reduces to such matters as government, agreement, inflectional paradigms, and the like. This decision seems to me no more defensible than a decision to restrict the study of language structure to phonetic patterning.

such – that most of them, on the contrary, are composed on the spur of the moment”, and that “one of the fundamental errors of the old science of language was to deal with all human utterances, as long as they remain constant to the common usage, as with something merely reproduced by memory” (Paul, 1886, 97-8). A theory of language that neglects this “creative” aspect of language is of only marginal interest.

On the basis of a limited experience with the data of speech, each normal human has developed for himself a thorough competence in his native language. This competence can be represented, to an as yet undetermined extent, as a system of rules that we can call the *grammar* of his language. To each phonetically possible utterance (cf. § 4.2), the grammar assigns a certain *structural description* that specifies the linguistic elements of which it is constituted and their structural relations (or, in the case of structural ambiguity, several such structural descriptions). For some utterances, the structural description will indicate, in particular, that they are perfectly well-formed sentences. This set we can call the *language generated by the grammar*. To others, the grammar will assign structural descriptions that indicate the manner of their deviation from perfect well-formedness. Where the deviation is sufficiently limited, an interpretation can often be imposed by virtue of formal relations to sentences of the generated language.

The grammar, then, is a device that (in particular) specifies the infinite set of well-formed sentences and assigns to each of these one or more structural descriptions. Perhaps we should call such a device a *generative grammar* to distinguish it from descriptive statements that merely present the inventory of elements that appear in structural descriptions, and their contextual variants.

A generative grammar contains a *syntactic component* and a *phonological component*. The former generates strings of minimal syntactically functioning elements (following Bolinger, 1948, let us call them *formatives*) and specifies their structural interrelations. The latter converts a string of formatives of specified syntactic structure into a phonetic representation. This much structure is common to all theories of generative grammar. Beyond this minimal specification, important differences emerge.

The generative grammar internalized by someone who has acquired a language defines what in Saussurian terms we may call *langue* (with a qualification to be specified directly below). In performing as a speaker or hearer, he puts this device to use. Thus as a hearer, his problem is to determine the structural description assigned by his grammar to a presented utterance (or, where the sentence is syntactically ambiguous, to determine the correct structural description for this particular token), and using the information in the structural description, to understand the utterance. Clearly the description of intrinsic competence provided by the grammar is not to be confused with an account of actual performance, as de Saussure emphasized with such lucidity (cf. also Sapir, 1921; Newman, 1941). Nor is it to be confused with an account of potential performance.³ The actual use of language

³ The common characterization of language as a set of “verbal habits” or as a “complex of present dispositions to verbal behavior, in which speakers of the same language have perforce come to resemble

obviously involves a complex interplay of many factors of the most disparate sort, of which the grammatical processes constitute only one. It seems natural to suppose that the study of actual linguistic performance can be seriously pursued only to the extent that we have a good understanding of the generative grammars that are acquired by the learner and put to use by the speaker or hearer. The classical Saussurian assumption of the logical priority of the study of *langue* (and the generative grammars that describe it) seems quite inescapable.

In the background of the discussion below there will be two conflicting models of generative grammar. The first – which I will call the *taxonomic model* – is a direct outgrowth of modern structural linguistics. The second – which I will call the *transformational model* – is much closer to traditional grammar. It should be noted, however, that modern grammars are typically not conceived as generative grammars, but as descriptive statements about a given corpus (text). Hence the taxonomic model, as described below, is no more than an attempt to formulate a generative grammar which is in the spirit of modern procedural and descriptive approaches. The essential reliance on procedures of segmentation and classification, and on statements of syntagmatic and paradigmatic distribution, is widely shared, however (cf. de Saussure, Hjelmslev, Harris, among others); and these notions clearly suggest a generative grammar with the characteristics of the taxonomic model, as considered here.

The taxonomic model is far simpler, more “concrete” and more “atomistic” than the transformational model. We can characterize it briefly in the following way. Each rule is of the form: element A has the member (variant, realization) X in the context Z-W. Let us call such a rule a *rewriting rule*. The syntactic component consists of an unordered set of rewriting rules, each of which states the membership of some phrase category or formative category in some context.⁴ The structural

one another” (Quinc, 1960, 27) is totally inadequate. Knowledge of one’s language is not reflected directly in linguistic habits and dispositions, and it is clear that speakers of the same language or dialect may differ enormously in dispositions to verbal response, depending on personality, beliefs and countless other extra-linguistic factors.

⁴ On the syntactic level, the taxonomic model is a generalization from Harris’ morpheme-to-utterance statements, which constitute the nearest approach to an explicit generative grammar on this level. Furthermore, most modern work in syntax is actually more adequately formalized in terms of rewriting rules with null context (i.e., *context-free grammar* – in particular, this seems to be true of Pike’s tagmemics, as of most work in IC analysis). Similarly, most, if not all of the work involving use of computers for analysis of sentence structure seems to fall within this narrower framework (cf. Gross, 1962). This is to say that both the sets of sentences generable and, much more importantly, even the systems of structural descriptions generable within the framework of IC analysis seem to be adequately represented by the mechanism of generation of strings and of structural descriptions (Phrase-markers) formalized within this theory (cf. Postal, 1964, in this connection). Though abstract study of such systems is recent, there is already a fairly substantial body of results. Cf. Chomsky (1963), Schützenberger and Chomsky (1963) for summaries of recent work. I do not think that the variations that have been proposed within this general framework have any bearing on the conclusions developed below, regarding the taxonomic model. From the point of view of linguistic adequacy, of course, the important question about a theory of grammar (e.g., the taxonomic model, or the theory of context-free grammar) is not so much the question of the sets of strings that are generable (the *weak generative*

description that it provides can be regarded as a labelled bracketing of the string of formatives, indicating the category of each substring which is a constituent. Let us call such a labelled bracketing, obtainable automatically from a single derivation, a *Phrase-marker* of this string of formatives. The phonological component consists of two distinct sets of rewriting rules. The first set (morphophonemic rules) states the phonemic constitution of morphophonemes or formatives with respect to stated contexts. The second set (phonetic rules) states the phonetic constitution of phonemes, with respect to stated contexts. Each of these sets is unordered.

The transformational model is far more complex and highly structured. The syntactic component is assumed to consist of two subcomponents. The first (constituent structure) subcomponent consists of an ordered set of rewriting rules that generate strings of formatives that we may call *C-terminal strings*. These constitute either a finite set, or a highly restricted infinite set. The second (transformational) subcomponent consists of a partially ordered set of complex operations called (*grammatical*) *transformations*, each of which maps a full Phrase-marker (or a pair, triple, etc. of Phrase-markers) of some terminal string (or a pair, triple, etc. of terminal strings) into a new *derived Phrase-marker* of a *T-terminal string*. Some of the rewriting and transformational rules may be obligatory, while others are optional. Application of all obligatory and perhaps some optional rules of the syntactic component, observing order, will give a T-terminal string with a derived Phrase-marker. The structural description of this string will be a set of Phrase-markers (one for each underlying C-terminal string, and, in addition, the derived Phrase-marker of the full string) and a representation of its "transformational history". We will see below that all of this information plays a role in determining the interpretation of an utterance.⁵

The phonological component of a transformational generative grammar consists of an ordered set of rewriting rules, an ordered set of transformational rules, and an ordered set of rewriting rules, in that order. The transformational rules, furthermore, apply in a cycle, first to the smallest constituents of a string, then to the next largest constituents, etc., until the maximal domain of phonological processes is reached.

capacity of this theory), but rather that of the sets of structural descriptions that are generable within the framework of this theory (its *strong generative capacity*), and it is this that I will consider below. However, it is interesting to observe that several examples are now known of subparts of natural languages that are beyond the weak generative capacity of the theory of context-free grammar (cf. Postal, 1961, 1964; Miller and Chomsky, 1963). Though this is not the linguistically most significant deficiency of this theory, it is sufficient to show that in attempting to enrich the theory of grammar to overcome the inadequacies of such systems, we must not only go beyond them in strong generative capacity, but we must develop a theory that exceeds the theory of context-free grammar in weak generative capacity as well.

⁵ The most accessible summary of formal properties of grammatical transformations, from this point of view, is in Chomsky (1961a). For further details, see Chomsky (1955, chapters 8, 9). The most extensive study of English grammar within this framework is Lees (1960a). See the bibliography of the second printing (1961) of Chomsky (1957a) for references to much recent work. In addition, cf. Schachter (1961, 1962), Postal (1962).

These are, technically, transformational rules since they involve the constituent structure of the utterance. This *transformational cycle* determines the phonetic form of syntactically complex units from the underlying (abstract) phonemic form of their components, using the manner of composition specified by the derived Phrase-marker.⁶

Notice that in the case of the transformational model, the symbols and structures that are manipulated, rewritten and transformed as a sentence is generated may bear no very direct relation to any of its concrete subparts, whereas in the case of the taxonomic model each of the symbols that is rewritten in the generation of a sentence stands for a category to which some subpart of this sentence belongs (or category symbol by which it is represented). It is in this sense that the taxonomic model is both more concrete and more atomistic.

1.2. Before continuing, it is instructive to consider these notions from the point of view of traditional grammar, as well as that of classical linguistic theory and of modern taxonomic linguistics.

It would not be inaccurate to regard the transformational model as a formalization of features implicit in traditional grammars, and to regard these grammars as implicit transformational generative grammars. The goal of a traditional grammar is to provide its user with the ability to understand an arbitrary sentence of the language, and to form and employ it properly on the appropriate occasion. Thus its goal is (at least) as far-reaching as that of a generative grammar, as just described. Furthermore, the rich descriptive apparatus of traditional grammar far exceeds the limits of the taxonomic model, though it is largely, and perhaps fully formalizable within the framework of the transformational model. However, it is important to bear in mind that even the most careful and complete traditional grammar relies in an essential way on the intuition and intelligence of the user, who is expected to draw the correct inferences from the many examples and hints (and explicit lists of irregularities) presented by the grammar. If the grammar is a good one, the user may succeed, but the deep-seated regularities of the language that he somehow discovers escape explicit formulation, and the nature of the abilities that enable him to perform this task remain a complete mystery. The vastness of these gaps can be appreciated only when one makes an attempt to construct explicit rules to account for the full range of structural information available to the mature user of a language.

Focusing on the notion of "creativity", one can distinguish two conflicting views regarding the essential nature of language in Nineteenth Century linguistic theory. On the one hand, we have the Humboldtian view that "man muss die *Sprache* nicht

⁶ For examples of the operation of the transformational cycle in English, see Chomsky, Halle, Lukoff (1956), and improved statements in Miller and Chomsky (1963). For examples in Russian, see Halle (1961b). For examples in Latvian, see Halle and Zeps (forthcoming). The structure of the phonological component of a transformational grammar, with particular reference to English, is discussed in more detail in Halle and Chomsky (1960, forthcoming).

sowohl wie ein todtes *Erzeugtes*, sondern weit mehr wie eine *Erzeugung* ansehen" (1836, § 8, p. LV). The essence of each language is what Humboldt designates as its characteristic *Form* (not to be identified with "inner form"). The form of language is that constant and unvarying factor that underlies and gives life and significance to each particular new linguistic act. It is by having developed an internal representation of this form that each individual is capable of understanding the language and using it in a way that is intelligible to his fellow speakers. This characteristic form determines and inheres in each separate linguistic element. The role and significance of each individual element can be determined only by considering it in relation to underlying form, that is, in relation to the fixed generative rules that determine the manner of its formation. It is this underlying generative principle that the linguist must seek to represent in a descriptive grammar.

Cf., for example, such representative passages as these: „. . . [die Form] . . . ist in ihrer Natur selbst eine Auffassung der einzelnen, im Gegensatze zu ihr als Stoff zu betrachtenden, *Sprach-elemente* in *geistiger Einheit*. Denn in jeder Sprache liegt eine solche [Einheit], und durch diese zusammenfassende Einheit macht eine Nation die ihr von ihren Vorfahren überlieferte Sprache zu der ihrigen. Dieselbe Einheit muss sich also in der Darstellung wiederfinden; und nur wenn man von den zerstreuten Elementen bis zu dieser Einheit hinaufsteigt, erhält man wahrhaft einen Begriff von der Sprache selbst, da man, ohne ein solches Verfahren, offenbar Gefahr läuft, nicht einmal jene Elemente in ihrer wahren Eigentümlichkeit, und noch weniger in ihrem realen Zusammenhange zu verstehen“ (§ 8, p. LXII). „Es versteht sich indess von selbst, dass in den Begriff der Form der Sprache keine Einzelheit als *isolirte* Thatsache, sondern immer nur insofern aufgenommen werden darf, als sich eine Methode der Sprachbildung an ihr entdecken lässt“ (§ 8, p. LXII). „Die charakteristische Form der Sprachen hängt an jedem *einzelnen* ihrer kleinsten Elemente; jedes wird durch sie, wie unerklärlich es im Einzelnen sei, auf irgend eine Weise bestimmt. Dagegen ist es kaum möglich, Punkte aufzufinden, von denen sich behaupten liesse, dass sie an ihnen, einzeln genommen, entscheidend haftete“ (§ 8, p. LIX). „Denn die Sprache ist ja nicht als ein daliegender, in seinem Ganzen übersehbarer, oder nach und nach mittelbarer Stoff, sondern muss als ein sich ewig *erzeugender* angesehen werden, wo die Gesetze der Erzeugung bestimmt sind, aber der Umfang und gewissermassen auch die Art des Erzeugnisses gänzlich unbestimmt bleiben“ (§ 9, p. LXXI). „Die Sprache besteht, neben den schon geformten Elementen, ganz vorzüglich auch aus Methoden, die Arbeit des Geistes, welcher sie die Bahn und die Form vorzeichnet, weiter fortzusetzen“ (§ 9, p. LXXVII). „Das in dieser Arbeit des Geistes, den articulirten Laut zum Gedankenausdruck zu erheben, liegende Beständige und Gleichförmige, so vollständig, als möglich, in seinem Zusammenhange aufgefasst, und systematisch dargestellt, macht die *Form* der Sprache aus“ (§ 8, p. LVIII).

In Humboldt's sense, *Form* extends beyond grammatical form (beyond "Redefügung" and "Wortbildung") to encompass also the substantive characterization of the sound system (§ 8, p. LX) and the principles of concept formation as embodied in the system of stems ("Grundwörter") (§ 8, p. LXI). "Überhaupt wird durch den Begriff Form nichts Factisches und Individuelles ausgeschlossen . . ." (§ 8, p. LXII).

From this conception of the nature of language, Humboldt derives his views concerning understanding of speech and acquisition of language. Speaking and under-

standing are, in his view, differing manifestations of the same underlying capacity, the same generative principle, mastery of which provides the speaker-hearer with the ability to use and understand all of the infinite range of linguistic items (“Mit dem *Verstehen* verhält es sich nicht anders. Es kann in der Seele nichts, als durch eigne Thätigkeit vorhanden sein, und Verstehen und Sprechen sind nur verschiedenartige Wirkungen der nämlichen Sprachkraft. Die gemeinsame Rede ist nie mit dem Übergeben eines Stoffes vergleichbar. In dem Verstehenden, wie im Sprechenden, muss derselbe aus der eigenen, innern Kraft entwickelt werden; und was der erstere empfängt, ist nur die harmonisch stimmende Anregung. Es ist daher dem Menschen auch schon natürlich, das eben Verstandene wieder gleich auszusprechen. Auf diese Weise liegt die Sprache in jedem Menschen in ihrem ganzen Umfange, was aber nichts Anderes bedeutet, als dass jeder ein . . . geregeltes Streben besitzt, die ganze Sprache, wie es äussere oder innere Veranlassung herbeiführt, nach und nach aus sich hervorzubringen und hervorgebracht zu verstehen” – § 9, p. LXX). Furthermore, since language consists essentially of a “System von Regeln” as well as a “Vorrath von Wörtern” (cf. § 9, p. LXXVIII), common to speaker and hearer, it follows that “Das *Sprechenlernen* der Kinder ist nicht ein Zumessen von Wörtern, Niederlegen im Gedächtnis, und Wiedernachlallen mit den Lippen, sondern ein Wachsen des Sprachvermögens durch Alter und Übung” (§ 9, p. LXXI). “. . . [Die Sprache] . . . lässt sich . . . , wenn es auch auf den ersten Anblick anders erscheint, nicht eigentlich lehren, sondern nur im Gemüthe wecken; man kann ihr nur den Faden hingeben, an dem sie sich von selbst entwickelt” (§ 6, p. L).

It is just this point of view concerning the essential nature of language that underlies and motivates recent work in generative grammar. Furthermore, the Humboldtian views concerning perception and acquisition have re-emerged, in many particulars, in the course of this work (cf., e.g., Chomsky, 1957a, 48; 1960; 1961a, §§ 1,2; and the references of note 50, below). A generative grammar, in the sense sketched above, is an attempt to represent, in a precise manner, certain aspects of the *Form* of language, and a particular theory of generative grammar is an attempt to specify those aspects of form that are a common human possession – in Humboldtian terms, one might identify this latter with the underlying general form of all language (“Die Formen mehrerer Sprachen können in einer noch allgemeineren Form zusammenkommen, und die Formen aller thun dies in der That, insofern man überall bloss von dem Allgemiesten ausgeht” . . . “dass man ebenso richtig sagen kann, dass das ganze Menschengeschlecht nur Eine Sprache, als das jeder Mensch eine besondere besitzt” – § 8, LXIII). There is one respect (to which we return directly below) in which this work diverges in principle from the Humboldtian framework; beyond this, the narrower limitations within which it has concretely developed (in particular, insofar as little was said until recently concerning semantic or conceptual structure) is a result not of any point of principle, but rather of the fact that there has been little to say about these further matters that can withstand serious analysis (cf. § 2.3.).

For further discussion of Humboldtian general linguistics, see Viertel (forthcoming).

In sharp contrast to the Humboldtian conception, in the general linguistics of the Nineteenth Century, is the view that is perhaps expressed most clearly by Whitney (1872); namely, that "language in the concrete sense . . . [is] . . . the sum of words and phrases by which any man expresses his thought" (372); that study of speech is no more than study of a body of vocal signs; and that study of the origin and development of language is nothing more than study of origin and development of these signs. The problem of accounting for the acquisition of language, so conceived, disappears. ". . . the acquisition of language by children does not seem to us any mystery at all." It is not at all astonishing "that a child, after hearing a certain word used some scores or hundreds of times, comes to understand what it means, and then, a little later, to pronounce and use it . . .".

This narrowing of the scope of linguistics to the study of inventory of elements was occasioned not only by the dramatic successes of comparative linguistics, which operated within these limitations, but also by the unclarity and obscurities of formulation of Humboldt ("a man whom it is nowadays the fashion to praise highly, without understanding or even reading him" – Whitney, 1872, 333) and his successors. Furthermore, there were some serious confusions concerning the notion of "creativity". Thus it is significant that the comments of Paul's quoted above are from a chapter that deals with analogic change. He makes no distinction (just as Humboldt makes no clear distinction) between the kind of "creativity" that leaves the language entirely unchanged (as in the production – and understanding – of new sentences, an activity in which the adult is constantly engaged) and the kind that actually changes the set of grammatical rules (e.g., analogic change). But this is a fundamental distinction. In fact, the technical tools for dealing with "rule-governed creativity," as distinct from "rule-changing creativity", have only become readily available during the past few decades in the course of work in logic and foundations of mathematics. But in the light of these developments, it is possible to return to the questions to which Humboldt addressed himself, and to attempt to represent certain aspects of the underlying "Form of language", insofar as it encompasses "rule-governed creativity", by means of an explicit generative grammar.

Saussure, like Whitney (and possibly under his influence – cf. Godel, 1957, 32-3), regards *langue* as basically a store of signs with their grammatical properties, that is, a store of wordlike elements, fixed phrases and, perhaps, certain limited phrase types (though it is possible that his rather obscure concept of "mécanisme de la langue" was intended to go beyond this – cf. Godel, 1957, 250). He was thus quite unable to come to grips with the recursive processes underlying sentence formation, and he appears to regard sentence formation as a matter of *parole* rather than *langue*, of free and voluntary creation rather than systematic rule (or perhaps, in some obscure way, as on the border between *langue* and *parole*). There is no place in his scheme for "rule-governed creativity" of the kind involved in the ordinary everyday use of language. At the same time, the influence of Humboldtian holism (but now restricted to inventories and paradigmatic sets, rather than to the full-scale generative processes

that constitute *Form*) is apparent in the central role of the notions “terme” and “valeur” in the Saussurian system.

Modern linguistics is much under the influence of de Saussure’s conception of *langue* as an inventory of elements (de Saussure, 1916, 154, and elsewhere, frequently) and his preoccupation with systems of elements rather than the systems of rules which were the focus of attention of traditional grammar and of the general linguistics of Humboldt. In general, modern descriptive statements pay little attention to the “creative” aspect of language; they do not face the problem of presenting the system of generative rules that assign structural descriptions to arbitrary utterances and thus embody the speaker’s competence in and knowledge of his language. Furthermore, this narrowing of the range of interest, as compared with traditional grammar, apparently has the effect of making it impossible to select an inventory of elements correctly, since it seems that no inventory (not even that of phonemes) can be determined without reference to the principles by which sentences are constructed in the language (cf. § 4.3-4). To the extent that this is true, “structural linguistics” will have suffered from a failure to appreciate the extent and depth of interconnections among various parts of a language system. By a rather arbitrary limitation of scope, modern linguistics may well have become engaged in an intensive study of mere artifacts. We return to this matter below.

It is, incidentally, interesting to take note of a curious and rather extreme contemporary view to the effect that true linguistic science must *necessarily* be a kind of pre-Darwinian taxonomy concerned solely with the collection and classification of countless specimens, while any attempt to formulate underlying principles and to concentrate on the kinds of data that shed some light on these is taken to be some novel sort of “engineering”.⁷ Perhaps this notion, which seems to me to defy comment, is related to the equally strange and factually quite incorrect view (recently expressed, e.g., by Joos, 1961; Reichling, 1961; Mel’chuk, 1961; Juilland, 1961) that current work in generative grammar is in some way an outgrowth of attempts to use electronic computers for one or another purpose, whereas in fact it should be obvious that its roots are firmly in traditional linguistics.

1.3. The issues involved can be clarified by setting linguistic theory within the general framework of the study of human intellectual capacities and their specific character. Still remaining within the classical framework, as modified above, we can take as an objective for linguistic theory the precise specification of two kinds of abstract device, the first serving as a perceptual model and the second, as a model for acquisition of language.

- (1) (a) utterance \longrightarrow A \longrightarrow structural description
 (b) primary linguistic data \longrightarrow B \longrightarrow generative grammar

⁷ See Bolinger (1960) for an elaboration of this point of view. See also the Introduction to Joos (1957).

The perceptual model A is a device that assigns a structural description D to a presented utterance U, utilizing in the process its internalized generative grammar G, where G generates a phonetic representation R of U with the structural description D. In Saussurian terms, U is a specimen of *parole* interpreted by the device A as a "performance" of the item R which has the structural description D and which belongs to the *langue* generated by G. The learning model B is a device which constructs a theory G (i.e., a generative grammar G of a certain *langue*) as its output, on the basis of primary linguistic data (e.g., specimens of *parole*), as input. To perform this task, it utilizes its given *faculté de langage*, its innate specification of certain heuristic procedures and certain built-in constraints on the character of the task to be performed. We can think of general linguistic theory as an attempt to specify the character of the device B. We can regard a particular grammar as, in part, an attempt to specify the information available in principle (i.e., apart from limitations of attention memory, etc.) to A that makes it capable of understanding an arbitrary utterance, to the highly non-trivial extent that understanding is determined by the structural description provided by the generative grammar. In evaluating a particular generative grammar, we ask whether the information that it gives us about a language is correct, that is, does it describe correctly the linguistic intuition of the speaker (Saussure's "conscience des sujets parlants", which to him, as to Sapir, provides the ultimate test of adequacy for a linguistic description). In evaluating a general theory of linguistic structure that is sufficiently explicit to offer an actual hypothesis about the character of B, we ask whether the generative grammars that it selects meet the empirical criterion of correspondence to the speaker's linguistic intuition, in the case of particular languages.

I will try to show that the taxonomic model (or any of its variants within modern study of language) is far too oversimplified to be able to account for the facts of linguistic structure and that the transformational model of generative grammar is much closer to the truth. To show that modern linguistics seriously underestimates the richness of structure of language and the generative processes that underlie it, it is necessary to sample the range of problems that cannot be attacked, or often even posed within the narrow limits that it sets. A variety of examples of this sort will be considered in the following sections. I will also try to show that these inadequacies and limitations may in part be traceable to an impoverished conception of the nature of human cognitive processes, and that a return to traditional concerns and viewpoints, with the higher standards of explicitness that have emerged in modern linguistics, can perhaps provide new insights concerning perception and learning.

2. LEVELS OF SUCCESS FOR GRAMMATICAL DESCRIPTION

2.0. Within the framework outlined above, we can sketch various levels of success that might be attained by a grammatical description associated with a particular linguistic theory. The lowest level of success is achieved if the grammar presents

the observed primary data correctly.⁸ A second and higher level of success is achieved when the grammar gives a correct account of the linguistic intuition of the native speaker, and specifies the observed data (in particular) in terms of significant generalizations that express underlying regularities in the language. A third and still higher level of success is achieved when the associated linguistic theory provides a general basis for selecting a grammar that achieves the second level of success over other grammars consistent with the relevant observed data that do not achieve this level of success. In this case, we can say that the linguistic theory in question suggests an explanation for the linguistic intuition of the native speaker. It can be interpreted as asserting that data of the observed kind will enable a speaker whose intrinsic capacities are as represented in this general theory to construct for himself a grammar that characterizes exactly this linguistic intuition.

For later reference, let us refer to these roughly delimited levels of success as the levels of *observational adequacy*, *descriptive adequacy*, and *explanatory adequacy*, respectively. In terms of the notions of the preceding section, a grammar that aims for observational adequacy is concerned merely to give an account of the primary data (e.g., the corpus) that is the input to the learning device (1b); a grammar that aims for descriptive adequacy is concerned to give a correct account of the linguistic intuition of the native speaker; in other words, it is concerned with the output of the device (1b); and a linguistic theory that aims for explanatory adequacy is concerned with the internal structure of the device (1b); that is, it aims to provide a principled basis, independent of any particular language, for the selection of the descriptively adequate grammar of each language.

Modern linguistics has been largely concerned with observational adequacy. In particular, this is true of post-Bloomfieldian American linguistics (cf. below, § 4.3-4), and apparently, of the London school of Firth, with its emphasis on the ad hoc character of linguistic description.⁹ Traditional grammar, on the other hand, was explicitly concerned with the level of descriptive adequacy (and this interest persists, explicitly, in Sapir's work, as well as in current work in the traditional mold – cf. Sapir, 1933; Long, 1960). This difference between traditional and modern points of view is made particularly clear in modern critique of traditional grammars. Thus Nida, in his valuable study (1943) of English syntax within the immediate constituent framework, criticizes Jespersen sharply for his "serious distortion and complication

⁸ Innocuous as this comment may seem, it still requires qualification. What data is relevant is determined in part by the possibility for a systematic theory, and one might therefore hold that the lowest level of success is no easier to achieve than the others. As noted above, the fact that a certain noise was produced, even intentionally, by an English speaker does not guarantee that it is a well-formed specimen of his language. Under many circumstances it is quite appropriate to use deviant utterances. Furthermore, under normal conditions speech is subject to various, often violent distortions that may in themselves indicate nothing about the underlying linguistic patterns. The problem of determining what data is valuable and to the point is not an easy one. What is observed is often neither relevant nor significant, and what is relevant and significant is often very difficult to observe, in linguistics no less than in the freshman physics laboratory, or, for that matter, anywhere in science.

⁹ Cf. Firth et al. (1957).

of the formal and functional values" in assigning to "the doctor's arrival", but not "the doctor's house", a structural description that indicates that the Subject-Verb relation appears in the former but not in the latter phrase. But clearly Jespersen's account is correct on the level of descriptive adequacy, and the fact that the data-processing operations of modern linguistics fail to provide the correct information indicates only that they are based on an erroneous conception of linguistic structure, or that observational adequacy is being taken as the only relevant concern.¹⁰ On the other hand, Jakobson's attempts to formulate universal phonological laws (cf. § 4.2, below) might perhaps be regarded as indicating a concern for explanatory adequacy, on at least one level of grammar. It is clear that the question of explanatory adequacy can be seriously raised only when we are presented with an explicit theory of generative grammar that specifies the form of grammars and suggests a mechanism for selecting among them (i.e., an evaluation procedure for grammars of a specified form). The difference between observational and descriptive adequacy is related to the distinction drawn by Hockett (1958) between "surface grammar" and "deep grammar", and he is unquestionably correct in noting that modern linguistics is largely confined in scope to the former.

2.1 *Levels of adequacy in phonology*

A few linguistic examples may help to clarify the distinction between these various levels of adequacy. Consider first the case of so-called "accidental gaps" in the lexicon. Thus in English there is a word "pick" /pik/, but no /blik/ or /ftik/. The level of observational adequacy would be attained by a grammar that contained the rule: $N \rightarrow /pik/$, but no lexical rule introducing /blik/ or /ftik/. To attain the level of descriptive adequacy, a grammar would have to provide, in addition, a general rule that sets up a specific barrier against /ftik/, but not against /blik/ (which would thus qualify as an accidental gap, a phonologically permissible nonsense syllable). This level would be achieved by a grammar that contained the generalization that in initial position before a true consonant (a segment which is consonantal and non-vocalic, in terms of Jakobson's distinctive features), a consonant is necessarily /s/. The level of explanatory adequacy would be attained by a linguistic theory that provides a principled reason for incorporating this generalization in a grammar of English, and for excluding the (factually correct) "rule" that in the context /#b-ik#/ a liquid is necessarily /r/. Thus the theory might provide a general evaluation measure (simplicity measure) which would show how the former, but not the latter rule gives a more highly valued grammar. Such a theory would suggest an explanation for the linguistic intuition that /blik/, but not /ftik/, is a "possible" word, though neither has been heard. This is the intuition that would result from observation of

¹⁰ Nida also criticizes Jespersen on essentially the same grounds, for describing "barking" in "the barking dogs" as an attributive of the same rank as "barks" in "the dog barks". Again Jespersen's decision seems to me unquestionably correct from the point of view of descriptive adequacy, though internally unmotivated (i.e., deficient from the point of view of explanatory adequacy).

actual utterances by a learner who constructs the most highly valued grammar of the appropriate form, as specified by this theory.¹¹

Consider now the matter of predictable phonetic variants. Thus in my speech, the lexical item “telegraph” appears in many phonetic shapes, depending on context, in particular, the shapes (2i-iii) in the contexts $\#-\#$, $-ic$, $-y$, respectively:

- (2) (i) tɛligræf
 (ii) tɛligræf
 (iii) tɛlɛgrɪf.

Observational adequacy would be achieved by a grammar that merely states the facts, as I have just done, thus reproducing the observed data in a convenient arrangement. Such a grammar (called, technically, an *item-and arrangement grammar*) in effect treats the item “telegraph” as an exception, exactly as it treats “see”-“saw”, “man”-“men”, etc. Thus the grammar would be no more complex if the facts were, instead, that (2i) appears in the context $-y$, (2ii) in the context $\#-\#$, and (2iii) in the context $-ic$, the rest of the language remaining fixed. Within this framework, there are no further questions to be raised, and there is nothing more to be said.

To achieve the level of descriptive adequacy, in this case, a grammar must treat the variants of “telegraph” as a special case of general rules applying as well to many other items. It must be able to account for the fact that the phonetic variation of “telegraph” is obviously not capricious, given the rest of English, as is the variation of “man”. Not having heard the form “men”, it is impossible for the linguist or learner to predict it. But this is not true in the case of (2).

The grammar would achieve the still higher level of explanatory adequacy, in this case, if the linguistic theory associated with it provides a framework for phonological rules and an evaluation measure meeting the following condition: the most highly valued set of rules of the appropriate form selected to generate a set of items from which the variants of “telegraph” are excluded would be the set of rules that in fact predict this contextual variation for “telegraph”. In this case, the linguistic theory would provide a basis for explaining the facts presented in (2), in terms of other

¹¹ A theory that attempts to reach the level of explanatory adequacy, in this case, is developed in Halle (1959a, 1959b) and Halle and Chomsky (forthcoming). Halle shows how consistent adherence to the general principle of minimizing feature specifications in the phonological component provides a principled basis for the distinction between accidental and non-accidental gaps. To my knowledge, this is the only attempt to provide a general basis for this distinction, though lists and charts that state much of the data that is to be explained have frequently appeared.

In his review of Halle (1959b), Ferguson (1962, 292) describes Halle’s discovery of the role of “morpheme structure rules” as “a misfortune” not too different from certain defects of taxonomic grammars that Halle exposes (cf. § 4.3, below). This is an extremely peculiar conclusion. No generalization is lost by distinguishing morpheme structure rules (which are obviously needed in a full grammar, and which, as Halle shows, play a distinctive role in accounting for an otherwise unexplained area of linguistic fact) from other phonetic rules differing from them both in formal properties and in the phenomena that they describe. On the other hand, the deficiency of taxonomic grammars to which Ferguson alludes involves their inability to state certain generalizations, that is, their inability to achieve descriptive adequacy.

aspects of English and certain assumptions about the general character of grammars. It would make clear, in other words, the respect in which the actual contextual variation differs from the alternative mentioned directly below (2). The latter would lead to a less-highly valued grammar – it would not be predicted by the highest-valued grammar based on data that excludes (2). The theory of item-and-arrangement grammar obviously cannot meet this condition, and for this reason (which, clearly, generalizes to a host of similar examples) cannot be regarded seriously as a theory of grammar.¹² In such cases as this, neither the level of descriptive nor explanatory adequacy is easy to meet, and it is a fact worth considering that despite the extensive investigations of English phonology in recent years, no attempt has even been made to meet them.

The point becomes even clearer when we consider phonetic variants that are syntactically conditioned. Thus English “tórrent“ /tɔrent/ (cf. “torrential”) has the reduced vowel [ɪ] in the second syllable, while the noun “tórment” /tɔrment/ retains the vowel [e]. The level of observational adequacy is attained by the preceding sentence. The level of descriptive adequacy would be achieved by a description that managed to relate these observations to the fact that there is a verb “tormént”, but no verb “torrént” in English, by means of general rules about stress shifts in nouns derived from verbs (“pérmit”, “permít”, etc.), and about the role of stress in preventing vowel reduction. The level of explanatory adequacy requires a phonological theory that prescribes the general form of such syntactically determined phonetic processes, and that shows how the appropriate generalizations, in this case, would appear in the highest-valued grammar of the prescribed form, even if these items were not part of the observed data from which this grammar is constructed. Similarly, in the case of such familiar examples as “light house keeper” (with stress patterns 132, 213, 313), the level of descriptive adequacy requires, beyond a statement of these facts, a general account of the rules by which such stress patterns are assigned in syntactic constructions, and the level of explanatory adequacy will be achieved only when a general theory of such processes is forthcoming. It is examples of this sort that provide the motivation for the transformational cycle of the phonological component, since in these cases the phonetic shape of the full phrase is determined by that of its constituents. Cf. p. 918 above.

2.2 *Levels of adequacy in syntax.*

Consider next a few syntactic examples. Suppose that the sentences

- (3) John is easy to please
- (4) John is eager to please

are observed and accepted as well-formed. A grammar that achieves only the level of

¹² See the references of the preceding footnote, and also Chomsky (1959, 1960), Miller and Chomsky (1963), for discussion of the problem of developing a phonological theory that meets this condition, for such cases.

observational adequacy would, again, merely note this fact in one way or another (e.g., by setting up appropriate lists). To achieve the level of descriptive adequacy, however, a grammar would have to assign structural descriptions indicating that *John* in (3) is the direct object of *please* (the words are grammatically related as in “This pleases John”), while in (4) it is the logical subject of *please* (as in “John pleases someone”). A theory of grammar that does not allow structural descriptions of this sort cannot achieve the level of descriptive adequacy. In cases of this sort, the taxonomic model of generative grammar discussed above (or any of its variants) cannot achieve the level of descriptive adequacy, since information of this kind cannot be represented in the Phrase-marker that it provides as the full structural description on the syntactic level. The transformational model does, however, make grammars available that can supply structural information of this sort, and therefore can, in this case at least, achieve the level of descriptive adequacy. In § 4.1 we will return to a more detailed consideration of the problem of assigning to (3), (4) structural descriptions that provide the full range of syntactic information.

How might a transformational grammar achieve the level of explanatory adequacy in such a case as this? To achieve this level, the theory must provide for the selection of a descriptively adequate grammar, given such data as (3), (4), “John’s eagerness (*easiness) to please . . .”, “to please John is easy (*eager)”, “John is an easy (*eager) fellow to please”, “it pleases John”, “John pleases everyone”, “John is easy (eager) for us to please”, “it is easy (*eager) to please John”, “John is a person who (it) is easy to please”, “this room is not easy to work in (to do decent work in)”, “he is easy to do business with”, “This knife is very difficult to cut (meat) with”, “a hotel lobby is difficult (a difficult place) to meet people in”, “he is not easy to get information from”, “such flattery is easy to be fooled by”, and many other similar structures.

The general theory, in other words, would have to make possible the formulation of the underlying generalizations that account for this arrangement of empirical data, and to distinguish these real and significant generalizations from vacuous pseudo-simplifications that have no linguistic consequences. In so doing, the theory would suggest an explanation for the linguistic intuition of native speakers as regards (3) and (4). This explanation would rest on the assumption that the concepts of grammatical structure and “significant generalization” made explicit in this theory constitute the set of tools used by the learner in constructing an internal representation of his language (i.e., a generative grammar), on the basis of presented linguistic data. There is fairly good reason to believe that in the case of (3), (4), the theory of transformational grammar can reach the level of explanatory adequacy, and can suggest an explanation for the speaker’s linguistic intuition.¹³ That is, the grammar that assigns the correct structural descriptions contains generalizations that are not

¹³ See Miller and Chomsky (1963). Cf. also Lees (1960b) for detailed discussion of a class of similar cases. For discussion of measures of evaluation that select grammars with significant generalizations over those that do not contain such generalizations, cf. Chomsky (1955, chapter 3; 1962); Halle (1961a); Halle and Chomsky (forthcoming).

expressed in grammars that fail to provide the correct structural descriptions, and is thus higher-valued, in a sense which can apparently be made precise without much difficulty.

As a second syntactic example, consider the following arrangement of sentences and non-sentences: (“John found the book”-“John was a farmer”), (“the book was found by John”-*“a farmer was been by John”), (“did John find the book?”-*“did John be a farmer?”), (*“found John the book?”-“was John a farmer?”), (“John didn’t find the book”-*“John didn’t be a farmer”), (*“John foundn’t the book”-“John wasn’t a farmer”), (“John DID find the book”-*“John DID be a farmer”), (“Bill found the book and so did John”-*“Bill was a farmer and so did John”), (*“Bill found the book and so found John”-“Bill was a farmer and so was John”), etc. In short, as is well-known, there are a variety of respects, of which these are a sample, in which “be” behaves quite differently from “find”. Similarly, “be”, but not “find”, is an Auxiliary. Traditional grammars merely list these facts as anomalous, and make no attempt to relate them. It can easily be shown, however, that a transformational grammar with a constituent structure subcomponent containing the rules:

- (5) (i) $VP \rightarrow Aux + VP_1$
 (ii) $Aux \rightarrow Aux_1 (Aux_2)$
 (iii) $Aux_1 \rightarrow Tense (Modal)$
 (iv) $Aux_2 \rightarrow (have + en) (be + ing)$
 (v) $VP_1 \rightarrow \begin{cases} Verb + NP \\ be + Predicate \end{cases}$

(an analysis which has many independent motivations) will automatically provide for just this range of phenomena, thus reducing a mass of apparent idiosyncracies to underlying regularity (cf. Chomsky, 1955, chapters 7,9; 1957a). In fact, a transformational grammar would have to be complicated considerably to generate the excluded sentences. Here again, then, it seems that the level of explanatory adequacy can be met by a transformational grammar and the theory associated with it.¹⁴

¹⁴ The well-known (and different) apparent anomalies of “have” are also largely accounted for by (5) and the rules for forming questions, negations, etc. Notice that from these facts one is led to the conclusion that “be”, the modals and the auxiliary “have” are not Verbs, in contrast to the familiar treatment of these items as “defective verbs” (cf., e.g., Bloomfield, 1933, 223; or Austin, 1956, who discusses the fact that modals have no progressive or participial forms, and compares them in this respect to “know”, etc. – actually, there is no more reason to comment on the lack of “to-”, “-ing”, or “-en” forms of modals than on the fact that nouns do not appear in these positions). Notice also that there is no optional rule of the grammar that allows one to select “be” (though there is an optional rule that allows one to select “be + Predicate”). In this respect, “be” is quite different from most lexical items. In general, it seems reasonable to regard an item as meaning-bearing just in case selection of it is subject to an optional rule (thus most lexical items are meaning-bearing, as are optional transformations and constructions given by rewriting rules, but not, e.g., phonemes). Where the grammar provides for an optional choice, it makes sense to search for the conditions under which it is appropriate to make this choice (this being one aspect of the study of meaning). Thus it would seem reasonable to inquire into the meaning of “Predication” (i.e., choice of “be + Predicate” in rule (5iii)), but not into the meaning of “be”, which is no more subject to independent choice than are its particular variants or their individual phonemes.

A similar problem is posed by certain English comparative constructions. We have such sentences as “John received a warmer welcome than Bill”, “John is a kinder person than Bill”, and “John knows a kinder person than Bill”, where only the last is ambiguous (“than Bill is”, “than Bill does”). Furthermore, although we can have such sentences as “Bill bought a bigger house than John did”, “Mary has a bigger red balloon than John”, we do not have “Bill bought the bigger house than John did”, “Mary has a red bigger balloon than John”, “Mary has a bigger redder balloon than John than Bill”, etc. At the level of observational adequacy, a grammar might simply state a variety of facts of this kind. But we can in fact reach a higher level of adequacy in this case. Suppose that we have a transformational grammar of English constructed so as to generate in the most economical way the full range of adjectival constructions, excluding comparatives. It can be shown (cf. Smith, 1961) that a large variety of constructions involving comparatives will be generated automatically by the grammar, with the right arrangement of ambiguities, instances and apparent “exceptions”, if we add to this grammar, at the appropriate point in the sequence of ordered rules, the generalized transformation that forms the simplest comparative constructions (namely, those of the form “John is taller than Bill (is)” from “John is tall”, “Bill is tall”). Here, then, is an interesting case where it seems proper to say that the general theory of transformational grammar provides an explanation for a complex array of superficially quite disordered data.

The same point can be illustrated by an interesting example of a rather different sort. Consider the sentences:

- (6) (i) who(m) did Mary see walking to the railroad station?
 (ii) do you know the boy who(m) Mary saw walking to the railroad station?
- (7) Mary saw the boy walking to the railroad station.
- (7) is multiply ambiguous; in particular it can have either the syntactic analysis (8i) or (8ii)
- (8) (i) NP – Verb – NP – Complement
 (ii) NP – Verb – NP

where the second NP in (8ii) consists of a NP (“the boy”) with a restrictive relative clause. The interpretation (8ii) is forced if we add “who was” after “boy” in (7); the interpretation (8i) is forced if we delete “ing” in (7). But (6i,ii) are not subject to this ambiguity; the interpretation (8ii) is ruled out, in these cases. Once again, these are facts that a grammar would have to state to achieve descriptive adequacy. (Notice that there is a further ambiguity, where “Mary” is taken as the subject of “walk”, but this is not relevant to the present discussion).

The problem of explanatory adequacy is, again, that of finding a principled basis for the factually correct description. Consider how (6i) and (6ii) must be generated in a transformational grammar for English. Each must be formed by transformation

from a terminal string *S* underlying (7). In each case, a transformation applies to *S* which selects the second NP, moves it to the front of the string *S*, and replaces it by a *wh*-form.¹⁵ But in the case of (7) with the structural description (8ii), this specification is ambiguous, since we must determine whether the second NP – the one to be prefixed – is “the boy” or “the boy walking to the railroad station”, each of which is an NP. Since transformations must be unambiguous, this matter must be resolved in the general theory. The natural way to resolve it is by a general requirement that the dominating, rather than the dominated, element must always be selected in such a case. This general condition, when appropriately formalized, might then be proposed as a hypothetical linguistic universal. What it asserts is that if the phrase *X* of category *A* is embedded within a larger phrase *ZXW* which is also of category *A*, then no rule applying to the category *A* applies to *X* (but only to *ZXW*).

But now consider (7) with the analysis (8ii). Observing the general condition just given, we can apply the question transformation to this giving “whom did Mary see?” (by prefixing to (7) the full, dominating NP “the boy walking to the railroad station”, and replacing it by “whom” – but see note 15); and applying the relative transformation to (7), we can form, ultimately, “do you know the boy (who is) walking to the railroad station whom Mary saw?” (by prefixing to (7) the full, dominating NP “the boy walking to the railroad station”, replacing it by “whom” to give “whom Mary saw”, and inserting this expression in the “matrix sentence” after the full

¹⁵ This is not strictly correct. A closer analysis shows such questions to be derived from singular indefinite NP's (thus “you know a boy with (who has) a scar” – “who do you know with (who has) a scar”, “I know a boy who was expelled” – “who do I know who was expelled”, etc.; but not “you know a boy with (who has) the scar” – “who do you know with the scar”, “who do you know who were expelled”, etc.). A still closer analysis shows that they are derived from sentences with the singular indefinite unspecified NP (namely, an NP of the form “someone *X*”, “something *X*”), which is moved to initial position where the indefinite “some” is replaced by “*wh*” (giving finally “who *X*”, “what *X*”, where the *X* under certain circumstances may move again to the end of the sentence, as in “who do you know who comes from Philadelphia”). This decision is necessary to account for many details of distribution, e.g., the fact that we have “he found something of yours” – “what did he find of yours”, “he found a friend of yours”, “he found someone else” – “who else did he find”, but not “he found someone of yours”, “who did he find of yours”, “he found a boy else”, etc. Similarly, we have such phrases as “someone's book”, “whose book”; but not “something's cover”, “what's cover” (though we have “its cover”, alongside of “his book”).

Notice that if *wh*-questions were to be formed by “questioning” arbitrary NP's, then there would be many (in fact, infinitely many) sources for each such sentence – thus “who is here” could be derived from “the boy is here”, “the young boy is here”, “the tallest of all the boys in the school is here”, etc. But in fact each such question is derived from a single source where the NP is “someone (thing) *X*”. Notice, on the other hand, that the otherwise rather similar relative transformation does not restrict the NP replaced by “who (what)” in this way, presumably, because the actual form of this NP is still determinable from the full transform, in this case, since the NP is shared by matrix and constituent sentences. This illustrates what is apparently another general feature of a transformational grammar, namely, that major categories have associated with them a “dummy terminal symbol” as a member (which may actually be realized, e.g., “it” for abstract Nouns, “someone (thing)”), and that this representative of the category is what actually must appear in the underlying strings for those transformations where the transform carries no indication of the actual terminal representative of this category in the underlying string. This fact is particularly important for the study of the limits on generative capacity of transformational grammars.

NP “the boy (who is) walking to the railroad station”). But neither the question transformation nor the relative transformation would now be applicable to the dominated NP “the boy” in (7) with the Phrase-marker (8ii); hence neither (6i) nor (6ii) is derivable from (7) with this Phrase-marker, and neither (6i) nor (6ii) can have this interpretation. Thus the general condition just proposed explains the fact stated in the descriptively adequate grammar. It predicts, on general grounds, that this must be the linguistic intuition of anyone who constructs for himself a transformational grammar to deal with the linguistic data to which he has been exposed.

This proposed explanation receives further support from many other examples. Thus we cannot derive “what did he know someone who has (of yours)?” from “he knew someone who has something (of yours)”, where the NP “something (of yours)” is embedded within the NP “someone who has something (of yours)”. But we can derive “who did he know who has something (of yours)?” (through the intermediate stage “who who has something of yours did he know” – cf. note 15 – where the full dominating NP *someone X* is prefixed). Similarly, we can derive “what did he see the man read?” from “he saw the man read the book”; and we can derive “what that was on the table did you see the man read?” (optionally, “what did you see the man read that was on the table?” – cf. note 15) but not “what did you see the man read the book that was on?” from “you saw the man read the book that was on the table”. Similarly, from “it is difficult for me to understand him”, where the NP “him” is not embedded within another NP, we can form, ultimately, “he is a person whom it is difficult for me to understand”. But from “for me to understand him is difficult”, where the NP “him” is embedded within the NP “for me to understand him”, we cannot, by the same process, derive “he is a person whom for me to understand is difficult”.

The same principle applies to the formation of relative clauses. Without going into details,¹⁶ it is clear that sentences with relative clauses are constructed from a pair of underlying sentences that share an NP. By the general principle just suggested, this shared NP must not be contained within another NP. But notice, in fact, that we cannot form “I saw the boy who who had the book left” from the pair (“I saw *the boy*”, “*the boy* who had the book left”), where the underlined NP is shared. Nor can we form “I read the book that the boy who had, left” from (“I read *the book*”, “the boy who had *the book* left”), where again the shared NP is dominated by an NP. Notice also that such sentences as “the man who comes from Philadelphia who you met is retired” is not ambiguous, as it should be if it were derivable from either (“*the man who comes from Philadelphia* is retired”, “you met *the man who comes*

¹⁶ It is hardly necessary to warn the reader of the informality of these descriptions. Notice in particular that where sentences are said to be “derived from other sentences”, what should be said, actually, is that they are derived from the abstract forms (categorized terminal strings) underlying other sentences. Notice also that such rules as those describing the wh-question and relative transformations must be regarded as constituting not a transformation, but rather a family of transformations, in the sense of Chomsky (1955, chapter 8), the k^{th} member of which takes the k^{th} available NP and performs the appropriate operations.

from Philadelphia”) or (“the man who you met is retired”, “the man comes from Philadelphia”). But in fact the latter pair is ruled out as a possible source by the general condition suggested above, since the shared NP “the man” is embedded within a dominating NP “the man who you met”. Similarly, from the pair (“the decision was discussed”, “the decision to leave surprised me”) we cannot form “the decision which to leave surprised me was discussed” or “the decision which was discussed to leave surprised me”. This again follows from the proposed general principle, since the phrase “the decision” in the second sentence of the pair is embedded within the NP “the decision to leave”.

Although this account still leaves much unsaid, and several qualifications are necessary, the principle seems well-supported and formally well-motivated, and thus can be proposed as a general hypothesis concerning linguistic structure, to be tested in terms of the consequences to which it leads in various languages.

Consider now one final example from the domain of syntax. Such sentences as

(9) I don't approve of his drinking (cooking, driving, etc.)

are ambiguous (. . . the fact that he drinks, cooks, etc.; the manner in which he drinks, cooks, etc.)¹⁷ An explanation for this is proposed in Chomsky (1955), and it can now be given a much better formulation as well as stronger support by several ingenious observations of Lees (1960a, 64f.) and Klima (personal communication). Among the many ways of converting declarative sentences into NP's in English (cf. Lees, 1960a), we have, in particular, two that can be described informally as follows:

(10) NP – Aux₁ (Aux₂) VP₁ ⇒ NP + Possessive – ing (Aux₂) VP₁

(11) NP – Aux – Verb – (NP) ⇒ NP + Possessive – nom + Verb – (of + NP).

The transformation (10) gives such noun phrases as “his refusing (having refused) to participate”, “his rejecting the offer”, “his (having been) destroying property”, etc.; while (11) gives such examples as “his refusal to participate”, “his rejection of the offer”, “his destruction of property”, etc. But the phrases constructed by (10), (11) must be inserted into other sentences in the NP position by a generalized transformation. And this insertion is carried out differently in the two cases. In the case of (10), the transform as a whole replaces the NP of the sentences into which it is inserted; thus the derived Phrase-marker of “his rejecting the offer surprised me”¹⁸ will indicate simply that “his rejecting the offer” is an NP. In the case of (11), however, the element NP + Possessive replaces the Determiner of an NP of the form Determiner + Noun, while the element nom + VP₁ replaces the Noun of this NP.

¹⁷ In the case of “cooking”, there are, in fact, two more interpretations, since “cooking” is a Noun independently of the transformations (10), (11) below and “cook” is one of those Verbs that undergo the transformation of NP₁ – V – NP₂ to NP₂ – V (cf. Chomsky, 1958; Gleitman, 1960) giving “NP cooks” (which is then subject to (10)) from, e.g., “they cook NP”.

¹⁸ For discussion of how transformations impose derived phrase structure, see Chomsky (1955, 1961a), Matthews (1962), Postal (1962).

Thus the derived Phrase-marker of “his rejection of the offer surprised me” will indicate that “his rejection of the offer” is an NP, that “his” is a Determiner and that “rejection of the offer” is a Noun. There are several facts that motivate this decision. For one thing, note that in the case of the phrases formed by (11) (but not those formed by (10)), adjectives can be inserted. Thus we can have “his strange refusal to participate”, “his unexpected rejection of the offer”, “his wanton destruction of property”, etc.; but not “his strange refusing to participate”, “his unexpected rejecting the offer”, “his wanton destroying property”. But adjectives are introduced by transformation¹⁹ in the position Determiner — Noun. Consequently, for the adjectivalization transformation to operate properly, this structure must be specified in the derived Phrase-marker of the NP formed by (11). Secondly, note that the position of the “NP + Possessive” construction in an NP formed by (11), but not (10), can be filled by “the” (“the refusal to participate”, “the rejection of the offer”, “the destruction of property”; but not “the refusing to participate”, “the rejecting the offer”, “the destroying property”). This indicates that paired with (11) is an otherwise identical transformation that replaces the Noun of the matrix sentence by “nom + Verb (of NP)”, leaving the Determiner “the” unaffected, and again shows that the paired transformation (11) replaces the Determiner (which is, in fact, “the”) of the matrix sentence by the “NP + Possessive” construction, which thus takes on the structure Determiner by the general rule for substitution transformations (cf. references of note 18).

But now observe that although (9) is ambiguous, both (12) and (13) are quite unambiguous:

- (12) I don't approve of his drinking the beer (driving a sports car)
 (13) I don't approve of his excessive drinking (careless driving).

Furthermore, they have opposite interpretations. Thus (12) refers to the fact of his drinking the beer, driving a sports car, etc.; while (13) refers to the manner of his drinking (of the beer), of his driving, etc. The fact that adjectives can appear in (13) implies that in this case the phrases “his drinking”, “his driving”, etc., have the derived phrase structure Determiner-Noun, as in the case of “his rejection of the offer”. They must thus be formed by the transformation (11). And observe, in fact, that there is no other nominalized form of these verbs (as “refusal” and “rejection” contrast with “refusing” and “rejecting”). Hence we conclude that there is an obligatory rule that assigns to the nominalizing morpheme *nom* introduced in (11) the shape /*ing*/ when it is affixed to “drink”, “drive”, etc., just as it assigns to *nom* the shape /*æl*/ when it is affixed to “refuse” and the shape /*yɪn*/ when it is affixed to “reject”.

It follows that “drinking”, “driving”, etc., will be formed in two distinct ways,

¹⁹ In Chomsky (1955, 1958) this is given as a separate adjectivalization transformation, but J. Aplegate has since pointed out that modifying adjectives must rather be introduced by a transformation of sentences with relative clauses, and this is the method that has been followed in Lces (1960), Smith (1961).

by (10) and by (11). Since these verbs are, furthermore, optionally intransitive, the full NP "his drinking", "his driving", etc., will also be generated in two ways, once by (10) (with the derived structure NP and the interpretation "fact that") and once by (11) (with the derived structure Determiner + Noun, as well as NP, and the interpretation "manner of"). Noting that adjectives cannot be inserted in (12) (giving, e.g., "I don't approve of his excessive drinking the beer"), we conclude that this is unambiguously derived by (9), consistent with its interpretation, in this case.

Notice that as the wh-question transformation was formulated, it does not yield "whose book (did you find)?", "which book (did you find)?", etc. To form these, it must be extended to apply also to underlying strings of the form X – Determiner + Noun – Y (note that possessive NP's are Determiners, replacing the definite article, in fact, by a transformation). Applying this observation to the present case, we see that this transformation will yield "whose excessive drinking surprised you?", etc., as it should, but that it will exclude "whose drinking the beer surprised you?", etc. (again, correctly), since the underlying NP in this case is not of the form Determiner + Noun. Similarly, "whose drinking surprised you?" will be derived from only one source (and it is, in fact, unambiguous), since only one of the potential sources is of the required form Determiner + Noun.

The sentences of (9) provide an extremely interesting example of syntactic ambiguity. Syntactic ambiguity is generally traceable to derivations from different underlying sentences (e.g., "flying planes can be dangerous" from "they fly planes" or "planes fly"). But in this case, there appears to be a single source for (9) (namely, the pair of terminal strings underlying the pair: "I don't approve of it", "he drinks"). Furthermore, both derivations of (9) follow essentially the same transformational path – in both cases a nominalized version of "he drinks" replaces the abstract NP of "I don't approve of it". The two alternative transformations (10) and (11) generate the same string, in this case, but assign to it different derived Phrase-markers. In this rather subtle difference between the two transformations, and the semantic property associated with them, lies the ambiguity of (9), so it appears.

It seems clear that examples such as these are totally beyond the range of any version of the taxonomic model, as so far conceived. But again, it seems possible to achieve the levels of descriptive and even explanatory adequacy with a transformational grammar.

2.3. *Levels of adequacy in semantics.*

I have given several examples of how a higher level of adequacy might be achieved by linguistic theory in the domains of phonology and syntax. It remains to consider the third major part of a synchronic description, namely, its semantic aspect. Here the problem is much more obscure. One might perhaps maintain that the condition of observational adequacy would be met by an account of situational regularities associated with actual discourse;²⁰ and that the condition of descriptive adequacy

²⁰ What are called "semantical regularities" by Ziff (1960a).

is in part achieved by a set of appropriately interrelated dictionary entries, an explicit portrayal of the structure of certain "semantic fields", a list of terms that enter into specific meaning-relations, e.g., synonymy, etc.

How might one hope to achieve a higher level of adequacy, in this case? It might plausibly be maintained that certain semantic features of a language can be partially explained in terms of underlying syntactic processes. As an example, consider the discussion of (9), above. Or consider the case of such adjectives as "interesting", "astonishing", "intriguing", etc., which have the semantic property that they are "connected with a specific human 'reaction',"²¹ even where no explicit reference is made to the person who is interested, astonished, intrigued ("it was an intriguing plan", as distinct from "it was an elaborate plan", etc.). These adjectives have in common many important syntactic features that distinguish them from other Verb + ing forms (e.g., "the plan seems intriguing (*failing)", "a very intriguing (*failing) plan", etc.). Furthermore, they would be derived, in a transformational grammar, from sentences in which they appear as Verbs ("the plan intrigues one", etc. – cf. Chomsky, 1958). But the class of verbs from which these adjectives derive are pure transitives with human objects.²² Thus the structural description of the sentence "it was an intriguing plan", as provided by a transformational grammar, will contain the terminal string underlying "the plan intrigued one (i.e., unspecified human)" just as explicitly as it contains the past tense morpheme; and this fact might be suggested as the explanation for the cited semantic feature.

In general, as syntactic description becomes deeper, what appear to be semantic questions fall increasingly within its scope;²³ and it is not entirely obvious whether or where one can draw a natural bound between grammar and "logical grammar" in the sense of Wittgenstein and the Oxford philosophers. Nevertheless, it seems clear that explanatory adequacy for descriptive semantics requires, beyond this, the development of an independent semantic theory (analogous, perhaps, to the general theory of grammar as described above) that deals with questions of a kind that can scarcely be coherently formulated today, in particular, with the question: what are the substantive and formal constraints on systems of concepts that are constructed by humans on the basis of presented data? Observe that the problem posed in § 1 for general linguistics is a special case of this question, where the system of concepts that is acquired consists of the notions "well-formed sentence of L", "grammatical relation in L", "sound pattern of L", etc. Perhaps it is not too much to hope that this parti-

²¹ Cf. Nowell-Smith (1954, 85). Other adjectives may also be characterizable in this way for some different reason, but this is irrelevant to the correctness of the present observation.

²² That is, "intrigue", "astonish", etc. do not undergo optional deletion of the object, as do "cook", "eat", etc.; and such sentences as "John amused the book" are clearly deviant. These observations are not refuted by the fact that deviant utterances with object deletion can be attested (cf., e.g., Wilson, *The American Earthquake*, 481: "The American Legion Posts, which dominate the later sections, startle trouble and shock", where all three verbs belong to the category in question), just as the distinction between the classes of adjectives noted above is not obscured by instances such as "if the sea was not very raging, . . ." (Russell, *Inquiry into Meaning and Truth*, 84). See note 2 and references there.

²³ For discussion see Harris (1954), Chomsky (1957a), Ziff (1960a), Katz and Fodor (1963).

cular problem may serve as a useful paradigm case. We return to this speculation below in § 5. In any event, it seems that formulation of a general semantic theory of some sort, independent of any particular language, is perhaps not an unreasonable task to undertake, and is a precondition for any far-reaching attempt to attain a level of explanatory adequacy in semantic description.

2.4. *Comprehensiveness of grammars.*

In the preceding discussion, three levels of adequacy have been loosely sketched that might be attained by a linguistic description in the areas of phonology, syntax, and semantics. Of these, only the levels of descriptive and explanatory adequacy (and, ultimately, only the latter) are of sufficient interest to justify further discussion. Notice, however, that these levels of success are discussed only for grammars that are paired with some linguistic theory. It is always possible to describe the linguistic intuition of the native speaker in a completely ad hoc way, in any particular case, if we drop the requirement that the grammar be constructed in accordance with some fixed model or if we allow the associated linguistic theory to be completely general and without content (e.g., if our linguistic theory merely states that a grammar is an arbitrary computer program). In this case, we can simply add any given fact to the grammar. Presumably, this possibility needs no further discussion. It is important to bear in mind that a grammar that assigns correctly the mass of structural descriptions (remote as this is from present hopes) would still be of no particular linguistic interest unless it also provided some insight into those formal properties that distinguish a natural language from an arbitrary enumerable set of structural descriptions. At best, such a grammar would help clarify the subject matter for linguistic theory, just as a fourteenth century clock depicting the positions of the heavenly bodies merely posed, but did not even suggest an answer to the questions to which classical physics addressed itself.

In connection with the question of levels of success, we must also briefly consider the matter of coverage of data. Sapir's often quoted remark that "all grammars leak" is extremely misleading, insofar as it implies that there are grammars so comprehensive that the question of completeness of coverage can seriously be raised. But this is patently false. In the case of traditional (i.e., inexplicit generative) grammars the gaps are not easy to locate because of the vagueness of the rules and the essential reliance on the linguistic intuition of the reader. One of the merits of an explicit generative grammar is that these gaps are immediately exposed. Anyone who is actively at work on a linguistic description can cite innumerable examples that fall beyond the range of rules as so far formulated, or that are incorrectly handled by these rules – it is, in fact, sufficient to open a book or to listen to a conversation at random to find countless examples of sentences and sentence types that are not adequately dealt with in traditional or modern grammars.

Comprehensiveness of coverage does not seem to me to be a serious or significant goal in the present stage of linguistic science. Gross coverage of data can be achieved

in many ways, by grammars of very different forms. Consequently, we learn little about the nature of linguistic structure from study of grammars that merely accomplish this. Higher levels of adequacy, in the sense described above, have been achieved so far only in limited areas. But only by studying the properties of grammars that achieve higher levels of adequacy and by gradually increasing the scope of description without sacrificing depth of analysis can we hope to sharpen and extend our understanding of the nature of linguistic structure.

It is important to bear this in mind in considering the masses of linguistic data that lie beyond the scope of an explicit generative grammar, proposed for some fragment of a language. It is no criticism of this grammar to point to data that is not encompassed by its rules, where this data has no demonstrated bearing on the correctness of alternative formulations of the grammar of this language or on alternative theories of language. Until incorporated in an explicit generative grammar, such examples simply stand as exceptions, no more relevant to the correctness of the already formulated rules than strong verbs and irregular plurals. Listing of innumerable examples is neither difficult nor very interesting; it is quite another matter to find rules that account for them, or a general theory of such rules.²⁴

It is necessary to distinguish between exceptions to a grammar, and counterexamples to a proposed general theory of linguistic structure. Examples that lie beyond the scope of a grammar are quite innocuous unless they show the superiority of some alternative grammar. They do not show that the grammar as already formulated is incorrect. Examples that contradict the principles formulated in some general theory show that, to at least this extent, the theory is incorrect and needs revision. Such examples become important if they can be shown to have some bearing on alternative conceptions of linguistic structure.

3. ON OBJECTIVITY OF LINGUISTIC DATA

When we discuss the levels of descriptive and explanatory adequacy, questions immediately arise concerning the firmness of the data in terms of which success is to

²⁴ These comments apply, it seems to me, to most of the examples presented by Bolinger (1960, 1961). These lists of examples could be extended indefinitely. In the form in which they have been presented, they have, for the most part, no obvious bearing on the correctness of formulations of English grammar that have been proposed for certain fragments of the language, or of the theories that underlie them.

Bolinger does suggest (1961, 381) that his examples are in conflict with certain theories of generative grammar, and that they support an alternative view about the nature of language, about which he offers only the following hint: in a grammar of the sort he envisions, "constructions are not produced one from another or from a stock of abstract components, but filed side by side", and the speakers do not 'produce' constructions, but rather "reach for" them, from a preestablished inventory". It is difficult to comment on the proposal in this form, because of the vagueness of the notions "construction" and "filed". If by "construction" Bolinger means something like "sequence of word classes", then his proposal is ruled out at once. It is clear that the variety of normal sentences is so great that the number of word class sequences associated with them is far larger than the number of seconds in a lifetime. For quantitative estimates bearing on this question (which are furthermore highly conservative) see Miller, Galanter, Pribram (1960), Miller and Chomsky (1963). If he has in mind some more abstract principle by which constructions are "filed", it remains to be seen whether this proposal, when clearly formulated, will differ from current theories of generative grammar.

be judged (nor are difficulties lacking even on the level of observational adequacy – cf. note 8). For example, in the case of (3), (4) one might ask how we can establish that the two are sentences of different types, or that “John’s eagerness to please . . .” is well-formed, while “John’s easiness to please . . .” is not, and so on. There is no very satisfying answer to this question; data of this sort are simply what constitute the subject matter for linguistic theory. We neglect such data at the cost of destroying the subject. It is not that these introspective judgments are sacrosanct and beyond any conceivable doubt. On the contrary, their correctness can be challenged and supported in many ways, some quite indirect. Consistency among speakers of similar backgrounds, and consistency for a particular speaker on different occasions is relevant information. The possibility of constructing a systematic and general theory to account for these observations is also a factor to be considered in evaluating the probable correctness of particular observations (as in the case of any data – cf. note 8). Consequently the fact that a certain grammatical theory has had explanatory value in dealing with data from one language may be an important factor in determining the validity of data from some different language. Operational tests that consistently supported introspective judgment in clear cases would, were they available, also be relevant in determining the correctness of particular observations.

It is sometimes assumed that operational criteria have a special and privileged position, in this connection, but this is surely a mistake. For one thing, we can be fairly certain that there will be no operational criteria for any but the most elementary notions. Furthermore, operational tests, just as explanatory theories, must meet the condition of correspondence to introspective judgment, if they are to be at all to the point. Thus a test of degree of grammaticalness that failed to make distinction between, e.g., “colorless green ideas sleep furiously” and “furiously sleep ideas green colorless” would, to this extent, prove itself to be an uninteresting test. When a criterion (operational or not) is proposed for some notion, we must first inquire, where the criterion is clear, whether the concept it delimits is at all close to the one in which we are interested.

It is surprising how frequently this obvious point is overlooked. Thus many linguists have proposed that synonymy be measured somehow in terms of degree of distributional similarity (cf., e.g., Hoenigswald, 1960; Frei, 1961), and have then concluded that such pairs as “bachelor” and “unmarried man” are not synonymous, since one, but not the other, can occur in the context – *hood*, etc. But all that this observation shows is that the proposed criterion is entirely wrong, as, indeed, it clearly is.²⁵ However synonymy may ultimately be analyzed, it is a fact that a speaker of English need undertake no empirical investigation to determine whether some bachelors are married, as he must to determine whether some bachelors are red-

²⁵ A critical and still unanswered objection to any such approach has been given by Bar-Hillel (1954, 233). Frei also gives a “distributional” argument against the existence of homonyms (40), but again this is simply a proposal for terminological revision. He regards these terminological innovations as refuting the position (argued in Chomsky, 1957a) that there is no evidence for the claim that the notion of phonemic contrast can be defined in terms of sameness of meaning in a way which will

haired; and such facts as this provide the basis for the conclusion that there is a meaning relation between “bachelor” and “unmarried man”. An analysis (such as the proposed distributional analysis) of these meaning relations which is inconsistent with these facts is, to that extent, shown to be wrong.

Similarly, consider Quine’s proposed quasi-operational definition of a concept of “stimulus meaning” (1960). As this is defined, the stimulus meaning of a word varies widely with level of attention, set, gullibility, mood, visual acuity, cortical lesions, etc., while the meaning and reference of a term are independent of these factors. These, and the many further discrepancies,²⁶ suggest that the concept has little relevance to the study of meaning and reference; consequently, it is not at all clear why any serious consideration should be given to this particular operational test. Quine’s concern with it appears to stem from his belief that it provides all of the objective information that can be obtained about any language (e.g., 39), and that all additional assumptions about a language are “arbitrary” and “unverifiable” (71-2, 80) since they are “undetermined by the speech dispositions” and might conceivably be “due to linguistic ingenuity or lucky coincidence” (Quine’s thesis of “indeterminacy of of translation”, and, also, of grammar, since he regards grammar as somehow based on translation – cf. 68f.). But he offers no argument for the belief that this particular operational test, among the many that might be proposed, has some unique significance; and the thesis of indeterminacy seems to amount only to the assertion that a significant empirical assertion has logically conceivable alternatives, which is true but unexciting.²⁷

provide a semantic basis for phonology. But in fact, he mistakes the question at issue, which was this: given a set of sentence tokens to which meaning is somehow assigned, can this information be used to determine which of these tokens contrast? Presumably, those who maintain that phonology can or must be based on meaning are claiming that the answer is affirmative. But if Frei is correct in assuming (41-2) that meaning can be assigned only to an element of *langue*, not to tokens (as, in fact, is also argued in Chomsky, 1957a, 98), then the claim under discussion is automatically shown to be vacuous.

²⁶ The stimulation X belongs to the (affirmative) stimulus meaning of the sentence Y if presentation of X prompts assent to Y (with various qualifications that are not relevant here). But in general, an object is correctly called a Y not just because of its appearance, but because of its function, or even its “history” (cf. comments by P. Foot, 1961, 47f). The other notions defined in terms of “stimulus meaning” are likewise of dubious interest. Thus “stimulus analyticity”, as defined, would seem to hold of many universally shared beliefs (e.g., “there have been some black dogs”, or “the world is flat”, at one period), and thus sheds little light on the important (but, as Quine has elsewhere demonstrated, quite obscure) notion of “connection of meaning”.

²⁷ What seems open to question in this account is only the use of the words “arbitrary” and “unverifiable” to apply to empirical hypotheses that do not merely summarize evidence, that is, to all non-trivial assertions of science or common sense, to X’s belief that Y is using “tomorrow” in the sense of X’s “tomorrow” and not his “yesterday”, etc. Furthermore, it seems that Quine’s own discussions of indeterminacy of reference (e.g., 52f.; cf. also 78-9) should be unintelligible, on his own grounds, for his hypothesis that his readers do not understand his “rabbit” in the sense of “rabbit stage”, etc., is “unverifiable and “arbitrary”, as he uses these terms.

Notice, in this connection, that though given a finite amount of evidence, it is trivially true that there are conflicting hypotheses compatible with it, it does not follow that there are certain conflicting hypotheses among which no decision can be made by any possible obtainable evidence. Given a decision to restrict evidence to “stimulus meaning”, one no doubt could find irresolvable conflicts, but this would be an uninteresting consequence of an arbitrary decision.

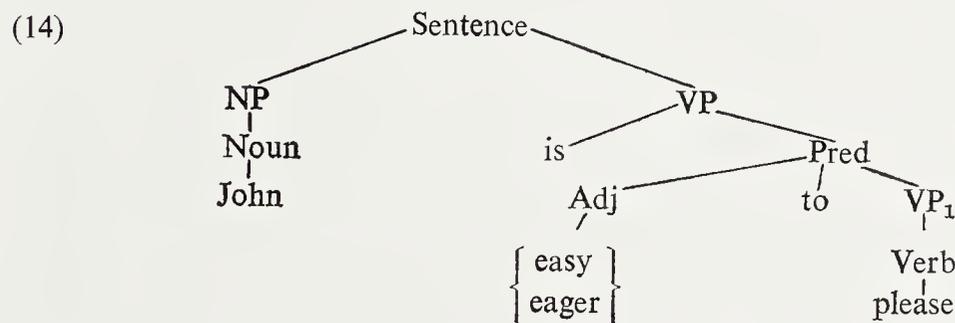
In these and many other cases, what has not been shown is that the concept defined by the proposed operational criterion has some importance. In fact, at the present stage of the study of language, it seems rather obvious that the attempt to gain some insight into the range of data that we now have is likely to be far more fruitful than the attempt to make this data more firm, e.g., by tests for synonymy, grammaticality, and the like. Operational criteria for these notions, were they available and correct, might soothe the scientific conscience; but how, in fact, would they advance our understanding of the nature of language, or of the use and acquisition of language?

4. THE NATURE OF STRUCTURAL DESCRIPTIONS

A generative grammar contains a syntactic component and a phonological component. The former generates strings of formatives and specifies their structural features and interrelations. The latter converts a string of formatives with a specified syntactic structure into a phonetic representation. After a brief discussion of structural descriptions on the syntactic level, we will turn to a more detailed account of alternative views as to the nature of phonological representation.

4.1. *The syntactic component.*

A structural description on the syntactic level must indicate how a string of formatives is subdivided into constituents of varying scope (from formatives, at one extreme, to the full sentence, at the other) and what are the categories to which these substrings belong (Noun, Verb, Noun Phrase, Relative clause, etc.).²⁸ Such information can be presented as a labelled bracketing of a string of formatives or in some equivalent notation, e.g., a labelled tree such as (14) for the sentences (3), (4).



Such a representation is what we have called a *Phrase-marker*. In terms of such Phrase-markers, we can define grammatical relations, as certain subconfigurations.

²⁸ The goal of traditional "universal grammar" was, of course, to give a substantive general account of these categories, thus fixing a universal "vocabulary" for the generative grammars of all languages. Presumably, such fixed universal category symbols would have to be defined in terms of formal properties of grammars and, perhaps, language-independent semantic properties of some sort. Whatever the feasibility of this enterprise may be, we will not consider it here, and will regard the category names for the time being as only conventional.

Thus the Subject-Predicate relation might be identified as the subconfiguration (Sentence; NP, VP), in which case it would hold between "John" and "is easy (eager) to please" in (14); and the Verb-Object relation could be defined as the configuration (VP₁; Verb, NP), in which case it would hold between "please" and "John" in "this may please John", with the obvious Phrase-marker; etc. Alternatively, these relations can be defined in terms of the heads of such configurations. For detailed proposals along these lines, see Chomsky (1955, chapter 6). It is the great merit of Pike's recent work in tagmemics to have focused attention on the importance of these notions.

Whatever exact decisions are made, it is evident, as observed in § 2, above, that a great deal of relevant information is not representable in terms of a single Phrase-marker such as (14). Thus there is no way to indicate, in such a representation, that when the adjective is "easy", the relation of "please" to "John" in (14) is that of Verb-Object, as in "this pleases John"; and that when the adjective is "eager", the relation of "John" to "please" in (14) is that of Subject-Verb, as in "John pleases us". Similarly, there is no way to indicate by a single labelled bracketing that in the sentence (15) the expressions "John", "please" and "gift" are related as they are in (16):

(15) did John expect to be pleased by the gift?

(16) The gift pleased John.

For reasons such as this, the taxonomic model of modern linguistics (cf. § 1, above), which provides a single Phrase-marker of the type of (14) as a structural description of a string, must be regarded as descriptively inadequate.

One might attempt to overcome this inadequacy by extending the definition of "grammatical relation" in the following way. Let us say that a grammatical relation holds of a certain pair (triple, etc.) of expressions (I) if they form part of a configuration of a Phrase-marker, as described above, or (II) if a "co-occurrence relation" of an appropriate sort²⁹ holds between the pair in question and a pair that has this grammatical relation in the sense of (I). Accordingly, we would say that in

²⁹ For a careful definition of one such notion, see Hiz (1961). This notion was introduced by Harris (1952a, 1952b) and studied in detail (Harris, 1957) as the basis for a theory of grammatical transformations. It is also mentioned in a similar connection by Bazell (1953), and is applied to Russian in Worth (1958). A grammatical transformation is defined, from this point of view, as a (symmetrical) relation holding between two sentence forms if corresponding positions in the two forms are filled by the same n-tuples of expressions. This relation is not part of generative grammar, as is the notion "grammatical transformation" of § 1 and the references cited there, but is a structural relation holding of sentences and sentence forms described by a taxonomic, IC grammar (as in Harris, 1951a, chapter 16). The notions of "co-occurrence relation" and "generative transformation" are rather different in formal properties as well as in their role in actual syntactic description, and a great deal of confusion can result from failure to distinguish them. Thus it makes no sense to arrange co-occurrence relations "in sequence", but generative transformations can (and, in practice, must) be ordered and applied in sequence. The examples of § 2 depend essentially on appropriate ordering and sequential application of transformational rules, and on appropriate choice of base versus derived forms (a distinction which is also not definable in terms of co-occurrence). Furthermore, co-occurrence is a relation defined on actual sentences, while generative transformations apply to abstract structures that often bear no close relation to actual sentences. Note also that in a generative transformational grammar, a direct, one-step transformational relation would hold between (16) and each of the sentences of (17);

- (17) (i) did the gift please John?
 (ii) John was pleased by the gift.

where the grammatical relations are not expressible directly in terms of subconfigurations of the Phrase-marker, the Subject-Verb and Verb-Object relations hold of the pairs "the gift" – "please" and "please" – "John", respectively, because any triple of expressions that can replace "the gift", "please" and "John" in (17) can also (with appropriate reordering) fill the positions of these expressions in (16), where the grammatical relations are definable directly in terms of the Phrase-marker.

However, this approach seems to me to face insurmountable objections. Thus although it is true that a co-occurrence relation of the appropriate sort holds between (16) and (17), it does not hold between (15) and (16), or (18i) and (16). Thus "please" – "John" can be replaced by "bring" – "happiness" in (16), but not in (15) or in (18i); but in all three cases these expressions are related as Verb-Object. And if some modification is proposed to deal with this discrepancy, will it be able to distinguish the grammatically related "please" – "John" in (15) from the same pair, grammatically unrelated, in "did John expect you to be pleased by the gift?" Or consider the sentences (18ii-iv):

- (18) (i) the gift pleased John but not Bill
 (ii) the book is what I want
 (iii) I want the book
 (iv) the clever boy saw the friendly man

In both (18ii) and (18iii), the Verb-Object relation holds of the pair "want"–"the book"; but only in (18iii) can this pair be replaced by "met"–"the boy". In (18iv), "clever" and "boy" are related as in "the boy is clever"; but in the latter, though not in (18iv), the pair "plan"–"intriguing" can replace "boy"–"clever". Furthermore, it seems that any pair that can replace "clever"–"boy" in (18iv) can replace "clever"–"man" in the same sentence, though no grammatical relation at all holds of this pair.

It is, of course, impossible to show that no possible modification of the notion of co-occurrence could deal with such problems. However, for the present it seems

a somewhat more devious relation would hold between (16) and (15), which is derived by a sequence of transformations from a pair of strings, one of which underlies (16); and no relation at all would hold between (15)-(17i), or (17i)-(17ii), though all would be based on the terminal string underlying (16). From the point of view of co-occurrence, however, there is a "one-step" relation between both (16)-(17), and (17i)-(17ii), and no relation at all (because of "the gift brought happiness", etc.) between (15) and (16). Similarly, no co-occurrence relation would hold between (18iii) and (18ii) (because of "I met the boy"), though the latter is derived from the former by a sequence of generative transformations. There are many other differences.

Harris' notion of transformation as a co-occurrence relation developed in the course of his work in the late 1940's on analysis of the structure of extended discourse. At the time, I was attempting to construct generative grammars for Modern Hebrew and English using Harris' morpheme-to-utterance procedures, with variables over 'long components', as a model for the syntactic component. There were serious difficulties in this, and the notion of grammatical transformation, when adapted and redesigned to enter the syntactic component of a generative grammar with ordered rules, seemed to overcome most of these.

clear that any such theory as the theory of phrase structure grammar that assigns a single Phrase-marker such as (14) to an utterance is incapable of expressing deeper structural relations, and must therefore be ruled out by considerations of descriptive adequacy.³⁰

In a transformational grammar, the syntactic description of a string of formatives consists of a set of Phrase-markers (one for each of the underlying simple strings from which the string is derived), a *derived Phrase-marker* such as (14), giving its superficial constituent structure (its IC structure) and grammatical relations, and a *Transformation-marker* which expresses the manner of its derivation from underlying strings.³¹ In the case of the examples that we have discussed, the deeper structural information would be provided by the Phrase-markers of the underlying strings (for details concerning the particular example (14), see Miller and Chomsky, 1963; see the references cited previously for the other cases). For the present, the transformational model for generative grammar is unique in that it allows for the generation of structural information of a variety rich enough to account for facts of the kind discussed here and in § 2, above – and, furthermore, to do so in many cases in a principled way, thus reaching the higher level of explanatory adequacy – though it is by no means without its problems.

4.2. *The phonological component.*

The phonological component of the grammar can be regarded as an input-output device which operates on a string of formatives, provided with a structural analysis by the syntactic component, and assigns to this string a representation as a string of phones. It is, in part, an open question to what extent structural information on the syntactic level is relevant to determining the phonetic form of a string of formatives. There is no doubt that information of the kind provided in the derived Phrase-marker is essential,³² and there are scattered examples that suggest that deeper syntactic features may also play a role in determining the details of phonetic shape.

A rather classical view of the structure of the phonological component might be something like this. Formatives are of two types: grammatical and lexical (among the grammatical we count, as subtypes, class markers and junctural elements introduced by syntactic rules, e.g., word boundary). Each grammatical formative is represented by a single symbol. Each lexical formative is represented in a systematic orthography as a string of symbols, each of which is assigned to certain categories (Vowel, Consonant, Voiced, etc.). Each symbol can, in fact, be regarded as an abbreviation for the set of categories to which it belongs, and each lexical item can thus be represented by a *classificatory matrix* in which the columns stand for what we

³⁰ Many other difficulties in the theory of phrase structure grammar are discussed in Chomsky (1955, 1957a, 1961a), Postal (1961, 1964).

³¹ For discussion of these matters, see the references of note 18. There are many open and difficult questions here, but the general outlines of a satisfactory theory seem clear.

³² See the references of notes 6, 11, and 12 for details. All of these studies are based on the notion “transformational cycle” sketched in § 1.

can call "segments" and the rows, for categories; the entry in the i^{th} row and j^{th} column indicates whether the j^{th} segment belongs to the i^{th} category. These categories, we might call (classificatory) distinctive features. Some squares of the matrix may be blank, where the feature in question can be supplied by a general rule (e.g., the entry for Rounding in the case of English Lax Front Vowels).

The rules of the phonological component are ordered, and apply in sequence to the string of formatives (utilizing, when this is relevant, the associated syntactic information) until ultimately a representation is reached in terms of a universal phonetic alphabet. The symbols of this alphabet are specified in terms of a set of phonetic features; hence the output of the phonological component can again be regarded as a matrix in which columns represent phones and rows, phonetic features of the universal system. The entry in the i^{th} row and j^{th} column indicates whether the j^{th} phone of the generated utterance possesses the i^{th} feature, or the degree to which it possesses this feature (in the case of such features as stress). Classificatory distinctive features are by definition "binary"; phonetic features may or may not be. A representation in terms of phonetic features we can call the *phonetic matrix*, again regarding the symbols of the universal phonetic alphabet as mere conventional abbreviations for sets of feature specifications.

The universal phonetic alphabet is part of a universal phonetic theory. In addition to a fixed set of features, such a theory should contain general laws concerning possible combinations and contrasts. Steps toward such a theory are found in the work of the classical British phoneticians (Bell, Ellis, Sweet); in the "phonologie" of de Saussure's 1897 lectures;³³ and again in Jakobson's theory of distinctive features and phonetic universals (e.g., Jakobson, Fant and Halle, 1952). This theory constitutes a part of general linguistic theory, exactly as do the set of restrictions on the form of rules and the other constraints on the structure of a generative grammar. We will refer below to the requirement that a general linguistic theory must incorporate a universal phonetic theory, with a fixed alphabet, as the condition of *phonetic specificity*. Note that a universal phonetic alphabet is the counterpart of a substantive theory of syntactic categories (see above, note 28) that assigns a fixed significance to the labels used in the syntactic component; but in the case of a phonetic alphabet, the construction of a concrete and substantive theory has, of course, been much more fully realized.

Let us assume that at a certain stage in the application of the rules of the phonological component, all grammatical formatives except junctures will have been eliminated and we will have a representation in terms of classificatory matrices and junctures alone (with derived phrase structure indicated). At this point, for example, English "saw", which at the input stage is /sī/ + *past*, would be represented /sō/

³³ Thus, for example, he claims that Nasalization is never distinctive for liquids in any language (1916; 74), and consequently need not be specified in the representation of, e.g., the nasalized /l/ of French "branlant". If this is true, then Nasalization need not be specified for liquids in the phonetic matrix for any language, just as Rounding need not be specified for Lax Front Vowels in the classificatory matrix for English.

(though English “heard”, which at the input stage is /hīr/ + *past*, might be represented /hīr≠d/, since the general rules that convert ī to e in many contexts, and that convert lax, non-compact vowels to [i] before final /r/ (+ Consonant), would presumably not yet have applied. Similarly, at this stage, such a phrase as “telegraphic code” (at the phonetic level, perhaps [t³h⁵eligræfikk²həwd⁴]¹) might still be represented /tele + græf + ik ≠ kōd/, or, more fully,

(19) [NP[Adj[N[Pre tele] [stem græf]] ik] ≠ [N kōd]],

where the notation [AX], with paired brackets, indicates that the bracketed string x is a string of the category A. This representation in terms of segments and junctures with the derived constituent structure of the string still marked (since it plays a role in the determination of phonetic shape by subsequent phonological rules), we will call, tentatively, the level of *systematic phonemics*, implying by the word “systematic” that the choice of elements at this level is deeply determined by properties of both the syntactic and the phonological component. The representation in terms of the phones (and, possibly, phonetic junctures) that constitutes the output of the phonological component, we will call the level of *systematic phonetics*.

So far as I can see, there is no other significant level of representation that can be isolated in the phonological component. The input to the phonological component is, in effect, the lowest level of syntactic representation (“l'étage inférieur de la morphologie” of de Saussure, cf. Godel, 1957, 166) where segments are classified in terms of what will ultimately be phonetic characteristics (“caractères phoniques”, *op. cit.*). The output of this component, as mentioned above, is essentially de Saussure's “phonologie”, or the “narrow transcription” of the British phoneticians. The level of systematic phonemics is, essentially, the “phonological orthography” of Sapir (cf. Sapir, 1933), his “ideal sounds” and “true elements of the phonetic pattern” (cf. 1925, note 2); whereas systematic phonetics is his “phonetic orthography” (1933) or “objective phonemes” (1925). Similarly, systematic phonemics seems to be, in essence, the phonemics of Bloomfield's practice (1933) (in particular, when his “secondary phonemes” are not represented), though it is difficult to say whether it is in accord with his phonological theory, which is hardly a model of clarity.³⁴ Systematic phonemics

³⁴ It is instructive, in this connection, to recall the controversies aroused by Bloomfield's *Language*. In particular, Kent's review (1934) criticized it from the point of view of traditional (systematic) phonetics. Kent argues that “the difference between [s] and [š] is functional in English: shall we disregard it in citing Japanese, because it is not functional – even though we have the machinery for marking the distinction”. In this vein, he criticizes Bloomfield's phonemicization of “secretary” [sekriterij] as /sekretejrij/ (which Bloomfield justifies, presumably, by reference to “secretarial” [sekritcrijij]), etc. In responding to the review, Bolling (1934) comments that to mark predictable phonetic variants, in particular, reduced variants of unstressed vowels, “would be like the meaningless underlining of a schoolgirl”; and he supports Bloomfield's phonemicizations by the argument that they mark only what is not predictable. It is interesting to note that the position that Bolling is attacking is, on many points, just the one that is adopted by the “neo-Bloomfieldian” linguists of the 1940's and 1950's, who characteristically criticize Bloomfield for failure to separate levels, and who return to a much “narrow-

would now generally be called "morphophonemics", in one of the several senses of this term. This terminological innovation is justified if there is a third, intermediate level of systematic representation, more closely related to sound and quite independent of syntactic structure, such as the system of representation now called "phonemic". However, as I will attempt to show below, the existence of an additional level is highly dubious, and for this reason I have preferred to keep the older term, modified by "systematic" to avoid confusion.

In general, we can say, with Palmer (1958), that the place of the phonological component is "that of an ancillary technique; it provides a bridge between the grammatical statement and the direct observations that are reported in phonetics". For linguistic theory, the significant questions concerning the phonological component have to do with the choice of phonetic features (and, more generally, the universal phonetic theory), and with the conditions on the form and ordering of rules. The latter question, in particular, is of great importance, and phonological theory has suffered seriously from its neglect. As soon as the attempt to construct explicit rules to determine the phonetic shape of a string of formatives passes the most superficial and introductory stage, it becomes obvious that a fairly strict ordering must be imposed on phonological processes, if they are to be describable in full generality. Thus most of the examples in Sapir (1933) involve ordering, though he does not explicitly mention this fact. Bloomfield was much concerned with questions of ordering,³⁵ and his Menomoni morphophonemics (1939) is the first modern example of a segment of a generative grammar with ordered rules. Bloomfield does not discuss the extent or depth of ordering in this grammar, and it is not easy to determine this from the examples that he gives. It apparently does not exceed five (cf. Bever, forthcoming). In the segment of the phonological component for Modern Hebrew pre-

er" transcription. In particular, the marking of reduced variants of unstressed vowels is considered one of the major innovations in this development. We return to this issue directly.

The controversy between Kent and Bloomfield-Bolling concerns the choice between systematic phonetics and systematic phonemics. But it is clear that these are not alternatives, and that in fact both levels are significant in the description of a language. It was Bloomfield's summary rejection of phonetics as without scientific value or status, rather than his development of a higher level of representation, that should really have been at issue here.

³⁵ Cf. Bloomfield (1933, 213). He regarded ordering of rules as an artifact – an invention of the linguist – as compared with order of constituents, which is "part of language". But this depreciation of the role of order of synchronic processes is just one aspect of the general antipathy to theory (the so-called "anti-mentalism") that Bloomfield developed and bequeathed to modern linguistics. This tendency fitted well with the operationalism, verificationism and behaviorism that formed a dominant intellectual mood in the early 1930's. Harris showed (1951a, 237) that some of Bloomfield's examples of ordering can be handled by an unordered set of rules that state the phonemic composition of a morphophoneme in a strictly morphophonemic context. But his method does not generalize to such examples as the one given directly below; and, furthermore, it is not clear whether this requirement on morphophonemic rules is compatible with the procedures by which they are established, since these procedures set up morphophonemes (similarly, phonemes) in terms of phonemic (respectively, phonetic) or mixed environments. There are important questions of principle here that have not been sufficiently clarified.

sented in Chomsky (1951), a depth of ordering that reaches the range of twenty to thirty is demonstrated,³⁶ and this is surely an underestimate. Recent work (see note 32) gives strong support to the belief that ordering relations among phonological processes are quite strict; and, furthermore, it provides evidence that the ordering is not strictly linear, but is in part cyclic (see § 1). Resolution of these questions seems to me the outstanding problem for contemporary phonology. Although several cases of ordering will be presented below, it is important to bear in mind that scattered examples cannot give an accurate indication of the extent or significance of ordering in a full grammar.

To make the discussion somewhat more concrete, consider the following simple example from English. We find such phonological regularities as the following (where the notation $[s_1, s_2]$ is used for the “archiphoneme” consisting of the features common to s_1, s_2):³⁷

- (20) (i) $\begin{Bmatrix} k \\ t \end{Bmatrix} \rightarrow s$ in the context: $- + [i, y]$
 (ii) $[s, z] + [i, y] \rightarrow [\check{s}, \check{z}]$ in the context: $- \text{Vowel}$.

Thus we have “opaque”–“opacity”, “logic”–“logicism”, “democrat”–“democracy”, “pirate”–“piracy”, in case (i); “race”–“racial”, “express”–“expression”, “erase”–“erasure”, “enclose”–“enclosure”, “revise”–“revision”, in case (ii). Although various qualifications are needed, clearly rules such as these are needed in any grammar. But if these are regarded as purely classificatory, unordered rules to the effect that “morphophoneme” X has the “phoneme” Y as member (or realization, etc.) in the context Z–W, then they must be supplemented by the additional rule

- (21) $\begin{Bmatrix} k \\ t \end{Bmatrix} + [i, y] \rightarrow \check{s}$ in the context; $- \text{Vowel}$,

to account for “logician”, “delicious” (cf. “delicacy”), “relate”–“relation”, “ignite”–“ignition”, etc. But clearly this rule is unnecessary if (20ii) can apply to the result of application of (20i), that is, if the rules are ordered as in (20).

The grammar containing just (20i), (20ii), in that order, will provide such derivations as:³⁸

³⁶ That is, it is shown that a sequence of something like twenty-five rules can be formed such that any interchange of adjacent rules will lead to a reformulation that increases complexity (and hence reduces generality). In the light of more recent work, the grammar presented there would have to be modified in many respects, but the conclusion concerning ordering, so it appears, would, if anything, be strengthened.

³⁷ A natural evaluation measure (“simplicity” measure) for the phonological component (cf. Halle, 1961a) is the number of feature specifications it contains. In particular, then, the grammar is more highly valued (and more general) if rules are stated in terms of archiphonemes (and, furthermore, “generalized” archiphonemes such as C, V, etc.) rather than segments.

³⁸ As throughout, irrelevant details are omitted. In particular, for reasons beyond the scope of this discussion, the first vowel in “logic” should actually be not /a/ but /ə/, and /i/ should actually be the “archiphoneme” lax vowel.

- (22) lajik + yin prezident + i prezident + i + æl
 lajis + yin prezidens + i prezidens + i + æl (by (20i))
 lajišin prezidenš + æl (by (20ii))

The top line in (22) is the systematic phonemic representation, in each case, and the last line becomes the systematic phonetic by additional rules. But none of the intermediate stages has any systematic status at all, apparently. For each linguistic form, the number of intermediate representations will depend on the number of rules in the ordered sequence that apply to it, and this number will differ for different forms, indeed, for different subparts of the same sentence, phrase, or word.

Clearly a grammar that contains (21) as a rule is missing a generalization. In fact, consideration of additional examples shows immediately that several generalizations are being missed. Thus observe that alongside of (20) there is also the rule

- (23) $z \rightarrow s$ in the context: - + iv,

as in “abuse”–“abusive”. But consider the forms “persuade”–“persuasive”–“persuasion”, “corrode”–“corrosive”–“corrosion”, etc. In a taxonomic grammar with no provision for applying rules in sequence, these regularities must be accounted for by two entirely new rules, independent of (20), (21), (23), namely:

- (24) (i) $d \rightarrow s$ in the context: - + iv
 (ii) $d + [i,y] \rightarrow \check{z}$ in the context: - Vowel.

If we allow rules to apply in sequence, the rules (24) are entirely superfluous. It is simply necessary to generalize (20i) to apply to [d,t] instead of simply /t/,³⁹ thus giving for “persuasive” the derivation (25) and for “persuasion” the derivation (26):

- (25) perswēd + iv, perswēz + iv (by (20i)), perswēsiv (by (23))
 (26) perswēd + yin, perswēz + yin (by (20i)), perswēžin (by (20ii)),

where again the first is the systematic phonemic and the last the systematic phonetic representation (details omitted).

Again, it is obvious that a grammar that accounts for this variety of phonetic facts by the rules (20) (suitably generalized) and (23), which are independently motivated, is much to be preferred, on grounds of descriptive adequacy, to one which contains in addition the rules (21), (24). The latter grammar is simply leaving significant generalizations unexpressed. But a descriptively adequate grammar in this case again requires that the rules be applied in the sequence as given.

Finally, let us extend the analysis to include the forms (27), illustrating a point to which we will return below:

³⁹ To this extent, this adjustment of (20i) simplifies the grammar (cf. note 37). Several qualifications are needed, however, which make the effect of the adjustment neutral, as regards complexity.

- (27) (i) decide [dīsa·yd]
 (ii) decided [dīsa·yDīd] – [D] = alveolar flap
 (iii) decisive [dīsaysiv]
 (iv) delight [dīlayt]
 (v) delighted [dīlayDīd].

To account for such facts as these, we must add to the phonological component containing the rules (20) and (23), the rules (28) and (29), where the order is now: (20i), (20ii), (23), (28), (29).

(28) a → a· in the context: – (Glide) Voiced

(29) [t,d] → D in the context: Stressed Vowel – Unstressed Vocalic.

Again, these can be generalized in familiar ways, and each is required, independently, by many other examples. With the rules so ordered we have such derivations as the following:

(30)	<i>decide</i>	<i>decided</i>	<i>decisive</i>	<i>delight</i>	<i>delighted</i>	<i>Rule</i>
(a)	dīsayd	dīsayd#d	dīsayd+iv	dīlayt	dīlayt#d	
(b)	„	„	dīsayz+iv	„	„	(20i)
(c)	„	„	dīsays+iv	„	„	(23)
(d)	dīsa·yd	dīsa·yd#d	„	„	„	(28)
(e)	„	dīsa·ydid	„	„	dīlaytid	
(f)	„	dīsa·yDīd	„	„	dīlayDīd	(29)

Again details and well-known rules are omitted. Line (a) is the systematic phonemic and line (f) the systematic phonetic representation. At no other stage does the set of representations have any systematic character that I can detect. Perhaps (c) is what would be called “phonemic” by many structural linguists (though not, e.g., by Bloch). If so, it is to be observed that ordering of rules is also necessary to convert the “phonemic” representation to the phonetic one, in the optimal way, since clearly if (28) and (29) are not given in this order, the correct output will not be achieved. Thus the [D] of “delighted” is phonetically voiced, but functionally is Voiceless, for the application of rule (28) – thus it has the classificatory distinctive feature of Voicelessness and the phonetic feature of Voiced, in the framework proposed above.

As we enlarge the range of examples considered, the depth of required ordering increases (as does its complexity, when we introduce the transformational cycle). Investigation of this question has, so far, failed to reveal any systematic set of representations that might be taken as constituting a level of representation” at any intermediate point in the operation of the phonological component, and therefore it seems necessary to conclude that systematic phonemics and systematic phonetics are the only two levels of representation that appear in structural descriptions provided by the phonological component. To fortify this conclusion, I would like to consider briefly the status of modern taxonomic phonemics, as seen from this point of view.

4.3. *Taxonomic phonemics.*

Sound pattern has been taken as the primary object of study in modern, structural linguistics; it has, furthermore, been studied in relative or complete isolation from the syntactic setting within which phonological processes operate. In both of these respects, structural linguistics marks a departure from a more traditional point of view, which again emerges in recent work in generative grammar, as sketched above. Though modern phonologists have not achieved anything like unanimity, a body of doctrine has emerged to all or part of which a great many linguists would subscribe. Abstracting away from much variation, let us coin the term "taxonomic phonemics" to refer to this body of doctrine, thus emphasizing its striking reliance, in almost all versions, on procedures of segmentation and classification (identification of variants).⁴⁰

Under discussion, then, are four potential levels of representation associated with the phonological component, namely, the levels of:

- (31) (i) physical phonetics
 (ii) systematic phonetics
 (iii) taxonomic phonetics
 (iv) systematic phonemics

Physical phonetics is the study referred to by Troubetzkoy (1939) as "the science of the sounds of *parole*", a study with methods and goals entirely different from those of phonology (the "science of the sounds of *langue*"). It provides Bloomfield's "mechanical record of the gross acoustic features, such as is produced in the phonetics laboratory" (1933, 85); its status is not in question here, and no further attention will be given to it.

I will assume, for the purposes of this discussion, that the status of systematic phonemics ("morphophonemics", in one sense of the more usual modern phrase) is also not in question.

The status of systematic phonetics and the condition of phonetic specifiability (cf. p. 945, above), however, has been very much in question, and it has, in fact, been explicitly rejected in many theoretical discussions. Thus for Bloomfield (*op.cit.*), the only kind of linguistic record that is "scientifically relevant", aside from that provided by physical phonetics, "is a record in terms of phonemes, ignoring all features that are not distinctive in the language". Phonetic transcription is dismissed as haphazard, limitless, accidental, and of no scientific value; and Bloomfield maintains

⁴⁰ I naturally cannot hope to survey all contemporary points of view in the space of this paper, and I will concentrate on those that seem to me the clearest, referring to Troubetzkoy, Harris, Bloch and Jakobson, among others. I will not consider glossematics (which, for reasons unclear to me, is often referred to as extremely rigorous and of high "operational preciseness" – cf., e.g., Haugen, 1951; Diderichsen, 1958), or the prosodic analysis of the London school, since I have been unable to find abstract formulations of these positions that are explicit enough to show what evidence might count either for or against them, though the latter, in particular, seems to have certain relations to the point of view sketched above in § 4.2.

that in phonology “we pay no heed to the acoustic nature of phonemes but merely accept them as distinct units and study their distribution” (p. 137). Troubetzkoy sometimes refers to phonemes as completely “abstract” units serving only a distinctive function. But elsewhere, he pays a great deal of attention to the systematization of the universal phonetic features that play a distinctive role in some language (structural phonetics – cf. 1939, 93f.). Bloomfield’s apparent rejection of the level of structural phonetics reappears in an extreme form in Joos’ (1957) summary of what he takes to be the characteristic view of American linguistics, namely, that “languages could differ from each other without limit and in unpredictable ways” (96), that “distinctive features are established ad hoc for each language or even dialect”, and that “no universal theory of segments can be called upon to settle the moot points” (228). Similarly, Hjelmslev appears to deny the relevance of phonetic substance to phonological representation.

Nevertheless, it seems to me correct to regard modern taxonomic phonemics, of all varieties, as resting squarely on assumptions concerning a universal phonetic theory of the sort described above. Analysis of actual practice shows no exceptions to the reliance on phonetic universals. No procedure has been offered to show why, for example, initial [p^h] should be identified with final [p] rather than final [t], in English, that does not rely essentially on the assumption that the familiar phonetic properties (Stop, Labial, etc.) are the “natural” ones. Harris might be interpreted as suggesting that a non-phonetic principle can replace reliance on absolute phonetic properties when he concludes (1951a, 66) that “simplicity of statement, as well as phonetic similarity, decide in favor of the p-p^h grouping”; but this implication, if intended, is surely false. The correct analysis is simpler only if we utilize the familiar phonetic properties for phonetic specification. With freedom of choice of features, any arbitrary grouping may be made simpler. From innumerable examples of this sort, it seems that we must conclude that, despite disclaimers, all varieties of taxonomic phonemics rely essentially on the condition of phonetic specifiability. Furthermore, actual practice shows remarkable agreement as to which features constitute the universal phonetic system that is implicitly assumed.

It appears, then, that the status of systematic phonetics is also beyond dispute, though there is room for much discussion as to what is the actual character of the universal phonetic theory that underlies all descriptive practice. In any event, we can assume that each utterance of any language can be uniquely represented as a sequence of *phones*, each of which can be regarded as an abbreviation for a set of features (those that constitute the universal theory in question), in terms of which “phonetic similarity”, “simplicity of statement”, “pattern congruity”, and so on, are defined.

Let us turn then to a more detailed investigation of taxonomic phonemics, taking this to be a theory that requires that phonological representations must, in addition to the condition of phonetic specifiability, meet conditions which, for the sake of this discussion, I will designate by the following terms:

- (32) (i) linearity
 (ii) invariance
 (iii) biuniqueness
 (iv) local determinacy.

The linearity condition (32i) requires that each occurrence of a phoneme in the phonemic representation of an utterance be associated with a particular succession of (one or more) consecutive phones in its representing matrix, as its "member" or "realization"; and, furthermore, that if A precedes B in the phonemic representation, then the phone sequence associated with A precedes (is to the left of) that associated with B in the phonetic matrix. This condition follows from definitions of the phoneme as a class of phone sequences (as in post-Bloomfieldian American linguistics, typically)⁴¹ or as a bundle of distinctive features (Bloomfield, Jakobson) or a minimal term in a phonological opposition (Prague circle).

The invariance condition (32ii) asserts that each phoneme P has associated with it a certain set $\varphi(P)$ of *defining features* (that is, $P \neq Q$ if and only if $\varphi(P) \neq \varphi(Q)$) and that wherever P occurs in a phonemic representation, there is an associated occurrence of $\varphi(P)$ in the corresponding phonetic representation. The invariance condition has no clear meaning unless the linearity condition is also met; I will assume, then, that it is inapplicable when linearity is violated. The invariance condition, in the form stated above, is required explicitly by Bloomfield, Troubetzkoy, Jakobson and Bloch, for example, and appears to be implicit in many other conceptions. Where linearity and invariance are both met by a taxonomic phonemic representation, the string of phones is segmented into successive segments, each of which contains, along with redundant (determined) features, the defining features $\varphi(P)$ of some phoneme P, and the phonemic representation is just the sequence of these phonemes.

One can distinguish two versions of the invariance condition, depending on whether the features are taken to be *relative* (i.e., more or less along a certain phonetic dimension) or *absolute*. Jakobson explicitly accepts the relative version of the invariance

⁴¹ In the case of Bloch's very careful system of definitions (cf. Bloch, 1950, for a lucid sketch), the linearity condition is not necessarily met, but it is met, apparently, insofar as linear order is defined on phonemes at all. There are various unclaritys here, despite the care of Bloch's presentation. Thus as the definitions stand, it is impossible for English [p^h] to be a member of the phoneme /p/ (with [p]), since the defining qualities for /p/ are not coextensive with [p^h] (or if a defining quality need qualify only part of a phone, it would follow that, e.g., [sp] could be assigned to /p/ and to /s/). It is also unclear what is meant by the statement that the phonemes of a dialect must "accommodate all the phones". Thus English "solely" has a doubled [l], phonetically. By definition, this pair of successive segments constitutes a phone. Must this phone be a member of a phoneme, or can the phonemic representation have two /l/'s, given the requirement that the phonemes must accommodate the phones? Bloch's work illustrates an important point, namely, that as the explicitness of formulation of taxonomic phonemics increases, the difficulty of giving a consistent and descriptively adequate interpretation also increases. Thus as compared with the other phonemic theories under consideration here, Bloch's is quite explicit; but the difficulty of determining whether the conditions (32) are met is at least as great in the case of his phonological theory as in the case of the others.

condition, and Bloch, as I understand his account, seems to accept the absolute version. Under the absolute invariance condition, partial overlapping is excluded. If a certain occurrence of a phone P is assigned to a phoneme \bar{P} , then every other occurrence of P must be assigned to \bar{P} . Under the relative version of the invariance condition, certain cases of partial overlapping are permissible.

There are, however, some unresolved conceptual difficulties concerning the formulation of the relative invariance condition. Consider, e.g., a binary feature F such that a phone P in a certain context X–Y is assigned the feature [+F] or [–F] depending on its relation, in terms of the feature F, to some other phone Q in the context X–Y. But how is the context X–Y in question to be specified? If in terms of phones, then in general we can expect that the contrasting element Q will not appear in the context X–Y, but in a context X'–Y', where X' belongs to the same phoneme as X and Y' to the same phoneme as Y. If in terms of phonemes, then what happens when features that define X and Y are again relative to a context which, in this case, includes P and Q? For some discussion, see Chomsky (1957b).

Technically, the biuniqueness condition (32iii) asserts that each sequence of phones is represented by a unique sequence of phonemes, and each sequence of phonemes represents a unique sequence of phones.⁴² The biuniqueness condition is very widely maintained by modern phonologists, in particular, by those mentioned above. However, it is very difficult to formulate this condition in a manner that is actually in accord with their intentions. Consider, for example, Hockett's explicit discussion of it (1951). He considers a hypothetical language with no morphophonemic contrast between voiced and voiceless stops and with the rule:

(33) Stop → Voiced, medially, in words.

Thus morphophonemic *pat#atak* becomes phonetic [patadak], while morphophonemic *patat#ak* becomes phonetic [padatak]. But, Hockett argues, if we hear [padatak] we do not know whether to transcribe /patat#ak/ or /pata#tak/. Consequently the morphophonemic representation fails the biuniqueness condition, and cannot be taken as the phonemic representation, which, in this case, must mark the distinction between voiced and voiceless consonants. This illustrative example, however, leaves many questions unanswered. Suppose, following Hockett, "that there is no word /pada/, or no word /tak/, or that, both of these words existing, they would not occur in this sequence". Or, suppose that there is a general rule to the effect that no word ends in a vowel. In any such case, "we can conclude that the proper representation would be *patat ak*" (/patat#ak/), and the morphophonemic representation would, technically, meet the biuniqueness condition and would thus qualify as phonemic, if we take this condition literally.

Hockett does not state whether he would accept this system as phonemic, in this

⁴² In this form, the condition is of course rarely met. What is intended, rather, is that each sequence of phonemes represents a sequence of phones that is unique up to free variation.

case, but it is fairly clear from the context that he would not. In fact, a decision to accept it as phonemic would seem to be inconsistent with his principle of separation of levels, to which we return below, under any reasonable interpretation of this. It is fairly clear that linguists who accept the so-called biuniqueness condition would regard the situation just described as still being a violation of "biuniqueness" in the intended sense. That is, they do not mean by "biuniqueness" simply one-one correspondence, but rather a correspondence such that the unique phonemic representation corresponding to a given phonetic form can be determined by "purely phonetic" considerations, or perhaps, considerations involving only "neighboring sounds". This convention, which is rather difficult to state precisely, is what I have called the condition of local determinacy (32iv). Apparently it is this, rather than literal biuniqueness in the technical sense, that is required in taxonomic phonemics.

Notice that from the linearity and absolute invariance condition one can deduce a particularly strong form of the biuniqueness and local determinacy conditions, namely, as noted above, the condition that the phoneme corresponding to a particular phone can be determined independently of the context of this phone. That is, even partial overlapping is disallowed, and (32iv) is vacuous. Although, as noted above, the situation is still somewhat confused in the case of the relative invariance condition, it is clear that proponents of such positions (e.g., Jakobson, Harris) would disallow complete overlapping but not partial overlapping, since however they interpret the invariance condition, they do insist on some sort of "biuniqueness".

Although conditions (32i-iv) are (with a qualification to which I return below in § 4.3) quite generally accepted, and though they do follow from familiar definitions of the phoneme, there are many examples showing that they are untenable. Consider first the linearity condition. Of the many examples that illustrate its incorrectness,⁴³ perhaps the simplest is one presented in a recent paper by Malécot (1960). He observes that Lax Vowel + Nasal is often realized as Nasalized Vowel before Unvoiced Stop, in English, so that e.g., phonemic /kænt/ is phonetic [kæ̃nt], though phonemic /hænd/ is phonetic [hæ̃nd]. In the face of this evidence, no linguist would conclude that vowel nasalization is distinctive in English, and that "can't"–"cat" constitute a minimal pair, while "can't"–"canned" do not. Rather, in such a case, the linearity condition would be disregarded. Furthermore, there can be no doubt that this decision is correct. The phonetic representation can be derived from the phonemic, in this case, by the phonetic rules (34), ordered as given:

- (34) (i) Vowel → Nasalized in the context: – Nasal Consonant
 (ii) Nasal → Ø in the context: Lax Vowel – Unvoiced Stop.

Though perfectly general and straightforward, these rules happen to lead to a violation of the linearity condition.

A second and more extreme example of the violation of linearity is the case of

⁴³ For several, see Harris (1951a, chapters 7,9).

the a-a' contrast, discussed above (p. 950, § 4.2). The rules (28), (29), applied in this order, convert the systematic phonemic representations of row (I) of (35) first to row (II) and then to the systematic phonetic representation of row (III):

- (35) (I) rayt#r rayd#r (“writer”, “rider”, respectively)
 (II) rayt#r ra·yd#r (by (28))
 (III) rayDir ra·yDir (by (29), etc.)

But here words which differ phonemically only in their fourth segments differ phonetically only in their second segments. Hence if phonemic representation is to play any significant role in linguistic description (if it is to be part of a grammar that achieves descriptive adequacy), the linearity condition must be rather grossly violated.

These violations of the linearity condition incidentally show, in yet another way,⁴⁴ the incorrectness of the claim that phonology can (or, even more unaccountably, that it must) be based on synonymy, in its usual formulation to the effect that phonetically similar sounds are not assigned to the same phoneme if and only if replacement of one by the other in some context leads to a change of meaning (cf., e.g., Diderichsen, 1949). If what is meant by “context” is “phonetic context”, then the criterion would give the result that V- \tilde{V} and a-a' constitute a phonological opposition (contrast) in English. If what is meant is “phonemic context”, then obviously the question at issue is simply being begged. In general, it should be observed that “minimal pair” is not an elementary notion. It cannot be defined in phonetic terms, but only in terms of a completed phonemic analysis. Consequently, the “commutation test” is

⁴⁴ For further discussion, see Chomsky (1957a). Notice, for example, that such a pair as [r], [D] are in free variation and are assigned to the same phoneme in the context /θ - Vowel/ (“three”, “throw”, etc.) in many English dialects, but replacement of one by the other in /bæ-|/ leads to a meaning difference (“battle”, “barrel”) (whereas, on the other hand, /t/ and /d/ can replace one another in the context /birn-/ (“burned”, “burnt”) with no change in meaning, though they would never be assigned to the same phoneme), so that the semantic criterion is falsified from right to left. And [ə], [r], though phonetically similar, clearly cannot be assigned to the same phoneme (cf. below) though they never contrast (with or without change of meaning), so that the criterion is falsified from left to right.

The history of the notion “contrast” in modern linguistics is very curious. Bloomfield (1926) took it as a primitive notion, and Harris provided a fairly effective operational test (1951a, 32f.), which is the only known device that can be used when the problem of determining contrast actually arises in practice. The only coherent attempt to define “contrast” has been Bloch's careful distributional definition. This has been frequently criticized, mainly on grounds of impracticality. Insofar as the criticism is valid, it shows only that “contrast” must be taken as a primitive notion. However, the criticism has almost universally been taken as showing that “contrast” must be defined in terms of “synonymy of utterance tokens” (e.g., Diderichsen, 1958), and in the background of the entire development has been the assumption that there is such a definition. Obviously, however, difficulties in one analysis do not show that another analysis is correct. And in fact there is no proposal for defining “contrast” in terms of “synonymy” that does not have obvious objections to it. In fact, the only definition I have been able to find or to construct that does not immediately fail (Chomsky, 1957a, 95-6) not only requires (with Bloch) that each token appear in each possible context, but that it occur in each possible context with each “meaning”, so that the “impracticality” of Bloch's proposal is compounded manifold. Perhaps some semantic criterion for “contrast” exists. This we will not know, however, until proponents of this view take the same care in formulating their proposal as Bloch did in formulating his. Until such time, it can only be dismissed as a totally unsupported claim.

of only marginal interest if formulated, in the usual manner, as a procedure for phonemic analysis.

Such violations of the linearity condition have not gone unnoticed by careful taxonomic phonologists, and it is instructive to consider the steps that have been taken to meet them. Troubetzkoy gives an example quite analogous to (34) both in the *Anleitung* and the *Grundzüge* (1939, 46). He observes that the following phonological rules operate in Russian:

- (36) (i) $o \rightarrow \text{ɔ}$ in the context: - 1
 (ii) $1 \rightarrow \text{Ø}$ in the context: Vowel - Nasal Consonant.

Thus phonemic /sólncă/ ("sun") is phonetic [sɔncə], and there is no necessity to set up /ɔ/ in contrast to /o/ as a new phoneme. Here the linearity condition is violated, as in (34); and, furthermore, the rules must be ordered as given. To account for such violations of linearity, Troubetzkoy proposes a general rule for phonemicization which we can state as follows:

- (37) If the phone A is phonetically similar to the phone sequence BC, and A-BC are in free variation or complementary distribution, and BC is a realization of the phoneme sequence PQ, then A is to be regarded as a realization of PQ.

Thus [ɔ] is phonetically similar to and in complementary distribution with [ɔl], which is a realization of /ol/; thus [ɔ] is a realization of [ol].⁴⁵ Similarly, nasalized vowels, at least in some English dialects, are in complementary distribution with Vowel + Nasal, and could thus be regarded as a realization of Vowel + Nasal, thus dealing with the violation of linearity caused by (34), in these dialects. Similarly, one might use the same argument to justify representing intervocalic and word final English [ŋ] as /ng/ (though to apply the argument in this case, complementary distribution would have to be defined in terms of phonemically specified, rather than phonetically specified contexts).

However, the rule (37) seems to me not at all satisfying. It is entirely ad hoc, and it can only be taken as indicating that the definition of the phoneme as a minimal term of a phonological opposition is incorrect. More seriously, it cannot be applied in general, without absurdity. Thus, in English, the pairs [ŋ]-[ny], [yũ]-[y] are phonetically similar and in complementary distribution, but it would be absurd, following the rule, to phonemicize [kitŋ] ("kitten") as /kitny/ or [yat] ("yacht") as /yũat/. Even more serious for the taxonomic phonemicist, is the fact that this rule can lead to a violation of biuniqueness. Thus consider the English [ã]-[a·] contrast ("write"-"ride"), discussed above. [ãy] appears only initially or after a consonant, and before an unvoiced consonant; [y] can never appear in this position. Since [y] and [ãy] are phonetically similar and [ãy] is a realization of /ay/, by Troubetzkoy's rule, [y] is a realization of /ay/. Aside from the absurdity, this leads to a violation of biuniqueness.

⁴⁵ Note that Troubetzkoy's rule must be modified, for adequacy, since as it stands it would require that [ɔl] be regarded as a realization of /oll/.

ness, in this case, since /y/ and /ay/ contrast (“ion” /ayan/ – “yon” /yan/). Hence aside from being ad hoc, this rule cannot be regarded as an extension of the notion “phoneme” to deal with the case of violation of linearity.

Troubetzkoy’s informal comments and discussion of examples indicate that the rule, as he stated it, perhaps does not conform to his actual intentions. Suppose, in fact, that we were to restrict application of the rule (37) to the case in which B is a Lax Vowel and C a Liquid or Nasal. Then the violations of linearity in the Russian example (36) and the English example (34) (but not the example of English /ng/) would still be handled, while the counter-examples of the preceding paragraph would be ruled out. But now the entirely ad hoc character of the rule becomes even more clear, and surely with such a restrictive formulation as this no one would seriously regard it as constituting part of the definition of the fundamental concept “phoneme”. Furthermore, it is still not difficult to construct counter-examples. Thus in many American dialects, [e] of “get” is in complementary distribution with [ɛr] of “berry”, which is a realization of /er/; so that by the rule, even as amended, [e] must be regarded as a realization of /er/, and “get” must be phonemicized /gert/.

The rule (37) is a typical example of an ad hoc device invented to remedy an inadequacy of some general notion of “taxonomic phoneme”, and this discussion of difficulties that it faces could be duplicated for other principles of this sort. These ad hoc revisions of a basically inadequate notion do not succeed in touching the central issue. In such cases as those discussed above, it is clear that the acceptability of an analysis hinges on its effect on the grammar as a whole. Thus the rules (34i) and (34ii) are quite general and are independently motivated. A grammar that incorporates them is materially simpler than one that does not. But the rules: /yu/ → [y] before Vowels, or /er/ → ε before Consonants, as in the absurd examples given above, obviously do not simplify the grammar of English. Similarly, Troubetzkoy’s Russian example is well-motivated by general systematic considerations; e.g., by the existence of such forms as /sólŋ,ešnij/, [sólŋ,išnij], and by the fact that were (36) not incorporated in the grammar, then each occurrence of /o/ in the lexicon would have to be marked as distinct from /o/, greatly complicating the grammar (cf. note 37). Similarly, the necessity of assigning English [ŋ] to /n/ (more accurately, to the archiphoneme Nasal) becomes obvious only when the full range of examples involving Nasal + Stop in various syntactic positions comes under investigation. The fact that considerations of this sort are crucial suggests that any such “atomistic” rule as the one that Troubetzkoy suggests will fail.

General systematic considerations are, however, foreign to the point of view of taxonomic phonemics, and, in fact, they have often been criticized as circular (cf., e.g., Twaddell, 1935, 66). This criticism is correct, given the general “procedural” bias of modern phonology; but it shows only that the attempt to develop a taxonomic phonemics on the basis of analytic procedures of segmentation and classification, supplemented by such ad hoc rules as (37), is ill-conceived from the start.

The more extreme case of violation of linearity posed by “writer”–“rider” (which

is beyond the range of (37) or any modification of it) is discussed by Harris (1951a, 70). He proposes that [ayD] be assigned to /ayt/ as a unit, and [a·yD] to /ayd/ as a unit, on general grounds of symmetry of distribution. But this is a rather vague notion, and it is not at all clear how it would fare once clarified. Furthermore, suppose that somehow a criterion of distributional symmetry can be formulated that has just the desired effect in this case. This result would still seem to be accidental and beside the point, since clearly in this case the critical factors are, once again, the generality and independent motivation of the rules (28), (29), and the relation of the forms in question to others; in particular, the relation of "writer" to write" and "rider" to "ride", which would surely be expressed, on syntactic grounds, in the systematic phonemic representation. But these factors have nothing directly to do with distributional symmetry. They are, once again, of a general systematic character, and thus lie beyond the narrow scope of taxonomic phonemics.

It seems to me, then, that the ad hoc devices for dealing with the violations of linearity are not defensible, and that the definition of a phoneme as "a bundle of [phonetic] distinctive features", "a class of phones in free variation or complementary distribution", or "a minimal term in a phonological opposition" can be maintained only if we are willing to tolerate such absurdities as the phonemic representations /kæ̃t/, /rayDir/, /ra·yDir/ for "can't", "writer", "rider", and so on, in many other cases.

Consider now the invariance condition. Notice first that it fails in the case of violations of linearity such as those discussed above. However, it seems to me untenable even when linearity is preserved. Phonemic overlapping provides the clearest example of this. Thus consider an English dialect in which [D] is the allophone of /r/ in "throw" and of /t/ in "Betty" (where it contrasts with the /r/ of "berry" – cf. Bloch, 1941). Following the principle of invariance, we must assign [D] to /t/ in the context #θ–, contrary not only to the speaker's intuition but also to the otherwise valid rules of consonant distribution. The situation is worse in dialects in which [D] and [r] are in free variation in this context and in intervocalic contrast, in which case no coherent solution is possible within the framework of (32), although the description of the facts is perfectly straightforward. The situation is still worse if we accept the absolute invariance condition, particularly if (as in Bloch, 1950) the features ("qualities") are defined in auditory terms. For it is known that in this case, not even the correct analysis of English stops is tenable, since /p/, /t/ and /k/ overlap (Schatz, 1953). For reasons such as these, then, it seems that the invariance condition cannot be accepted, however the condition of linearity is treated.

The biuniqueness condition is difficult to discuss because of the unclarity of formulation noted above. Nevertheless, certain consequences of accepting it are clear, and it seems to me that these are quite devastating, for anyone concerned with descriptive adequacy. Halle has pointed out that it is generally impossible to provide a level of representation meeting the biuniqueness condition without destroying the generality of rules, when the sound system has an asymmetry. Thus he gives the

following, quite typical example from Russian (Halle, 1959b). In (38) the four forms in column I are given in systematic phonemic representation and in column III in systematic phonetic representation:

(38)	I	II	III
	d'at, l,i	d'at, l,i	d'at, l,i
	d'at, bi	d'ad, bi	d'ad, bi
	ž'eč l,i	ž'eč l,i	ž'eč l,i
	ž'eč bi	ž'eč bi	ž'eǰ bi

The forms of column III are produced from those of column I by the general rule:

(39) Obstruent \rightarrow Voiced in the context: – Voiced Obstruent.

But the representations in column I fail the condition of biuniqueness as usually construed (in terms of local determinacy), and consequently would not be accepted as taxonomic phonemic. The representations in column II would be accepted as “phonemic” by taxonomic phonologists, because of the fact that t, -d, contrast while č-ǰ do not. But if the grammar is to provide II as a level of representation, then it cannot incorporate the general rule (39), but must have in its place the two rules (40i) and (40ii), the first of which is taken as a rule relating “morphophonemic” to “phonemic” representation, and the second as relating “phonemic” to phonetic representation:

(40) (i) Obstruent \rightarrow Voiced in the context: – Voiced Obstruent, except for c, č, x;
 (ii) c, č, x \rightarrow Voiced in the context: – Voiced Obstruent.

It seems to me that the force of this example has not been sufficiently appreciated by taxonomic phonemicists. Where it has been noted at all, the discussion has not been adequate. Ferguson, in his review (1962) of Halle (1959b), discusses not the example given in the book under review (and reproduced above), but instead a Turkish example that had at first been proposed by Lees as analogous to Halle's, and then withdrawn by Lees as inappropriate (Lees, 1961, p. 63). Insofar as Ferguson's discussion carries over to the correct example that Halle gives, it amounts only to the observation that from the phonetic record alone it is possible to determine the underlying systematic phonemic (in his terms, morphophonemic) form in the case of c, č, x, but not in the case of the other obstruents. This is correct but irrelevant, since this information is provided just as explicitly in the grammar which incorporates only systematic phonemics and systematic phonetics as in the grammar which, in addition, adds an intermediate level of taxonomic phonemics. Thus the fact remains that in this case, the only effect of assuming that there is a taxonomic phonemic level is to make it impossible to state the generalization.

In the face of Halle's example, I do not see how one can fail to be uncomfortable in attributing to Russian a level of taxonomic phonemics. Furthermore, similar examples are not difficult to find in other languages. Bloch, in fact, gave a rather

similar example in his discussion of phonemic overlapping (Bloch, 1941). In his dialect of English there are forms that might have the systematic phonemic representations of column I and the systematic phonetic representations of column III of (41):

(41)	I	II	III
	“nod”: nad	na·d	na·d
	“knot”: nat	nat	nat
	“bed”: bed	bed	be·d
	“bet”: bet	bet	bet.

Column I does not meet the biuniqueness condition because of such contrasts as “balm”–“bomb”, “starry”–“sorry”, “father”–“bother”, and because of the fact that the vowel of “Pa’d (do it)” is that of “pod”, phonetically. Column III can be derived from column I by the familiar rule of lengthening before voiced segments (of which (28) is a special case).⁴⁶ But Bloch is forced, by the biuniqueness condition, to accept II as the phonemic level of representation. Thus a full grammar of English, meeting this condition, would have to replace the general rule of vowel lengthening by two rules, the first of which applies only to /a/ and the second to all other vowels. The first would relate “morphophonemic” and “phonemic”, and the second “phonemic” and phonetic representations. The situation is exactly analogous to the Russian example just given, and again we see that the effect of the biuniqueness condition is to complicate the grammar, that is, to prevent it from achieving descriptive adequacy.

The complicating effect of the biuniqueness condition has been commented on by several of its proponents. Thus Bloch remarks at once, in discussing the preceding example, that it leads to a loss of symmetry. Similarly, he remarks (1950, note 3) that the National Romanization which influenced his earlier, non-biunique analysis of Japanese, though “neat and systematic”, is not as close to a “phonemic notation” as the Hepburn Romanization, “unsystematic and cumbersome as it seems to be”. Similarly, Hockett (1951) compares Bloch’s “deceptively simple” non-biunique analysis with his later “quite complicated . . . but obviously more accurate” taxonomic phonemic analysis. In fact, however, the “greater accuracy” of the latter seems to reside in nothing other than its observance of conditions (32i-iv). We return below to the question of why this is regarded as a sign of greater accuracy.

⁴⁶ This discussion is quite unaffected by the residual cases of a-a. contrast. For Bloch’s dialect “father”, and “bother” have different vowels, quite independently of how we analyze the forms of (41). In fact, it is no accident that the short vowel in the a-a. pairs is generally spelled “o” while the long one is spelled “a”. A good case can be made for the conclusion that the vowel phoneme of “nod”, “knot”, “bomb”, etc., is actually /ə/, which in certain dialects goes to [a·] (merging with the variant of /a/), in others goes to [a] (giving the a-a. contrast), and in others remains [ə]. This assumption is required by many other considerations, e.g., to describe in the most general way the familiar ē → æ and ō → a alternations. Cf. Halle and Chomsky (forthcoming) for a detailed discussion. The issue is further complicated by dialects (cf. Sledd, 1959) in which liquids drop pre-consonantly (giving long variants of short vowels in such words as “absolve” /æbsəlv/ – cf., “absolution” – etc.). This is just one of the many examples that show how wide a range of information is necessary to determine what is in fact a minimal pair.

We have, as yet, said nothing about the principle of complementary distribution, which is the central concept of taxonomic phonemics as developed, for example, by Jones, Troubetzkoy, Harris and Bloch. This principle is, basically, the principle of biuniqueness converted into a procedure. Regarded as an analytic procedure, its goal is to provide the minimally redundant representation meeting the conditions of biuniqueness and local determinacy. We will show, however, that it is in general incapable of providing the minimally redundant analysis meeting these conditions, and furthermore, that it may even lead to a non-biunique analysis.

We can formulate the principle in this way (following Harris, 1951a, chapter 7): Given a set of representations in terms of phones, let us define the distribution $D(x)$ of the phone x as the set of (short-range) phonetic contexts in which x occurs. The relation of complementary distribution holds between phones x and y if $D(x)$ and $D(y)$ have no element in common. A *tentative phoneme* is a class of phones related pair-wise by the relation of complementary distribution. Some would require further that a defining phonetic property be associated with each tentative phoneme, marking each of its members and no other phone (the invariance condition).⁴⁷ A *tentative phonemic system* is a family of tentative phonemes meeting a condition of exhaustiveness. We find *the* phonemic system (or systems) by applying additional criteria of symmetry.

But consider the example of phonemic overlapping due to Bloch that was discussed above, namely, the case of a dialect with [D] as the realization of /r/ in “throw” and of /t/ in “Betty”, where it contrasts with the [r] of “Berry”. The requirement of biuniqueness is preserved if we set up the phonemes /t/, with the allophone [D] in intervocalic, post-stress position, and /r/, with the allophone [D] after dental spirants. Given a phone in a phonetic context, we can now uniquely assign it to a phoneme; and given a phoneme in a phonemic context we can uniquely determine its phonetic realization (up to free variation). However, this solution, which is the only reasonable one (and the one Bloch accepted in his 1941 paper), is inconsistent with the principle of complementary distribution. In fact, the allophones [D] and [r] of /r/ are not in complementary distribution, since they both occur in the context [be-iy] (“Betty”, “berry”). Hence complementary distribution is not a necessary condition for biuniqueness. Furthermore, the class of “tentative phonemic systems” as defined in the preceding paragraph will not include the optimal biunique system as a member, and no supplementary criteria will suffice to select it from this class.

But now observe further that the class of tentative phonemic systems, as defined, will contain systems that fail the principle of biuniqueness. Thus, for example, [k] and [ã] are in complementary distribution in English (and, furthermore, share features shared by nothing else, e.g., in Jakobson’s terms, the features Compact, Grave, Lax, Non-Flat). Hence they qualify as a tentative phoneme, and there is a tentative

⁴⁷ This would be required by Troubetzkoy, Jakobson and Bloch, but not by Harris (cf. 1951a, 72, note 28). He maintains that “any grouping of complementary segments may be called phonemic”, and that further criteria have to do only with convenience, not with linguistic fact.

phonemic system in which they are identified as members of the same phoneme /K/. But in this phonemic system, "socked" [säkt] and "Scot" [skät] will both be represented phonemically as /sKKt/. Similarly, [ə] and [r] are in complementary distribution (and share defining features) and thus qualify as a potential phoneme. But if they are identified as variants of /R/, we will have "prevail" /pRRvəl/, [prəveyl], "pervade" /pRRvəd/ [pərveyd], which is a violation of local determinacy, and of biuniqueness as generally construed. Consequently the principle of complementary distribution does not even provide a sufficient condition for biuniqueness. Since it provides neither a necessary nor a sufficient condition for biuniqueness, and, apparently, has no motivation except for its connection with biuniqueness, the principle of complementary distribution appears to be devoid of theoretical significance.

Related questions have been discussed by taxonomic phonemicists, but the general problem has apparently escaped attention. Troubetzkoy considers the example of English [r] and [ə], and gives a rule (1935, Rule IV; 1939, Rule IV) that would prevent them from being assigned to the same phoneme in case the sequence [ər] is in contrast with [ə]. This rule, as formulated, is not pertinent to the problem of preserving biuniqueness, and does not cover either of the examples of the preceding paragraph. It is, furthermore, entirely ad hoc, and thus simply serves to indicate a theoretical inadequacy of taxonomic phonemics.

Apparently only Harris has considered a special case of this problem explicitly. He points out (1951a, 62, note 10) that we might have phonetic representations [t̄ray], [kray] for "try", "cry", where t̄-k and r-r are in complementary distribution. But if we were to set up a tentative phonemic system in the manner described above, we could have a phoneme /T/ with allophones [t̄] before [r] and [k] before [r], and a phoneme /R/ with allophones [r̄], [r]. But now both "try" and "cry" would be represented /TRay/. To avoid this, Harris suggests that we first group [r̄] and [r] into /r/, and then redefine distributions in terms of the newly specified contexts, in which [t̄] and [k] now contrast before /r/. This procedure will avoid the difficulty in the particular case of "try", "cry", but not in the cases described above. Furthermore, the same procedure could just as well be used to group [t̄] and [k] into /T/, thus keeping [r̄] and [r] phonemically distinct (in further justification, we could point out that this regularizes distributions, since now /t/ occurs neither before /r/ or /l/, instead of, asymmetrically, only before /r/). Hence, as in the case of the procedures discussed above, it fails to distinguish permissible from impermissible applications. Finally, the procedure as stated is inconsistent with Harris' general requirement on the set of linguistic procedures (1951a, 7), namely, that operations must be "carried out for all the elements simultaneously" without any "arbitrary point of departure". In fact, this requirement was what made it possible for Harris to avoid Bloomfield's use of descriptive order (cf. note 35, above). But it is violated by the procedure just discussed.

4.4. *Criteria for systematic phonemics.*

Systematic phonemics in the sense of Sapir or of § 4.2 does not observe the con-

ditions (32) and is not based on such techniques as complementary distribution or, for that matter, on any analytic procedures of segmentation and classification.⁴⁸ Furthermore, construction of the set of ordered rules constituting the phonological component cannot be undertaken in isolation from the study of syntactic processes, just as study of the syntactic component cannot proceed without regard to the simplicity and generality of the rules that convert its output into a phonetic representation.

In analyzing a particular language, we must assume given a theory of generative grammar that specifies abstractly the form of grammars and a measure of evaluation for grammars. To fix the level of systematic phonemics for this language, we must attempt to construct the most highly valued grammar compatible with the primary data from this language (cf. § 1). The level of systematic phonemics will consist of the set of representations that appear in derivations provided by this grammar at the point where grammatical morphemes other than junctures have been eliminated. It is certainly conceivable that there exist procedures of some sort that would facilitate the task of selecting this level of representation, but they are not, to my knowledge, available today. It is hardly likely that elementary taxonomic procedures of the kind that have been studied in modern structural linguistics can lead to the discovery of this level of representation. For the present, it seems that the most promising way to give a closer specification of this level of representation and the criteria that determine it is by refining the abstract conditions on the form of generative grammar, the measure of evaluation and the universal features that define the phonetic matrices in terms of which the primary data is represented.

We observed in § 4.2 that if a grammar is to achieve the level of descriptive adequacy, the rules of its phonological component must be ordered; and, in general, a derivation will contain many representations between the systematic phonemic and the systematic phonetic. We suggested that there is no set of intermediate representations that has any systematic significance. Whether or not this is true, we have now, in § 4.3, accumulated evidence showing that if a level meeting the conditions associated with taxonomic phonemics is incorporated in a grammar, then many generalizations will not be expressible and descriptive adequacy cannot be achieved. It is important, then, to see whether there is some way of justifying the assumption that a level of taxonomic phonemics actually constitutes a part of linguistic structure.

4.5. *The motivation for taxonomic phonemics.*

We are now concerned with the question: why should it be assumed that a grammar must generate representations meeting the conditions (32), as part of the structural descriptions of utterances? What, in other words, is the justification for the theory of taxonomic phonemics, in any of its modern varieties?

Many linguists would perhaps take a position of the sort expressed by Twaddell (1935). In opposition to the "mentalist" approach of Sapir (that is, the approach

⁴⁸ In the case of Sapir, it seems that the choice of examples in his important psychological reality paper (1933) was motivated by his rejection of these (at the time, still unformulated) conditions.

that is concerned with descriptive and explanatory adequacy), he proposes a method of phonemic analysis for which the following is "the only defense that may be offered": "this procedure . . . appears to be characterized by a minimum of the undemonstrable. With one coherent set of assumptions and conventions, which are indispensable to all scientific linguistic study, and one sound laboratory generalization, we may apply strictly mathematical methods and deduce a logically unimpeachable definition of some entity". (74). Thus the phoneme is "a mere terminological convenience" (68). There is no necessity for demonstrating "psychological reality" (i.e., descriptive adequacy), because "this demonstration would be a convenience rather than a necessity for linguistic study: it would represent a summary of the behavior of native speakers, a behavior which is already available for the student of language, though in less concentrated form" (58). Thus all that is asked of a linguistic notion or a linguistic description is that it meet the requirement of *consistency* and what we may call *convertibility* (namely, the account must be explicit enough to be convertible into some other, equally arbitrary framework) and, perhaps, in some sense, *simplicity* and *convenience*.

In part, Harris seems to take a similar position in his *Methods* (1951a, chapter 1). He describes his procedures as "merely ways of arranging the original data". The only general condition that they must meet is the biuniqueness condition, which is not justified on any external count, but simply is taken as defining the subject. The procedures must be "based on distribution, and be unambiguous, consistent and subject to check". The criteria for selecting phonemes are stated only "to make explicit in each case what method [of data arrangement] is being followed" (63). Thus only consistency and convertibility (and convenience, for one or another purpose) is required of a linguistic theory or a grammatical description. But Harris also states (372-3) that "the work of analysis leads right up to the statements which enable anyone to synthesize or predict utterances in the language", that is, to a generative grammar. This constitutes a truth claim for the procedures, a claim which surely cannot be maintained if conflicting procedures meeting the conditions of consistency and convertibility are equally valid, and which would appear to be incompatible with Harris' earlier remark that the "overall purpose . . . [of the procedures] . . . is to obtain a compact one-one representation of the stock of utterances in the corpus" (366). Furthermore, there are no known procedures which lead to this more ambitious, and far more significant goal. These conflicting remarks concerning what Hockett has called "metacriteria" (1955) illustrate a general ambivalence concerning goals that makes evaluation of modern taxonomic linguistics on its own terms rather difficult.

Insofar as consistency and convertibility are taken as the only valid metacriteria, linguistic theory is concerned only with the level of observational adequacy. This theory makes no claim to truth; no evidence conflicts with it, just as none can be offered in its support. The only criticism that is relevant is that taxonomic phonemics, as indicated above, seems more of an inconvenience than a convenience, if embedded

within a full grammatical description. This point of view takes a theory to be, essentially, nothing more than a summary of data. In contrast, it has been repeatedly pointed out (most forcefully, by Karl Popper) that the prevailing attitude in the sciences is to regard data as of interest primarily insofar as it has bearing on the choice among alternative theories, and to search for data, however exotic, that will be crucial in this sense.

If one is unwilling to settle for just consistency and convertibility, what further justification can be offered for taxonomic phonemics? I have tried to show above that the internal linguistic evidence does not support taxonomic phonemics. Taxonomic phonemic representations do not contribute to the simplicity or generality of a grammar, but, in fact, have just the opposite effect. Therefore one must search for external evidence. In particular, it is important to ask whether reasonable requirements for a perceptual model ((1a) of § 1.3) or a learning or discovery model ((1b) of § 1.3) have any bearing on the validity of taxonomic phonemics. Considerations of this sort may actually have been at the core of some theoretical and methodological studies.

One might try to justify the conditions (32) by arguing that speech perception involves two successive stages: the hearer first uses only local phonetic cues to identify the invariant criterial attributes that determine the successive taxonomic phonemes; and he then goes on to determine the deeper structure of the utterance (in particular, its systematic phonemic representation and its syntactic structure). This clearly seems to be the view of Jakobson (cf. Jakobson, Fant and Halle, 1952) and of Joos (1957, 92),⁴⁹ among others. However, there is no real basis for this account, and it is scarcely in accord with what little is known about complex perceptual processes, or, for that matter, about speech perception. Thus it is well-known that intelligibility is preserved under gross phonetic distortion, which may be completely unnoticed when grammatical constraints are met; and brief exposure to an unfamiliar dialect is often sufficient to overcome unintelligibility or even an impression of strangeness (note that related dialects may differ greatly, sentence by sentence, in phonetic and taxonomic phonemic representations, though perhaps hardly at all on the level of systematic phonemics – cf. in this connection Halle, 1962; also Chomsky, 1959, for an analysis of some of the data presented by Sledd, 1955, 1958, from this point of view). Sapir is the only linguist to have presented careful observations of native perceptual responses relevant to this question, in his classic paper on psychological reality (1933), and his reports are directly counter to the taxonomic account of speech perception. Surely one would expect that in identifying an utterance, the hearer will bring to bear the full grammatical apparatus that determines the space of possibilities from which this utterance is

⁴⁹ To illustrate his point, Joos cites the example of someone who responded to “he has poise” with “what’s a poy?” But this seems rather dubious support for his position, since the hearer in this case was puzzled by the apparent application of the unfamiliar constituent structure rule: $N \rightarrow \text{poy}$, and had clearly assigned a full syntactic structure to the utterance. Thus this example does not support the independence of phonemic representation from syntactic structure in perception.

drawn and the nature and interrelations of these objects. That is, one would naturally expect that, as in the case of other perceptual processes, the hearer's knowledge will provide a complex schema within which the actual signal is interpreted. To the extent that this is true, the "atomistic" view of the taxonomic phonologists will be in error. In any event, presently available evidence does not support the taxonomic model given above as an adequate general account of speech perception.⁵⁰

It remains to consider the status of taxonomic phonemics with respect to a model of acquisition of language. There is, in fact, an approach to the question on these grounds.

Suppose that we impose on the acquisition model the condition of *separation of levels*, which we can interpret as requiring that the level of systematic phonetic representation must be "rationalized" and converted to a level of taxonomic phonemic representation without reference to any morphological or syntactic information.⁵¹ Observe that this condition is not to be confused with the conditions of biuniqueness and local determinacy. These (as all of the conditions (32)) pertain to the "perceptual model"; they assert that the phonemic correspondent to a given phonetic sequence must be determinable by operations involving only neighboring sounds, *once the phonemic system is fixed*. But the condition of separation of levels is not a formal condition on a phonemic system and the rules that relate it to sound; it is a methodological condition on information relevant to determining the correct choice of a phonemic system. It thus pertains to an acquisition model such as (1b), rather than to a perceptual model such as (1a).

Nevertheless, there is a connection between the condition of separation of levels and the conditions of biuniqueness and local determinacy. If no higher-level information is relevant to determining what is the taxonomic phonemic system, it is natural to require that once the taxonomic phonemic system is fixed, on purely phonetic grounds, no higher-level information should be relevant to determining what is the sequence of taxonomic phonemes corresponding to a given sequence of phones. Consequently, an argument in support of the condition of separation of levels would, indirectly, provide a motivation for imposing the conditions of biuniqueness and local determinacy on the perceptual model as formal conditions on the notion "phoneme".

This is apparently the line of reasoning that has been followed insofar as justification for the conditions of biuniqueness and local determinacy has actually been offered.

⁵⁰ For further discussion, see Halle and Stevens (1961), Miller and Chomsky (1963), and references there cited. For discussion in a similar vein on the syntactic level, see Matthews (1961).

⁵¹ One or another form of this is implicit in all substantive discussions of linguistic procedures that I have been able to locate. Some linguists (e.g., Pike and Harris) would allow restricted use of certain higher level information in phonology, where this can be obtained by "cyclic" or "spiral" procedures (cf. Pike, 1947, 1952; Harris, 1951a), but many American linguists insist on strict separation. Glossemicians also mention successive and intricately interwoven procedures of analysis and synthesis (Diderichsen, 1958). The kinds of procedures they have in mind also allow for some sort of interdependence of levels, but the reference to procedures is too vague for the extent of permitted interdependence to be determinable in this case.

Thus, for example, Hockett gives only one argument in support of these conditions in the review cited above (Hockett, 1951), namely, that given these conditions “one knows definitely to what level each fact applies”. Otherwise, we have a “hodge-podge arrangement”. He is concerned here with the context of discovery, not perception, and is offering an argument in support of the condition of separation of levels rather than in support of the biuniqueness and local determinacy conditions directly. Similarly, in his important paper on phonemic overlapping (1941), Bloch offers only one argument (an argument that Joos, in his comments, 1957, considers conclusive) to show why the biuniqueness condition must be maintained, namely, this: “Suppose that we are studying a new and unfamiliar dialect of English, and that we have succeeded in pairing the stressed and the unstressed vowels of such words as *at*, *them*, *could*, *will*, *so* and the like; if we now hear a phrase like *oút of tówn*, with the unstressed vowel of the second word perceptually the same as those which we have already identified with various stressed alternants, how are we to treat this? We must defer the phonemic analysis until we chance to hear a stressed form of the same word, which may not occur at all in the dialect we are studying, or which, if it does occur, we may fail to recognize as ‘the same word’.”

Both Bloch and Hockett are proposing that the condition of biuniqueness must be imposed on the notion “phoneme” because the model for acquisition must meet the condition of separation of levels. But it is important to observe that both of them are presenting an argument that is methodological rather than substantive. They do not suggest that an accurate model of the process of acquisition of language must incorporate the condition of separation of levels – that this is a fact about the design of language and about the intrinsic characteristics of an organism capable of learning a language under the empirically given conditions of time and access. They are considering rather the problems of gathering and organizing data, and thus their indirect argument for the conditions of biuniqueness and local determinacy at most shows that it would be convenient for the linguist if there were a level of representation meeting these conditions, but it does not bear on the question of the existence of this level as a part of linguistic structure.

Let us turn to the question of separation of levels as a substantive issue. As in the case of the conditions (32), two kinds of considerations are relevant: external considerations pertaining, in this case, to language acquisition rather than perception; and purely internal linguistic considerations. As to the former, Hockett has in fact suggested in various places (e.g., 1948) that the successive steps of the analyst should in some way parallel those of the language learner. But clearly the child does not master the phonology before proceeding to the syntax, and there is no possible justification for the principle of separation of levels from considerations of this sort.

It remains then to ask whether this condition can be justified (thus indirectly providing a justification for the biuniqueness and local determinacy conditions) on internal linguistic grounds, that is, by a demonstration that it contributes to the clarity, generality or coherence of a full grammar. But it seems clear that this principle

has rather the effect of detracting significantly from these qualities, and, in fact, that adherence to this principle makes it impossible to attain the levels of descriptive or explanatory adequacy. Consequently, the principle seems to be entirely superfluous, in either its stronger or weaker forms (see note 51).

The effects of strict application of a principle of separation of levels have often been discussed. The matter of word boundary that Hockett cites in his invented example discussed above illustrates the problems that arise when it is adopted. It has long been recognized that a phonemic system is quite unacceptable if no junctures are recognized. Consequently, linguists who adopt the principle of relative or complete separation of levels have attempted to devise analytic procedures that would make it possible to place junctures in appropriate places on the basis of phonetic evidence alone. These procedures make use of phonetic features that appear at utterance boundary to determine the position of junctures medially in utterances. Thus a juncture would be marked in "night rate" because it contains an utterance-final allophone of /t/ followed by an utterance-initial allophone of /r/. Apart from the counter-examples that have already been offered to this principle (and that remain unanswered – cf. e.g., Leopold, 1948; Harris, 1951a, 87; Chomsky, Halle, Lukoff, 1956, § 2) it is clear that it cannot succeed because of examples of the following kind. In many dialects of English, /t/ has the allophone [D] in word final position after a weak stress and before a main stress – thus we have [iDédz] ("at Ed's"), [iDéwr] ("at our"), [ðæDæd] ("that ad"), contrasting with [iténd] ("attend"), [iták] ("attack," "a tack") and with [idépt] ("adept"), [idépt] ("adapt"). But [D] occurs only medially, never finally. Thus any consideration involving utterance boundary will place junctures in exactly the wrong places. Alternatively, if no junctures are placed, [D] must be taken as a third alveolar stop, giving an equally unacceptable phonemic analysis. We must conclude, then, that there is no known method for assigning junctures in terms of phonetic evidence alone. Present methods do not distinguish permissible from impermissible applications, and, consequently, are useless as they stand. It seems unlikely that this difficulty can be remedied, and unless it is, the principle of separation of levels is entirely untenable.

As a second example, consider the much debated subject of English vocalic nuclei. According to a view that is widely held among American structuralists,⁵² these are to be analyzed as short vowels plus one of the glides /y/, /w/ or /h/. On the purely phonetic grounds on which the question must be discussed by those who accept the principle of separation of levels, this is a very neat and well-motivated description. In particular, the post-vocalic /h/, representing a centering glide, can be used to account for such contrasts as "real" /rihl/, "really" /rihliy/ versus "reel" /riyl/, "Greeley" /griyliy/, etc.

If, however, we are concerned with selecting a phonemic system that will be compatible with a full descriptively adequate grammar, this analysis becomes quite un-

⁵² For an account of its background, see Gleason (1961, chapter 19). An important critique is presented in Sledd (1955).

acceptable. Thus observe that on the level of systematic phonemics, the words “real”, “really” will be represented /riæl/, /riæl+li/ (because of “reality”), just as “total”, “totally” are represented /tõtæ/, /tõtæ+li/ because of “totality”, and “mobile” is represented /mõbil/ because of “mobility”. Furthermore, the glide of “real”, “really” is not to be distinguished on the level of systematic phonetics, from that of “total”, “totally”, “mobile” (or, for that matter, “dialect”, “betrayal”, “refusal”, “science”, etc.), namely, [i]. Hence in all of these cases the systematic phonetic representation can be derived from the systematic phonemic by the very general rule of English phonology that:

(42) Vowel → i when unstressed.⁵³

If, however, we wish to provide the taxonomic phonemic representations /rihl/, /rihliy/, /towtıl/, /towtılıy/, /mowbıl/, /dayılekt/, /biytreyıl/, etc., as an intermediate stage of formal description, we must replace the general rule (42) by the three rules:

- (43) (i) Vowels → i post-consonantly, when unstressed
 (ii) Vowels → h post-vocalically, when unstressed
 (iii) h → i post-vocalically,

where the first two relate “morphophonemic” and “phonemic” representations, and the third relates “phonemic” and phonetic representations. Thus again we find that what may very well be the optimal taxonomic phonemic system is not incorporable into a descriptively adequate grammar. The failure to achieve descriptive adequacy, in this case, is traceable to the requirement of separation of levels in the underlying theory.

In his recent review of Halle (1959b), Ferguson (1962) criticizes Halle for his rejection of the biuniqueness and local determinacy conditions (condition (3a) in Halle’s presentation), and offers a defense of these conditions. But he presents the issue incorrectly, and as a result neither his critique of Halle’s position nor his arguments in support of biuniqueness and local determinacy are to the point. Since Ferguson’s is the only recent discussion of this issue from the point of view of taxonomic phonemics, it is important to trace the argument with some care. Ferguson argues for what he calls “the autonomy of phonology”, that is, the view that phonology is entirely independent of syntax and morphology, and that the biuniqueness and local determinacy conditions are thus reasonable. Halle’s position – and the one that I have advocated here – is the direct contradictory of this, namely, the view that *some* phonetic processes depend on syntactic and morphological structure so that phonology as a whole cannot be studied, without distortion, in total independence of higher level structure. Let us call this the view that phonology is “non-autonomous”. A third possible position we may call the assumption of “inseparability of phonology”,

⁵³ This rule is of course incorrect as stated (cf. “relaxation” [rılækséyšin], “condensation” [känděnséyšin], etc.) if it is one of a set of unordered rules of a taxonomic grammar. But it is correct if it is embedded into a transformational cycle of the kind discussed above. Cf. references of note 6 for details.

that is, the view that *all* phonetic processes depend essentially on syntactic and morphological structure. This view has certainly never been advocated by anyone, and it is unnecessary to refute it. But it is the assumption of inseparability of phonology, not the assumption of non-autonomy of phonology, that Ferguson imputes to Halle, and against which he presents a series of arguments (to which we return directly). These arguments against the inseparability of phonology have no bearing on the question of autonomy of phonology. This failure to observe the distinction between inseparability of phonology and non-autonomy of phonology in fact vitiates Ferguson's argument entirely.

Specifically, Ferguson cites in favor of his position the undeniable fact that syntactic and morphological structure are not involved in certain sound changes and in certain aspects of language learning and dialectal variation. This observation is irrelevant to the issue of autonomy or non-autonomy of phonology (though it successfully demolishes the absurd thesis of inseparability of phonology). It is also apparent that morphology and syntax play an important role in specifying the range and character of certain sound changes (cf. much of Kuryłowicz' recent work, or e.g., Twaddell, 1935, p. 79, etc.), of certain aspects of phonological development in language learning, and of certain aspects of phonological dialectal variation. Consequently, to the extent that considerations of the sort that Ferguson adduces are relevant, they show nothing more than the untenability of the thesis of autonomy of phonology. It is true that in plotting isoglosses, "it is often quite clear that subareas of different phonological systems do not coincide well with subareas of grammatical systems and lexical inventories" (Ferguson, 290), just as it is clear that isoglosses drawn for vocalic systems often do not coincide with those drawn for consonantal systems. The argument from this to autonomy is equally apposite in both cases. Similarly, in the case of Ferguson's other examples.⁵⁴

Finally, I should like to comment on Ferguson's assertion that Halle's theory (as the theory of the present paper) does not provide machinery for describing phonetic data that is accounted for adequately by his autonomous phonology. He cites, e.g., the word *Audrey* with the cluster /dr/ as compared with *bedrock* with /d+r/ and *bedroom* with variation between /dr/ and /d+r/. In this case, a "non-autonomous" generative grammar would give rules stating that in *bedroom* the morpheme boundary sometimes does and sometimes does not become a phonetic juncture (depending on dialect or style, as the facts indicate). It would, on the other hand, make no such statement about *Audrey* (with no boundary) or *bedrock* (where the boundary always becomes phonetic juncture). I do not see what is the problem here, or how an

⁵⁴ Ferguson's claim that a phonological theory that does not observe Halle's condition (3a) (biuniqueness and local determinancy) makes diachronic change incomprehensible is particularly astonishing. Would anyone really be willing to maintain that the phonology of, e.g., Sapir and Bloomfield, cannot accommodate sound changes that have been exhibited and explained by the post-Bloomfieldian linguists who have insisted on these conditions? His assertion that the principles of biuniqueness and local determinancy (note that it is just these that are at issue at this point in his discussion) underlie the achievements of the last century represents a curious interpretation of the history of linguistics.

autonomous phonology of the type that Ferguson proposes would handle the situation any differently. Ferguson's example simply shows the absurdity of the claim that *every* morphemic boundary is a phonetic juncture, but surely no one has ever maintained this. What has been maintained is that syntactic and morphological considerations must be taken into account in determining when to handle phonetic facts by placement of junctures, and when to handle them by postulation of new phonemes, and Ferguson's remarks have no bearing on this question.

Summarizing, then, it seems that if we are concerned with descriptive and explanatory adequacy, only two levels of representation can be justified in structural descriptions provided by the phonological component, namely, the levels of systematic phonemics and systematic phonetics. The level of taxonomic phonemics is not incorporable into a descriptively adequate grammar. As noted in § 4.2, this conclusion is close to the position of de Saussure and Sapir, and is close to Bloomfield's practice, though perhaps not his theory.

It is interesting to consider the kinds of criticism that have been offered by taxonomic linguists against de Saussure, Sapir and Bloomfield. Wells (1947) criticizes de Saussure for not making use of the principle of complementary distribution with respect to a particular language in his "phonologie" (but only the analogous principle with respect to all languages). In his long review of Sapir's collected papers (1951b), Harris devotes very little attention to Sapir's fundamental theoretical papers on phonology (Sapir, 1925; 1933), and remarks only (293) that they confuse phonology and morphophonemics. Similarly Joos comments (1957, 92) that "when we look back at Bloomfield's work, we are disturbed at this and that, but more than anything else Bloomfield's confusion between phonemes and morphophonemes disturbs us". But it is important to observe that these and other critics have not actually demonstrated that the position of de Saussure, Sapir or Bloomfield is in any way confused. The criticism relies on the assumption that systematic phonetics has no significant status (so that de Saussure's phonologie goes only "half way" towards Wells' taxonomic phonemics), and that taxonomic phonemics is a significant intermediate level of linguistic structure (so that Sapir and Bloomfield appear to be confusing morphophonemics and taxonomic phonemics in their systematic phonemics). Hence the criticism amounts only to the comment that de Saussure, Sapir and Bloomfield have not developed the level of taxonomic phonemics, but only the levels of systematic phonetics and systematic phonemics. The criticism, then, is only as well-founded as is the status of taxonomic phonemics.

There is, in fact, a real confusion in Bloomfield, and this has perhaps played a role in the development of taxonomic phonemics in American linguistics, at least. Bloomfield's assertion that only two kinds of representation are scientifically relevant on the level of sound (cf. above, p. 951) has had a significant impact on later developments. One of Bloomfield's significant levels is physical phonetics. The other, if we follow his descriptive practice, is close to Sapir's systematic phonemics; or, if we follow his "bundle of distinctive features" theory (1933, 79), it is close to post-Bloomfieldian

taxonomic phonemics. In any event, he explicitly denies any status to systematic (universal) phonetics. (Similarly, Troubetzkoy, despite his thoroughgoing reliance at every step on a universal phonetics, tends to disparage it in his theoretical remarks.) However, as we noted above, phonology of any sort is unthinkable without assumptions involving phonetic universals, and Bloomfield uses them constantly, as do all phonologists. Hence there are implicit assumptions concerning systematic phonetics in his descriptive and theoretical work. Furthermore, from the rejection of a level of systematic phonetic representation as the "lowest level" of representation to be provided in a grammar, post-Bloomfieldian linguists were forced to the conclusion that the phonemic level must be the lowest level of representation. Consequently, phonemic representation must be much closer to actual sound than in the case of the systematic phonemics of Sapir or of much of Bloomfield's practice. In particular, the conditions (32) become well-motivated, for this lowest level of representation, and the principle of complementary distribution is invoked to eliminate obvious redundancy (supplemented by various ad hoc, and ineffective rules of the kind we have discussed above to take account of cases where the representations meeting (32) are too unintuitive).

In short, we find that there is a gradual return, in post-Bloomfieldian phonological theory, from the systematic phonemics of Sapir and (to a large extent) Bloomfield, to a much "narrower" system not too far removed from that of the phoneticians who were Bloomfield's critics (see note 34). It is in this sense that modern taxonomic phonemic representations are "more accurate" (cf. page 961 above), and it is for this reason that they are far more complex than the earlier systematic phonemic representations. In this way, the fundamental insights of the pioneers of modern phonology have largely been lost.

5. MODELS OF PERCEPTION AND ACQUISITION

A concern with perception and acquisition of language has played a significant role in determining the course of development of linguistic theory, as it should if this theory is ever to have broader scientific significance. But I have tried to show that the basic point of view regarding both perception and acquisition has been much too particularistic and concrete. It has failed totally to come to grips with the "creative" aspect of language use, that is, the ability to form and understand previously unheard sentences. It has, in general, failed to appreciate the degree of internal organization and the intricacy of the system of abstract structures that has been mastered by the learner, and that is brought to bear in understanding, or even identifying utterances. With regard to perceptual models, these limitations reveal themselves in such conditions as linearity, invariance and biuniqueness; with regard to models of acquisition, in such methodological conditions as the principle of separation of levels, the attempt to define grammatical relations in terms of co-occurrence, and, in general, the emphasis

on elementary procedures of segmentation and classification that has dominated modern linguistic theory.⁵⁵

These taxonomic models of acquisition are not far removed from the extremely limited paradigms of learning and concept formation, based exclusively on some notion of matching or similarity or possession of a common property from some fixed set of available properties, that are to be found in recent cognitive psychology. But it does not seem plausible that the kind of generative grammar that seems to be descriptively adequate might be acquired in a reasonably brief time (if at all) by an organism that brings to the learning task only a "quality space" and a "distance measure" along these dimensions. Evidence of the kind discussed above suggests that each natural language is a simple and highly systematic realization of a complex and intricate underlying model, with highly special and unique properties. To the extent that this observation can be substantiated, it suggests that the structure of the grammar internalized by the learner may be, to a presently quite unexpected degree, a reflection of the general character of his learning capacity rather than the particular course of his experience. It seems not unlikely that the organism brings, as its contribution to acquisition of a particular language, a highly restrictive characterization of a class of generative systems (potential theories) from which the grammar of its language is selected on the basis of the presented linguistic data. There is no a priori reason to expect that these potential theories are of the highly simple taxonomic variety with which modern linguistics has been preoccupied, and the linguistic evidence seems to show, in fact, that they are not.

In the case of perception of language, as noted above in § 4.4, the step-by-step analytic models of taxonomic linguistics are not in the least convincing. The process of coming to understand a presented utterance can be quite naturally described, in part, as a process of constructing an internal representation (a "percept") of its full structural description. There is little reason to doubt that the full apparatus of the generative grammar that represents the hearer's linguistic competence is brought to bear immediately in carrying out this task. In particular, much of the perceived phonetic shape of an utterance (e.g., in English, the complex arrangements of reduced and unreduced vowels and stress contours) is a reflection of its syntactic structure. It would not be surprising to find that what the hearer (or the phonetician) perceives is an ideal pattern, not incompatible with the signal that actually reaches his ears, that is projected by the phonological component of his grammar from the syntactic description that he has assigned to this signal (cf. references of note 50).

In part, these questions belong to theoretical psychology. But purely linguistic research can play a fundamental role in adding substance to these speculations. A perceptual model that does not incorporate a descriptively adequate generative grammar cannot be taken very seriously. Similarly, the construction of a model of acquisition (whether a model of learning, or a linguistic procedure for discovery of grammars)

⁵⁵ One might cite de Saussure as a source for this preoccupation with inventory and with taxonomic procedures. Cf. (1916, 154).

cannot be seriously undertaken without a clear understanding of the nature of the descriptively adequate grammars that it must provide as output, on the basis of primary linguistic data (cf. § 1.3). It presupposes, in other words, a general linguistic theory that achieves the level of explanatory adequacy. It is clear that we have descriptively adequate grammars, and underlying theories that reach the level of explanatory adequacy, only for a rather narrow range of linguistic phonemata in a small number of languages. It seems to me that present theories of transformational generative grammar provide a basis for extending and deepening our understanding of linguistic structure. In any event, whether or not this hope is ultimately justified, it seems clear that to pursue the goals of § 1 in any serious way, it is necessary to go far beyond the restricted framework of modern taxonomic linguistics and the narrowly-conceived empiricism from which it springs.

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DISCUSSION

ŠAUMJAN:

Professor Chomsky's extensive paper covers such a variety of problems and is so rich in ideas that in my brief discussion of the paper I shall not be able to dwell on the issues brought up therein. Therefore I shall have to pick out for discussion only three problems that seem to be especially important to me personally:

(1) the relation of the theory of generative grammars to linguistic engineering; (2) the subject of systematic phonemics; (3) the essence of a structural description.

1. I fully share the speaker's opinion that it would be a mistake to regard the theory of generative grammars as some novel sort of linguistic engineering. It is true that progress in machine translation and the use of electronic computers for linguistic engineering are extremely important not only from the practical point of view; they also set new tasks before the theory of generative grammars. However it is necessary to emphasize the fact that treating the theory of generative grammars as a variety of linguistic engineering is utterly erroneous.

Every science has two principal functions, cognitive and utilitarian. Although both these functions are closely linked with each other they should be strictly distinguished. A confusion of theoretical and practical tasks of science is seriously detrimental not only to theory but to practice as well. If we wish theory to serve practical purposes and be really fruitful we should not lose sight of the fact that an inner logic exists in the development of science, determining the emergence of new theories and raising it to higher levels. As far as the theory of generative grammars is concerned, it has emerged precisely as a result of the logical development of linguistic science. The theory of generative grammars raises linguistic science to a higher explanatory level and can therefore serve as a more perfect basis for every kind of linguistic engineering. One should bear in mind however, that sometimes theoretical research outstrips practice leaving it far behind. As such it is valuable not from the point of view of immediate practical applications, but from the point of view of scientific strategy as pioneering work with far-off targets. It seems to me that at the present stage of its development the theory of generative grammars should be evaluated from the point of view of the general strategy of linguistic research – as pioneering work with far-off targets that raise linguistic science to a higher level of cognition.

2. I now proceed to discuss the subject of the scientific discipline which Mr. Chomsky calls systematic phonemics.

Chomsky distinguishes four levels in the study of language sounds:

- (1) Physical phonetics.
- (2) Systematic phonetics
- (3) Taxonomic phonemics
- (4) Systematic phonemics.

The difference between physical and systematic phonetics is clear and need not be commented upon.

In order to define the subject of systematic phonemics Chomsky starts by criticizing traditional phonology, which he calls taxonomic phonemics

According to Chomsky, the following conditions are characteristic of taxonomic phonemics:

- (1) linearity
- (2) invariance
- (3) biuniqueness
- (4) local determinacy

After considering in detail those conditions the speaker shows convincingly that they are untenable. Consequently taxonomic phonemics in its present shape is no less untenable.

Having pointed out the untenability of taxonomic phonemics Chomsky proposes to replace it by a scientific discipline which he calls systematic phonemics. In his paper he adduces a great number of examples to illustrate the essence of this new discipline. They give us reason to think that Chomsky's systematic phonemics is nothing but well-known morphonemics in the disguise of a new phonemic terminology. We do not doubt that morphonemics is an important scientific discipline covering the most essential part of the expression plane structure, but it is not clear what it benefits by being disguised in phonemic terminology.

I should like to add that Chomsky has put forward a really fruitful idea – the necessity to replace the usual unsystematized morphonemic rules by ordered rules arranged in a definite hierarchic order. A consistent application of the ordering principle is sure to bring about a radical reform of morphonemics. Chomsky's profound idea of the necessity to introduce the ordering principle into morphonemics is of fundamental importance for a further study of the expression plane of language. Yet it is not clear why, to realize this idea, we have to give up the terms "morphonemics" and "morphonemic rules" in favour of the terms "systematic phonemics" and "phonemic rules".

I admit that the choice of terms is very much a matter of taste. From this point of view there is no reason why a morphoneme should not be called a phoneme. Yet I presume that disguising morphonemic terminology as phonemic may have a bad effect upon definite aspects of linguistic research, as a number of fundamental prob-

lems in the study of language sounds may thereby be obscured and left out of account.

I have already mentioned that Chomsky's criticism of traditional phonemics seems quite sound to me, yet I do not think that phonemics should be replaced by morphonemics. In my opinion we must seek for a way of building a system of phonemics that will overcome the difficulties considered by Chomsky. A possible way has been suggested in my book *Проблемы теоретической фонологии*, in which I expounded a "two-level theory" of phonology. Other ways are also possible.

I cannot go here into the two-level theory of phonology. I shall merely say that it is founded upon principles diametrically opposite to those Chomsky criticised.

Chomsky justly denies the principle of linearity; in this connection I should like to mention that the two-level theory of phonology asserts that the flow of phonemes is not linear, whereas the flow of sounds is.

The speaker rightly criticises the principle of bi-unique-ness. Let me point out here that in the two-level theory of phonology there is no one-to-one correspondence between the phonemic and the physical levels of language.

The same is true of the other principles criticised by Chomsky. The problem of invariance, for example, has found a new solution in the two-level theory of phonology etc.

I should not like to be misunderstood. There are many various ways to solve the fundamental problems of modern phonology. One can choose whichever way one likes, except doing away with phonology as an independent scientific discipline.

3. I shall turn now to the question of a structural description.

I fully agree with Chomsky that the observation level must be distinguished from the explanatory level in evaluating the effectiveness of a structural description. A structural description at the explanatory level should be considered the most effective.

In his writing Chomsky has given a profound criticism of the IC model and has convincingly shown its merits and demerits. One of the fundamental difficulties the IC model runs into consists in the fact that it does not permit of any rearrangement of symbols in the process of generation. As a result the IC model proves inadequate for a description of some important aspects of natural languages; cf. the system of conjugation in English which contains discontinuous morphemes in constructions with auxiliary verbs. In the phrase *I have spoken*, for instance, we have a discontinuous morpheme *have . . . en* whose elements are separated by the morpheme *speak*.

To overcome this and other defects of the IC model Chomsky has suggested a transformation model that permits, for example, the rearrangement of symbols. He has convincingly shown that the IC model, supplemented by the transformation model, makes a powerful generative grammar which adequately describes language.

However I should like to draw attention to a fact which was left out of account in Chomsky's work. Here I mean that the IC model cannot do without spatial consi-

derations confining it to the level of observation. What I have in mind is the linearity of elements. The IC model does not permit of the rearrangement of symbols bound by the relation of concatenation. Although the transformation model permits the rearrangement of symbols, it is not free of spatial considerations which in the nature of things pertain to the observation level.

In view of all this a more general question suggests itself: is it possible to build up a model completely free of spatial considerations which are in principle incompatible with the explanatory level?

As a matter of fact such a model is possible. In one of my papers I have made an attempt to build up a generative model based upon the operation of setting up domination relations among the elements irrespective of their arrangement in the flow of speech. The operation of setting up domination relations is called application, and the model itself – an application generative model. As it is necessary to divest the model of any spatial considerations, I introduce the concepts of linear and metrical distribution. By linear distribution I mean spatial distribution which is given at the observation level. By metrical distribution I mean the underlying relations among the elements independent of spatial considerations. It is precisely with metrical distribution that the application generative model is concerned with.

Transformations are assigned a definite place in the application model. However they are used here not to overcome spatial considerations but to establish the relations of invariance among strings. In other words they are assigned a purely explanatory function which they are made to fulfil in Chomsky's generative grammar as well. Thus his profound ideas about transformations as an effective means of cognizing language at the explanatory level preserve their full significance within the framework of the application generative grammar.

Those are some of the ideas that Professor Chomsky's brilliant paper suggested to me.

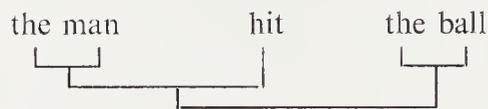
UHLENBECK:

As it is impossible to react adequately in three minutes to the many problems raised by Prof. Chomsky in his stimulating and challenging paper, I will limit myself to three remarks.

(1) Prof. Chomsky is of the opinion that immediate constituent analysis cannot give us a satisfactory and complete syntactic description. Therefore this analysis ought to be supplemented in his opinion by a set of so-called transformational rules. This means however that immediate constituent analysis still occupies a place and an important place in his analytic apparatus.

Immediate constituent analysis is hardly different from the traditional method of parsing, that is it consists of a kind of gradual division of the content of the sentence. Accordingly the sentence *the man hit the ball* is first described by Chomsky as consisting of a noun phrase *the man* and a verb phrase *hit the ball* and these two phrases are further analysed in the usual way into smaller elements till the level of the word is

reached. Actually a pure linguistic analysis of the relational structure of this simple sentence unfettered by logical influence gives a quite different picture. The hearer of this sentence after having heard the first three words, will establish two connections (1) between *the* and *man* and (2) between the *man* and *hit*. Then the fourth word *the* cannot be integrated with the preceding three, but it can be connected with the next word *ball*. After this last word has been perceived by the hearer all connections are established. *The ball* can now be integrated with the already existing group *the man hit*. The syntactic structure of the whole sentence can therefore be represented by the following graph



On page 922 Chomsky remarks that the roots of his theory are firmly in traditional linguistics. I am afraid that his reliance on immediate constituent analysis shows that the roots of his theory are much too firmly in traditional ways of thinking.

(2) My second remark deals with the problem of the relation between syntax and the semantic aspect of language. On page 920 Chomsky remarks that the semantic aspect of *la langue* was not discussed by him, simply because there seems to be little to say that can withstand serious analysis. I am of the opinion that as long as the semantic aspect of language and its relation to syntax has not been clarified, every theory of syntax will remain unsatisfactory and I can find support for this opinion in p. 93 of Prof. Chomsky's well-known book *Syntactic Structures*, where he writes: "There is no aspect of linguistic study more in need of clear and careful formulation than that which deals with the points of connection between syntax and semantics." "The real question" – I am still quoting Prof. Chomsky – "that should be asked is How are the syntactic devices available in a given language put to work in the actual use of this language." Indeed this is the question which ought and in my opinion can be answered and I regret that Prof. Chomsky has not even tried to give to his own, very relevant question even the beginning of an answer.

My third and last remark has to do with the phenomenon of ambiguity. On p. 935 of his paper we find the following statement: "Syntactic ambiguity is generally traceable to derivations from different underlying sentences." I don't think that this is true. There are of course cases of real syntactical homonymy as in *Old men and women*, but it is highly important to notice that most sentences become ambiguous as soon as one starts looking at them detached from their actual, situational and referential setting. Sentences which consist of the same elements and which have the same relational structure may have a different content. This implies that in the study of syntax it is an unpermissible a priori to postulate the existence of syntactical, that is relational differences between two given sentences on the ground that these sentences are "intuitively felt" to be different in content. Language is not a self-

contained system used situation and content free. As soon as it is treated as such one cannot avoid giving a distorted view of the linguistic facts.

CHOMSKY (to Uhlenbeck):

I find nothing in my present paper which is at variance with my earlier remarks about semantics quoted by Prof. Uhlenbeck. From the fact that a subject is intrinsically important, it does not follow that there is something important that can be said about it. I have made a few (quite unsatisfactory) remarks about the relation between syntax and semantics, and how it might be studied, in my paper (§ 2.3), and I would be interested to see how Prof. Uhlenbeck proposes to go more deeply into these questions.

On the question of ambiguity, there is surely no issue. Certain sentences (e.g., "I disapprove of John's drinking", "flying planes can be dangerous") admit of alternative syntactic analyses; others (e.g., "I disapprove of John's drinking the beer", "flying planes are dangerous") do not. A grammar that fails to exhibit this difference in the proper way does not attain descriptive adequacy, and cannot serve as the basis for semantic description. Context is often sufficient to resolve the ambiguity (in ways which are, for the most part, quite poorly understood), a fact which obviously has no bearing whatsoever on the distinction between syntactically ambiguous and syntactically unambiguous sentences, and the necessity for a descriptively adequate grammar to mark this distinction appropriately.

On the matter of simple declaratives such as "the man hit the ball", I am afraid that I see no objection to the traditional analysis into NP and VP, which seems to me both highly intuitive and supported by a variety of linguistic facts. Thus, for example, the rules that form nominalizations require this analysis ("for the man to hit the ball", which is of the form "for NP to VP"; "the man's hitting the ball", which is of the form "NP+Possessive Ing+VP", where Ing attaches to the next item), as do the rules for natural conjunctions (cf. Chomsky, 1957a, p. 35), etc., whereas there is no rule of English, so far as I know, that requires the alternative analysis (((the man) hit) (the ball)), which, it is claimed, is the result of a "pure linguistic analysis". Similarly, the traditional analysis (but not Uhlenbeck's suggested alternative) permits determination of prosodic features by general rules (e.g., the general "nuclear stress rule" of English that weakens the stress of the verb to secondary in the Verb-Object construction requires this analysis - cf. Chomsky, Halle, Lukoff, 1956 - as, apparently, do the rules that determine pitch). In short, it seems to me that the available evidence is entirely in favor of the traditional analysis.

Uhlenbeck's alternative might, indeed, follow from the assumption that in determining the structure of a presented sentence, the hearer proceeds *strictly* from "left-to-right", making all decisions finally about each word before the next word appears, so that only left-branching constructions are tolerated (or from various slightly weaker, strictly left-right assumptions that limit recognizability to essentially left-branching structures). But I find it difficult to believe that anyone would seriously

offer this today as a perceptual model. In fact, were it proposed, it would seem to be adequately refuted by the fact that the analysis NP – VP is supported by internal linguistic evidence. Thus in this, as in many other cases, it seems to me that the traditional analysis is clearly correct, and that the serious problem for linguistics is not to invent some novel and unmotivated alternative, but to provide a principled basis to account for the correct traditional analysis.

HAUGEN:

It is very reassuring to be told by Professor Chomsky in this paper that transformational analysis is “a formalization of features implicit in traditional grammars” (p. 918). While some of us have suspected this, I for one found it helpful to have the originator of this brilliant new trend in analysis make it explicit. The so-called “item-and-arrangement” grammar was an attempt to get away from the quasi-historical statements of structural relationship of these grammars and set up statements which could be regarded as purely synchronic. The results have been unsatisfying because they lacked a dimension which transformation grammar has now restored. They gave a flat, unsubstantial dissection of the utterance instead of a time-dimensioned picture of the process of linguistic creation. Transformational theory seems valuable to me because it puts the *dissecta membra* back together. Without confusing the process of individual construction of sentences with the historical change of language, it permits us once more to state as a synchronic fact the processes which grammarians had always assumed took place during speech and listening.

However, this is not to say that I am entirely happy with all the details of the theory as it has so far been propounded. Here is one which I would like to have clarified. A central feature of transformational practice is the use of an arrow pointing from left to right, which we are told to read “rewrite as”. Two criticisms can be made of this symbol. (1) All analysis appears to proceed from larger to smaller units, in the order which Hjelmslev has called “deductive”. Thus the analysis of a sentence into a noun phrase and a verb phrase is written $S \rightarrow NP + VP$. Is there any compelling reason for this practice, or could we not have a synthesis instead, what Hjelmslev has called “inductive” procedure? As far as I can see, these two approaches are inverse images of one another, and there are cases where I think it would make better sense to say that $NP + VP \rightarrow S$. (2) This leads to the other question, concerning the value of the arrow and the meaning of the term “rewrite as”. As it stands, this definition is meaningless; it refers only to marks on paper, and it says nothing at all about the relation of the symbols to the left and the symbols to the right. In point of fact, it covers at least three quite different relationships, and includes an ambiguity which often has to be resolved by explicit statements in the accompanying text. (a) In the statements of the kernel sentences it means “consists of” or “may be analyzed into”; the item on the left equals the sum of the items on the right, as in the S-formula I have already quoted. (b) In the optional transformations it means “is transformed into, becomes”; these are essentially recipes for changing construction A into con-

struction B by changing its elements in such and such ways, as when a kernel sentence is changed into a relative clause or its verb into a passive. (c) In the rules for derivation it means the opposite of (a), e.g. that when suffix X is added to V, the sum of these is an N. This is different from (b) because here the resultant class is stated, while in (b) it is only described. It may give the impression of a highly unified procedure to use the same arrow throughout a whole grammar, but when we compare it with such standard mathematical symbols as the equals sign, we wonder whether there might not be call here for more than one symbol, or else some more precise delimitation of the meaning of this one.

In the current discussion on grammaticality I think it deserves to be recalled that Otto Jespersen touched the subject in passing a generation ago in his classic book *Mankind, Nation and Individual from a Linguistic Point of View* (Oslo, 1925). In the chapter where he discusses "standards of correctness" he has a section on logic as a guide to correctness. In general he dismisses the traditional application of logic to grammar. But on one point he is firm in defense of logic, viz. when linguists have claimed that it is possible to say absurdities in a linguistically correct way. He writes (p. 116): "I have several times put the question to philologists whether the sentence 'mit runde bord er firkantet' ('my round table is square') is correct, and got the answer that from a linguistic point of view it is irreproachable, while, for example, 'mit rundt bord er brune', contains two linguistic mistakes. (An English parallel would be 'mine round table is brown'.) They overlooked the fact that linguistic 'correctness' must attach just as much to the lexical as to the grammatical element. The two combinations, 'two and two are five' and 'my round table is square' are, as sentences, right grammatically, but lexically wrong: because they cannot be harmonized with the meanings of the words 'two' 'five' 'round'. They are therefore, in reality, linguistically incorrect."

Jespersen, in this passage, seems to be admitting the grammaticality of his absurd sentence. But he insists that beyond the grammar there are lexical rules which exclude this sentence from the language. This is the same kind of rule as would exclude the application of 'colorless' and 'green' to the same object. In transformation theory these would seem to come after the grammatical transformations, at the point where the terminal strings are turned into actual sentences. But it is clear that these rules are outside "grammar", at the same time as they are an extremely important part of our use of language. Without them, sentences would be in the strictest sense meaningless.

It is therefore necessary to set up rules of *lexicality* to supplement those of grammaticality; this will complete a triad which includes *phonemicity*, or should we say *phonematicity*. Rules of lexicality enable us to recognize and evaluate deviations from the norms, as when we say "black is white", or "might is right", proverbial phrases in which the lexical form is violated in order to say something dramatic. In Ibsen's *Peer Gynt* this paradoxical phrasing is common, e.g. in one passage where he says "vold er lempe og lempe er vold" (force is gentleness and gentleness is force). All

such expressions would lose their meaning entirely if there were not lexical rules which limited the availability of certain lexical items.

CHOMSKY (to Haugen):

As I understand Hjelmslev, he is referring to order of analytic procedure when he distinguishes “inductive” from “deductive” process. No decision as to order of analytic procedure is entailed by the manner in which generative grammars are formulated. Furthermore, although the rules of a grammar should determine uniquely the set of allowed derivations, they say nothing about how these derivations are actually to be constructed. If the grammar specifies that rule R_1 must precede rule R_2 , this means simply that in a completed derivation, it must be the case that if rule R_2 has been used to construct a structure S_2 , then R_1 must have been used to construct an “earlier” structure S_1 (where “earlier” is to be given no temporal connotation). How one actually is to construct a derivation (e.g., first using R_2 and then using R_1 to construct the structure presupposed by R_2 , or conversely) is not specified by the generative rules.

Perhaps the point can be clarified by an analogy. A metamathematical definition of “proof” may say that a proof consists of a sequence of lines, each of which is an axiom or follows from earlier lines by rules of inference. A “theorem” then is the last line of a proof. But such a specification says nothing about how one is to construct a proof – e.g., it does not specify that one must first write down the axioms, then the lines that follow from them; or that one must first write down the theorem to be proven, then the lines from which it follows, etc., terminating with the initial axiom; or any of the other possible alternatives. In the one case, the class of well-formed proofs is specified; in the other, the class of well-formed derivations of strings is specified. In neither case is anything said about how one might go about actually forming these objects.

The arrow has been used in various ways in informal presentations, and perhaps this has led to some confusions. I think these can be clarified by referring to more formal presentations, e.g., Chomsky (1955, 1961a) in the bibliography.

I’m not convinced that rules of “lexicality” are necessarily all outside of grammar (for some discussion, see Chomsky, 1961b), though some kinds clearly are (see Katz and Fodor, 1963, for discussion of lexical distinctions that are not incorporable into grammar).

HALLIDAY:

It seems to me that a descriptive linguist today faces the question whether to adopt transformation theory in his own work; and that he faces this question both in his research and in his teaching of linguistics, and in linguistics both “pure” and applied, though the answer might conceivably be different in those different spheres. Some of us who never followed “post-Bloomfieldian” linguistics find that transformation theory has very much more in common with our own ideas; and we need to consider

whether it can account so effectively for what is now known about language that it can be taken as "the" descriptive linguistics of 1962, and all other theories discarded. It would be a great advantage if linguistics had reached this monolithic state. Chomsky has formulated the issue (p. 916) as a choice between a transformational and a taxonomic model for description, and the distinction will serve as a basis for discussion even if ultimately its significance needs to be carefully examined.

The view of language as "rule-governed creativity" is especially to be welcomed, and linguistic theory must certainly allow for a description to be generative in the sense that Chomsky has indicated (pp. 915-18). (It should also in my view allow for textual analysis, so that a description proceeding from exponent to category can be as "theoretical", in the sense that its statements are constrained by the theory, as one proceeding from category to exponent; and for statements of contextual meaning.) Furthermore most linguists would I think agree that the aim of a description is "explanatory adequacy": "finding a principled basis for a factually correct description" (p. 924); we all, for example, distinguish explained absences from unexplained gaps, and seek to reduce the latter to a minimum, just as we avoid *ad hoc* categories and seek those with maximum generality. Chomsky has claimed that a theory based on a taxonomic model cannot, by its nature, attain the explanatory power of one based on transformation. In my view it has been shown that a particular taxonomic model, that of "post-Bloomfieldian" linguistics which is the one that, very naturally, Chomsky has been most concerned to criticise, cannot attain this power; but this has not been shown to be true of a taxonomic model *per se*.

No one can question, it seems to me, the explanatory power of transformational description. But we must distinguish between acceptance of the transformational model on the one hand and rejection of all other models on the other; the two views are not mutually presupposing. (Even if one model is shown to be superior for certain purposes for which linguistic theory is put to use, it is not necessarily superior for all purposes; the derivational history of an utterance, for example, is not necessarily to be equated for all purposes with the patterned properties of the utterance when generated.) It is not enough to dismiss other theories because they appear to fall short *when discussed within the logical framework of transformation theory*, as if the latter was at one and the same time both a theory of language and a theory of linguistic theories. Nor can one invoke some external transcendent logic which can measure and evaluate different theories in a particular science. Each theory must be examined in its own right, and in use, for an assessment of its explanatory power.

This of course applies equally to transformation theory. It would not for example, be relevant to its assessment to point out, from the standpoint of another theory, in which "system" is a primitive term, that transformation fails to distinguish between inter-system and intra-system derivations. Provided always that the results obtained from the use of a theory are "interesting", what counts is its internal validation. In order to evaluate transformation theory, those operating within a different framework have to try to understand not only the account it gives of language but also what

it holds to be the nature and aim of a linguistic theory. This is not easy. A colleague at this congress has likened transformation theory to an iceberg, nine tenths of which is under water, inaccessible to view. From what may be a very imperfect understanding of the not inconsiderable tenth that is showing, I should like to mention some problems that I find in attempting to see inside the transformational model of the working of language.

1) In handling, as it does, very delicate distinctions in a language, how does transformation theory first show the likeness between partially like utterances before showing how they differ?

2) Beyond a certain, often not very advanced, degree of delicacy, languages tend to exhibit cross-classification (subclasses that cut across each other); how is this handled?

3) Correlation between systems in a language ranges from complete dependence through all points to complete independence. For example, the relative probability of positive and negative *polarity* may be partially dependent on the choice of perfective or imperfective *aspect*: an imperfective clause may be less likely to be negative than a perfective one. In the absence of specific determining factors (these may not be discoverable, or they may entail distinctions not yet made at that point), how is this accounted for?

4) Some systems in the grammar of a language may be expounded (carried, manifested) directly by phonology. For example in English, there are systems expounded by intonation (including a number that are by no means very delicate, such as the contrast between neutral and echo WH – question, //1 where are you / going // versus //2 where are you / going //); but the fact that these systems are expounded by intonation in no way determines their place in the grammar. Even if the sentence is not first “cut” into nucleus plus intonation pattern – a cut which seriously undermines a description of English – does the relation imposed by transformation theory between the syntactic and the phonological components permit the correct placing of such systems in the grammar, and their linking with other systems which are not directly referable to phonology?

5) With “sentence” as a primitive term of the theory, how are grammatical relations above the sentence accounted for?

6) How revealing is the transformational model for the description and comparison of texts, for example literary texts?

7) Descriptive adequacy is equated with “a correct account of the linguistic intuition of the native speaker” (p 924). As a native speaker I find some statements about English highly “counter-intuitive” (for example $S \rightarrow NP + VP$, and derivations involving deletion); the required “intuition” seems to me to derive either from traditional grammar or from IC analysis, to neither of which I was exposed in my more impressionable years. What theoretical status, if any, have such references to intuition?

8) Grammaticalness is an important concept, provided it is recognised to be a

matter of degree and provided it is supplemented by "lexicalness"; but the distinction between perfect well-formedness and deviation from well-formedness, as it appears to be drawn for English, I find similarly counter-intuitive. While it is necessary that a theory should be able to account for the variable status of different utterances on this dimension, this need not imply that a decision on the status of each utterance is a precondition of its being described. Is such a decision required by transformation theory, and if so what advantage is gained by this requirement?

9) The formal item ("formative", p. 915) seems to be restricted to morpheme rank, as is "classical" morphemics. This may follow, as other things follow (cf. e.g. 4) above), from the unidirectional order of the generative procedure; what advantage is gained from this (e.g. in textual studies, in linking utterance to situation and in integrating grammatical with lexical statements), by contrast with a theory which allows for formal items of any rank, including that of sentence?

10) What is the theoretical status of (among others) the categories "word" and "phrase" which appear in transformational descriptions? In what relation do they stand to each other and to "sentence"?

11) How are distinctions of register (variety of language according to use) allowed for in the description?

12) Is it assumed that all lexical relations can be handled within the syntactic component? If not, how is a theory of lexis to be integrated with transformation theory?

13) Similarly, how does transformation theory accommodate statements accounting for the relation between utterances and the situation in which they are used? Can a theory of contextual meaning (semantics) be adapted to the transformational model?

14) Different applications of linguistic theory may call for different types of statement, and for descriptions of varying degrees of delicacy. Descriptive statements may take various forms and still be subject to rigorous theoretical constraints. What is the advantage of insisting on certain formulaic statement types ("rules") as the only valid method of description?

Whatever the answers to question such as these, the validity and power of the transformational model as proposed by Chomsky can be regarded as fully demonstrated. What is not demonstrated is that transformation theory has the monopoly of such validity and power. It may be that if the constituent structure subcomponent of a syntax is based on IC analysis there needs to be a transformational subcomponent as well. But to show that the model yielded by the apparatus of phonemics, morphemics and IC analysis is weak, and can be readily outstripped by transformation theory, is not to show that all taxonomic models are weak, or lacking in explanatory power.

If therefore some linguists concerned with the description of languages continue to use, and to develop, theories other than transformation theory, this is not from perversity or insularity but from a conviction that it is in the best interests of linguistics to build on the experience gained in other traditions that have shown themselves to be fruitful. It is clear from the present Congress that the divergent trends in des-

riptive theory are moving closer together all the time. But no one theory can yet claim to have incorporated all the major achievements of midcentury (and earlier) scientific linguistics. Nor have the full possibilities of observation-oriented taxonomic description yet been exploited; and it is important that they should be.

CHOMSKY (to Halliday):

I can obviously make no attempt here to do justice to the many questions raised by Mr. Halliday. However, I would like to emphasize three points. First, I have certainly not proposed that *no* alternative theory of language can ever be constructed that will have advantages over the one I presented. Such a claim would clearly be utterly absurd. Rather, I was concerned to show that a *particular* alternative model – namely, the taxonomic model of modern structural linguistics – is seriously deficient in respects in which the transformational model is not. Secondly, this taxonomic model is, it seems to me, the only alternative theory of generative grammar that has been presented with sufficient clarity so that questions of adequacy can be raised. Finally, no discussion can be undertaken as to the relative merits of generative and non-generative grammars (and the theories that underlie them). A generative grammar has the goal of presenting explicitly the full range of structural information about a given language, and if someone doubts that such grammars are the central concern of linguistic theory, I can imagine no argument that might convince him. This is a question of goal, not of fact.

Questions of fact are those that can be fruitfully discussed. I have tried to show that the theory of transformational grammar is capable (and, for the present, is uniquely capable) of encompassing a wide range of linguistic fact and motivating descriptively adequate accounts (such as, e.g., the many genuine insights of traditional grammar, about which I am apparently much less skeptical than Halliday). Clearly it is not impossible that an alternative may be constructed which will lead to the rejection of this theory. There is no point in discussing this possibility, which clearly exists and will always remain. What is important is to formulate a substantive theory of language with sufficient clarity so that its empirical adequacy can be tested and, where there are alternative assumptions, so that a choice can be made among them on empirical grounds. This I have tried to do with respect to the transformational and taxonomic models, and it is this that must be done, if linguistic theory is to progress, whenever genuine alternatives are proposed.

PIKE:

I deeply appreciate Prof. Chomsky's work, from which I have profited and expect to profit very greatly. Occasionally, however, there appear to be problems in his presentation.

In § 3 of his paper, Chomsky states that data of "introspective judgments" "constitute the subject matter of linguistic theory" – and can only be neglected "at the cost of destroying the subject". How extraordinary it appears, therefore, to see that

in the opinion of some of us he has neglected such data, with the result which he himself predicted – the destroying of the level of phonemics in the conventional sense.

Some of us, however, find introspective judgments less useful – in dealing with preliterate cultures – than the study of objectively *observable* reactions of native speakers as they learn to read, write, or become bilingual. On these slim grounds of our direct experience, for example, I have for a decade and a half insisted that phonemic theory and practice must rest. Nor am I the first to do so – witness Sapir's material. Chomsky's attempt to reject the relevance of Sapir's¹ data to the phonemic level, on the ground of some genuine additional evidence of morphophonemic relevance, to me appears abortive.

For one, I continue to grant the need of a total study of language, including the relation of the phonological hierarchy to the grammatical one. Yet, whereas for a previous decade I urged the relevance of grammatical prerequisites to phonemic analysis, in this decade we similarly need an article on phonemic requisites for grammatical analysis.

The underlying source of Chomsky's rejection of a conventional phonemic level, to me seems to be in the retention of traces of a view of language hierarchy on a modified Trager model (see his *Field of Linguistics*) – a model now abandoned by Trager. Whereas Trager had a single hierarchy from phonetics, to phonemics, to morphology, to syntax, to metalinguistics, Chomsky carries on a related *mono-hierarchical* view which in its logical presentation of rewrite rules of its phrase structure starts from the sentence down. Our experience suggests that the phoneme layer becomes inevitably transparent when initial native-reaction data concerning the phoneme are ignored.

¹ My comment about Sapir is based more on the "Sound Patterns in Language" (1925, *Selected Writings*, pp. 33–60) than on the Psychological Reality article which seems to have been more relied on by Chomsky for his references to Sapir's morphophonemic views. Although Sound Patterns also has morphophonemic data, it has clear data of the conventional phonemic type: e.g., reference to the vowels of *bat* versus *bad* (p. 37), which are "not psychologically parallel" to *bid* versus *bead*. Note, also, the contrast between the "mechanical variant" – subphonemic – of Upper Chinook [t] [d], in which the [d] "exists only as a mechanical variant of t; hence this alternation is not the same psychologically as the Sanskrit sandhi variation -t : -d" (p. 38). Sapir here, in my opinion, differentiates sharply between the psychological status of subphonemic variants and those many of us would now call morphophonemic.

In addition, my own experience would lead me to believe that linguistic experiments would show a substantial degree of psychological reality (1) to conventional phonemes, as units of the phonological hierarchy, and (2) to morphemes, as units of the lexical hierarchy. Reaction to morphophonemic "invariants" of the content of morphemes would then be evidence for the psychological reality of morphemes. I wish, then, to retain a component of psychological reality both for conventional phonemes and for the morphophonemic constancy of lexemes. The nature of their reinforcement and clash needs study in a multi-hierarchical framework. (Compare my *Phonemics*, 1947, pp. 64–65, 160; and Fries and Pike, "Coexistent Phonemic Systems", *Language*, 25, 1949, 29–50).

See, also, documentation for certain environment types for which native reaction implies that a phonemic notation is best used and for others a morphophonemic notation is preferable, in Sarah Gudschinsky, Native Reaction to Tones and Words in Mazatec, *Word*, 14 (1958), 338–45; across word boundaries, morphophonemic tonal notation was preferable; within words, across morpheme boundaries, phonemic notation was needed.

Granted the importance of many of the problems in phonemics pointed out by Chomsky, it nevertheless appears to me that a *multi-hierarchical* approach is needed – (1) A phonological hierarchy which includes not only phonemes but syllables, rhythm units, etc.; and also (2) Lexical and (3) Grammatical hierarchies quasi- (but not totally) independent and interlocking with it.

If this latter approach is used, one finds that Chomsky is right in much of what he asserts, wrong in some of that which he denies.

CHOMSKY (to Pike):

Sapir, in his two fundamental papers on phonology, discusses a system of representation which he calls “phonological” and for which I suggested the name “systematic phonemics”. In later developments, it was proposed that in addition to this level, there is a level meeting the conditions discussed in §§ 4.3, 4.4 of my paper, a level which I called “taxonomic phonemics”. Sapir did not, of course, discuss taxonomic phonemics explicitly. The purpose of his papers was, rather, to present evidence in support of the level of systematic phonemics.

Sapir makes it abundantly clear, both in “Sound patterns” and “Psychological reality”, that systematic phonemics fails the conditions for taxonomic phonemics. Every example in the latter paper illustrates this; in the former, this is shown by his discussion of complete overlap in footnote 2, his explicit statement on relevance of morphological processes to phonetic pattern on p. 42 (in the *Selected Writings*), his discussion of English /ng/ on p. 43, etc.

In my paper, I assumed that the status of systematic phonemics is not in question (cf. p. 951), and I take it that Pike agrees with me on this. I then raised the question whether there is any evidence in favor of the assumption that taxonomic phonemics constitutes a significant level of representation. I tried to show that the internal linguistic evidence is opposed to this assumption, and that there is no supporting external (psychological) evidence of any substance. With reference to Sapir, I claimed that his papers, which provide the only extensive discussion of the issue of psychological reality, offer no support for the existence of a separate level of taxonomic phonemics.

Pike takes issue with the latter claim. He states that I reject the relevance of Sapir’s data to the taxonomic phonemic level, and he maintains that Sapir did present evidence in favor of taxonomic phonemics. In support of his view, he cites two examples from Sapir’s “Sound patterns”: (1) the relation of the vowels of *bit-bead* is not psychologically parallel to the (physically similar) relation of vowels of *bat-bad*; (2) the relation of Chinook t-d is not parallel to the relation of Sanskrit sandhi alternants t-d.

These examples are both characteristic of Sapir’s views, and both seem quite plausible. However, neither supports Pike’s assertion.

Consider first (1). In this case, the taxonomic phonemic representation and the systematic phonemic representation coincide; in both, the vowels of *bid* and *bead*

are distinguished while those of *bat* and *bad* are identified. Clearly the only evidence that has any bearing on the question of psychological reality of taxonomic phonemics as a new and independent level (or on the question of how Sapir viewed this issue) is evidence that falls in the area of *divergence* between systematic and taxonomic phonemics. Consequently, the example in question is entirely irrelevant to the point at issue. Since taxonomic and systematic representations coincide, in this case, Sapir's argument in support of the reality of the representation does not show that he assigned any status to taxonomic phonemics as an independent level.

Consider now (2). In this case, the systematic phonemics of Sapir (and of my paper) will represent Chinook [t]-[d] with a single element, and will represent word-final Sanskrit /t/ as distinct from word-final /d/, even where they fall together as phonetic [d] by the sandhi rules. Consequently, Chinook t,d are treated differently from the Sanskrit sandhi variants t,d in the systematic phonemic representations. The systematic phonemic representations therefore account fully for the intuitive difference, which is presumably why Sapir gave the example. Here too, then, the example has no bearing on the question of the reality of taxonomic phonemics, and clearly does not indicate that Sapir implicitly recognized the reality of this system.

In both cases, then, Sapir's examples simply support his argument for the reality of a level of systematic phonemics, and have no bearing on the question at issue, namely, whether his data supports the assumption that taxonomic phonemics constitutes a separate and independent level, in addition to his systematic phonemics. My claim, concerning Sapir, is that insofar as taxonomic phonemics and systematic phonemics diverge, his data supports only the reality of the latter, and that the whole point of his papers is to show that only the level of systematic phonemics has "psychological reality", in his sense. The examples that Pike cites have no relevance to this claim.

There is no doubt that speakers of a language can make distinctions of varied sorts, and can be made aware of many different facets of language structure. Thus it would surely come as no surprise to find that a speaker reacts differently to syntactically conditioned and syntactically unconditioned alternations, or to alternations that involve "deeply embedded rules" and those that involve "superficial rules" (in some appropriate sense of these terms), just as one would expect to find differences in awareness of rule-governed alternations in the consonantal, vocalic and prosodic systems. Any evidence of this sort would be of value to the phonologist. Pike feels that "linguistic experiments would show a substantial degree of psychological reality to conventional [taxonomic] phonemes." This hope that future experiments or empirical observations may lend some support to taxonomic phonemics is also expressed by Ferguson (1962, note 1), and is no doubt shared by many linguists. I did not, in my paper, deny that this is possible; I claimed only that there is no known evidence of any substance to support this view and, in particular, that Sapir's evidence does not support it.

Pike claims actually to have evidence from "the study of objectively *observable*

reactions of native speakers” that would support the level of taxonomic phonemics. The only study in the literature dealing with this question is Sapir’s, and his conclusion is that the “objectively observable reactions” relate to systematic phonemic representation, and do not correspond to taxonomic phonemic representation. If there is any objective evidence to the contrary, it would seem reasonable to expect that after some thirty years of investigation of taxonomic phonemics, someone would have seen fit to present it. If it is true, as I have tried to show in my paper, that taxonomic phonemics has no justification on internal linguistic grounds, then presentation of this objective evidence from observable reactions becomes quite crucial, since it would constitute the only support for the assumptions that have dominated the study of sound structure since Sapir.

Pike interprets my discussion of taxonomic phonemics as directed against a “modified Trager model”. I do not see how he comes to this conclusion. In fact, Trager’s is one of the few structuralist conceptions not referred to explicitly in the discussion.

W. HAAS:

Mr. Chomsky reproaches “modern linguistics” for being preoccupied with mere inventories and classifications. This reproach, it seems to me, applies only to that poor emaciated version of modern linguistics – those IC-phrase-markers (NP, VP, etc.) – which we can manage to find room for in a generative grammar. Mr. Chomsky’s reproach applies to the taxonomic model of a generative grammar. Here, all that is interesting – principles of classification, operations of analysis, and an order of linguistic facts defined operationally – is dismissed as irrelevant, as some sort of “discovery-procedure”, which is supposed to be devoid of theoretical interest. What is left then, *is* uninteresting.

However, we can gratefully accept a good deal of what Mr. Chomsky argues for, without rejecting what he argues against. His present argument is very largely for “transformations”. It seems to me that we can use transformation-techniques outside generative grammar – use them together with other techniques, and for the purpose of establishing syntactic structures, i.e. we can use them as a “discovery-procedure”. As such, they can help us to cure some of the defects which Mr. Chomsky has pointed out in traditional grammatical analysis. Our techniques of analysis need not be mechanical. Nor need we engage in any mechanical generation of sentences or mechanical evaluation of given grammars.

Let me take one of Mr. Chomsky’s examples:

- (1) John is easy to please.
- (2) John is eager to please.

Without a generative grammar, should we really be incapable of assigning different structures to these two sentences?

As a chemist applies a certain set of test-operations to a substance in order to

describe it adequately – he may weigh it, apply litmus paper or acids or X-rays, etc. –, so the linguist disposes of a certain limited set of test-operations for describing the structures of given sentences. E.g.

(a) *Substitutions*: Can we distinguish our two sentences with regard to permissible substitutions? We find *John is eager to please/go/sleep/laugh/talk/*, etc., but not **John is easy to go/sleep* etc; and we find, on the other hand; *John is easy to please/avoid/rouse/talk to/laugh at*, etc., but not **John is eager to avoid/talk to* etc. We note that, in our description of English we find extensive use of these two distinctive classes of substitutables; they are familiar, *V_i* and *V_t*.

(b) *Expansions*: There are distinctive expansions by members of a familiar grammatical category in (2): *John is eager to please*, viz. *us/everybody/his wife*, etc., but not **John is easy to please us*, etc.

(c) Lastly, we have a distinctive *transformation*: *John is easy to please*. → *To please John is easy*, but not **to please John is eager*.

Such tests will show us that the two sentences have different syntactic structures.

I should be grateful to Mr. Chomsky if he would tell me what he finds wrong with this kind of non-generative analysis – a description on the lines of a structural *recognition grammar*. We shall of course have to admit that there are still many gaps to be filled in this kind of grammar, just as Mr. Chomsky admits that there are many gaps in his generative grammar. But I must confess that the sort of description, which obtains and defines grammatical structures by reference to analytic test-operations, appears to me to be more informative, and also far nearer to the choices and constructions performed by speakers and listeners, than a generative mechanism could ever hope to be.

CHOMSKY (to Haas):

Before turning to Mr. Haas' comments, I would like to make a few terminological remarks concerning the terms "generative grammar", "discovery procedure" and "recognition procedure".

A generative grammar is a device (or procedure) which assigns structural descriptions to sentences in a perfectly explicit manner, formulated independently of any particular language. A particular theory of generative grammar (the taxonomic model, the transformational model) specifies a class of potential grammars (that is, a schema for grammars), and provides a procedure by which a structural description is assigned to an arbitrary sentence by an arbitrary grammar of this class – that is, it explains how a grammar of the specified form provides structural information about sentences. A theory of grammar is open for discussion only insofar as it meets these conditions. To the extent that it fails to provide an explicit characterization of the class of grammars and of the manner in which grammars assign structural descriptions, it cannot be confronted by evidence, and can receive neither criticism nor support.

A "discovery procedure" is a method which, applied to a mass of linguistic data, provides a grammar that purports to give an adequate account of the language of

which this data is a sample. A proposed discovery procedure merits discussion just to the extent that it meets the following two conditions: (I) the grammar that it provides is a generative grammar, as defined above; (II) the procedure is explicit and independent of any particular language (i.e., non-*ad hoc*). Again, to the extent that it fails either (I) or (II), it is untestable. This is obvious when we observe that a proposed discovery procedure can be evaluated only by considering whether the grammar that it provides, given appropriate data, is descriptively adequate (in the sense of my paper, p. 924) and that descriptive adequacy is a meaningful concept only for generative (i.e., explicit) grammars.

Not to be confused with a discovery procedure is a "recognition procedure". Such a procedure, specific to a particular language, operates on an input sentence and by successive analytic operations and predetermined tests, assigns to it a structural description. Insofar as it is explicit, then, a recognition procedure for a particular language is, in fact, a generative grammar of this language. A recognition procedure is, however, more ambitious than generative grammars of the type that I dealt with in my paper, in that it attempts to deal simultaneously, in a single algorithm, with two separable questions: (A) what information about sentences is available in principle to the person who knows the language; (B) how is this information put to use in interpreting an input sentence. The taxonomic and transformational models for generative grammar, as discussed in my paper, attempt to answer only (A), and say nothing about the strategies and heuristic devices that might be involved in (B). Thus in my paper I take the view that to construct an adequate perceptual model, questions (A) and (B) must be dealt with separately; that there is an independent and systematic approach to (A) which can attain a high level of adequacy as long as it does not deal with (B), but not otherwise; that (B) can be successfully studied only on the assumption that the recognition (perceptual) device incorporates a generative grammar that answers (A) as well as a strategy for putting to use the information about sentences represented in this grammar (for further discussion, see Matthews, 1961, and the references of Note 3 of my paper). I do not wish to deal with this question now, but only to clarify what is at stake. Again, a recognition procedure is worth discussing only to the extent that it is explicit, and a theory of "recognition grammar" is worth discussing only to the extent that it is both explicit and language-independent, for just the reasons mentioned above.

In § 1.3 of my paper I have a brief discussion of recognition procedures and discovery procedures, and I express the view there that study of each presupposes a theory of generative grammar, and that (A) and (B) should be dealt with separately. However, Mr. Haas is quite wrong in attributing to me the view that a discovery procedure would be "devoid of theoretical interest". As I tried to make clear in my paper, a discovery procedure would constitute a scientific advance of the highest importance. It would, in effect, constitute a theory of language learning, a hypothesis about the intellectual capacities that are utilized to perform this task. My view is not that development of a discovery procedure would be uninteresting, but rather that

it is, for the present, hopelessly out of the question, and that we do not know enough about language structure to pose this problem seriously.

In the light of these remarks, I would like to turn to Mr. Haas' question. Mr. Haas does not distinguish "discovery procedure" from "recognition procedure". He begins by talking about "general principles of classification, operations of analysis, and an order of linguistic facts defined operationally", and ends by discussing a "structural recognition grammar" which is apparently to be specific to a particular language. These are entirely different notions, however, and it is difficult to proceed not knowing which he has in mind.

Mr. Haas does not (here or elsewhere) present an explicit general discovery procedure or an explicit recognition procedure for a particular language; nor has he described the character of a procedure of either sort in a form which permits discussion or evaluation. Rather, he takes it for granted that structural linguistics has already somewhere developed such procedures, and he simply provides ad hoc suggestions, not stated in any general terms but specific to a particular example, as to how these procedures might operate. But in fact it is entirely beyond argument that structural linguistics has presented no explicit and general discovery procedure, in the sense defined above, and that it has, furthermore, never presented for any particular language a recognition procedure for assigning structural descriptions to sentences of this language, nor has it even proposed any theory as to the character of such recognition grammars. This is not a matter of a few unfilled gaps, as Mr. Haas asserts. Rather, there is not even the hint of an explicit procedure, of either sort, in modern structural linguistics. This is not surprising, since the problems of constructing an adequate acquisition model (discovery procedure) or perceptual model (recognition grammar) is extremely difficult, and, in my opinion, hopelessly premature (particularly in the case of the former).

Failure to appreciate the extent and seriousness of the "gaps" may perhaps be traced to two causes. First is the general taxonomic bias of modern linguistics, that is, its preoccupation with principles of segmentation and classification to the exclusion of any concern with the form of a generative (i.e., explicit) grammar and the manner in which it provides structural information. The second cause is no doubt the extremely vague way in which the term "procedure" is often used in structural linguistics (for some examples, see Chomsky, 1960, or the last reference of note 51 of my paper).

Mr. Haas asks me to comment on his proposal for a "non-generative recognition grammar". Insofar as this recognition grammar is explicit, it is a generative grammar; hence the issue between us is not over generative vs. non-generative grammar. Insofar as this recognition grammar is not explicit, there is nothing to discuss. Thus the only issue that we can discuss is how Mr. Haas' theory of generative grammar will compare with the taxonomic and transformational theories of generative grammar; and this question will be open for discussion only when his theory receives an explicit formulation, and particular explicit recognition grammars are constructed in accordance

with it. But Mr. Haas goes on to say that, in his view, there is no need for his procedures to achieve explicit formulation (i.e., to be “mechanical”). I can only interpret this as a refusal to submit his theoretical conceptions, or his proposed grammars for particular languages, to empirical test. Consequently, I can make no attempt to answer Mr. Haas’ final question. He has asked for evaluation of a kind of grammar that is not, and apparently will not be formulated in such a way as to permit discussion or evaluation.

LONGACRE:

Mr. Chomsky says: “It seems clear that examples such as these are totally beyond the range of any version of the taxonomic model, as so far conceived.” He gives us an analytical chestnut, sentence (9): “I don’t approve of his drinking.” This sentence, he shows us, is ambiguous in that it can mean either the *fact* of his drinking (“and is expandable to “his drinking the beer”) or can mean the *manner* of his drinking (and is expandable to “his excessive drinking”).

In the Summer Institute of Linguistics we have for several years now been analyzing languages on a world-wide basis within the framework of tagmemics. We have no difficulty with analytical chestnuts of the sort here illustrated and handle them frequently. Yet, in fn. 3, Chomsky classifies tagmemics as a taxonomic model essentially of the immediate constituent model. It therefore seems evident that if tagmemics handles problems of this sort – problems not amenable to taxonomic models as defined by Chomsky – then it follows that either (1) tagmemics is not narrowly taxonomic, or (2) taxonomic grammars are not as ineffectual as claimed. A third possibility may be that the opposition between taxonomy and generation is not a useful one.

We would handle the ambiguity in sentence (9) by setting up two contrasting total formulae both of which may have homophonous minimal manifestations:

- + Possessive + Gerund ± Object
- + Possessive ± Adjective + Nominal

Please note that these formulae are not analyses of particular sentences. Functional segments indicated as plus-minus in the formulae must be present or absent within a given sentence; they cannot be both present and absent. These formulae are not therefore mere labelled segmentations of particular sentences, and tagmemics is not preoccupied with endless segmentation and labelling. Although the formulae are ultimately based on the analysis of some sort of corpus they extrapolate beyond the corpus and make possible novel formations. Furthermore, such formulae are not merely ad hoc devices trumped up to solve particular problems but are tools which we use regularly. These tools are based in a theory not only of language but of language in relation to human behaviour as a whole. I would claim for them, therefore, the level of explanatory adequacy as defined by Chomsky.

CHOMSKY (to Longacre):

The taxonomic model is proposed as a formalization of those devices made avail-

able for generation of strings and structural descriptions by ordinary IC analysis. The formulas that Mr. Longacre proposes can be stated as well within the framework of ordinary IC analysis as within the tagmemic framework, namely (using the notation I proposed for the taxonomic model), with the rewriting rules:

- (1) NP → Possessive Gerund (Object)
- (2) NP → Possessive (Adjective) Nominal
- (3) Gerund → drinking; Possessive → John's; Object → the beer;
Nominal → drinking (of the beer); Adjective → excessive, etc.

With this analysis, "John's drinking" is assigned two structural descriptions. But within the IC (and tagmemic) framework, the analysis seems to me formally unmotivated. It can, e.g., be replaced by the incorrect, but simpler, analysis that keeps (2) and (3), but replaces (1) by

- (1') NP → Possessive Gerund Object

where optionality of Object need not be stated, and where "John's drinking" is no longer assigned two structural descriptions. Thus I do not see how explanatory adequacy can be attained within the framework of the taxonomic model. There do not seem to be any formal grounds for selecting the correct alternative.

In any event, this discussion of explanatory adequacy is beside the point in this case, since not even descriptive adequacy is attained by the assignment of structural descriptions given by either (1)–(3) or (1')–(3). Within IC analysis, tagmemics, and the taxonomic model generally, the structural descriptions assigned would be, in each unambiguous case, a single labelled bracketing. This simply fails to express the relevant information concerning the generated strings. Thus it does not convey the information that the grammatical relations between "John", "drink" and "the beer" are the same in "John's drinking the beer", "John's drinking of the beer", and "John drinks the beer" (as, furthermore, in "the beer is (too strong) for John to drink", etc.) a structural fact that underlies the correct semantic interpretation of these sentences. Nor is it capable of expressing the equally important fact that (i) "John's drinking the beer" and (ii) "(the fact) that John drinks the beer" are structurally similar to one another, e.g., in the respect that neither has the *internal* structure of a Noun Phrase, but only the internal structure of a full sentence; while both are structurally distinct from (iii) "John's (excessive) drinking of the beer". This parallel between (i) and (ii), as against (iii), appears to be what underlies the semantic interpretation of these sentences (and the ambiguity of "John's drinking"). Hence just those deeper aspects of structural description that are revealed by the transformational analysis (and that seem, furthermore, to be formally motivated in the transformational generative grammar) are not expressed in the structural description provided by the taxonomic systems.

It is for such reasons as these that it seems clearly necessary to develop a richer notion of structural description than is provided by the taxonomic models (and I still

see no respect in which tagmemics diverges from this framework), e.g., as suggested in my paper, a theory that assigns multiple Phrase-markers to utterances. The transformational model seems to me, for the present, to be the only one that succeeds in assigning a sufficiently rich and intuitively correct structural description in a formally motivated way in such cases as the one under discussion.

To repeat the crucial point, the problem in such cases as this is not to devise a set of formulas which will cover an ambiguous sentence in two different ways. It is, first, to devise a set of rules which will assign descriptively adequate structural descriptions to each sentence; and, second, to motivate this set of rules on general, non ad hoc grounds. It is in the latter two respects that taxonomic models seem to me deficient.

GLINZ:

1. Ich bin sehr einverstanden mit der ersten und zentralen Feststellung von Chomsky, dass als Ausgangspunkt aller Sprachtheorie festzuhalten ist (S. 914 oben): "a mature speaker can produce a new sentence of his language on the appropriate occasion, and other speakers can understand it immediately, though it is equally new to them". Ebenso bin ich sehr glücklich darüber, dass Chomsky sich an wesentlichen Stellen auf *Proben* stützt, auf mögliche Umwandlungen eines gegebenen Satzes (z.B. S. 928f.).

2. Um sicher zu sein, dass ich richtig verstehe, möchte ich Chomskys Charakteristik von "generative grammar" und "descriptive grammar" (S. 915) in meiner eigenen Ausdrucksweise wie folgt wiedergeben:

generative grammar	descriptive grammar
gibt eine Rechenschaft von den <i>möglichen</i> Verwirklichungen in einer Sprache, beschreibt das <i>Funktionieren</i> der Sprache (in der Annahme, dass das die Hauptaufgabe einer Grammatik sei).	registriert und ordnet nur die <i>schon erfolgten</i> Verwirklichungen, die als feste Gestalten in einem <i>Korpus</i> vorliegen (in der Annahme, dass damit das Wesentliche geleistet sei).

Ebenso möchte ich mich vergewissern, ob ich die Stelle S. 935 unten richtig verstehe, wenn ich "different underlying sentences" (die für syntactic ambiguity verantwortlich sein sollen) mit "verschiedene zugrundeliegende *Satzpläne*" übersetze.

3. Wenn diese Wiedergaben den Sinn des von Chomsky Gesagten richtig treffen, dann ist meine ganze Arbeit (vgl. *Die Innere Form des Deutschen*, Bern, 1952, 3 1962) zum grossen Teil "generative grammar", wie Chomsky sie fordert; sie beschränkt sich nicht darauf, die in einem Korpus vorkommenden sprachlichen Gestalten einfach zu registrieren, sondern arbeitet die *Möglichkeiten* des Gestaltens überhaupt in der betr. Sprache (in meinem Fall im Deutschen) heraus. Ebenso ist meine ganze Arbeit per definitionem "transformational grammar", indem alle ihre Ergebnisse auf möglichen Umformungen von Sätzen basieren (Klangproben, dann vor allem Ersatz-

proben und Verschiebeproben). Freilich ist mein Ziel dabei nicht ganz dasselbe wie bei Chomsky: ich gehe nicht darauf aus, diese Umformungen in einer Folge von stets nur zweigliedrigen Schritten zu fixieren und das Gesamt dieser Schritte in ein möglichst einfaches algebra-ähnliches System zu bringen, sondern ich betrachte Transformationen aller Art nur als das gegebene wissenschaftliche Mittel, um unabhängig von logischen, psychologischen oder andern Vorurteilen die spracheigenen Einheiten und Kategorien aufzufinden und abzugrenzen. Ich halte nämlich, mindestens zu einem grossen Teil, *nicht* die Transformationen für das Elementarste in der Sprache, sondern die *Sprachinhalte* (Kategorien, Strukturinhalte und Wortinhalte) selbst; ich glaube, dass an sich, sachlich, die Möglichkeit oder Unmöglichkeit aller Transformationen auf der Einzelbeschaffenheit und dem Strukturzusammenhang dieser Inhalte beruht, und nicht umgekehrt. Das verträgt sich sehr wohl mit meiner Überzeugung (die ich in meiner Beschreibung des Deutschen in die Praxis umgesetzt habe), dass unser wissenschaftlich-methodischer *Weg* zu den Inhalten aller Art in erster Linie über das sorgfältige Sammeln und Vergleichen der naheliegenden und überhaupt möglichen Transformationen führt.

4. Schliesslich möchte ich in diesem Zusammenhang die bescheidene Bitte wiederholen, die ich schon vor fünf Jahren in Oslo an meine englischen und französischen Kollegen richtete (vgl. *Proceedings of the Eighth International Congress of Linguists*, Oslo, 1958, S. 209–10): dass man bei den Transformationen deutlich unterscheidet, ob man die Reihenfolge der Glieder ändert ("Yesterday John went to Detroit / John went to Detroit yesterday", von mir für das Deutsche "Verschiebeprobe" genannt) oder ob man ein Glied durch ein anderes ersetzt ("John/the boy/he went to Detroit", von mir "Ersatzprobe" genannt). Dann wäre es auch sehr nützlich, wenn man für jede von diesen beiden Transformations-Arten auch im Englischen einen eindeutigen Namen hätte (für die Verschiebeprobe scheint "substitution" als eindeutige Bezeichnung vorhanden zu sein; dagegen scheint mir das öfters anzutreffende "commutation" bald für die Verschiebeprobe, bald für die Ersatzprobe verwendet zu werden). Diese Bitte mag kleinlich erscheinen, aber ich habe mehr als einmal die Erfahrung gemacht, dass aus kleinen terminologischen Ungenauigkeiten die grössten Missverständnisse zwischen Linguisten entstehen können.

CHOMSKY (to Glinz):

In Glinz's remarks and my paper I think one can detect traces of a common approach to many linguistic questions, beneath what seems to be a rather wide divergence of both terminology and specific goals. Thus, e.g., Glinz's use of the word "transformation" covers both some of the kinds of process to which I have applied the word (in particular, permutations), as well as at least some that I would have described with phrase structure (rewriting) rules (e.g., his "Ersatzprobe"). Furthermore, he seems to be concerned with transformations only as a device for revealing categories, etc., rather than (as in work in generative grammar) as devices for determining the range of possible structures in the language as well. I believe we also

have a rather different estimate of the difficulty and significance of the task of constructing a systematic and consistent generative grammar that meets a high level of adequacy.

STUART:

Some important conclusions of Chomsky's paper receive an indirect support from work that I have been doing along quite different lines in the past two years. The need for an understanding of phonologic ordering relations in order to achieve empirical interpretations for derived theoretical forms suggests that we should re-examine our views as to the relative completeness of our understanding of what is and what is not phonologically important. Secondly, it seems necessary that we should pay strict attention to the *formal* definition of relations, especially at the syntactic level; relations tend to evaporate when labeled by such terms as "subject", "component", etc., but it is even more clear that attempts to define the relations underlying the physical exponents of a given language system by formulations made within that system can only lead to disaster.

More generally, the stress Chomsky lays on distinguishing taxonomic from theoretical studies is crucial, but can be overemphatic if it leads to a merely fashionable contempt for taxonomy. I should have thought it clear that we haven't nearly enough data yet in the form of taxonomic constructs; but he is right in the suggestion that we need more theorists of a logico-philosophical kind (who must also be well trained linguists with field experience). I think I am right in saying that Chomsky now accepts my view that transformation theory is itself a taxonomic theory, but this should not obscure the fact that it is a particularly interesting case. Important difficulties arise, however, when we consider the explanatory role of theory, and these difficulties are in no way less important in the case of theories which are logical constructs from empirical data, as in the case of transformation theory.

In particular, it seems to me that difficulties arise from Chomsky's view that a language may adequately be characterized as a set of sentences.

CHOMSKY (to Stuart):

In some sufficiently vague and general sense of the word "taxonomic", I have no doubt that this label can be applied to a transformational grammar. But I suggested "taxonomic" rather as a technical term to apply to a class of grammars based exclusively on segmentation and classification, without ordering of rules, and assigning only a single Phrase-Marker as full structural description on the syntactic level, while assuming the existence of a level of representation meeting the conditions of linearity, invariance, biuniqueness, and local determinacy, etc., on the phonological level. In this technical (and, I think, both useful and accurate sense), a transformational grammar is not taxonomic.

I am afraid I do not really see in what way the remainder of Mr. Stuart's comment applies to any position that I have advocated.

Incidentally, the attempt to demonstrate that the taxonomic model is ultimately inadequate is in no sense to be interpreted, as Stuart suggests, as an expression of "contempt" for this model. I cannot speak for others, but I, at least, find the model extremely interesting and have spent a great deal of time exploring its potentialities and formal properties (cf., e.g., the joint papers with Miller and Schützenberger in my bibliography, and the papers cited in these).

GAGE:

First, I would like to remark on what I felt were a couple of shortcomings of the report in its dealing with, as it is called there, "systematic phonetics". For one thing, the text makes it sound vaguely as though the mere assumption of the existence of the Universal Phonetic Alphabet and acknowledgement of our dependence upon it, apart from knowing it in detail is supposed to make phonology thinkable.

Another point the paper doesn't seem to make sufficiently clear is that taxonomic, or inventorial, or Tory phonemics, for short, and systematic phonemics are, as one might put it, perhaps analogous but certainly not homologous. The postulated level of systematic phonetics, on the other hand, fulfils all the alleged requirements on Tory phonemics: Relative to the level of phonic substance, or "physical phonetics", it has strict linearity, absolute invariance, bi-uniqueness, and self-determinacy to within universally insignificant limits of one phone slopping over onto its neighbors. Furthermore, it enjoys separation from other levels. What the present theory denies is that there is a level, call it functional phonetics, – a level on which users of a language identify certain unlike systemic phones which are non-contrastively distributed.

A further comment comes to mind when considering the logical basis of the model advocated in the paper. Considerable attention in the discussion of generative grammars has been given to the question of the power of various models, in the sense of their ability to impose complicated restrictions on what they specify as output. Some have held that only a model of at least transformational power is capable of giving in any straightforward way an account of the dependencies found in languages. If the rules in the transformational cycle in the middle of the phonological component are, as their name implies, truly transformational, than why isn't it otiose to introduce this degree of power twice? In fact, there would seem to be a strong burden of proof put on any assertion that it is indispensable both for phonology and also in the transformational part of the syntactic component.

Most basically, I fail to see how the model presented can be considered a theory of language until it has bridged the gap to "linguistic performance". Viewed from the outside, either casually or as investigated by Tory grammarians, what has the appearance of being the language is the set of all tolerable utterances. It is in effect one contention of the present paper that the "language generated by the grammar" is the language as viewed from the inside. But what a language really is ought in some way to be connected to what we can find out about it. (To take one way of looking at it, if we imagine some sort of a density function defined on C^* , the set of tolerable

utterances showing the same regularities as those found in some finite corpus, C , there is a nagging suspicion that the "language generated by the grammar" would be a subset of measure zero in C^* .) There needs to be some orderly procedure relating the grammatical to the tolerable.

CHOMSKY (to Gage):

This is not the place, and I am not the person, to discuss systematic phonetics in any detail. Some references are given in my paper (§ 4.2). For a recent discussion of what seems to me the most interesting and promising approach to a theory of systematic phonetics, see Halle (1959b). I would not, incidentally, wish to assume that systematic phonetics meets the analogues of the conditions (32) of my paper.

The issue of generative capacity does not arise in the case of the phonological component, since in any case, this simply represents (up to free variation) a many-one mapping of the output of the syntactic component into strings of phones. The transformational rules of the phonological component are those that specify the processes that determine the phonetic shape of utterances in terms of their syntactic structure. Insofar as such processes exist (which is surely undeniable) the assertion that the phonological component must contain transformational devices along with rewriting rules (as these terms have been defined) needs no justification; it is simply a statement of fact.

Clearly many factors are involved in determining what is a "tolerable utterance", grammatical structure being only one. There is no reason why one might not try to investigate the problem of presenting a closer characterization of the notion "tolerable utterance" than is given in the study of grammatical well-formedness (generative grammar), by considering the effect of these other factors in determining the set of well-formed sentences. Contrary to what Mr. Gage implies in his comment, this issue has never even been raised, let alone studied in taxonomic structural linguistics. Nor was this simply an oversight. As long as linguistic investigation is limited to systems of elements (inventories), and as long as no attempt is made to specify precisely the underlying processes that determine the full range of grammatical structures, the study of the delimitation of the set of "tolerable utterances" cannot be undertaken. In fact, the problem of delimiting the set of "tolerable utterances" was raised almost at the outset of work in generative grammar, and has been studied to some extent. In particular, there has been some investigation of the effects of limitations of memory in determining which of the grammatically permissible structures can be actually produced and interpreted, and there are some results bearing on this question. See Chomsky, *Information and Control*, 1959, and, for further discussion, the items Chomsky (1961a), Miller and Chomsky, Schützenberger and Chomsky, in the bibliography at the end of my article above.

FRANCESCO:

This paper suggests some significant psychological implications. At the end

Chomsky seems to point out that there may be some relation between the highly complicated system of transformational grammar he sets up, and the way the human mind is acting in the process of first language learning. How do you expect to be able to put together this possibility with the wide range of actual language structures? In fact there is here the danger of calling again for the old problem of "Weltanschauung".

GALTON:

I wonder whether the purpose of explanatory adequacy is really served by a rule specifically excluding say *ftik* and words of similar build from the possible morphemes of English. It seems to me that all this is still only a matter of taking inventory; the observational, descriptive, and explanatory adequacy only correspond to successive levels of generalization, abstraction, and explicitness, but no value judgement such as is implied by Prof. Chomsky seems to me to be borne out by them.

What exactly is supposed to be explained at the explanatory level? We are simply up against the outcome of largely historical change here: *ftik*, the speaker's example, happens not to be Russian, but it might well be and serve as the designation of some new technical device (for instance) which could be taken over just as *sputnik* or *Blitz*. If that sort of thing happens, you have to alter your rules, always hobbling, as it were, after the facts. You really remain at the level of observational adequacy *all the time*. A real "explanation", I submit, is always historical.

CHOMSKY (to Galton):

To achieve explanatory adequacy, it is necessary not only to state a rule excluding *ftik* but, and this is the crucial point, to justify putting this rule into the grammar, and not putting into the grammar the rule excluding, e.g., *blik*. The grammar that the child has internalized excludes *ftik* explicitly, but not *blik* (though it does not list *blik* in the inventory of elements). Were we able to achieve explanatory adequacy, in this case, we would have a hypothesis about how the child comes to make this distinction; that is, about the general assumptions concerning the character of language that must be attributed to him to account for the fact that he learns the way he does. Obviously historical evidence is totally irrelevant; the native speaker who knows nothing of the history of English is perfectly aware of the distinction between accidental gaps (e.g., *blik*) and non-accidental gaps (e.g., *ftik*). What is to be explained is just his awareness of this distinction.

ABRAMSON AND LISKER:

We wish to take exception to the way in which acoustic phonetic evidence was used by Professor Chomsky in his argument against the invariance condition in phonemics. The general point to be made is that the notion of the phoneme, as it is generally used by linguists, is based on perceptual operations, not on the acoustic substance of speech; therefore, there is no reason for supposing that a string of phonemes will have

a one-to-one representation in the acoustic manifestation. All evidence is to the contrary (see Lisker, *Language*, 33, 370–374). What is typically found, instead, is a combination of physical features overlapping in the temporal and spectral domains, that serve as auditory cues to what the analyst, or indeed the native speaker, *hears* as a string of phones.

The case in point here is that of the perception of English initial stops. It is claimed that the experiments done by Carol Schatz (*Lg.*, 30, 47–56) show that “/p/, /t/ and /k/ overlap”, that is, that a given consonant may be heard as more than one of them, as the following vowel is varied. Interesting and important as her work was in 1954, moderate attention to related work reported at about the same time and later will show that no such interpretation is tenable.

A brief review of the genetic and acoustic features of English initial voiceless stops may be helpful here. There are two parallel sets of events underlying this kind of utterance: (1) a sequence of different shapes of the vocal tract, and (2) a sequence of different kinds of sound sources. Air pent up behind an occlusion appropriate to the stop to be uttered is suddenly released; what with this movement of the articulator away from the point of articulation, the tract starts changing its configuration into one, either steady or continuously varying, that is appropriate to the vowel that – in the sense of a string of phonemes – follows the stop. Acoustically, at the moment of release there is a shock excitation of the vocal tract giving a transient noise often called a burst. This is followed by changing formants. That is, as the articulator moves away from the stop occlusion, the formants bend in response to the changing configuration of the tract. This part of the syllable is what is covered under the label of formant transitions; in stressed syllables, much or all of the transition phase is filled with turbulence (aspiration) until glottal excitation (voicing) begins to take over.

The formants then assume frequency positions, fixed or varying, appropriate to the rest of the syllable.

Now, the question is, how much of such an utterance is relevant to the perception of the initial stop and how much to the vowel? In early work with synthetic speech (cf. Liberman, Delattre, Cooper. *Amer. J. Psych.*, 45, 497–516), the Haskins group varied the frequency position of a burst of noise before a variety of steady-state vowel formants. The steady-state formants played by themselves were heard only as vowels. With a burst before, they were heard as stop plus vowel. One dramatic result, often cited, was that a burst at 1440 cps yielded the syllable /pi/ when combined with the vowel /i/, /ka/ when combined with the vowel /a/, and /pu/ when combined with the vowel /u/.

It was not the burst alone but rather the particular combination of burst and formant pattern that was heard as a particular CV syllable. This was confirmed by a further experiment in which bursts presented in isolation were scarcely identifiable as stops. Since that study used the term “schematic stop” for the burst, in as much as this was the only acoustic cue provided for stop identification, it is under-

standable that Schatz equated stop and burst when she replicated these experiments using real speech. She manipulated bursts and vowels by cutting and splicing magnetic tape recordings and got rather similar results.

Although it was then evident that bursts furnished important cues to stop identification, it was already experimentally established that potent cues were also to be found in the formant transitions. (Cooper, Delattre et al., *JAcous. Soc. Am.*, 24, 597-606; Liberman, Delattre et al., *Psychol. Monogr.*, 68-8, 1954). Thus, a sharp segmentation in the acoustic domain between the stop and the vowel turned out to be impossible. That is to say, if one insists on assigning acoustic events to the phonemes, then the formant transitions belong to both the consonant and the vowel.

Professor Chomsky, then, is seizing upon one acoustic feature out of a complex of acoustic cues and basing an argument on the fact that this feature, the burst, is not always heard in its own right as the same phoneme. If you will allow a shift from the situation of partial temporal overlap to one of complete overlap, that is, simultaneous spectral overlap between phonemes, perhaps the following is a parallel case of equally unacceptable reasoning: the three high vowels of Standard Thai /i i u/ are all likely to have the same first formant (e.g. for a given speaker, ca. 360 cps.; cf. Abramson, *The Vowels and Tones of Standard Thai: Acoustical Measurements and Experiments*, Bloomington, Table 1.2, pp. 33-35). Now if you wished to pull out one prominent acoustic feature and use it in an argument against the notion of invariance, you could say that this first formant is heard as /i/ in one context, /i/ in another, and /u/ in a third. Such situations can easily be found in other languages.

Whatever the merits of Chomsky's arguments on the invariance condition in phonemic theory, he gains no support by equating one bit of the speech wave with a particular phoneme.

CHOMSKY (to Abramson and Lisker):

In my paper, I pointed out that the linearity and invariance conditions, as imposed by taxonomic phonemicists, are not consistent with acoustic evidence, one of my main points being exactly what Abramson and Lisker assert, namely, that "the notion of the phoneme, as it is generally used by linguists, is based on perceptual operations..." so that "there is no reason for supposing that a string of phonemes will have a one-to-one representation in the acoustic manifestation". I pointed out, however, that this fact is at variance with the theory of taxonomic phonemics, as formulated in its most careful presentations. Therefore, I do not see the issue that seems to motivate the comment by Abramson and Lisker. Apparently they have either understood me to be in support of the taxonomic position that I was criticizing, or else, perhaps, they think that I have misinterpreted the views of taxonomic phonemicists. To settle any doubts on the latter score, it is sufficient to compare the experimental procedures of Schatz (1954) with the methods of determining phonetic representation proposed, e.g., by Harris, 1951a, chapters 3, 4. In fact, in Lisker's 1957 paper referred to in the intervention, taxonomic phonemics is criticized for just these assumptions (though

Lisker, then at least, seemed willing to accept a modified version of the linearity principle).

The invariance and linearity conditions, as proposed by taxonomic phonemicists, constituted an interesting proposal concerning language, with a clear physical meaning. As linguistic and experimental evidence has accumulated, it has become clear that the proposal was incorrect, and that the defining features of phonemes must be a good deal more abstract than was originally thought. Thus Halle proposed an abstract set of acoustic invariants to account for the Haskins data on stops (*J. Acoustic Soc. of America*, 1956, p. 511); and, as an articulatory analogue, the Haskins group have recently moved from their earlier assumption that invariants of phonemes are to be found in articulatory movements, to a view rather like that referred to in Note 49 of my paper, namely, that the “invariants” are rather to be found in instructions on a “higher neural level” (cf. Lisker, Cooper, Liberman, *Word*, 18, 1962 82–106). I suggested in my paper that it would, in fact, not be too surprising to discover that in determining the phonemic representation of a presented utterance (i.e., in identifying it), the perceiver may even use high-level syntactic information about how this utterance is generated, in addition to acoustic cues, as one would expect in any complex perceptual process. At some “high enough” neural level, or in some sufficiently abstract sense of “neural representation,” it will presumably be possible to construct analogues to the invariance and linearity which were sought in the actual signal or the actual articulatory movements in taxonomic phonemics. This is the assumption embodied in the hypothesis that a level of systematic phonemics, where phonemes are characterized in terms of categorial features, constitutes a significant aspect of linguistic structure. It would be sheer obscurantism to confuse deeper and more abstract invariants, as they come to light, with the simple physical invariants assumed in taxonomic phonemics.

MORPHOLOGICAL ANALYSIS AND SYNTACTICAL RECONSTRUCTION IN ESKIMO-ALEUT

KNUT BERGSLAND

1. In Eskimo, as is well-known, there are two main types of verbal constructions, (1) intransitive: verbal form marked for one grammatical person (the subject), in number agreement with a noun in the so-called absolutive case (sg. zero, du. *-k*, pl. *-t*), e.g. Greenlandic indic. *teriangniaq pangalig-poq* "the fox ran", *teriangnia-t pangalig-pu-t* "the foxes ran"; (2) transitive: verbal form marked for two grammatical persons, the one (the object) in agreement with a noun in the absolutive case, the other (the actor) in agreement with a noun in the so-called relative case (sg. *-m*, Grl. *-p*, du. and pl. = abs.), e.g. *teriangnia-p orssoq neri-vâ* */-va-a/* "the fox ate the blubber", *teriangnia-t orssoq neri-vât* */-va-a-t/* "the foxes ate the blubber", *teriangnia-p nerdleri-t nererêrsima-va-i* "the fox had already eaten the geese".

Verbs that occur in both constructions have, in the intransitive one, (a) an active sense, e.g. *neri-voq* "he eats, ate", or (b) a reflexive sense, e.g. *toqúpoq* "he killed himself" vs. transitive *toqúpâ* "he killed him", passive *toqutauvoq* "he was killed" (passive participle + *-u-* "be") and *toqúneqarpoq* id. (verbal noun + *-qar-* "have"). From the latter, and from other transitive verbs, are derived so-called half-transitive verbs, active intransitive verbs which may have a complement in the instrumental case corresponding to the object of the underlying transitive verb, e.g. intr. part. *inung-mik toqut-si-ssoq* "who kills or has killed a person, murderer" (Kleinschmidt, 1851, 54 ff.).

From the point of view of the nominal cases, and with special regard to the verbs of the semantic type (b), one may say that the transitive construction is passive (Thalbitzer, 1911, 1058 f.). From a syntactically more comprehensive point of view it appears as active: a so-called 4., i.e. reflexive 3., person suffix refers to a subject/actor, Kleinschmidt's "projekt" (1851, 15), rather than to an absolutive term (subject/object), e.g. N.Alas. indic. *arna-t inî-mîg-nun* (sg/pl. 4. p. pl. + allative) *aillak-tu-t* "the women went home to their place", *aapa-ŋa-n* (sg.3.p.sg. + rel.) *uqallauti-ga-a irni* (sg. *irniq* + 4.p.sg. *-ni*) "the ("his", the son's) father said to his son"; W.Esk. Kuskokwim *qayâ-ne ayautâ* "he takes away (or removes) his kayak", half-tr. *qayâ-mi-nik ayautsioq* "he removes his kayak" (Hinz 1944, 43). In respect to alternative interpretations the transitive construction of Eskimo recalls the ergative construction of Georgian as discussed by Vogt (1950).

2. To a nominal subject/object in the absolutive case correspond subject/object suffixes for the 1., 2. and 4. persons (Hammerich 1936, 159 ff.), e.g. Grl. *akivu-nga /-ŋa/* “I answer”, *akivavsi-nga* “you (pl.) answer me”. In the contemporative mode, where the subject/actor usually is the same as that of the verb to which the contemporative is joined, there is normally one person suffix only, in either function, e.g. N. Alas. . . . *mīkī-plu-ni* (4.p.sg.) *inuiyuminait-lu-tīŋ* (4.p.pl.) *īluqarmīŋ isagutī-kp-a-tīŋ* (conditional 3.p.sg. + 4.p.pl.) “(they were afraid of him) he being small, being able to kill them all, if he attacked them”. The corresponding 3.p. suffixes, sg. *-gu*, du. *-gik*, pl. *-git*, WE *-ku*, *-kīk*, *-ki*, which occur also in other modes, are object suffixes only.

In the indicative and the participial, the suffixes for object 3.p. + actor 1-4.p. are the same as the suffixes found in nouns, in the absolutive case, for number + 1-4.p. possessor (Kleinschmidt, 1851, 51), cf. N. Alas. *tuttu-t ayulirī-t-ka* “I am missing the caribou (pl.)” and *qītunra-t-ka* “my children”; *arna-t qinīra-a-t* “the women looked at her” and *umialīkpai-t nīqa-a-t* “the rich people’s food”. With a 1., 2. and 4.p. possessor/actor the markers for numerus possessi/objecti can be identified as variants of the simple number suffixes, sg. zero, du. *-k* and pl. *-t*, which in intransitive forms mark 3.p. subject and so correspond to the subject/object suffixes for the other persons. With a 3.p. possessor/actor the suffixes for numerus possessi/objecti are sg. *-a /-ŋa-*, du. *-k-*, pl. *-i /-ŋi-*, followed by the simple number suffixes which here agree with the possessor/actor in the relative case (Hammerich, 1936, 203).

From the formal identity of these verbal and nominal constructions one has concluded that the verbal forms have a nominal nature (Thalbitzer, 1911, 1057 ff.) and become predicates only “durch das hinzutreten des existenzprädikats” and that there is in “*panīnguit asavara* ‘dein töchterchen [ist] mein geliebtes/meine liebe’ genau dasselbe subject-prädikatverhältnis wie in *panīnguit tikīpoq* ‘dein töchterchen kommt’” (Hammerich, 1936, 210 ff.; cf. Sauvageot, 1953). However, in the former sentence an additional “her/she alone”, for example, would have to be in the 3. person (*kisiat*), whereas in the latter it would have to be in the 4. person (*kisime*), so the interpretation is reconstructive rather than descriptive. What is a participle in one dialect may be an indicative in another, but in general an Eskimo noun, unless verbalized with a derivational suffix (e.g. *-u-* “be”), does not constitute a predicate in the way of a verb (Kleinschmidt 1851, 67 f.; Hammerich, 1936, 212).

3. The actor markers that precede the object suffixes in the dependent modes – causative (conjunctive, relative preterit) etc. and conditional (subjunctive, relative future) – can be identified as modified forms of the subject markers in the same modes. (With some exceptions the same holds good for the interrogative and the imperative-optative.) These subject/actor suffixes can be further identified with the suffixes of nouns in the relative sg. + 1-4.p. possessor (Kleinschmidt, 1851, 50). Cf. N. Alas. *paqīt-ku-ma* “when I find”, *naat-ku-p-kit* (WE *-m-ki*) “when I finish them” and *nuna-ma* “(of) my land”. Before the object suffixes, as before local case suffixes, most of the

3.p. forms, which have special final alternants of the relative suffix, look like absolutive forms (Hammerich, 1936, 203), e.g. *-a-ŋa* "he me" vs. intr., sg.3.p.sg. rel. *-a-n* (Grl. *-at*); WE, N. Alas. *-at-ŋa* (Grl. *-aŋŋa*) "they me" vs. intr., 3.p.pl. rel. *-at-a*. This can be accounted for in terms of assimilation and syncope, cf. 1.p.sg. *-p-/-m-* and *-ma* above, and, for example, the different treatment of *t+ŋ* in WE Kusk. */tikiz-ŋaitut/* "they will not arrive" (*tikit-*), N. Alas. *min-ŋaujaqtuaq* "which has alighted" (*mit-*).

Unlike nouns in the relative case, the verbal relative forms have no correlate in a superordinate term with a 3.p. possessor/actor suffix, cf. N. Alas. *aŋugaatsia-m iġlu-a-n kīlu-a-ni* (loc.) "in the rear of the elderly man's house" and *iqsinaqtaaq ili-kp-a-n irni aanruaqarnira-a* "in case that a monster turn up, she gave her son an amulet" (the final *-a* refers to *irni*, not to *ilikpan*). To reduce the verbal construction to the nominal one, one has assumed ellipsis of a term corresponding to *kīlu-a-ni* in our nominal construction (Thalbitzer, 1930, 327) or else ellipsis of a more "abstract" nature (Hammerich, 1936, 217 ff.). Actually, the constructions differ also in respect to the other term of the relation: with a nominal term like *iġlu-a-n* goes a nominal term in the relative case (the possessor *aŋugaatsia-m*), with an intransitive verb form like *ilikp-a-n* goes a nominal term in the absolutive case (the subject *iqsinaqtaaq*). Thus, the "same" suffix complex *-a-n* enters into quite different syntactical constructions.

4. In the indicative and the participial, one finds before the 1., 2. and 4.p. object suffixes the same actor markers as in the dependent modes i.e. relative forms (except that in the WE 3.p. du=pl. actor + du. and pl. object the object suffixes seem to be added to the 3.p.pl. + 3.p.pl., e.g. Kusk. indic. *-it-kut*, depend. *-at-kut* "they us", cf. indic. and depend. *-a-kut* "he us"; EE *-atigut* "he/they us", which like WE *-at-kut* seems to reflect 3.p.pl.rel. **-ata+kut*, belongs to both sets of modes).

These forms can hardly be explained by the fact that they are transitive (Hammerich, 1936, 201) – transitive verb forms have a nominal actor in the relative case, but here the verb itself has the shape of a relative form. Actually, apart from the intransitive participle, there are only faint traces of a case distinction in the Eskimo verb. In the conditional the 4.p. forms mostly have the absolutive shape (sg. *-ni*, du. *-nik* and pl. *-niŋ* with *-n-* from the singular, Siberia *-yik*, *-yij*) but the opposition to the other, relative, forms is one of person rather than one of case. In other words, "relative" is here an exclusively morphophonemic term.

5. A descriptive analysis of Eskimo that works down from the higher levels yields structural units (e.g. object 3.p. sg.) which can not be profitably isolated by any uniform method of morphophonemic cutting but can easily be described in terms of overlapping morphophonemic units (e.g. *-a* "he him/his one"). It is reasonable to assume that the overlapping is the result of a historical process, and by assigning uniform meanings and functions to the morphophonemic units one can no doubt build up some other structure behind the actual one. However, if the actual structure is assumed to be the outcome of a unique process that would constitute the only

possible “explanation” of it, how could one select the diagnostic meanings and functions? If it is assumed to be of a TYPE that can have only one type of antecedent, the descriptive analysis would seem to be sufficient. And if one admits the possibility of more than one type of antecedent, how could one determine which one it actually was? A tentative syntactical reconstruction seems to be of interest only for the comparison with one or more different languages assumed, on the basis of specific correspondences on the morphemic level, to reflect the same proto-language. In our case, then, what can the comparison with Aleut tell us about the structure of Proto-Eskimo?

6. In Aleut there are object suffixes only for the 3. person, sg. *-ka*, du. *-ki-g*, pl. *-ki-n*, Atka *-ki-s* (*-n/-s* = Esk. *-t*), in the contemporative, the imperative and in a dependent mode (cf. 2.). For the other persons there are personal pronouns, independent or proclitic as object, enclitic as subject, e.g. *tiŋ ukurtanar-tgici* “you (pl.) have seen me” (without *tgici*: “he has seen me”). These pronouns, which consist of a stem *tgi-/ti-* + possessive suffixes, probably are the cognates of the Eskimo subject/object suffixes; 2.p.sg. *tgin* accounts for both alternants of the Eskimo suffix *-t̄in/k̄in-* (Hammerich, 1936, 172) and the comparison can easily be justified phonologically also for the rest of the set. Of the Eskimo independent pronouns, some contain the respective subject/object suffixes (1.p.sg. *uva-ŋa* etc.), the other ones have another set of Aleut cognates (2.p.sg. *il-vit* etc., Aleut *i-min*, Atka *i-mis* “to, for you”, etc.).

If from these specific morphemic correspondences one may infer that the independent pronouns have become object suffixes in Eskimo, and that they have become so on the analogy of the 3.p. object suffixes, a simple explanation of the enigmatic relative forms in the Eskimo indicative presents itself: together with the new object suffixes the relative suffix could have been transferred from dependent modes. If the pronouns have become object suffixes in part by directly joining a preceding subordinate verb form, the displacement of the pronoun could also have turned the Aleut type *tgin asratikur* “he killed himself” into the above-mentioned Eskimo semantic type (b) *toqúpoq*, cf. Atka *tayarur anrarinas tunurta:saku: tgin haqatalaga: ran artakur* “the man / the people / [whom, while] talked about / himself / did not know, i.e. the man did not know that the people were talking about him”. Quite possibly, then, the Eskimo opposition intransitive vs. transitive (1.) reflects an opposition of the type found in Aleut.

7. As first realized by Waldemar Jochelson (1912, 1044 f.), the Aleut forms with actor/possessor suffixes differ from the morphologically corresponding Eskimo forms in two important syntactical respects, exemplified by Jochelson as follows: (1) *anrari-r* (abs.sg.) *qa-r* (do.) *su-ku-r* (indic. 3.p.sg.) “the person took the/a fish”, with fully explicit object (*qa-r*) and no suffixal reference to it in the verb, vs. *anrari-m* (rel. sg.) *su-ku-*: “the person took it”, with suffixal reference (-: = post-consonantic *-a*, Esk. *-a*) to the implicit object, and *qa-r anrari-m su-ku-*: “the fish, the person took it”, with

reference to the absolutive term preceding the actor. (2) *ula-m il-a-n* (3.p.sg.loc.) *uŋuci-ku-qin* (= *-ku-r + tiŋ*) "I am sitting in the house", with fully explicit local complement, vs. *il-a-n uŋuci-ku-ŋ* "I am sitting in it", with suffixal reference (*-a-* and *-Ø-ŋ*) to the implicit part of the complement. For the last point cf. Atka *ukina-r* (explicit object) *ila-ŋi:-n* (3.p.pl.ablat.) *agati + arta-ku-ni-ŋ* "I took the knife away from them (*-ŋi-*, *-ni-*)".

To a term with a 4.p. suffix, which is fully explicit in the indicated sense, there can be no suffixal reference in the final verb, cf. Atka *fuřa:ski:-n curta-ku-r hiŋa* "he has his cap on, that [person]". Just as here the demonstrative stem form *hiŋa* points to an "implicit" subject, it points to implicit terms of the two other kinds in *su-ku-ŋ hiŋa* "I took it, that [object]", *asa:- haqata-lakari-ŋ hama* "his name I do not know, that [person's]". So far one is free to interpret the type *qa-r anrari-m su-ku:-* as "passive" (although there are also derivational passive forms, with removal of the actor, e.g. *qa-r su-lga-ku-r* "the fish was taken"). However, if *qa-r*, the absolutive term to which there is suffixal reference, is the grammatical subject here, the same holds good, not only for *anrari-r* in *anrari-r qa-r su-ku-r*, but also for *itraygi-r* in the type *itraygi-r cja:- usa-ku-r* "the reindeer, its hair falls off, i.e. the reindeer sheds its hair" (not the same as **itraygi-m c.u.* "the reindeer's h.f.o."), so the predicates are CLAUSES.

8. The Aleut indicative, which seems to be the cognate of the Greenlandic transitive participle and the usual indicative in N. Alaskan, does not differ morphologically from a noun and so fulfills the morphological conditions for interpreting the verbal constructions in question as "nominal", but it does not fulfill the syntactical expectations that are implicit in this interpretation, cf. Atka *kimi-ku-r ukurtal aŋali-ŋ* "it [the airplane] descending / I saw it today", *aniqdu-m su-ku:-n uqidusadu:ka-lakar-a* "the child / what it takes / it will not return it". The suffixal reference in the final verbs (*-Ø-ŋ* and *-a*) shows that *kimi-ku-r* and *su-ku:-n* have one more "slot" than the nominal objects *qa-r* and *fuřa:ski:-n* in *anrari-r qa-r su-ku-r* and *fuřa:ski:-n curta-ku-r hiŋa* above. When the slot is filled, for example with an explicit "airplane", there is no reference to it in the final verb.

In Aleut, as in Eskimo, the indicative constitutes a clause by itself, whereas a noun normally needs "das hinzutreten des existenzprädikats", viz. the verb *a-* "be".

9. The Aleut participles (agentive *-na-*, cf. Esk. verbal noun *-niq*; passive *-qa-/-ka-*, WE *-kaq*, EE *-gaq*; indefinite *-Ø-*) share with the indicative the function of a final predicate but differ from it in other syntactical respects, cf. *ayugi:rin + arta-na-s ukurta-na-r-t[gin] i:* "did you see some going out?", *igarta-r haqa-na:- ukurta-na-r-t[gin] i:* "did you see the airplane coming?". Both are full sentences, as shown by the final predicate (*-na-r + tgin*). In the former, the participle (*-na-s* pl.) alone fills the "object slot" of the transitive verb *ukurta-* and so turns out to have no subject slot itself. In the latter, the 3.p. suffix (*-:*) adds that slot and refers to the absolutive term *igarta-r* (incidentally an indefinite participle, "flyer"). In this sense also the condi-

tional, which seems to be the cognate of the Eskimo conditional, is a participle, cf. *igarta-r ama:nu-gu-*: "when the airplane leaves". Here, as in the Eskimo intransitive dependent verb forms, the 3.p. "possessive" suffix refers to an absolutive, non-subordinate, term. In other phrases it refers to a term in the relative case, e.g. *aqali-m haqa-*: "the coming of the daylight, the coming daylight", and, with one more slot, *anrari-m su-ku-*: "the person took it". The reference is the "same", the constructions are different.

The Aleut conditional shows case distinction, e.g. 4.p.sg. abs. *sara:ran aqali-gu:n* "whenever he was about to sleep (he at the same time . . .)", rel. *tgin saraniqada-gu:m tgin quytukalizar* "when he has fallen asleep, he usually starts snoring". Here, as in the case of the Eskimo dependent modes, there is no term with 3.p. reference to the relative conditional. In this respect the conditional differs from a noun and goes with the indicative, cf. *anqarta-ku-m* (rel.sg.) *haqa:ran + arikur* "he went away but will come back in a moment". The relative case may be said to mark subordination but it too can enter into different syntactical constructions.

10. If the comparison with Aleut indicates a possible structural background for the morphological peculiarities of the Eskimo verb, it does not explain all of its constructions. The Eskimo half-transitive verbs (1.) are comparable to Aleut intransitive derivatives such as *asrat-gari-(laga-da* "thou shalt not) kill", but the instrumental complement of an Eskimo half-transitive verb has no other analogue in Aleut than the absolutive object of an ordinary transitive verb. The absolutive object is in keeping with the absolutive pronouns which in Eskimo have become object suffixes (6.) but in Aleut there is no instrumental case at all. The morphological analysis of the Aleut case forms shows that the system of local cases has been reduced, so it is reasonable to assume that the instrumental has been lost, but to determine its exact functions in the proto-language and the causes and effects of its loss in Aleut is hardly possible. Starting with this aspect of the comparative problem one could build a different model of the proto-language, and it is evident that the comparative method does not permit a syntactical reconstruction in any real, historical, sense.

The comparison of Eskimo and Aleut demonstrates the impossibility of determining the syntactical structure from the morphemes and their meanings. Without a syntactical frame of reference, however, comparisons of grammatical elements are vague and can claim no great interest. Our case shows that the syntactical frame can not be taken for granted but has to be established by comparison. In other words, syntactical reconstruction is equivalent to stating correspondence formulae on the higher levels which can make the comparisons on the morphemic level more meaningful. By comparison one can get no additional material for linguistic typology but one can gain insight into how linguistic structures are interrelated and to what extent the common elements are determined by their own setting.

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PROGRESS IN OTOMANGUEAN RECONSTRUCTION

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0. Some thirty native languages of Middle America can be considered to belong to the Otomanguean stock.¹ Although these languages were first grouped together somewhat hesitantly on the basis of inspection and conjecture, it is now becoming increasingly probable that the relationship long assumed will soon be validated. Comparative reconstruction of all six traditional branches of Otomanguean is either a present reality or currently in process. When all projects now underway are consummated, six reconstructed corpora will be available for further comparison with each other. The resultant reconstruction will give us Proto-Otomanguean. Once formulated, Proto-Otomanguean will be a piece of detailed reconstruction in Middle America which, in probable time depth and diversity of included structures (but not in geographical spread), will be not incomparable with the accomplishment of Indo-European scholarship. In this progress report, we list briefly Otomanguean languages with rough indication of their geographical spread, indicate published and unpublished work in various branches of the stock, summarize reconstructed systems, and attempt thereby to gain a preview of what Proto-Otomanguean itself will look like.

1. The central mass of Otomanguean is found in the state of Oaxaca, Mexico, and in surrounding areas of the states of Guerrero, Puebla, and Vera Cruz. To the west within this general area are found the Mixtean branch (including Mixtec, Cuicatec, and Trique), the Popolocan (Popoloc, Ixcatec, Chocho, and Mazatec), and the Amuzgoan. The latter has traditionally been considered to belong to the Mixtecan family but appears to the writer of this paper to share no significant structural innovation with Mixtecan. To the east within the central mass lie Chinantecan and Zapotecan. Each of these two is commonly spoken of as comprising one language but contains such marked dialect variation that each may be considered to contain at least six or seven separate languages.

The Otopamean branch (Otomí, Mazahua, Pame, Chichimeco-Jonaz and, Matlatzinca-Ocuilteco) lies north of the central mass of Otomanguean in the states of Hidalgo, Querétaro, Guanajuato, and San Luis Potosí. Pame in particular lies north

¹ For an outline sketch of the development of the term "Otomanguean" (and the proposed linguistic grouping thus labelled) see Fernández de Miranda and Weitlaner, 1961, pp. 4-9.

of the cultural boundary of Mesoamerica as set by Kirchhoff in 1943 (see bibliography, p. 1025). Two extinct languages, Chiapanec and Mangué, are southern outliers. The former was spoken until quite recently in the state of Chiapas in and around the town of Chiapa de Curzo. Mangué, which became extinct earlier, was spoken along the Pacific coast of Central America in Honduras, Nicaragua, Costa Rica, and El Salvador with some extensions into the interior. Mangué appears in the records under various names such as Nagrandan, Dirian, Orisian, and Nicoyan (cf. McQuown, 1955). Chiapanec and Mangué appear to have been very similar. Evidently they comprised a group which emigrated southward from the central mass of Otomanguean languages, the Chiapanecs stopping off in Chiapas while the Mangués continued to disperse further southward where they formed the southernmost extension of Mesoamerica as a cultural area.

2. Swadesh's article on Proto-Zapotecan (1947) marks the first application of the comparative method to a branch of Otomanguean. This article, however, was not intended to be a full treatment of Proto-Zapotecan. María Teresa Fernández de Miranda is revising and amplifying Swadesh's work so as to provide us with an etymological dictionary of Proto-Zapotecan with data from seven dialects in some 300 sets of cognates. In 1950, Stanley Newman and Robert Weitlaner published two articles giving us a preliminary reconstruction of Proto-Otomí and Proto-Otomí-Mazahua. Doris Bartholomew (1960) has revised the Newman-Weitlaner line-up of Proto-Otomian consonants and has sent to press a series of four articles on Proto-Otomí-Pame. Although the reconstruction of Otopamean is still incomplete, her present work plus that of her predecessors is sufficient for tentative comparison of the Otopamean branch with other branches of Otomanguean. Meanwhile, in 1957, my volume *Proto-Mixtecan* was published, bringing together Mixtec, Cuicatec, and Trique in 279 cognate sets with demonstration that Trique belongs to the Mixtecan family properly conceived. Gudschinsky's *Proto-Popotecan* (1959) involved a triple piece of reconstruction: Proto-Mazatec (on the basis of four Mazatec dialects), Proto-Popolocan (comparison of Popoloc, Ixcatec, Chocho, and Proto-Mazatec), and Proto-Popolocan-Mixtecan (comparison of Proto-Mixtecan and Proto-Popolocan). I have recently (1962) published an amplification of Gudschinsky's Proto-Popolocan-Mixtecan reconstructions, containing some seventy further cognate sets between these two branches of Otomanguean. While Gudschinsky has the distinction of being the first to bring together two branches of Otomanguean according to the comparative method, Fernández de Miranda and Weitlaner (1961) have now published a monograph which brings together Chiapanec and Mangué, and incorporates both with Proto-Popolocan-Mixtecan. The sixth traditional branch of Otomanguean, Chinantecan, is currently being reconstructed by Calvin Rensch on the basis of descriptive data from seven Chinantecan languages.

An unpublished study of mine indicates that a seventh Otomanguean branch, Amuzgoan, needs to be added to the six traditional branches. Amuzgo has traditional-

ly been considered to be a Mixtecan language. I have assembled 81 cognate sets embracing Proto-Popolocan, Proto-Mixtecan, and Amuzgo (abbreviated: PPn, PMx, and A), as well as 65 cognate sets embracing only PMx and A. (I have found a disproportionate number of PMx-A sets solely because I have used Mixtecan languages, especially Trique, as a finding device in searching for Amuzgo cognates.) While this is admittedly a small corpus, it nevertheless is sufficient to enable us to sketch the structure of a reconstructed phonological system including PMx, PPn, and A. A preliminary study of the developments from this reconstructed system down to the various descendants reveals but one possible shared structural innovation between A and PMx, viz. both have developed a prenasalized stop series. Yet even here the parallelism is only partial. Proto-Popolocan-Mixtecan (abbreviated: PPnMx) clusters of *n plus stop or spirant became the PMx prenasalized series, while in A apparently only clusters of *n and stop thus coalesced. Furthermore, examples of A ^ht in contrast with nt are plentiful while examples at other points of articulation (e.g. ^hk vs. nk) are not plentiful and usually involve free variation between unit phoneme and sequence (which are markedly different phonetically).² In effect, then, A shares with PMx the development: PPnMx *nt > unit phoneme. Moreover, if development of a prenasalized series is considered to be diagnostic of Mixtecan, then Chiapanec-Manguéan, which displays the same feature, would have to be considered Mixtecan also – a possibility which no one has as yet been rash enough to suggest.

In spite of this one possible shared innovation, there are several reasons for considering A as a separate branch: (1) In PMx there is complete merger without trace of PPnMx *t with *tʷ and of *θ with *θʷ, but the reflexes of the two series remain distinct in A where there is a series of palatalized consonants. (2) Four PMx vowels, *i, *ī, *a, and *o occur before postvocalic *m. When PPn is compared with PMx, however, it is necessary to reconstruct five vowels (the above plus *e) before *m. Taking into account A compels us to reconstruct all six vowels (adding *u) before *m. It is reasonable to suppose that the horizon on which all vowels occurred before *m is the earliest horizon of reconstruction reached yet. In that *m was lost after *e and *u in PMx it scarcely seems that A (by whose crucial witness all six vowels are found to occur before *m) can fit into Mixtecan proper. Rather, addition of A seems to broaden our knowledge of a horizon at least as early as that reached by comparing Popolocan and Mixtecan. (3) Furthermore, in Mixtecan there is evidence of postposed *m and *mʔ. Taking into account Popolocan makes possible the reconstruction of a fuller system of postposed elements in which *m or rearticulated stem vowel figured along with *ʔ and *x (i.e. with certain laryngeal phonemes). Amuzgo agrees with Popolocan in preserving witness to a much fuller system of postposed elements than need to be reconstructed for PMx. At any rate, the most adequate preview yet

² For these and other details of Amuzgo phonology I am indebted to Amy Bauernschmidt's unpublished paper, "Amuzgo Phonology in Relation to Syllable Dynamics". The Amuzgo dialect is of Xochistlahuaca, Guerrero.

available as to the structure of Proto-Otomanguean can be obtained by adding Amuzgo to Popolocan-Mixtecan reconstructions plus Chiapanec-Manguean. It seems that Amuzgo contributes considerably to our knowledge of an early layer which is either Proto-Otomanguean or some important sub-group (Popolocan-Mixtecan-Amuzgoan-Manguean?) within that stock.³

3. We here present summaries of salient features of reconstructed phonological systems in various branches of Otomanguean (exclusive of Chinantecan) in order to gain a preview of the structure of Proto-Otomanguean.

3.1. Aside from Proto-Otomí-Pame, reconstructed vowel systems are not strikingly divergent. For PMx (as now revised by my more recent work)⁴ six vowels are reconstructed: *i, *e, *ī, *a, *o, *u. For PPn Gudschinsky reconstructs: *i, *e, *a, *o, *u. For both PPnMx (Gudschinsky's work as revised in my critique, p. 232) and PPnMx plus A, six vowels of the sort reconstructed for PMx seem to be adequate. For Proto-Chiapanec-Manguean (abbreviated: PChM), Fernández de Miranda and Weitlaner reconstruct: *i, *e, *ī, *a, *u. The same two writers reconstruct eight vowels for Proto-Popomanguean (comparison of Popolocan, Mixtecan, and Chiapanec-Manguean): *i, *e, *ë, *ī, *a, *ə, *o, *u.⁵ But reconstruction of PPM *ë and *ə is based on prior work of Gudschinsky who reconstructed both these vowels for PPnMx. In turn, Gudschinsky's reconstruction of *ə was based on my own erroneous reconstruction of this vowel in PMx. In my current critique of Gudschinsky's work, I demonstrate that neither *ë nor *ə need be reconstructed in PPnMx. It seems probable that elimination of these vowels from Gudschinsky's reconstructions would also eliminate them from Proto-Popomanguean – although I have not yet studied this problem in detail. For Proto-Zapotecan (abbreviated: PZ), Fernández de Miranda reconstructs:⁶ *i, *e, *ë, *a, *o, *u. For Proto-Otomí-Pame (abbreviated: POP), Bartholomew reconstructs: *i, *e, *a, and *o.⁷ It seems possible that one vowel system, *i, *e, *ī, *a, *o, *u, can be reconstructed as lying back of PPn, PMx, A, and PChM. It may also be possible that Proto-Zapotecan harks back to a similar but slightly different system. Proto-Otomí-Pame possibly reflects a simplification of some such system by merger of certain Proto-Otomanguean vowels. The question as to what vowel system characterized Proto-Otomanguean, however, must be left open until sound correspondences can be set up among the various branches.

3.2. Reconstructed consonant systems diverge considerably. Nevertheless, taking

³ Joseph Grimes' paper, "Measures of Linguistic Divergence" (to be read before this same congress), presents data which seem to me to support my contention that Amuzgo should be considered a separate branch of Otomanguean rather than Mixtecan as such. A recent attempt to sustain the old Mixtec-Cuicatec-Amuzgo grouping (and to exclude Trique from Mixtecan proper) is found in Swadesh, 1960, and Arana, 1960. See my rebuttal to both: Longacre, 1961.

⁴ Longacre, 1962, pp. 231-2.

⁵ Fernández de Miranda and Weitlaner, 1961, pp. 33-7.

⁶ By permission of Fernández de Miranda, from her current researches.

⁷ Bartholomew, *Proto-Otomí-Pame*, III.

the following system as lying back of PPn, PMx, A, and PChM, it is possible to rationalize the differences:

PPnMx plus A plus PChM (abbreviated: PPnMx+)

*t	*t ^y	*k	*k ^w	*ʔ
*θ	*θ ^y	*x	*x ^w	
*n			*m	
	*y		*w	

Proto-Popolocan had a similar system of consonants, except that (1) PPnMx+ *θ and *θ^y split into PPn *c, *č, *s, and *š.⁸ (2) PPn *ñ developed from fusion of PPnMx+ *ny (Gudschinsky, 1959, 43-5). (3) A PPn phoneme *l probably needs to be posited; it possibly harked back to an allophone of PPnMx+ *y which split off under obscure conditions (cf. laterals in PMx, A, and POP).

		PPn		
*t	*t ^y	*k	*k ^w	*ʔ
*c	*č			
*s	*š	*h	*h ^w	
*n	*ñ		*m	
*l(?)	*y		*w	

Proto-Mixtecan merged PPnMx+ *t^y with *t, and *θ^y with *θ.⁹ Furthermore, PMx developed a prenasalized series by fusion of PPnMx+ clusters composed of *n followed by stop or spirant (the top two rows of PPnMx+ consonants minus *ʔ).¹⁰ PMx *l may have been a phoneme but more probably was an allophone of *y. (Longacre, 1961, 27).

		PMx		
*t	*k	*k ^w	*ʔ	
*θ	*x	*x ^w		
*nd	*ng	*ng ^w		
*n		*m		
*l(?)	*y	*w		

Proto-Chiapanec-Manguan had, like PMx, a prenasalized series. It had, however, both a stop *p and a prenasalized *mb not found in PMx. Significant here may be the absence of any readily identifiable reflex of PPnMx+ *k^w in some environments. In this case, PChM *p might prove a reflex of PPnM+ *k^w while PChM *mb might prove a reflex of PPnMx+ *nk^w cluster. PChM merges reflexes of *t^y with that of *t

⁸ Longacre, 1962, p. 229-231.

⁹ Gudschinsky, 44. For my suggestion that *θ^y be added to the inventory of PPnMx phonemes (with merger of *θ^y and *θ to PMx *θ), see Longacre, 1962, p. 229.

¹⁰ See Longacre, 1959, pp. 28, 54-5; Gudschinsky, 1959, p. 50.

but PPnMx+ *tʷ and *y merge into PChM *y before *u).¹¹ Reflex of PPnMx+ *θʷ is not listed by Miranda and Weitlaner in that their work was based on Gudschinsky's who did not reconstruct such a phoneme for PPnMx+ (I have, however, felt it necessary to reconstruct such a phoneme in my critique of Gudschinsky). PChM *s, *h, and *hʷ are clearly a continuation of the PPnMx+ spirant series minus the alveopalatal position. PChM *ñ and *l are reconstructed by Miranda and Weitlaner (pp. 13, 15) with some hesitation in that reflexes of the former overlap with those of *n while reflexes of the latter overlap with those of *r. At any rate, PChM *r (with *l as a possible allophone) is at once better attested to and harder to account for historically than is the PPn and PMx *l of doubtful phonemicity.

PChM				
*p	*t	*k		*ʔ
	*s	*h	*hʷ	
*mb	*nd	*ng		
*m	*n	*ñ(?)	*M [hm]	
*w	*y			
	*l/r			

By contrast, modern Amuzgo looks quite different from the above systems. But the contemporary descendants of Proto-Popolocan or Proto-Mixtecan also appear quite diverse from reconstructed PPnMx+. In treating any contemporary phonological system we are several stages removed from PPnMx+ while in giving the phonology of a reconstructed branch we are perhaps but one stage removed from PPnMx+. Nevertheless, Amuzgo consonant structure with all its modern complications may be shown to hark back to the consonant system of PPnMx+. We give first the Amuzgo consonants then follow with the discussion:

Amuzgo				
p	t	tʷ	k, kʷ, kʰ	ʔ
	c	č		
b	s	š	h	
^m p	ⁿ t	ⁿ tʷ	ⁿ k	
m	n	ñ		
w	l	y		
	r, ʀ			

The bilabial series contains not only m and w but further members, p, b, and ^mp, which have no corresponding PPnMx+ reconstructed phonemes. These latter three phonemes are, however, quite infrequent. Setting aside occurrence of these three phonemes in onomatopoeic words and in Spanish loans, we find that p, b, and w (with ^mp considered to be a special case of fusion with preposed nasal) tend to occur

¹¹ Fernández de Miranda and Weitlaner, 1961, p. 28.

in mutually exclusive distribution before various Amuzgo vowels. It is therefore plausible to assume that the entire Amuzgo bilabial series exclusive of *m* harks back to the same reconstructed phoneme (probably $PPnMx + *w$). Amuzgo *c*, *č* *s*, and *š*, are developments of $PPnMx + *θ$ and $*θ^v$. The prenasalized series (which might better be considered to be occluded nasals than prenasalized stops) are reflexes of $PPnMx +$ nasal plus stop. The regular reflex of $PPnMx + *y$ is Amuzgo *l*, but reflex *y* occurs in postposed syllables. The vibrants *r* and *ř* are also very rare phonemes found almost exclusively in onomatopoeic words and in Spanish loans.

Compared with the above systems, the consonant systems of Proto-Zapotecan and Proto-Otomí-Pame appear on first inspection to be quite different. But it is chiefly the presence of a fortis-lenis or geminate-single contrast that makes these systems appear so different.

In giving the consonant system of Proto-Zapotecan (taken from Fernández de Miranda's study which is still in process), all consonants not in italics have a fortis (or according to Swadesh [1947], a geminated) counterpart:

PZ					
*p	*t	*tʷ	*k	*kʷ	*ʔ
	*c				
	*s	*š			
*m	*n				
	*l				
	*r				*R
*w	*y				

The presence of a bilabial **p* as well as a **kʷ* in PZ (and possibly also in POP), compels us to face squarely the problem of whether Proto-Otomanguean contained both a bilabial stop and a labiovelar stop. PZ **c*, **s*, and **š* are probably developments of the Proto-Otomanguean phonemes similar to $PPnMx + *θ$ and $*θ^v$. Sources for PZ **l* and **r* must await further study. In the Mixtecan and Popolocan languages *r* phonemes are clearly developments from such earlier phonemes as **t* (cf. Trique; Longacre, 1959, 68-70), **l* (cf. early borrowings in Mixtec and Cuicatec where Spanish loans with *r* substitute *l* for *r*; Longacre, 1959, 20), and **tʷ*, **θ*, or $*θ^v$ (cf. Popolocan languages; Gudschinsky, 25-31) – plus introduction of *r* from Spanish loans. It is entirely possible that some such source could explain PZ **r* as well. **R* occurred only in postposed elements and possibly had a velar (or uvular) quality. In that PZ has no labiovelar spirant (as do PPn, PMx, and PChM), and in that the velar spirant testified to in other branches seems to have been prominent in postposed position, it seems possible that PZ **R* may be a reflex of Proto-Otomanguean velar spirant.

The phoneme **m* was so rare in PZ that Swadesh did not reconstruct it at all. This is unusual in that **m* is a very common phoneme in every other reconstructed branch of Otomanguean – including Chinantecan which is not taken account of here. Could it be that the drastic restriction in the distribution of **m* in PZ could give us a clue as to

the development of PZ fortis consonants? Evidence is not lacking that these two problems are linked together. I have suggested elsewhere that Proto-Otomanguean *m was possibly restricted to preposed and postposed elements.¹² Postposed *m leaves no direct trace in PZ where not even nasalized vowels (the most likely reflex of postposed *m) are found – unless some of the vowel alternations in PZ are due to postposed elements such as *m which have been lost without further trace. Preposed *m, however, could conceivably have coalesced with a following consonant to form a geminate which in time came to pattern as a fortis consonant. I suggest, therefore, that PZ fortis consonants hark back to Proto-Otomanguean *mC clusters, while PZ lenis consonants hark back to consonants without the preposed *m.

Proto-Otomí-Pame as reconstructed by Bartholomew is not equivalent to Proto-Otopamean, the reconstruction of which is projected by Bartholomew. Otomí-Pame has yet to be incorporated with the Weitlaner-Newman reconstruction of Otomí-Mazahua as well as with Matlazinca and Chichimeco-Jonaz. Nevertheless, in that Otomí and Pame are presumably two of the most diverse members of Otopamean, the common reconstruction of the two may be assumed to tell us quite a bit about the structure of Otopamean. The consonant system of Proto-Otomí-Pame follows; each consonant not italicized may occur geminated:

POP					
*p	*t	*c	*k	*k ^w (?)	*ʔ
*m	*n	*s	*h	*h ^w (?)	
*w(?)	*l				

Bartholomew has hesitated to reconstruct labiovelars and *w in that evidence is not as solid here as for the reconstruction of the other POP consonants. She has assured me, however, that evidence for them is by no means entirely lacking and that it may well be that they should be reconstructed in an early stage of POP. She has also told me that reconstruction of *w is scarcely more tenuous than that of *l and that there was some sort of functional equivalence between them in that both patterned as semivowels. This fact, coupled with the consideration that no such phoneme as *y reconstructs in POP, makes it probable that POP *l is a reflex of a Proto-Otomanguean phoneme similar to PPnMx+ *y. Notice that if the reconstruction of *k^w and *h^w be accepted, then we have as with PZ a contrast between *p and *k^w. The origin of POP geminates is obscure at present. Should they prove to have an origin similar to that of PZ fortis phonemes, this would give an isogloss of shared innovation of considerable importance between Zapotecan and Otopamean branches.

3.3. Undoubtedly there was a system of postposed elements occurring post-accentual in Proto-Otomanguean. For Proto-Popolocan-Mixtecan plus Amuzgoan, I reconstruct tentatively the following system of postposed elements including the following

¹² Longacre, 1962, p. 235.

components: syllabic *m, rearticulated vowel of same quality as stem vowel (*V), and the laryngeals *x and *ʔ:

*-m	
*-xm	*-xV
*-xmʔ	*-xVʔ
*-ʔm	*-ʔ/ʔV

The above postposed elements are established by the PPn and A reflexes. PMx reduced postposed elements to *-m, *-ʔ and *-mʔ. In PChM the following postposed elements are found: *-mV, *-hV, *-hVʔ. Suffixes containing *m were evidently active morphemes in both Chiapanec and Mangué, e.g. (FM and W set 168) *-mihi “suffixed form which appears in the numerals 2 to 9”; (FM and W set 169) *-me “suffix which appears with frequency in adjectives”; (FM and W set 170) *-me, *-mu “verbal suffix which appears rather frequently”; and (FM and W set 183) *-mu “suffixed form which appears in the plural of personal pronouns”. These various elements appear clearly to be descendants of an earlier postposed system similar to that found in Proto-Popolocan-Mixtecan. PZ preserves little direct evidence of postposed elements besides the *-RV postposed element which may possibly be descended from some postposed element involving a velar spirant. Nevertheless, PZ contains instances of a rather puzzling vowel alternation in stems; all reconstructed vowels vary in at least one set to *a, and all reconstructed vowels vary in at least one set to *o. We may here have evidence of coloring of vowel quality by two contrasting sorts of postposed elements that have otherwise disappeared without trace (cf. effect on surrounding vowels of Indo-European laryngeals). In POP, Bartholomew finds evidence of the following postposed elements: *-m, *-hm, *ʔm, *-n, *hn, *-ʔn. There is also a feature of syncope in POP stems that involved the following sorts of alternations: *VhV ~ *VV, and *VʔV ~ *VV. The latter feature probably witnesses to postposed Proto-Otomanguean elements such as *-xV, and *-ʔV reconstructed for PPn. The postposed elements containing nasals resemble similar elements in PPnMx+ but involve not only *m but *n as well. Whether PPnMx+ has eliminated postposed elements containing *n or whether POP has developed such elements is a question the answer to which must await a full-scale reconstruction of Proto-Otomanguean.

3.4. Very probably Proto-Otomanguean contained some system of phonemic pitch contrasts. For Proto-Popolocan-Mixtecan a system of four phonemic pitch levels (with the highest tone restricted to sandhi variants) seemed indicated (Gudschinsky, 38-40). The Amuzgo dialect studied by Bauernschmidt has a three-register system. The tones of Chiapanec and Mangué (assuming that they were once present) are lost forever in that no sources recording these languages before their extinction indicate tone (or glottal stop). Zapotecan languages are characterized by systems of from two to four levels. Chatino, commonly assumed to be Zapotecan (but this problem should be examined in its own right), has four phonemic levels in the dialect thus far analyzed.¹³

¹³ McKaughan, 1954. Unpublished paper of Leslie Pride: “The Tonal Structure of Chatino”.

Chinantecan languages are characterized by systems of from three to five levels. The Otopamean branch displays systems of a somewhat different type. In Otomí, high and low tones are in contrast plus a rising glide (with a falling glide phonemic as well in Eastern Otomí). In Mazahua, high, low, rising, and falling tones are found. Donald Stewart reconstructs these four – high, low, rising, and falling – for Proto-Otomí-Mazahua.¹⁴ Pame has the same tonal contrasts – although the rising glide has only recently been found to be phonemic.

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¹⁴ Paper of Donald Stewart's read at the Mesa Redonda for Oaxacan Languages, Mexico City, May, 1957.

ALGONQUIAN LANGUAGES AND GENETIC RELATIONSHIP

KARL V. TEETER

1. This paper is concerned with the Algonquian family of languages, and with a few examples drawn therefrom which may help to clarify certain aspects of the concept of genetic relationship among languages. The Algonquian languages constitute the most widely spread group of languages of indigenous North America, with representatives of the family occupying the most easterly and most westerly points of the United States and a good deal of territory in between, as well as a sizable portion of what is now Canada.

2. The main body of the Algonquian languages stretches from the Rocky Mountains to the Atlantic Ocean. In the western plains the principal representatives of the family are Blackfoot, Arapaho, and Cheyenne, extending in roughly that order from Alberta to Colorado, east of the Continental Divide. Central and eastern Canada is the location of the widely scattered Cree dialects, including Montagnais and Naskapi to the northeast, and the dialects of Ojibwa (Chippewa) occupy southern Canada. In the Midwest, extending southward through present-day Wisconsin, Michigan, and Illinois, are the Menomini, the Fox and related dialects, the Potawatomi, and the mutually intelligible Miami, Peoria, and Illinois. From north to south along the Atlantic Coast, from New Brunswick to North Carolina, are found the Micmac, Passamaquoddy, Penobscot, Massachusetts, Natick-Narragansett, Mohegan-Pequot, Delaware, and Powhatan, to mention the more important Algonquian languages of the East. The southernmost representative of the main group is Shawnee, found in Tennessee and the Carolinas. Except for several of the languages indigenous to the Atlantic coastal areas, most of the Algonquian languages are still spoken, each by a small and typically diminishing number of people.

3. To locate the remaining Algonquian languages, Wiyot and Yurok, it is necessary to move to the Pacific Ocean, to the area of Cape Mendocino, Humboldt Bay, and the Klamath River, in northwestern California, an area more than seven hundred miles distant from the eastern slope of the Rockies and the geographically nearest close relative of Wiyot and Yurok, the Blackfoot. The two Pacific Algonquian languages occupy about a hundred miles of California coastline, extending a short distance inland along the rivers. Yurok has a few speakers left, and Wiyot in 1961 was recalled by one old woman in her eighties, who has recently died. The relationship of Wiyot and Yurok to Algonquian was not recognized until it was demonstrated

by Sapir in 1913.¹ Even after his work, skeptics continued to express doubts, which were not finally laid to rest until the work of Haas a few years ago.²

4. The Algonquian languages have played an important role in the development of American linguistics in the twentieth century. Each of the two greatest figures in the field, Edward Sapir and Leonard Bloomfield, especially the latter, worked extensively on Algonquian. Both used the results of their work in part to convince doubting colleagues that languages without a written tradition are subject to the same sorts of grammatical constraints as any other languages, and that they are equally accessible to the methods of comparative and historical linguistics.³

5. Sapir's work on Algonquian languages was largely limited to comparative linguistic papers, and Bloomfield studied the languages intensively from both the descriptive and comparative point of view. The height of the latter's published descriptive work is the paper Menomini Morphophonemics,⁴ in which he gives a brilliant and thoroughly documented treatment of descriptive order; this concept will figure in the discussion below. His work in comparative Algonquian is best exemplified by the sketch⁵ published in 1946, in which he reconstructs the prototype language of four groups of Algonquian dialects, Fox, Cree, Menomini, and Ojibwa. This reconstructed language is referred to as Proto-Central Algonquian (PCA).⁶ Many equations between the PCA languages and those of the Plains are suggested by Michelson,⁷ and a few between PCA and various eastern languages by Geary, Siebert, and Voegelin.⁸ Hockett has added Potawatomi to the PCA group.⁹ Aside from this, little but highly preliminary comparison and reconstruction has been published.

6. Today the work of data-gathering has proceeded apace, and although much more and better description is needed, the available material comprises a rich mine for comparative linguistic research. I am one of those who have been taking part in this task, and what I should like to do here is to draw on this work in a preliminary

¹ Edward Sapir, "Wiyot and Yurok, Algonkin Languages of California", *American Anthropologist*, 15 (1913), 617-646.

² Mary R. Haas, "Algonkian-Ritwan: The End of a Controversy", *International Journal of American Linguistics* 24 (1958), 159-173.

³ Sapir, "The Concept of Phonetic Law as Tested in Primitive Languages by Leonard Bloomfield", in Stuart A. Rice, ed., *Methods in Social Science* (Chicago, 1931), pp. 297-306, and reprinted in David G. Mandelbaum, ed., *Selected Writings of Edward Sapir* (Berkeley-Los Angeles, 1951), pp. 73-82; Bloomfield, "A Note on Sound-Change", *Language*, 4 (1928), 99-100.

⁴ *Travaux du Cercle Linguistique de Prague*, 8 (1939), 105-115.

⁵ "Algonquian", in Harry Hoijer and others, *Linguistic Structures of Native America* (= *Viking Fund Publications in Anthropology*, 6) (1946), 85-129.

⁶ Charles F. Hockett, "Implications of Bloomfield's Algonquian Studies", *Language*, 24 (1948), 117-131.

⁷ Truman Michelson, "Phonetic Shifts in Algonquian Languages", *IJAL*, 8 (1935), 131-171.

⁸ James A. Geary, "Proto-Algonquian *çk: Further Examples", *Language*, 17 (1941), 304-310; Frank T. Siebert, Jr., "Certain Proto-Algonquian Consonant Clusters", *Language*, 17 (1941), 298-303; Charles F. Voegelin, "Proto-Algonquian Consonant Clusters in Delaware", *Language* 17 (1941) 143-147.

⁹ Charles F. Hockett, "The Position of Potawatomi in Central Algonkian", *Papers of the Michigan Academy of Science, Arts and Letters*, 28 (1942), 537-542.

attempt to make sense of the concept of depth of genetic relationship. For the purposes of this discussion, I shall select two rules, from different parts of Algonquian grammar, which have correspondences in Bloomfield's PCA and in Wiyot.¹⁰

7. Linguists have long been intuitively aware that certain sorts of resemblances among languages are worth far more than others for making inferences as to genetic relationship. Sapir speaks of these as "fundamental features of structure hidden away in the very core of the linguistic complex".¹¹ Little attempt has been made to interpret the sense in which such features may be said to be "fundamental" or "deep", and as long as a reasonably explicit conception as to the form of grammars was lacking there seemed small hope of success for such an enterprise. Now, however, that linguists are again beginning to recognize the importance of descriptive order¹² in grammars, and that the construction of a comprehensive theory of language is underway,¹³ with already some suggestions as to its possible use in connection with the phenomena of linguistic change,¹⁴ I think that this question may be reopened with some hope of arriving at a meaningful answer.

8. If we think of a grammar as a series of sets of ordered rules, each set corresponding to a given level of description – thus there are syntactic rules, morphophonemic rules, etc. – then of necessity some rules in each set are relatively early and others are relatively late. I propose as a hypothesis for testing that we can think of "depth" as relative earliness in some subset of rules of the grammar of a language. Although it is by no means yet clear in detail precisely how the various subdivisions of grammatical rules are to be made, I believe that one can find numerous tentative confirmations of this general hypothesis, of which the two I shall present below are a sample. Note, by the way, that if relative ordering also applies among the sets of rules, as seems likely enough, the two "latest" and therefore least deep types of rule are the lexical rules, which occur at the end of the syntactic component of the grammar, and the phonetic rules, the last rules of the morphophonemic component. On this conception, then, it becomes clear that the rules of a language most subject to external influence, the least contextualized, are going to be those specifying particular lexical items and particular phones. This, of course, is exactly what happens in languages, as has repeatedly been confirmed empirically.

¹⁰ Karl V. Teeter, *The Wiyot Language: Grammar, Texts, and Lexicon*, to be published in the *University of California Publications in Linguistics*.

¹¹ Sapir, *Language* (New York, 1921), p. 219.

¹² Leonard Bloomfield, *Language* (New York, 1933), p. 213; id., "Menomini Morphophonemics" (fn. 4); Murray B. Emeneau, "A Sketch of Kota Grammar", in *Kota Texts (Part One)* (= *University of California Publications in Linguistics*, 2) (1944), 15-35; Noam Chomsky and Morris Halle, *The Sound Pattern of English*, forthcoming.

¹³ Noam Chomsky, *Syntactic Structures* ('s-Gravenhage, 1957).

¹⁴ G. H. Matthews, "Syntactic Change in Crow and Hidatsa", *Massachusetts Institute of Technology, Research Laboratory of Electronics, Quarterly Progress Report*, 58 (July 15, 1960), 281-284; id., "Morphophonemic Change in Crow and Hidatsa", *same publication*, 59 (October 15, 1960), 165-168; Morris Halle, "Phonology in a Generative Grammar", *Word*, 18 (1962), 54-72.

9. I want to show first an early morphophonemic rule, and then a complex early syntactic rule, of Algonquian. In each of these cases one finds a resemblance between PCA and Wiyot. In PCA there is a set of possessive prefixes which have the morphophonemic peculiarity of requiring the addition of /t/ before a stem beginning in a vowel:¹⁵ possessive + /vowel/ > possessive + /t/ + /vowel/. This is clearly an early rule in the morphophonemic section, since it applies before the forms of the individual prefixes have been specified; that is, it applies to the whole class of possessive prefixes. Now Wiyot, the westernmost and, with Yurok, geographically isolated member of the Algonquian family, also has a class of possessive prefixes. The language does not have stems beginning with vowels; instead there are a number of stems with initial /h/ plus a vowel which resemble PCA stems in initial vowels. And Wiyot has a morphophonemic rule: possessive + /h + vowel/ > possessive + /t/ + /vowel/. With the first person possessive prefix /du-/ and the stem /hikw-/ "louse" in Wiyot, for example, one obtains the form /dutíkw/ "my louse". In PCA the morphemes with the same functions are respectively /*ne-/ and /*ehkw-/, and the PCA form glossed "my louse" is /*netehkw-/.

10. The possessive prefixes of PCA are used to express person in respect to both nouns and verbs. Either sort of stem can tolerate only a single prefix, and a sequence of choice rule specifies which is used when more than one person is involved. This rule states that if any person involved is a second person, the second person prefix is employed; if there is no second person, but there is a first person, the first person prefix is used; otherwise, the prefix indicates the third person.¹⁶ Now Wiyot appears to have no provision for expressing more than a single person in relation to nouns, and does not use prefixes to express person with verbs, except in one isolated circumstance. A negative preverb, used with a verbal theme to indicate negation of what the theme says, functions not only as a negative, but has three different forms to express three persons. When more than one person is to be related to the verb, as in the case of transitive verbs, only one negative preverb may be used. And which it is is governed by a rule identical in all respects to the one of PCA just described. Thus, the first, second, and third person forms of the preverb are respectively /kado kho ko/, the language has verbal themes such as /walásah/ "I see you", /waláh/ "I see him", and /walíř/ "he sees him", and the complete negative mood phrases corresponding to these themes are /kho walásah/ "I do not see you", /kado waláh/ "I do not see him", and /ko walíř/ "he does not see him". Both in PCA and in Wiyot, then, the rule in effect states that there is an order of preference for the prefixal expression of person, and that this order is second-first-third.

11. Now the two rules I have just presented are, I wish to suggest, examples of deep rules, rules which in the course of linguistic change could still remain even when the overt forms of the elements to which they apply had changed completely in different

¹⁵ Bloomfield, "Algonquian" (cf. fn. 5), sections 28 and 30.

¹⁶ Stated e.g. in Bloomfield, "Algonquian", sections 28 and 37, and in his "Notes on the Fox Language", *IJAL*, 3 (1925), 219-232, and 4 (1927), 181-219, section 61.

directions, or even been replaced by borrowing. They are examples of Meillet's "procédés particuliers d'expression de la morphologie"¹⁷ as much as are his own favorite examples of paradigmatically correlated but otherwise formally different items, although it would appear today that his conception of "morphology" was too narrow.

12. In quantity these two rules are a small part of the evidence for the genetic relationship of PCA and Wiyot. But in quality they are not. A great deal of recent American work has been wasted on the manipulation of similarities in isolated lexical items, which are necessarily superficial, the product of late rules. The lexicon is an outward face of language, a face turned toward culture, and is typically the area in which borrowing takes heavy toll, obscuring the difference between similarities which are the product of diffusion and those due to genetic relationship. Much discussion in America in the last few years has centered on such questions as how many lexical similarities we need to conclude genetic relationship. Surely this search for a magic number is misguided. Two rules of the depth of those I have exemplified are enough, just as Meillet would require no more than French *il est, ils sont, je fus* as against Latin *est, sunt, fuī*, to say that French and Latin are genetically related.¹⁸

13. Much more work is required, of course, to make truly precise the concept of linguistic depth and its applications in comparative grammar. Too much effort, as I have said, has been expended on superficial comparison. It is just as true that too much description is shallow, writers often merely listing the facts they have accidentally recorded. Many descriptive grammars will have to be reappraised to correct this where possible, and much more description will have to be done before the task of comparative linguistics can be undertaken on the scale we hope it someday will be. But in the meantime there is much that can be accomplished. The Algonquian languages and other language groups without a written tradition represent a gigantic and still largely untapped source for the testing of assumptions and for the development and enrichment of the techniques of the Indo-European comparative method.

14. This paper has been concerned with two principal points, one a matter of theory, the other of opportunity. In the first place, I have made some preliminary suggestions as to how we may fruitfully construe one aspect of the concept of genetic relationship. This is the notion of genetic depth, which I have examined in terms of our growing understanding of the nature of language. In the second place, I have commended the increased attention of linguists to the data that is accumulating on the less well-studied languages of the world. In concluding I should like to enlarge briefly on these two main points.

15. We should by now have escaped from the unfortunate post-Saussurian tendency to regard the synchronic system of a language as static, an error repeatedly and incisively pointed out by Professor Jakobson. Yet the idea of each level of grammar as an inventory of elements all of the same status, a set of items in various arrangements,

¹⁷ A. Meillet, *La méthode comparative en linguistique historique* (Oslo, 1925), p. 25.

¹⁸ *La méthode comparative*, p. 27.

persists. In these terms there is no sense in talking about the diachronic implications of synchronic structure noted by Bloomfield.¹⁹ Nor is there any sense, if a synchronic system is static, in speaking of certain resemblances between languages as deep, and as therefore necessarily due to the universal nature of language or to genetic relationship, development from a common ancestor, and of other resemblances as shallow, and consequently possibly due not only to generic or genetic relationship, but also to chance or diffusion.

16. Yet we can only build a comparative grammar on resemblances which are sufficiently deep to exclude chance or diffusion as a hypothesis, and sufficiently contextualized to exclude origin in universals. It follows that if we cannot make sense of genetic depth, if we cannot draw diachronic implications from synchronic structure, we cannot do comparative grammar. What is available to us is only synchronic data – as Meillet has pointed out.²⁰ It is only the results of changes, not the changes themselves, which appear in our data. We must therefore reject the conception of a synchronic system as static, and the related model of a grammar as a collection of collections of elements is likewise to be discarded. If our model for a language system is not dynamic, it cannot explain the possibility of the recovery of past linguistic history.

17. This dynamic model, as far as the question of depth is concerned, is readily available to us in the requirement of ordering among grammatical rules, implicit already in the work of the Hindu grammarians, and today increasingly recognized, in America due first to Edward Sapir, and recently largely to the work of Noam Chomsky and Morris Halle. Exceptions and anomalies, as Professor Kuryłowicz reminds us in his presentation on internal reconstruction, are the probative evidence for establishment of genetic relationship, along with paradigmatically correlated sets of resemblances. But what are exceptions but rules which appear relatively early in some subset of rules of the grammar? Thus the irregular plural /-ən/ in English must be specified, for the morphemes to which it applies, before the regular plural, or our description will predict such non-occurrent forms as *oxes. Here is an early rule, and the resemblance in generation between English *oxen* and German *Ochsen* is a deep one. It is therefore no longer a mere resemblance, but a correspondence. This is the burden of my first point: if we wish to clarify the theory of comparative and historical grammar, we must refer to the theory of grammar. If we wish to understand genetic relationship, we must look at language systems.

18. The second point made in my paper I have called one of opportunity. It still remains true that in no other language family do we approach the admirable depth of comparison and historical understanding which there is in the Indo-European field. It is also true, I think, that in our current theoretical work on the history of language we are hampered by unclarity as to what applies to languages in general and what is

¹⁹ In the paper "Menomini Morphophonemics", cf. footnote 4, above.

²⁰ Meillet, *La méthode comparative*, p. 11.

peculiar to Indo-European. For a long time there was nothing much that could be done about this state of affairs, since the descriptive understanding of languages has been so far ahead for Indo-European languages of its state for any other group. Today this is not true to the extent that it once was. The linguistics of the last thirty or forty years has been brilliantly successful in the accumulation of relevant descriptive data from languages all over the world. The opportunity for linguistic comparison, to the end of writing the history of languages, is greater now than it has ever been.

19. The more examples which can be brought to bear on a problem the easier its solution. What is needed now is a massive and clear-headed attack on the data we have, to the end that our hypotheses and theories may receive the widest possible testing and validation. The Algonquian languages, from which I have drawn the examples used in my paper, are only one among the many groups which offer previously unparalleled opportunity for the comparatist. The second point, therefore, which has been the aim of my paper to stress, is that here, in the less well-studied languages, we are due for a renaissance of comparative linguistic work. Let us improve our data, but let us above all use what we have.

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DISCUSSION

BEVER:

It appears on the basis of Teeter's material that an even stronger equation can be made. If within Wiyot grammar one introduces /h/ before vowels after the rule requiring addition of /t/, the latter rule will become identical to the rule given for PCA, not merely similar.

LONGACRE:

Is Teeter arguing that depth excludes chance? If so, how can we tell when we have reached the level where a line may be drawn?

TEETER:

I am arguing precisely that. Deep rules reflect older processes, and it is just this which provides the basis for the comparative method and historical reconstruction for the deep rules are at the same time those found in contexts detailed enough to exclude chance and borrowing. As for the drawing of a line, I was especially careful to be vague in talking about subsets of rules within which relative depth matters, for the simple reason that our model of language is not able to say more. We can see enough now to be quite sure there are such subsystems, and to identify the more general of them, such as syntax and morphophonemics. How definite a line we can draw depends on how definite a model of language systems we have available, and all such models have a long way to go. We can, I think, explain the basis for a few of the

things the comparatist does, and I have endeavored to provide a sketch for one such explanation, but the practice of historical linguistics remains, and will remain, an art.

DYEN:

First, when we speak about "depth", as Teeter has, are we actually saying anything more than that, at some level, chance is excluded? Second, I think that we must be very wary of the search for a panacea, of a universal solution to linguistic problems.

TEETER:

With regard to the second comment, I could not agree more. I have said the same thing in my response to Professor Longacre. As for the first question, certainly we are saying more. We are saying that something which is in the nature of language, i.e. the ordering of rules, reflects depth, and that depth excludes chance.

HAMMERICH:

In reference to the last part of section 13 of Teeter's paper, his advocacy of comparative work on exotic languages, might it not perhaps be that even more could be accomplished toward a theory of linguistic change by continued work on Indo-European?

TEETER:

Nothing I have said should be construed as saying that Indo-Europeanists should stop work. Clearly, we need study of all of the languages and language families of the world, the more thorough the better.

DYEN:

Teeter claims in section 12 of his paper that we cannot use lexical elements as basic evidence in comparison. What would he use in Chinese, where morphophonemic irregularities, which have played such an important role in Indo-European studies, are virtually absent?

TEETER:

Lexical elements, I believe, are important not for their surface similarities but only when considered as endpoints of a system of rules, and it is the rules we compare. Professor Dyen no doubt has in mind Meillet's frequent remarks on the difficulty of applying the comparative method to various Far Eastern languages, and it is in this sense that I characterized Meillet's conception of morphology as too narrow (section 11, above). If we were indeed forced to rely on isolated lexical items in Chinese historical linguistics, the situation would be desperate. But we are not. Chinese is as rule-governed a language as any, but it is simply that the complications are rather in the syntax than the morphophonemics. This does not mean that there are not ordered rules in Chinese syntax, or that we cannot find paradigmatically correlated

sets of resemblances. Notice that there are paradigms in syntax, too, consisting of basic sentences and their transformations.

DYEN:

I doubt that in this way, if we had only syntax to work with, we could ever go much outside of a family such as Chinese, to show its wider relationships.

PRELIMINARIES TO THE RECONSTRUCTION OF INDO-EUROPEAN SENTENCE STRUCTURE

CALVERT WATKINS

Syntax is notoriously ignored in most studies of comparative and historical linguistics. When one turns from phonology and morphology to syntax, what is conspicuously lacking in any of the manuals are syntactic equations, of the sort on which one can build reconstructions. Yet there is only an apparent, not a real contrast between e.g. $*p > f$, and a comparable putative syntactic historical filiation. It would be absurd to think that the physical body of a phrase or similar utterance can be historically transmitted. But the underlying structure of a phrase, as in our case the rules of arrangement of the elements, can be presumed to constitute a linguistic system functioning in time, and as such to be susceptible to analysis by the comparative method as well as by other techniques of historical linguistics. The "tractability" of the syntactic system for historical investigation is only in degree different from that of the phonological or the morphological.

In this paper we seek to establish certain equations in phrase structure among certain of the older IE languages: Vedic, Hittite, Latin, Greek, and Old Irish. The elements of the sentence or phrase with which we will be concerned are four: the sentence connective (N), the enclitic pronominal element (E), the preverb (P), and the finite verb form (V). Some or all of these four elements may be combined to form what we may term the "verb phrase" of IE, and of the later historically attested languages, in independent of "principal" clauses. Our concern here may be described by the title of a significant article by Dell H. Hymes, "Positional analysis of categories: a frame for reconstruction" (*Word* 11.10-23 [1955]). Put more loosely, it is the problem of "word order", though it is clear in IE, and even clearer in Athapaskan, that the elements in question are not all "words". It is not of particular import whether we speak of this as a problem of syntax, or as a problem of morphology, as does Hymes; I prefer to call it syntax, since it is traditionally put there in IE comparative grammar.

It will be evident, I hope, that the formulae given below for the relative positions of different elements in the sentence in the various languages is not meant to constitute a set of hard and fast rules for these languages, to which there are next to no exceptions. Such an assumption in the field of syntax would be of course hopelessly unrealistic. But it is nonetheless an observable fact that, as J. Gonda has put it, "The grammatical procedures by which words and word groups were arranged and united into meaningful larger units were often highly conventional" (*Four Studies. . . Veda*, 8). We will be

concerned with such conventional patterns of word order in the languages under consideration.

These recurrent patterns may and do vary from language to language. Yet such patterns may also exhibit, among a set of languages, striking recurrent features of similarity. Where we know from other considerations that these languages are cognate, though this condition is by no means a necessary one, it is a legitimate hypothesis that such recurrent similarities of syntactic patterning may result from genetic filiation. This is of course the basic assumption behind any attempt at historical and comparative syntax, and has been with us since the early days of IE studies. Yet the atomistic approach of the neo-grammarians left its imprint on the study of word order as well. For Delbruck, the position of V was a separate question from that of the position of P, and of E. To state that the normal position of the verb in IE was sentence-final, as he does, hardly serves to characterize the IE phrase; sentence-final position for the finite verb is equally normal in, for example, Burushaski.

To the earlier atomistic approach to linguistic history we have for some time opposed the notion of language as a system of interrelated units, on whatever level; and this view must be applied to word order just as to phonology. In our concern here for the four elements of the verb phrase mentioned above, what is of significance is not the position of each element, but the position of each relative to the others, and the ensuing sentence patterns which we can formulate. It is on this basis that one can, I think, make a meaningful statement about IE phrase structure. So, for a simple case, we may contrast the IE type $\#E...V\#$, with verb at the end and pronominal object in second position, to the Burushaski type $\#...EV\#$, considering the Burushaski prefixal pronouns (obj. of trans. verbs, subj. of intrans. verbs), as E. The picture is entirely different from that to be gained from examining the position of V alone.

To take another example: one of the few generally accepted syntactic statements about IE is Wackernagel's Law, that enclitics originally occupied the second position in the sentence. Wackernagel based his formulation on the evidence of Greek, Indo-Iranian, and Latin, and recognized its application to Celtic (*v. infra*); later investigations of Balto-Slavic, and in particular the evidence of the subsequently discovered Anatolian languages, have abundantly confirmed his view. Kuryłowicz, however, has suggested (*Oslo Proceedings*, 613) against V. V. Ivanov (*l.c.*), that Wackernagel's Law belongs on the plane of a general linguistic feature rather than to the comparative grammar of the IE languages alone. I would disagree with this statement in any case; but what is decisive is not the fact that an enclitic (particle or pronoun) in the earlier IE languages usually occupies second position. This might conceivably result fortuitously from general conditions of phrase contour. But when we observe the same fixed order among two or more such enclitics in all the older languages; when unaccented connectives such as **k^we*, **de* (Gk. *δέ*, OIr. *-d-*), **yo* (Hitt. *-ya*, OIr. *-e*, *-a-*) always precede and never follow unaccented pronouns in Indic, Greek, Latin, Hittite with the other Anatolian languages, and Old Irish; then we are forced to assume that there is in fact a direct historical relation. The factor of chance can be ruled out.

For the situation in Vedic, we may reproduce the remarks of Gonda (*o.c.*, 7); "In the majority of the works examined, the final position [of the verb] is most usual. Although the initial position has never been predominant, the verb may, speaking generally, occur at the beginning in a well-definable number of cases." It is the great merit of Gonda's work to show that where the verb preceded by the object of subject is not in final position, such sentences are usually "amplified", i.e., "they are from their beginning until the verb complete in themselves"; the remainder may be left out without affecting the structure of the sentence. This provides additional corroboration for the basic final position of V; it is a pattern which recurs in other IE languages as well. The position of P has been described independently by Bonfante (*AGI*, 24.2.1-60 [1930]) and Kuryłowicz (*BPTJ*, 5.39-46[1936]): either it occupies initial position, separated from V ("tmesis") or else immediately precedes V, still written as a separate word. In subordinate clause we have already in the Rig Veda the "univerbation" of juxtaposed P and V, which becomes the rule later for principal clause as well, with the elimination of tmesis. This indicates that P and V even in tmesis are constituents of a single semantic "word". In both cases V occupies final position, with the reserve for the "amplified sentence". Enclitic particles and pronouns (E), when present, occupy regularly the second position, as established by Wackernagel. Hence we may set up the following formulae for the Vedic sentence or clause, marking the boundaries by #. Simple verb: a) #V(E) . . # (marked); b) #.(E) . . V # (unmarked); Compound verb: c) #P(E) . . V #; d) #.(E) . . PV #. We shall see that these patterns repeat themselves with extraordinary regularity in the other IE languages under consideration. Rigvedic examples: a) *bháranti vām* . . (1.151.8); b) *īm* . . *jabhāra* (4.27.4); c) *prá vām* . . *bharante* (7.72.4); d) *te* . . *prá bharāmasi* (8.66.11). There are also cases of V (which is unaccented in principal clauses) occupying second, i.e. enclitic, position. It may be preceded by P in initial position, and most frequently by the connective pronoun *sá* (N). When an enclitic pronoun (E) is also present, this precedes enclitic V (though the situation is often not distinguishable from an amplified sentence). Cf. *antáh paśyanti vṛjiná* "they see into evil" (2.27.3, #PV . . #); *sá veda yajñám* "(and) he knows the sacrifice" (3.11.1, #NV . . #); *etá vo vaśmi údyata* "I wish you these proffered (words)" (2.31.7, #EV . . #).

As Delbrück has noted (*Vgl. Synt.*, 3.51), where both enclitic particle and enclitic pronoun are present in second position, the particle precedes the pronoun: *dyáús ca tvā* . . (3.6.3). Here *ca* > **k^we* is properly an enclitic connective N. The pattern (NE) in enclitic position is likewise general in the other languages. Connective N may also occur accented; in this case N occupies initial position. The clearest evidence for initial N is furnished by Hittite, for which see below; but in the frequent concatenation of phrases by the pronoun *sá-/tá-* in initial position, Vedic has preserved a clear reflex of the older situation. The pronoun *sá* as N may be followed by E (*sá u* passim); including enclitic V (supra); more usually V is final. When V is a compound, the formula is always #N(E) . . PV #: *sá devó deván práti paprathe* "(and) the god has expanded as far as the (other) gods" (2.24.11). The older invariant form *sá*, meaning

simply “and”, is preserved in the conjunctions *sá yádi*, *sá yátra*, *sá céd* (Wackernagel, *Kl. Schr.*, 257-61). The reconstructible **so* was termed by Wackernagel a “blosses Fulcrum”; it is simply a fossilized connective N.

The order of elements in the Hittite sentence is in general far more rigid than that of Vedic. The normal position of V is sentence-final, but initial position exists here also as a stylistically marked variant. Both single E and “strings” of E (pronouns and particles) always occupy the second position, and are written as one word with the initial element of the sentence, whether this be N, P, V, or other lexeme. Hittite knows semantic composition of P and V, though the actual accentual “univerbation” has not yet taken place (as it has in the more recent Lydian, for example), and the two are always written as separate words. The far commoner position of P is immediately before V in final position; but examples are not rare of P in sentence-initial position, with V at the end. For sentences with P, E, V, then, we have the same four formulae as in Vedic: a) #V(E) . . . #; b) #.(E) . . . V #; c) #P(E) . . . V #; d) #.(E) . . . PV #. Examples from the Laws: a) *kuenzi-ma-an LUGAL-uš* “but the king can kill him” (II § 74); b) *tepu-ši išhiyanzi* “they bind little on him” (I § 94); c) *šēr-wa-ši šarnikmi* “I shall make restitution for him” (I § 95); d) *išhas-šeš-a šēr šarnikzi* “and his master makes restitution” (I § 99).

Among the enclitics in second position there is a fixed order of precedence; cf. Laroche, BSL 53.161-74 (1958) for Hittite, Luvian, and Hierogl. “Hit.”. We may note here that the enclitic connective *-(y)a*, the functional equivalent of IE **k^we* (cf. *kuiš-a* = Lat. *quis-que*), always precedes enclitic pronoun: KUR UGU-*ya-mu*. . . (Hatt. I 26), with the same order of enclitics (NE) as in Vedic.

It is for the sentence connective N in initial position that the Hittite evidence is uniquely valuable. In archaic Hittite, we have three connective particles, *nu*, *ta*, *šu*, which always occupy initial position, and serve to link the sentence or clause with what precedes; they may be translated “and”, or simply ignored in translation. Though the later language uses the connective (*nu* alone) with greater regularity than the earlier language, it is clear from our earliest texts the N in initial position, optionally followed by one or more E, is a solidly established syntactic feature. As long recognized, these forms represent deictic pronominal stems; but they show the bare stem alone, IE **nu* **to* **so* **e* (> Luvian *a-* = Hitt. *nu*), and are formally on an older level than the corresponding inflected pronouns themselves. Formally, *nu* has been equated with the Old Irish “empty” preverb *no*; we can make a syntactic equation as well (cf. Dillon, *TPS*, 1947, 23). Both occupy sentence-initial position and both are regularly followed by E. In both Hittite and (archaic) Old Irish, V is final; we can juxtapose Hitt. *nu-mu* ^d*IŠTAR*. . . *kaniššan hartá* “and Ishtar held me in favor” (Hatt. I 66) and and OIr. *no-m Choimmdiu -coíma* “the Lord cherishes me” (*Early Ir. Lyr.*, 2.2). The formula for both is #NE. . . V#. Where V is a compound, it is preceded in final position by P; the formula is thus #N(E) . . . PV#. just as above in Vedic. Cf. *nu-kán tamedani kuedanikki andan paitteni* “you go in to someone else” (v. Schuler, “Dienstanweisungen”, *SAG*, 1, I 19), which may be contrasted with *anda-kán tamen-*

dani lē kuedanikki paitteni “do not go in to some one else” (*ibid.*, I 25), showing #PE. . . V#, without N. As we shall see below, there is ample evidence in the other languages to justify attributing this type of construction with N to IE itself.

We may consider now the situation in Latin. Here final position of V has been long recognized as basic, and we may state in general that the older the text, the more regular this appears; cf. the SC de Bac., or the Columna Rostrata, where it is constant. On the other hand, there is good evidence for permissible initial position of V as a marked variant; cf. Kroll, *Glotta*, 9.112-23 (1918). For the original second position of E, (though this “law” ceased to operate during the course of the historical period) it is sufficient to refer to Wackernagel’s study. I point out only that earlier Latin as well regularly presents the order *-que* + pronoun (NE) among the enclitics, e.g. *dantque eum* (Enn. Euhem.). By the time of our earliest extended records, the univerbation of P and V is an accomplished fact, and initial or final position of V includes V > PV. But clear traces remain of an earlier stage preceding the univerbation; that commonly described as “tmesis”. For our type d) #.(E). . . PV# cf. *manum endo iacito* (XII Tab.); for type c) #P(E). . . V#, *prae tet tremonti* (Carm. Sal.), *sub uos placo* (Fest.). The latter are minimal verb phrases, in that nothing comes between E in second position and V in final position, and the pattern PEV comes to be normal for literary tmesis; but I suggest that we have the real separation in *ē nōs Lasēs iuuāte* (Carm. Arv.), where I take *ē* as the preverb (: Skt. *ā*) appearing also in Lat. *hērēs* > **ghēro-* + *ē-d-*: Skt. *ā-dā-*.

One of the striking characteristics of the earliest Latin prose is the almost universal use of *-que*, enclitic on the first word of the sentence or clause, which functions as a pure sentence connective, and not as the copulative conjunction of the classical language. Compare the inscription of the Columna Rostrate, of the Carmen Euocationis, where enclitic N *-que* is as constant as sentence initial N *nu* in classical Hittite. This stylistic feature shows the antiquity in Latin of sentences involving N. We have however in Latin one extraordinary archaism which proves the former existence of an initial N in Latin, of the same form as Hitt. *nu*: the adverb *nudius tertius* “day before yesterday”. Hofmann has shown that this is a nominal sentence, “(now) it is the third day” (*IF* 42.77 [1924]). We can set it beside Hitt. *nu* ITU 10 KAJ (Otten, *Kummarbit* 7) “and it is the 10th month”, which is an identically constructed sentence; the equation shows that Latin once possessed an N identical in form, function, and position with Hitt. *nu*.

For Greek we are faced at first sight with a bewildering variety of positions; there is little to be made from such contrasts as Hecataeus 1 F 1 τούτου δ’ Οίνεως ἐγένετο: Οίνεως δ’ ἐγένετο Αἰτωλός. See in general Dover, *Greek Word Order*; Greek would appear to have gone farther than any other IE language in the elaboration of a “free” word order. For V, however, the basic constituents would appear to be final position, initial position, and second (enclitic) position. The last is particularly notable in Mycenaean, after N (*ho*, see below) in initial position: *ho-deksato*. . . , #NV. . . #, which agrees with this type in Vedic.

The univerbation of P and V is older than Mycenaean times (*apudōke* etc.), but the separation of the two was preserved in the literary language. Wackernagel, *Vorl.*, 2.174, points out that “Von alters her tritt Tmesis am ehesten ein, wenn das Präverbium zugleich an der Spitze des Satzes steht”, which is precisely our type c) #P(E) . . . V(. . .) #, e.g. *κατὰ πίονα μηρί' ἔκηα* (A 40). The other Homeric type of tmesis, where P immediately follows V, as in *τότε δ' ἤδη ἔχεν κάτα γαῖα μέλαινα* (B 699), I suspect to be a (purely literary?) Greek innovation, based on the inherited syntagm noun + postposition (cf. Hittite). It would have replaced our type d) #.(E) . . . PV (. . .) #, with P immediately before V. The occasional presence of a caesura noted by Sommer between apparently “univerbated” P and V (e.g. *ἀμφι | πονησομεθ' Ψ 159*) proves the original existence of a word boundary between P and V in type d) in Greek, exactly as in Vedic and Hittite. The same is shown by the presence of a word divider between P and V (with V in final position) in Cypr. *εξ | ορυξε* (Schwyzer, 679.11).

The second position of E in early Greek has been amply documented by Wackernagel, and confirmed by Dover. Delbrück has furthermore noted, after Monro, that enclitic connective (e.g. *δέ*) in second position precedes enclitic pronoun (NE); the same is characteristic of Mycenaean, e.g. *dāmos-de-min phāsi, ekhei-de-min* (*Doc.*, p. 254) (cf. *dantque eum*). The former example appears to show an enclitic V following E, an enclitic series (NEV) beside the latter V(NE); it may be compared with Vedic (EV).

It is Mycenaean which preserves connective *ho* in its original form, function, and syntactic pattern: the particle *o-*, with a graphic variant *jo-*, which regularly begins a clause, and is always written together with the following word, usually a finite verb: *ho-wide* “and he saw”, *ho-ophēlonsi* “and they owe” *ho-agrēse* “and he took” (*Doc.*, 43). The negation of the simple verb *didonsi* is *ou-didonsi*; but the negation of *ho-agrēse* is *ou-k^{we} agrēse*, which proves that *ho-* and *k^{we}* have the same semantic content, that of a connective “and”. The same conclusion is offered by comparison of sentences of the type NN *mo(i)roppās toto wetos ho-agrēse* ZE 1, with those of the type NN *theoio doelā ekhei-k^{we} onāton*, which are of identical structure: “NN (is) shareholder this year; and he took 1 pair”; “NN (is) the servant of the god; and she holds a lease”. Even the convention of writing *ho-* as one word with what follows is exactly paralleled in Old Hittite usage with the connective *nu-* (Otten, *MDOG*, 86.59-61 [1953]). The great stylistic development of the use of connective particles in classical Greek thus has its origins in IE times; it may indeed be compared with the much humbler generalization of *nu* in classical Hittite.

We may take Old Irish as “tertium” comparisonis; it is of critical importance for its isolated position on the western fringe of the IE area, and its evidence for comparative syntax is essentially untouched. In classical Old Irish, V occupies sentence initial position; with compound verb, the complex P+V occupies the same position; pronominal objects (E) are always unstressed, and may occur either “infixd or “suffixed”; suffixed E occurs after uncompounded V, and infixd E occurs between P and V. The respective formulae are thus #V(E) . . . # and #P(E)V . . . #; it is evident that

the position of E is consistently second in the sentence, and that we have a clear conservation of Wackernagel's Law. The same fixed order of enclitics (NE) recurs in Old Irish as well, e.g. *nachim-* < **ne k^we me*, or the class B and C pronouns, e.g. *-dam-* < **de me* (-*d-*: δè). For OIr. *no*, the lexically empty preverb serving as "prop" to infix pronouns to a simple verb, which continues the IE connective **nu* in initial position, see the section on Hittite above. Not only have we *no*: Hitt. *nu* continuing N, but also *se-*: OHitt. *šu* as well. Cf. *se-ch is* = *no-ch is*; the apophony **se/so* is that of Lat. *olle* < **ol-se*: Gk. *ó* < **so*.

Now besides the above patterns for P and V in classical Old Irish, archaic Old Irish preserves two earlier syntactic patterns, which have so far gone unnoticed in comparative IE studies; yet together they exactly recover the situation in the other early IE languages. These are Bergin's Law (*Ériu*, 12.197-214[1938]), and "tmesis". In tmesis, P(E) occupies initial position, and V is sentence final; in Bergin's Law, both simple V and compound PV are sentence final. It is clear from the comparative data that initial position of V is inherited; we can combine this with tmesis and Bergin's Law, and the result is four sentence patterns for the earliest Old Irish: a) #V(E). . . #; b) #. . . V # (B's Law); c) #P(E). . . V # (tmesis); d) #. . . PV # (B's Law). One can finally show that the classical Old Irish type #P(E)V. . . # is a late development, representing merely the univerbation of the tmesis type (c).

We have considered the relative positions of four elements of the verb phrase in five earlier IE languages. The situation is such that there can be no question of the patterns of order of any one of these languages influencing the pattern of any of the others, in respect of the elements considered. For this reason the essential identity among the five languages in the patterns of structure of the verb phrase is sufficiently remarkable that we may safely exclude chance; we have to deal here with direct syntactic inheritances from common IE times. I would submit, then, on the evidence of the equations among all these five languages for the earliest discernible patterns of phrase structure, that we are entitled to reconstruct the following types for the IE verb phrase:

I) For the uncompounded finite verb, an opposition of sentence initial and sentence final position of V, with a permissible E or series of E in second position: a) #V(E). . . # b) #.(E). . . V #. The latter was evidently "normal", and the former the stylistically marked member of the opposition.

II) For the finite verb semantically compounded with a preverb, obligatory final position of V, and an opposition of initial and prefinal position of P, again with permissible E or series of E in second position: c) #P(E). . . V # d) #.(E). . . PV #.

III) For sentences in narrative or continuous style, a connective particle may appear in initial position, followed by a permissible E or series of E in second position, with V in final position, preceded by P in the case of a compound verb: e) #N(E). . . (P)V #.

IV) In all five types (a-e), a connective particle (N) may also appear in second position; in this case an enclitic pronoun (E), if present, always comes after enclitic N. Thus we have permissible phrases beginning #.N(E), #PN(E), #VN(E), and

even #NN(E), as in **so/e k^we* (Ved. *sá ca* = OIr. *se-ch*), **nu k^we* (OIr. *no-ch* = Goth. *nauh*).

For the enclitic (second) position of V, we have the evidence of Vedic and Greek. But since these are precisely the only two IE languages for which we have other good evidence for the atonic character of V in non-initial position, it is possible that this position is an independent innovation in both; cf. Kuryłowicz, *Accent.*, § 4.

It is obvious that we have not considered the evidence of all the IE languages, nor all the evidence in those we have treated. Iranian agrees essentially with Indic, and both Baltic and Slavic would afford striking confirmation. The historical transformations in Germanic require particular investigation. I have purposefully ignored the situation of the dependent or subordinate clause, for reasons of space; but in this syntactic type the same equations can be made among the same languages, with the addition of the “relativizing” element **yo-*, **k^wo*. This element is ultimately only a member of the class N, appearing in either initial or second (enclitic) position. Likewise worthy of investigation is the treatment of the negation, which closely parallels P in distribution. Wackernagel, *Vorl.*, 2.172, notes as “eigentümlich” Goth. *niþ-ni-qam* “did not go in with” ‘οὐ συνεισήλθεν’ (John VI 22); but cf. the normal Hitt. type *anda UL pait* “did not go into”.

It is worthwhile noting that just as we have tonic and atonic personal pronouns (our E concerns only the latter), the connective N may appear in either initial or second (enclitic) position. So **nu* is initial in Hitt. *nu*, OIr. *no*, Ved. *nú*, but enclitic in Arc. ὄ-νυ, Ved. *nú*; **to* is initial in Hitt. *ta*, Goth. *þauh* > **tu-k^we* enclitic in Hitt. *na-tta*, Ved. *tú*; **so* is initial in Hitt. *šu*, Gk. *ho-*, enclitic in Hitt. *na-ššu*. Similarly Lat. *quī* but *-que*, Gk. ὅς but Hitt. *-ya*, OIr. *-e*.

We may add finally that the doctrine of the sentence connective permits the explanation of a peculiar IE dialect feature: the augment *e-*. We have seen that OIr. *no* corresponds formally and functionally to the Hittite connective *nu*. But there is a secondary function of *no* in Old Irish: it always (except in archaic poetry) precedes verb forms which have (OIr.) secondary endings. In this function it is identical with the augment *e-* of Greek and Indo-Iranian. Luvian provides the missing link: Luvian *a-* corresponds functionally to the Hittite connective *nu*, and formally to IE *e-* (cf. Luv. *as-* “be”, *ad-/az-* “eat”). We may set up a square as follows: illustrating both the formal and functional correspondences:

OIr.	<i>no</i>	—————	Hitt.	<i>nu</i>
	IE	<i>*e</i>	—————	Luv.
				<i>a</i>

The Luvian connective *a-* may thus be exactly equated with the “IE augment”, which is thus revealed to be by origin a connective used with “injunctive” forms in the function of continuous narration.

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DISCUSSION

ROSÉN:

The statements made here on the development of adverb-syntagmas are so much oversimplified that they cannot preclude false interpretations. On one hand, needless to say, we have to subscribe to what has been said, in this discussion, by Miss Hahn about the inappropriateness of the term *tnesis*; I also hold that many of the cases termed *tnesis*, e.g., in Homer, involve, in fact, postpositional syntagmas (Cf. my *Laut- und Formenlehre der herodotischen Sprachform*, 168 s., 184.) But what evolves out of syntagmas is not univerbations, at least not in the commonly accepted sense of this term (J. Wackernagel, *Vorlesungen über Syntax*², II, 82). What is commonly termed “univerbation”, is the treatment, for all intents and purposes, of an originally bi-morphemic sequence as mono-morphemic; univerbation is attained in the type ἐκάθιζον. But, as we know, this is only rarely attained and only in very specific conditions (*Laut- und Formenlehre etc.*, 163 s.; cf. Meister, *Homerische Kunstsprache* 217). The intermediate stage in succession to an adverb – verb syntagma, and long before univerbation, is composition, and this is what we are presented with in the vast majority of features in historical Indo-European: the compound verb in the common and broad sense of the term. This should have been the immediate object of a treatment like Professor Watkins’. But we cannot easily say that what Watkins terms “univerbation” is what we term “composition”; there is no unique feature of Indo-European that may be included under a single heading “verbal composition”, rather within what is ordinarily termed “compounds” we have a host of diachronically as well as synchronically distinct types, different in their syntagmatic behavior and historical background. To quote just one example: the two verbs προσμένω, one of which yields the immediate-constituent analysis [προσ+(μέν+ω)] and means “wait (some time) longer” (intransitive), an accusative complement indicates the duration of the additional waiting), while the other one represents [(προσ+μέν)+ω], is transitive (the accusative complement indicates what is waited for, expected) and is best translated by “be looking forward to” (Soph., *OR*, 837 vs. Hdt. 1,199,5, Soph., *El.*, 1236). (Likewise, a descriptive analysis of decompounds is extremely illustrative of the syntagmatic history of simple compounds.) The domaine of “verbal composition” is, consequently of utmost importance in historical studies like the one here presented.

SEILER:

My comment is on the problem of what Mr. Watkins calls the *Connectives* (N). I assume they have been so labelled for a certain function they perform. But this function is precisely the thing to be investigated before any definite labelling takes place.

In this class N we find elements like Latin *que*, Sanskrit *ca*, Greek *te*, always in second position, never initially. We find other elements like Hittite *nu* always initially.

And we find related elements like the Vedic *ní* initially and *nú* non-initially. Thus, there is a difference not only in position but also in form. It is safe to assume that not only the form but also the function or meaning is different. An element like the Mycenaean *ho* (p. 1040: *ho agrēse*) neither distributionally nor semantically belongs to the same category as the element *k^we* (in *ou-k^we-agrēse*).

I should therefore propose to reduce the N-category, preserving the term for those elements which occur non-clitically (i.e. in first position); and to enlarge the E-category as to cover not only the enclitic personal pronouns but all the other elements (particles, as they are called) sharing the same distributional, and, we may add, the same semantic property. The E-class then might be further subdivided into pronominals on one hand and particles on the other. I feel sure that such a regrouping does justice to the factual hierarchy of criteria and that it would enhance further reconstruction of IE sentence structure.

BIRNBAUM:

I have just a few additional remarks and questions concerning the methods of reconstruction in IE syntax. I found Mr. Watkins's title misleading, for it seemed to promise to cover far more than a limited problem of word order in IE verb phrases. I had expected some more general remarks on the methods of what I would call comparative (rather than internal) reconstruction of PIE syntax.

I think it is methodologically important to know how many terms of comparison can be considered a safe or necessary minimum is historical syntax to make us accept a reconstruction as proved or at least highly probable. Recently in my own limited field of investigation (Slavic historical syntax) the point was made by Dostál (following Meillet) that one must have concordant or identical evidence from at least three old Slavic languages in order to establish Common Slavic syntactic data. The requirement has been reiterated because in Slavic linguistics many syntactic reconstructions have been suggested on the basis of only two languages, usually Old Church Slavonic and Old Russian. My question now is whether in reconstructing the far more remote IE syntactic system one could even consider the agreement of three relatively old IE languages as sufficient, provided of course no contradicting evidence is available.

One final methodological question. Of course one always looks for the earliest possible attested evidence. Yet in a case like that of the enclitics, which Watkins discussed, one might be entitled to use more recent data. For instance, the system of enclitics in Common Slavic was comparatively simple, following Wackernagel's law on the whole (cf. Berneker et al.). Serbo-Croatian has in historical times developed a much more sophisticated positional system of enclitics. Nevertheless I think it permissible to search in the underlying pattern of ranked word order (e.g. when SC verbal enclitics like *sam* or *ću* occur together with, say, the pronominal *ga* or *se*) for older, or even original, Common Slavic or perhaps PIE principles. If so, then in looking for evidence about the general model of IE sentence structure, should one not search for more recent data as well as the oldest attestations? Perhaps this is what

Watkins had in mind when he wrote, "both Baltic and Slavic would afford striking confirmation".

WATKINS:

Both the terms *tnesis* and *univerbation* appear with quotation marks in my paper; what I mean by them is clear from the context of the paper, and does not differ substantially from Dr. Rosén's remarks. I am basically in agreement with Dr. Seiler's suggestions; there are formal differences between initial and enclitic connectives, which must be accounted for. It is still true that connective *-que* in archaic Latin texts is functionally and stylistically equivalent to connective *nu* (in initial position) in classical Hittite, hence my treating both as N. Whether this was true for Indo-European itself, for which we can reconstruct both **k^we* and **nu*, is another matter, and here Dr. Seiler's suggestion may be quite productive. Yet the new category must account (e.g.) for both enclitic **k^we*, **yo*, and the pronominal stems **k^wo-*, **yo-*, which in certain IE dialects occupy clause initial position. Space does not permit the detailed consideration of the methodological points raised by Dr. Birnbaum. I will say in general that there is no magic number of agreeing linguistic traditions to establish an original; far more important is the nature of the particular correspondence, as is shown in Dr. Teeter's paper above.

I am grateful to Prof. E. Risch for pointing out to me that Wackernagel's equation Gk. ὄτε: Ved. *sá ca*, which I had originally accepted, cannot be correct, since we have *ote* (not *ok^we*) in Mycenaean.

A much fuller account of the syntactic reconstructions presented in this paper may be found in my study, 'Preliminaries to a historical and comparative analysis of the syntax of the Old Irish verb', in the Irish journal *Celtica*, vol. 6.

MEANING IN MORPHEMES AND COMPOUND LEXICAL UNITS

WILLIAM AMES COATES

Lexicology, the scientific study of vocabulary, has long been recognized as a branch of linguistics, but for some reason it is not yet generally recognized that it is an autonomous level of linguistic analysis on a par with phonemics, morphemics, etc.¹ Recent textbooks of linguistics almost without exception ignore lexicology and do not even hint at the possibility of treating the synchronic structure of vocabulary, although there has been no lack of specialized works in this field. Linguists are therefore faced with the task of integrating lexicology into the body of descriptive linguistics and examining its relations with other levels of linguistic analysis. It is the principal purpose of this paper to explore some of those relationships.

Every level has its own basic unit, distinct from all other linguistic units. The unit of lexicology may be defined as that unit on the expression side of language which corresponds directly to something on the content side; this relationship is what is known as *meaning*. There is a further limitation in that lexicology is concerned only with lexical meaning, not with grammatical meaning. Lexical meaning may be analyzed as including denotational and connotational meaning; the latter falls outside the scope of this paper.

One can still read assertions that the linguistic unit having lexical meaning is the morpheme.² This is an instance of confusion of levels: if the morpheme is the unit of morphemics, then it cannot be the unit of lexicology. The unit of lexicology must be a unit in its own right, which may be called the *lexical unit*. While this fact is being increasingly recognized, no one seems as yet to have drawn the logical corollary, that *a morpheme as such need not have lexical meaning*. This principle clearly has important consequences for morphemic analysis.

It is true, of course, that most morphemes do have lexical meaning, but they do so only because they are at the same time lexical units. Just as some morphemes may consist of a single phoneme (e.g. *a* in French), so too many lexical units consist of a single morpheme.

Every lexical unit, whether simple or compound, has denotational meaning, by definition. The constituent morphemes of a compound lexical unit in many cases also

¹ Furthermore, it is sometimes confused with lexicography, the practical science of writing dictionaries.

² I understand the term *morpheme* in its broad sense, and distinguish between content morphemes and function morphemes. Since this paper is primarily concerned with the former, the term *morpheme* by itself is used here to mean *content morpheme* unless otherwise specified.

have denotational meaning. This is true, for example, of most such morphemes which also occur as simplexes; e.g., there is an obvious connection between the meaning of *book-shelf* and the meanings of the morphemes *book* and *shelf* occurring as simplexes, and we can say that these morphemes retain their denotational meanings even as constituents of the compound lexical unit.

In the case of lexical units composed, wholly or in part, of morphemes which never occur as simplexes, i.e. bound morphemes, it may be difficult or even impossible to assign any denotational meaning to the latter. Yet they clearly bear a relationship to the meaning of the whole. Here we may apply the principle of differential meaning, which has proved so fruitful in linguistic analysis. The chief function of a morpheme in a compound lexical unit may be merely to distinguish that one unit from all others. Thus *de-* serves to distinguish *deceive* from *receive*, *perceive*, etc., while *-ceive* distinguishes it from *detect*, *destroy*, etc.; both therefore have differential meaning. (Some linguists deny morphemic status to *de-*, *-ceive*, and similar elements, partly because they discern no denotational meaning in them. Others see a common core of meaning in *deceive*, *receive*, *perceive*, etc. It is in any case certain that if *-ceive* has any denotational meaning, it is much vaguer than the meaning of *book* in *book-shelf*. But it is beyond doubt that it has differential meaning, and this is certainly primary.)

Denotational and differential meaning are consequently not mutually exclusive; a given morpheme in a given compound lexical unit may well have both. In fact, in compound lexical units the constituent morphemes always have differential meaning, even in cases where their denotational meaning is quite clear. So *shelf* serves to distinguish *book-shelf* from *bookcase*, *book-counter*, etc., while at the same time there can be no doubt about its denotational meaning.

In some cases the denotational meaning seems at first sight to be quite clear, but further analysis raises doubts. In the word *blackberry*, for instance, the morpheme *black* would seem to have its ordinary meaning; but the familiar childhood paradox "Blackberries are red when they are green" reminds us that blackberries are not necessarily black. The usual meaning of *black* is evidently not essential to the meaning of the whole lexical unit, though not unrelated to it; so once again the differential meaning is primary and the denotational meaning secondary.

A third type of meaning is seen in morphemes such as *-ice* in *justice*. While it is possible to see a certain degree of denotational meaning in it, it seems to serve principally to transform an adjective into a noun; its meaning is therefore primarily functional, and it may perhaps be assigned to the class of function morphemes, familiar in morphology and syntax. The process of derivation by which *justice* is formed from *just* is certainly part of morphology, but its relevance is lexical as well as grammatical.

Whether or not there is a fourth type of meaning that morphemes may have will depend on the particulars of morphemic analysis. Those who have used denotational meaning as a criterion for morpheme status do not consider elements such as the *er* in *ladder* to be morphemes; but once the requirement of denotational meaning is dispensed with, this is no longer so certain. The details of morphemic analysis lie outside the

scope of this paper; but it may be that the procedures can be set up in such a way as to either include or exclude such elements as *-er*. If *-er* is recognized as a morpheme, what can be said about its meaning? It certainly has no denotational meaning; and since *ladd-* is of unique occurrence, it has neither differential nor functional meaning, there being no other combinations for it to contrast with. Yet it is essential to the meaning of the whole; *ladd-* and similar elements never occur without it. This would appear to be its outstanding characteristic. It is thus a question of characteristic distribution; so we may say that if *-er* is to be considered a morpheme, it has distributional meaning.

There are, then, four types of meaning which morphemes may have: denotational, differential, functional, and distributional. The first three may occur together in many instances; the fourth becomes necessary precisely in those cases where none of the other three apply.

It has been generally recognized since Saussure that the linguistic sign is arbitrary in its ultimate nature, but that arbitrary elements can be put together in combinations which are not completely arbitrary. This is as true of vocabulary as it is of any other aspect of language. We can accordingly distinguish between lexical units with motivated meaning and those with unmotivated meaning, and this has proved to be one of the most fruitful concepts in lexicology. A simplex can have only unmotivated meaning, except for the relatively few instances of onomatopoeia and sound symbolism; a compound lexical unit may have either motivated or unmotivated meaning.

When the connection between a lexical unit and the concept it represents is completely arbitrary and conventional, we speak of lack of motivation; motivation means that this connection is not entirely arbitrary and conventional, but that the native speaker can see a direct connection between the structure of the lexical unit and its meaning. This should not be interpreted to mean that we can necessarily arrive at the meaning directly when we start from the structure; in many cases the connection becomes clear only when the meaning is known. For example, when we know the meaning of *forget-me-not*, we realize that it is a poetic figure of speech, and we can see how the meanings of its elements are related to the meaning of the whole; but starting from the lexical unit, we cannot arrive at its precise meaning: even if we know that it is a figure of speech and that the reference is to a flower, there is still nothing to indicate the particular species.

Motivation is commonly thought of as proceeding from the expression to the content. Complete motivation, however, is a reversible process. There is, to be sure, no way of knowing *a priori* that for a given concept a language will use a completely motivated lexical unit – here again the arbitrary nature of language manifests itself. But once this is known, we can construct the required lexical unit on the basis of the simplexes (or other constituent morphemes) and the prescribed methods of combination; for example, knowing *book* and *shelf* and the English rules of compounding, we can produce *book-shelf*, a fully motivated lexical unit.

It is clear from this that instances of onomatopoeia and sound symbolism can never be fully motivated. Different languages often differ widely in the onomatopoeic expressions they use for the same phenomena; and even where there is a marked resem-

blance between the expressions, there are usually smaller differences, as in English *bow-wow* and German *wau-wau*. It is consequently never possible to predict the exact form that an onomatopoeic expression will take; an element of the conventional is always involved.

Between the extremes of complete motivation and lack of motivation there exist various grades of partial motivation. Saussure spoke of "un phénomène qui permet de reconnaître des degrés dans l'arbitraire",³ but to the best of my knowledge this suggestion has never before been developed. It is possible to establish degrees of motivation in a fairly precise manner. The following points are to be considered:

1) The existence or lack of a simplex: a lexical unit containing one or more simplexes has a higher degree of motivation than one containing only morphemes which do not occur as simplexes. For example, both *endless* and *infinite* are motivated; but *endless* is derived from the simplex *end*, while analysis of *infinite* finally gives us the morpheme *fin-*, which is not a simplex; accordingly *endless* has in this respect one degree more motivation than *infinite*. A very large part of the vocabulary of English lacks simplexes, particularly words of French, Latin, or Greek origin. But there are also words of Germanic origin which lack simplexes, e.g. *cranberry*; and there are some words of Latin origin for which English does have simplexes, e.g. *unlimited* has a higher degree of motivation than *infinite* because the simplex *limit* does exist.

2) The use of a constituent morpheme in a sense different from any it has as a simplex. For example, the morpheme *case* does not have the same meaning in the lexical unit *bookcase* that it does as a simplex: the piece of furniture known as a *bookcase* would never be referred to simply as a *case*, while a *book-shelf* might often be called merely a *shelf*; and a *case of books* or a *case for books* is quite a different thing from a *bookcase*. *Bookcase* therefore has one degree less motivation than *book-shelf*.

3) Stages of derivation or compounding: a compound lexical unit may contain two morphemes or it may contain more, and derivatives in particular may include a whole series of affixes; each additional stage of derivation or compounding represents one less degree of motivation. Thus, while *endless* and *endlessness* are both motivated, *endless* is one stage closer to the simplex and so has one degree more motivation.

4) Phonetic resemblance: the greater the resemblance between the compound lexical unit and the simpler form from which it is derived, the higher the degree of motivation. For example, there is a close resemblance between Polish *ilość* "quantity" and *ile* "how many?", while the resemblance between the corresponding Russian *količestvo* and *skol'ko* is much less; *ilość* therefore has a higher degree of motivation than *količestvo*.

5) It is possible to analyze a lexical unit and see how many items of additional information must be provided in order to arrive at the meaning; each item means one degree less motivation. For example, the lexical units *red-cap* "railway porter (in the U. S.)" and *white-cap* "wave with a foaming crest" are entirely parallel in structure but diverge

³ Ferdinand de Saussure, *Cours de Linguistique Générale* (Paris, 1922), p. 181.

sharply in meaning; they also have different degrees of motivation. Both are composed of simplexes, used in their regular meanings, and the phonetic resemblance to the simplexes is complete; both represent the same stage of compounding. Additional information must be provided at the following stages: 1) both are bahuvrihi compounds (not formally distinguished in English from karmadharaya compounds); 2) *red-cap* refers to a man, *white-cap* to a thing; 3) *red-cap* refers only to a man with a particular occupation (not every man with a red cap is a *red-cap*), *white-cap* refers to a particular thing, i.e. a wave. For *white-cap* one additional stage is required: 4) the reference is metaphorical (the crest of a wave is only compared with a white cap, while a railway porter actually has a red cap). Thus *red-cap* is three degrees removed from complete motivation, *white-cap* four degrees. A similar analysis can be carried out in order to determine the degree of motivation of any lexical unit.

Earlier in the paper the term *lexical unit* was defined, but little was said about just what the term should be considered to include. This is a point on which opinions may differ considerably; I would suggest the following.

Two basic considerations must be borne in mind. First, the lexical unit is a unit on the expression side of language; it is not a unit on the content side, a semantic unit. Second, the identifying characteristic of the lexical unit is its connection with the content side, i.e. with one or more semantic units. In trying to decide just what is and what is not a lexical unit, criteria from both the expression side and the content side must be applied.

A lexical unit may be simple or compound. I use the term *compound lexical unit* to refer to any lexical unit consisting of two or more morphemes, regardless of the way they are put together. A compound lexical unit consisting of two morphemes may contain two free morphemes (e.g. *bookcase*, *get up*), one free and one bound morpheme (e.g. *justice*, *cranberry*), or two bound morphemes (e.g. *perceive*, *asinine*); with more than two morphemes the possible combinations naturally increase. A bound morpheme may recur in other combinations (e.g. *perceive*, *receive*, *perfect*), or it may be of unique occurrence (e.g. *cranberry*); in the case of two bound morphemes, only one can be unique, since otherwise there would be no basis for division into two morphemes.

Derivation and compounding are processes of word-formation found in many languages; each language has its own formal criteria for them. I consider all derivatives and all compounds to be lexical units. There may also be other fixed combinations which cannot be classed formally as either derivatives or compounds, i.e. they are normal syntactic groups except for the fact that they must be handled as single units. Such a fixed syntactic group is *black market*, as can be seen by comparison with the free syntactic group *illegal market*: we can say, for example, *illegal steel market*, inserting an extra element between the two components, but we cannot do the same thing with *black market*; we can only say something like *black market in steel*.

On the content side the basic criterion for lexical units is motivation: all combinations with unmotivated or partially motivated meaning are to be considered as lexical units, regardless of their formal status, i.e. including even free syntactic groups.

A given combination may be classed as a lexical unit in terms of criteria from either

the expression side or the content side, or from both together, but it is never necessary that both be applied together.

The application of the criteria may be seen from the following examples. A typical characteristic of English vocabulary are the familiar verb-adverb groups. From the formal point of view they are free syntactic groups; they consequently qualify as lexical units only if their meaning is not fully motivated. For example, *go in* is fully motivated and therefore is not a lexical unit; but *give up* in the sense of 'stop trying' is not fully motivated and accordingly is a lexical unit. On the other hand, *book-shelf* is fully motivated, but it is a lexical unit because it is a compound.

The phenomena of synonymy, polysemy, and homonymy raise problems in determining lexical units. All three arise from the fact that there is not always a one-to-one correspondence between lexical units and semantic units. Synonymy occurs when one semantic unit or two or more closely-related semantic units correspond to several linguistic forms; in such cases there are as many lexical units as there are linguistic forms. Polysemy occurs when one linguistic form corresponds to two or more related semantic units; in such cases there is only one lexical unit. Homonymy occurs when one linguistic form corresponds to two or more unrelated semantic units; in such cases there are as many lexical units as there are semantic units. It goes without saying that it may often be difficult in a particular case to decide between polysemy and homonymy.

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DISCUSSION

MATTOSO CAMARA:

I should like to call the attention of Prof. Coates to a passage of his paper (p. 1049) where he alludes to the concept of Saussure about arbitrary signs, unmotivated signs and motivated signs. He seems to consider *arbitrary* and *unmotivated* as equivalent terms. Such was not the thought of Saussure. By *arbitrary sign* he meant a form whose sounds and sound-pattern have no relation with physical features of the outer world; and he says that "the linguistic sign is essentially arbitrary", except for onomatopoeias and words with sound symbolism. Unmotivated, on the contrary is a linguistic form that cannot be deduced from the grammatical pattern of the language. Arbitrary inanalysable forms are unmotivated, but analysable ones are not. Onomatopoeias are not arbitrary but are unmotivated; and so on. I think that we should maintain the neat distinction that Saussure has made and not mix his concepts as does Prof. Coates.

STRANG:

The distinction of lexicology from morphemics, lexemes (in Dr. Haden's terms) from morphemes, is most useful, but there is ground for further clarification of the

notion of lexical unit as Dr. Coates presents it. He defines lexicology as the scientific study of vocabulary, and its units, lexical units, are apparently items of vocabulary. His further explanations make clear that vocabulary means more than words (in the popular senses) (cf. especially p. 1047), but one claim seems to indicate that it means less than words (in these senses). The claim is that "every lexical unit, whether simple or compound, has denotational meaning, by definition" (p. 1046). But closed-system items having the form of words may well not do so, consider prepositions (*of, by?*) in English.

WORTTHEORIE AUF STRUKTURALISTISCHER UND INHALTSBEZOGENER GRUNDLAGE

HANS GLINZ

A. GRUNDBEGRIFFE

1. Ein *Wortinhalt* (= eine kleinste im Satz bewegliche¹) semantische Einheit ist eine geistige Grösse eigenen Rechts, die zwar von einem *Wortkörper* (= Lautkomplex, phonematisches Korrelat der semantischen Einheit) getragen wird, aber grundsätzlich von diesem Wortkörper abgelöst werden kann und daher systematisch von ihm zu unterscheiden ist. Der Wortinhalt kann nicht direkt übermittelt und nicht durch direkte Nachahmung erlernt werden, wie der (hörbare, lautlich nachahmbare) Wortkörper. Bei jeder sprachlichen Verständigung wird beim Partner der Besitz der nötigen Wortinhalte schon vorausgesetzt; die geäußerten (gesprochenen, geschriebenen) und aufgenommenen (gehörten, gelesenen) Wortkörper dienen als auslösende Signale für diese (als geistiger Besitz schon vorhandenen) Wortinhalte. Dasselbe gilt für die "Formal-Werte" oder "Struktur-Inhalte" (= Inhaltswerte von Wortarten, Wortformen, Satzplänen).

Erlernung eines neuen Wortinhalts heisst also *nachvollziehende Neubildung* des betr. Inhalts im lernenden Gesprächspartner oder überhaupt Handlungspartner. Solche Neubildung kann in einem einzelnen Akt (Übernahmeakt, Verstehensakt) erfolgen, oder in einer Folge von Akten. Bei solchen Übernahmeakten wirkt der Wortkörper (der gehörte oder gelesene phonematische Träger) als Anstoss (als Reiz, als Katalysator). Aus den bekannten grammatischen Strukturen und den Wortkörpern mit schon bekannten Wortinhalten (= dem "Kontext" im engeren Sinn) sowie aus der gesamten Situation (= dem "Kontext" im weiteren Sinn) wird zunächst das hier und jetzt vom Sprecher/Schreiber *Gemeinte* erschlossen und dadurch (meist nicht in besonderer Absicht, sondern implizite) auch der neue Wortinhalt (als "Mitträger des Gemeinten an einer bestimmten Stelle", "lokalisierbarer Mitträger", "isolierbarer Mitträger"). Das Erlernen des Wortinhalts ist also ein Neubilden, ein eigener geistiger Akt, für den der Wortkörper nur als auslösendes Signal gelten kann, als Katalysator, *nicht* als materiale Ursache und gültige Determination.

2. Für neue Wortinhalte (= neue festzuhaltende Ergebnisse geistiger Prozesse, neue semantische Einheiten) werden aber sehr oft nicht völlig neue Wortkörper (= neue

¹ Ich nehme hier die von Eric Buysens in der Diskussion angebrachte Korrektur an meiner Ausdrucksweise in den "Preprints" gerne an.

phonematische Träger) geschaffen, sondern es werden Wortkörper herangezogen (einzeln oder als ganze Komplexe), die schon andere Inhalte zu tragen hatten und weiterhin tragen. Die Motive, die eine solche Wortkörper-Wahl bestimmen, können ganz zufällig sein; sie spielen für die Inhaltskonstitution als solche meist keine Rolle, und sie sind den lernenden Partnern (= neuen Übernehmern des Wortinhalts) meist gar nicht bekannt. Ein neuer Wortinhalt kann daher (auch von einem ideal ausgebildeten Partner) meist *nicht* aus dem ihn tragenden Wortkörper schlüssig konstruiert werden (auch nicht bei vollkommen regulärer Wortbildung). Der Wortkörper kann allerdings gewisse *Hilfen* bieten zum Erfassen und Behalten des Wortinhalts; aber die Tragweite und Zuverlässigkeit dieser Hilfen ist grundsätzlich *nicht* voraussagbar. Auch ein ganz regulär gebildeter Wortkörper fungiert daher in der natürlichen Sprache nicht als Determination, sondern nur als Reiz, als "anregendes Signal" für die nachvollziehende Neubildung des Inhalts im neuen Partner (vgl. S. 1053).

3. Da als Träger neuer Inhalte sehr oft die Wortkörper schon vorhandener Inhalte herangezogen werden, kann es in einer Sprache zusehends mehr Inhalte als Wortkörper geben, und ein Wortkörper kann verschiedene Inhalte zu tragen haben, die an sich nichts miteinander zu tun haben müssen. Doch wird ein Wortkörper von dem Inhalt, den er in erster Linie zu tragen hat, oft einen gewissen "Trägerwert" gewinnen, und dieser Trägerwert kann als Motiv wirken, wenn für einen neuen Inhalt geeignete Wortkörper gesucht und herangezogen werden. Auch wenn ein Wortkörper verschiedene, aber leicht aufeinander beziehbare Inhalte trägt, kann sich aus einem ihnen allen gemeinsamen Charakter ein gewisser "Grundwert" oder Trägerwert für den betr. Wortkörper bilden. Das wird vor allem der Fall sein, wenn der Wortkörper normalerweise nur als Teilträger in einem ganzen Komplex fungiert, z.B. eine Partikel wie "an" in "er fängt an", "er macht Licht an", "er lehnt sich an", "an dieser Stelle", "an sich" usw.

Diese Trägerwerte sind nicht zu verwechseln mit einer "Grundbedeutung" im diachronischen Sinn. Sie sind "allgemeine Inhaltstönungen, die sich für einen Wortkörper aus seinem Gebrauch für den und den Inhalt oder die und die Inhalte ergeben", sind also rein synchronische Werte und können sich demgemäß ändern, erweitern oder verengern, wenn der betr. Wortkörper für einen neuen Inhalt als Träger herangezogen wird.

B. FOLGERUNGEN FÜR DAS RICHTIGE VERSTÄNDNIS VERSCHIEDENER METHODEN

Es ist nun wesentlich, dass man bei jeder sprachwissenschaftlichen Arbeit klar sieht, ob die verwendeten Methoden als Resultat primär Wortinhalte oder Wortkörper oder ihre Trägerwerte liefern.

1. Die "mentalistischen" Verfahren älterer Art (z.B. in der Lexikographie) gehen

primär auf Wortinhalte, die je von einem festgelegten Wortkörper getragen werden ("Bedeutungen" eines Wortes oder eines Wortstamms); dabei ergibt sich oft die Gefahr, dass man sich von ausser- und übersprachlichen Kriterien leiten lässt und eine Mehrzahl von Bedeutungen ansetzt, wo die Sprache als solche nur *einen* Inhalt aufweist. Die "Bedeutungen" sind dann "mögliche mit dem Wort gemeinte Begriffe", aber nicht der Wortinhalt als geistige Einheit.

2. Um diese Fehlerquellen zu vermeiden, hat man (vor allem seit Saussure und Bloomfield) die "strukturalistischen" Verfahren – die früher stets auch im Spiel waren, aber meist unbewusst blieben – bewusst entwickelt: Ausgehen von einem festgelegten Korpus sprachlicher Äusserungen; Herausheben gleicher Teilstücke, die offenbar beim sprachlichen Handeln gleiches leisten ("recurring sames"); Aufarbeiten aller Möglichkeiten des Korpus durch Umformungs-, Ersatz- und Verschiebepробen, kontrolliert durch "Informanten", die die betr. Sprache als Muttersprache sprechen (falls der Forscher nicht seine Muttersprache untersucht, in der er sein eigener erster Informant ist und eine Vielzahl seiner Leser als kontrollierende Informanten betrachten kann²).

Diese Methoden, die sich durch eine leichte Kontrollierbarkeit und einen hohen Sicherheitsgrad auszeichnen, liefern in erster Linie *Strukturen* und *Wortkörper*. Wenn man sich nicht damit begnügen will, diese Strukturen und Wortkörper nur zu registrieren, wenn man ihre *Leistung* zu erfassen und zu beschreiben versucht, gelangt man zunächst zu *Trägerwerten*, sowohl für die Strukturen wie für die Wortkörper.³ Unter Umständen gelangt man auch direkt zu Struktur- oder zu *Wortinhalten* (wenn nämlich in dem betr. Fall nur *ein* Inhalt vom Wortkörper getragen wird und sich daher kein besonderer Trägerwert gebildet hat); doch lässt sich grundsätzlich nicht voraussagen, ob man bei strukturalistischem Angang unmittelbar auf einen Inhalt oder erst auf einen Trägerwert stösst.

3. Wenn man die Inhalte zuverlässig erfassen will – auch wo von ein und demselben Wortkörper verschiedene Inhalte getragen werden, und auch wo einheitliche Inhalte von ganzen Wortkörper-Komplexen getragen werden – muss man die rein geistige Konstitution solcher Inhalte (S. 1053) bewusst in Rechnung stellen und muss demgemäss die primären strukturalistischen Methoden erheblich verfeinern. Diese Verfeinerung dürfte auf dem Wege über Informanten nur schwer zu erreichen sein, sie setzt eine möglichst vollständige und "tiefe" Aneignung der betr. Sprache durch den Forscher voraus. Sie wird am ehesten erreichbar sein, wenn die untersuchte Sprache zugleich die Muttersprache des Forschers ist und wenn er neben der Alltagsrede auch die sprachlichen Kunstwerke in ihren verschiedenen Erscheinungsformen heranzieht.

² Vgl. Glinz, *Die innere Form des Deutschen, eine neue deutsche Grammatik*, 2. nachgeführte Auflage (Bern, 1961).

³ *Innere Form des Deutschen*, S. 5-7 und Beilage, sowie *Ansätze zu einer Sprachtheorie* (Düsseldorf, 1962), S. 83-84.

C. BEISPIELE VON ANALYSEN VERSCHIEDENEN GRADES

Eine Unterredung zwischen einem begutachtenden Arzt (Psychiater) und der Tochter eines Patienten, dessen Arbeitsunfähigkeit (zwecks Zuerkennung einer Rente) der Arzt feststellen soll, beginnt (nach Bandaufnahme von 1956, Namen geändert):

- Arzt: (1) Wie heissen Sie denn?
 Frau: (2) Ich heisse Telani geborene Sobitzki nich
 Arzt: (3) Telani geborene Sobitzki?
 Frau: (4) Ja ja
 Arzt: (5) Und jetzt erzählen Sie mir doch mal was Sie bei Ihrem Vater bemerkten
 Frau: (6a) Ach Gott er is in letzter Zeit sehr vergesslich er bringt alles durcheinander
 (6b) und wir können ihn gar nicht mehr wenn Mutti die wohnt in Münsingen ich wohn in Schifferstedt wenn er manchmal zu mir nach Schifferstedt kommt
 (6c) dann sag ich na Papa was sollst du denn nun holen was sollst du denn einkaufen
 (6d) ja weisst du ich habe alles vergessen ich weiss überhaupt nichts mehr nicht
 (6e) und dann unterhalten wir uns was und dann fängt er plötzlich von ner ganz andern Sache zu erzählen
 (6f) was gar nicht dazu passt
 (6g) ja und äh
 (6h) und er erzählt zum Beispiel viel von früher viel von früher so
 (6i) und in der heutigen Zeit kann er sich gar nicht mehr so richtig reinfinden und
 (6k) tja
 (6l) das ist natürlich nicht nicht mehr das Richtige nich

Durch Anwendung strukturalistischer Verfahren (z.B. durch Ersatzproben: "Wer sind Sie denn", "Ihr Name bitte", "Woher kommen Sie denn", "Warum kommen Sie denn", "Was wollen Sie denn") gewinnt man zunächst eine Satzstruktur:

Fragepartikel mit "w"	Personalform des Verbs	Personalpronomen	redesituierende Partikel
weitgehend aus- wechselbar	weitgehend auswechselbar	nur auswechselbar mit "Du" oder "Ihr", gekoppelt mit Ände- rung der Personalform	nicht auswechsel- bar, nur weglassbar

Ferner gelangt man zu den Wortkörpern "wie – heissen – Sie – denn".

2. Durch feinere Proben grenzt sich der hier von dem Wortkörper "denn" getragene Inhalt ab von andern Inhalten, die vom gleichen Wortkörper in anderer Stellung

getragen werden (“*denn* das war falsch” “so hat es ihn *denn* erwischt”). Das “denn” hat in den drei Stellungen eine verschiedene Ersatzreihe, die das Gemeinte in verschieden starkem Masse ändert:

“wo bist du denn”	“so hat es ihn denn erwischt”	“denn das war falsch”
	also	aber
	nun doch	also
		und
		das war nämlich falsch
		weil das falsch war

Bei Umstellung verändert das “denn” offensichtlich seinen Wert (“So hat es ihn denn erwischt” und “Denn es hat ihn so erwischt” sind inhaltlich sehr verschieden!). “Denn” erweist sich also als ein Wortkörper, der mindestens drei Inhalte trägt.⁴ Wie diese Inhalte beschaffen und wie sie geistig abgegrenzt sind, das geht aber aus der rein strukturalistischen Analyse (“Analyse von aussen”) *nicht* hervor. Hierfür braucht es eine “Analyse von innen”, die eine genaue Kenntnis der deutschen Sprache, ja eine innige Verwurzelung in ihr voraussetzt.

“Denn (1)”, endgestellt: Anschluss einer Frage an eine bestehende Situation; die Frage wird als aus der Situation herauswachsend dargestellt, sie ist aus der Situation nötig geworden.

“Denn (2)”: Bestätigung einer Erwartung. Es könnte folgen “ich dachte es ja”, “das konnte man voraussehen”, “hast du daran gezweifelt?”.

“Denn (3)”: Satzanfang, ohne Einfluss auf die Stellung der andern Satzglieder: der Satz wird als erkannte Ursache eines schon genannten Sachverhaltes oder Geschehens hingestellt.

Als gemeinsamer Trägerwert von “denn” lässt sich formulieren: “Situierung der Rede im Erwartungshorizont der Gesprächspartner”.

3. “Was Sie bei Ihrem Vater *bemerkten*”. Eine Ersatzprobe ergibt: “bemerkt haben”, “gesehen haben”, “sehen konnten”, “festgestellt haben”. “Bemerkten” trägt hier den Inhalt “sinnlich-geistiges Aufnehmen und Festhalten”, und der sozusagen gleiche Inhalt kann auch vom Wortkörper “sehen” getragen werden. Der Wortkörper “bemerken” kann aber auch einen andern Inhalt tragen, nämlich “etwas sagen”. Dieser Inhalt könnte sogar in der hier vorliegenden Satzstruktur auftreten, ohne jede Änderung der Wortkörper (nur das “bei ihrem Vater” trüge dann ebenfalls einen etwas andern Inhalt): Was Sie in Gegenwart Ihres Vaters bemerkten/sagten”. Das “bei” ist im einen Falle durch “an” ersetzbar (“an jemand etwas bemerken”), im andern Falle ist es durch “in Gegenwart von” ersetzbar. Wir haben also in “bemerken” einen Wortkörper, der zwei deutlich unterschiedene Inhalte trägt, den einen gemeinsam mit “sehen”, den andern gemeinsam mit “sagen”. Als Trägerwert (also

⁴ Wenn man den Gebrauch “besser *denn* je” (= *als* je) als altertümlich ausser Betracht lässt.

als "gemeinsame Inhaltstönung" oder als "kleinsten gemeinschaftlichen Nenner") kann man ansetzen: "geistig-sinnliches Handeln, zu dem es eine gewisse Aufmerksamkeit braucht".

4. "Er *bringt* alles *durcheinander*". Ersatzproben: "er bringt alles in Unordnung, verwechselt alles, kann nichts mehr richtig behalten". Es liegt also ein einheitlicher Inhalt vor, der durch den ganzen Wortkörper-Komplex "durcheinander bringen" getragen wird. Die beiden Wortkörper tragen aber daneben noch ihre eigenen, getrennten Inhalte, und diese könnten bei anderer Situation durch die genau gleichen Wortkörper in genau gleicher Satzstruktur wirksam werden: "er hat es vorher nicht geordnet, er bringt alles durcheinander"; hier wäre der Ersatz möglich "er *bringt* alles durcheinander *herbei*, er *trägt* alles durcheinander *herbei*". Ein einheitlicher Inhalt kann also von einem ganzen Wortkörperkomplex getragen werden; jeder beteiligte Wortkörper kann daneben in anderer Verbindung seinen sonstigen Inhalt tragen, und die Wortkörper können sogar in gleicher Kombination das eine Mal den höheren, einheitlichen Inhalt, das andere Mal eine einfache Verbindung ihrer gewöhnlichen Inhalte tragen.

5. Der Satz "und jetzt erzählen Sie mir doch mal, . . ." könnte auch lauten: "und jetzt sagen Sie mir doch mal / berichten Sie mir doch mal / beschreiben Sie mir doch mal / erklären Sie mir doch mal, . . ." Die Auswirkung der Änderung "erzählen/sagen" ist minim; es liegt offenbar ein höherer Inhalt vor "einen andern durch zusammenhängende Rede informieren", wobei der Inhalt der Information in einem Gliedsatz gegeben wird (" . . . , was Sie bei Ihrem Vater bemerkten"). Im Zusammenhang dieser Satzstruktur kann man also geradezu einen höhern Wortinhalt ansetzen (also etwa "sprachlich klar machen"), und dieser höhere Inhalt kann von den zwei Wortkörpern "erzählen" und "sagen" fast gleichwertig getragen werden. Bei Verwendung des Wortkörpers "berichten" erscheint ein etwas intensiverer Inhalt, beim Wortkörper "beschreiben" erscheint der Inhalt genauer begrenzt; beim Wortkörper "erklären" erscheint ein Inhalt, der eine erste Kenntnis schon voraussetzt und nun eine genauere, logisch auseinanderlegende Darstellung suggeriert.

Die Wortkörper "sagen" und "erzählen" erscheinen aber nur gerade in dieser Satzstruktur als "sozusagen auswechselbare Träger *eines* höheren Inhalts". In Satz 6e ist nur die Ersatzprobe möglich "von ner andern Sache zu *berichten*", aber nicht "zu *sagen*, zu *erklären*"; es müsste dann schon heißen "etwas von ner ganz andern Sache zu sagen", und dabei würde das sprachliche Mitteilen als weniger intensiv hingestellt. Ähnliches gilt für Satz 6h ("er erzählt viel von früher / berichtet viel von früher / sagt viel von früher").

Eine genaue inhaltsbezogene Analyse ergibt also, dass oft nicht eindeutig entschieden werden kann zwischen "zwei benachbarte, aber getrennte Inhalte" oder "ein höherer Inhalt mit zwei möglichen Trägern". Hier, bei der Entstehung höherer Inhalte, die sich auf die (bis zur Auswechselbarkeit gehende) Nachbarschaft von zwei

oder mehr einfacheren Inhalten stützt (die dann im Extremfall zu blossen verschiedenen Wortkörpern mit verschiedenen Trägerwerten reduziert werden) – hier dürfte ein besonders heikles, aber auch besonders fruchtbares Forschungsfeld für feinere inhaltsbezogene Sprachwissenschaft liegen, sowohl in synchronischer wie in darauf gestützter diachronischer Arbeit.

D. ERGEBNISSE

1. Die Wortinhalte dürfen nicht als scharf abgehobene und einförmig strukturierte (oder algebraisch genau strukturierte) Schicht betrachtet werden, sondern sie sind in sehr verschiedener Stufung und Ausprägung zu finden. Gültige Wortinhalte und blosser Trägerwerte sind nicht scharf und wesensmässig voneinander geschieden, sondern nur gradmässig. Der Weg zu den Inhalten aller Stufen führt nicht über Deduktion und auch nicht primär über die in den Wörterbüchern bereitgestellten Materialien, sondern über die Feinanalyse ausgewählter Texte, möglichst eines repräsentativen Korpus, wobei die Ergebnisse der Wörterbücher von Fall zu Fall als Hilfe und Gegenprobe herangezogen werden können.

2. Strukturalismus und inhaltsbezogene Sprachwissenschaft sind keine sich ausschliessenden Gegensätze, sondern zwei Stufen; in einer zureichenden Worttheorie muss jede von ihnen zu ihrem Recht kommen. Der Strukturalismus allein, ohne inhaltsbezogene Fortführung, führt nur zu ungedeuteten Strukturen und Wortkörpern, u.U. zu Strukturwerten und Trägerwerten. Die Inhalte als solche, vor allem die höheren Inhalte, die den Hauptreichtum und oft das Charakteristische einer Sprache ausmachen, erreicht er nicht. Die inhaltsbezogene Sprachwissenschaft ohne solide strukturalistische Grundlage schwebt in der Luft und ist vielfacher Fehldeutung durch undurchschaute aussersprachliche Voraussetzungen ausgeliefert. Nur wenn beide Arbeitsweisen sachgerecht verbunden werden, kann die Worttheorie und überhaupt die Sprachwissenschaft das Ziel erreichen, das ihr nach ihrem Wesen gesetzt ist.

INTRODUCTION TO THE DISCUSSION

My paper concerns the *theory of the word*, and as such it is but a part of a more complex subject, i.e. of a *theory of language as a whole*. Of course, 6 pages are very few for that purpose, and I apologize if these 6 pages – written in German as my mother-tongue – are not so easy to read for an English or French reader. For detailed discussion of the problem, I may refer to my book *Ansätze zu einer Sprachtheorie* (Düsseldorf, 1962, 93 pp.); it contains the first three chapters – the fundamental chapters, I think – of my theory of language. The paper to be discussed here should be considered as the fourth chapter.

The basic notion I have developed for dealing with language is what I call a *Sprachinhalt*, and especially a *Wortinhalt*. In English I could perhaps say “a semantic unit” or “the unit of the content side”.

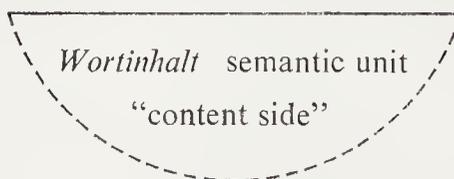


Fig. 1.

To represent one *Wortinhalt*, in order to keep it distinct from other *Inhalte*, there is a *sound form*, a unit of *expression*; I could say too “a lexical unit” in the terms of Mr. Coates. This unit I call a *Wortkörper*. The phonemes are but the *marks* used to distinguish one *Wortkörper* from an other.

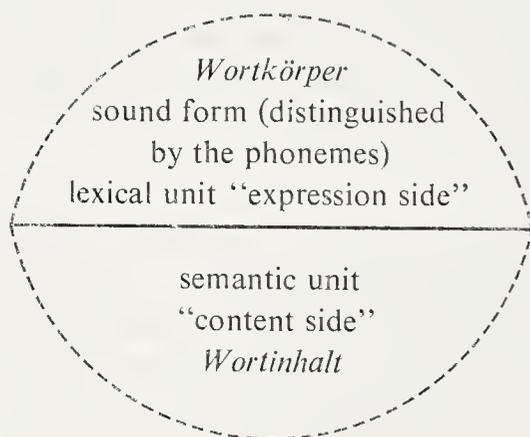


Fig. 2.

Now it seems to me very important to *distinguish* clearly the “*Wortinhalte*” and the *message* to be conveyed. This message is what I call “*Das Gemeinte*” (= “that which somebody *means*”).

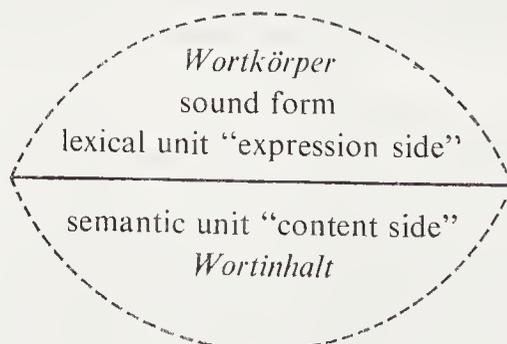


Fig. 3.

message (“thoughts and images” in the terms of Mr. Andreyev, also feelings, commands)
“*das Gemeinte*”

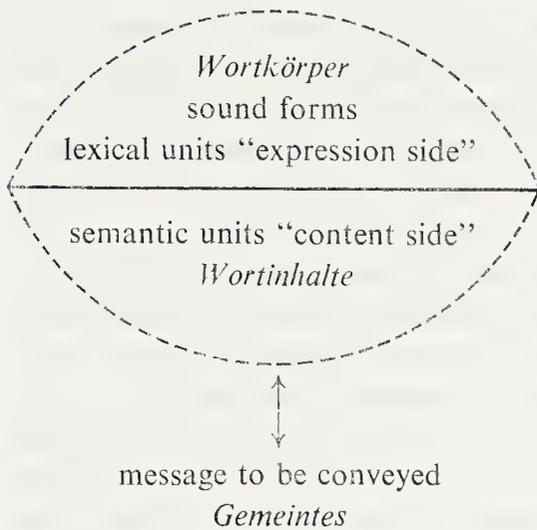


Fig. 4.

can be *heard* from everybody and can be identified with appropriate techniques

don't become conscious to the common man, must be recognized by the scholar

becomes conscious to every hearer (because he wants to *understand* the message) and becomes conscious to the scholar not speaking the language by use of informants

This notion "das Gemeinte" is very important for me, and I think here I make the decisive step beyond de Saussure. Now, we can describe the act of speech and the act of understanding as follows:

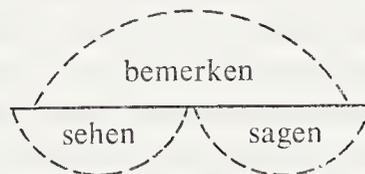
The older "mentalistic" methods assumed that the scholar could recognize these Inhalte by *introspection* only. This was, as we see today, to great extent a pious illusion ("eine fromme Selbsttäuschung"). The so-called *structural* methods – based on distribution and on transformational operations and tests – give as much more assurance and scientific control; but they first reach the *Wortkörper*, and when they reach *Wortinhalte*, they tell us only that this *must be* a separate Inhalt, but we don't know *what* Inhalt.

So we must *begin* as structuralists, but we must go *far beyond the limits* of pure structuralism in its present-day form; we must reach what we call in German, in the terms of Mr. Weisgerber, "inhaltsbezogen", i.e. trying to find the *true semantic units* in each given language at a given time – the true semantic units, to which the "simple lexical units" serve only as *vehicles*, as *carriers*.

Especially, we must develop *subtler methods*, methods *based* on structural principles, but *directed* to recognize the Inhalte. This is what I tried to do on pages 1056-1058.

Just an example: "bemerken" (in the sentence "was Sie bei Ihrem Vater *bemerkten*") is *one* Wortkörper, but it can serve as a carrier of *two* Wortinhalte:

er bemerkte = er *sah*
 er bemerkte = er *sagte*



From these *two* Inhalte it has to represent, the Wortkörper *as* Wortkörper has some "common semantic colour"; a learner can find that it is useful to have these two

Inhalte carried by one and the same Wortkörper. This "common semantic colour", this possible "Gedächtnis-Stütze" for the learner, or "Eignung des Trägers für bestimmte Inhalte" is what I call "*Trägerwert*" eines Wortkörpers. I could translate "the lowest common denominator" or "the common value of a lexical unit which is the carrier of several related semantic units". There is no time here to discuss this notion "*Trägerwert eines Wortkörpers*" in more detailed manner.

As a final remark, I would point out: it seems to me that the *levels* "Wortkörper – Wortinhalte – Gemeintes" are *not always clearly distinct*; really, a Wortkörper with its Trägerwert can be considered as "a frozen Wortinhalt", and a Wortinhalt is "a piece of Gemeintes which through frequent use has become a fixed unit".⁵

But now, I shall be glad to hear what you think about these notions and the methods developed on this basis.

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DISCUSSION

BUYSENS:

Dans la définition du sens du mot (Wortinhalt) comme "die kleinste semantische Einheit", l'emploi du mot "kleinste" ne me paraît pas justifié, et la suite de l'article le montre bien.

En effet, à la page 1054 je lis: "Wenn ein Wortkörper verschiedene, aber leicht aufeinander beziehbare Inhalte trägt, kann sich aus einem ihnen allen gemeinsamen Charakter ein gewisser Grundwert oder Trägerwert für den betreffenden Wortkörper bilden." Pour que divers sens aient un trait commun, il faut pouvoir les décomposer; et c'est ce que fait M. Glinz par exemple p. 1057 lorsqu'il commente le sens de "bemerkten": "Als Trägerwert (also als gemeinsame Inhaltstönung oder als kleinsten gemeinschaftlichen Nenner): geistig-sinnliches Handeln, zu dem es eine gewisse Aufmerksamkeit braucht." Ce que M. Glinz appelle "kleinste gemeinschaftliche Nenner" est nécessairement plus petit que chacun des sens comparés; le Wortinhalt n'est donc pas "die kleinste semantische Einheit". Peut-être serai-je plus clair si j'ajoute un exemple. Prenons le mot "oncle"; son sens est complexe: un oncle est le frère du père ou de la mère; le sens de "oncle" englobe donc ceux de "frère" et "père" ou "mère"; et les notions de "père" et de "frère" sont aussi des notions complexes. Je ne puis donc pas définir le Wortinhalt comme "die kleinste semantische Einheit" et je constate qu'en fait, sinon en théorie, M. Glinz est de mon avis.

Ma deuxième remarque concerne le restant de la première phrase de l'article: "Ein Wortinhalt ist eine geistige Grösse eigenen Rechts, die zwar von einem Wortkörper getragen wird, aber grundsätzlich von diesem Wortkörper abgelöst werden kann und daher systematisch von ihm zu unterscheiden ist." Et plus bas: "Das

⁵ Vgl. zu diesem ganzen Problem jetzt "Sprache und Welt", *Duden-Beiträge*, Heft 6 (Mannheim, 1962), S. 19-26.

Erlernen des Wortinhalts ist also ein Neubilden, ein eigener geistiger Akt, für den der Wortkörper nur als auslösendes Signal gelten kann, als Katalysator, nicht als materiale Ursache und gültige Determination."

J'admets parfaitement que notre connaissance du monde est indépendante du langage; j'ai plus d'une fois défendu cette thèse en m'appuyant entre autres sur la psychologie animale: les animaux ont des idées abstraites précises concernant le monde dans lequel ils vivent. Mais les idées qui forment notre connaissance ne s'identifient pas toujours avec le sens des mots et M. Glinz en fournit lui-même la preuve lorsqu'il parle de synonymes comme "sagen" et "bemerken" et lorsqu'il dégage ce qu'il appelle Trägerwert. Ce phénomène de Trägerwert est purement linguistique; M. Glinz n'en parle que parce qu'il existe la polysémie or les animaux ne connaissent pas la polysémie. Cette notion linguistique est donc causée, déterminée par la forme linguistique; M. Glinz n'a pas étudié le Wortinhalt indépendamment de la forme. C'est impossible.

En résumé, je constate un léger désaccord entre la théorie et la pratique de M. Glinz; sa théorie contient deux petites erreurs, mais sa pratique est excellente. C'est mieux que l'inverse.

ROSÉN:

Das von Professor Glinz gebrachte Beispiel des deutschen Wortes *denn* und die daran geknüpfte, auf "environments" gestützte Analyse ist besonders einleuchtend. Trotzdem knüpft sich hieran die Frage, ob und wie sich die Festlegung des "Gemeintem", des "Trägerwertes" und "Inhalts" in den Rahmen der *langue* und *parole* hineinbauen lässt oder ob sie unseren überkommenen Dichotomien der sprachwissenschaftlichen Methodologie zuwiderläuft.

HATTORI:

Glinz says:

"Der *Wortinhalt* kann nicht direkt übermittelt und nicht durch direkte Nachahmung erlernt werden, wie der (hörbare, lautlich nachahmbare) *Wortkörper*.

Erlernung eines neuen Wortinhalts heisst also *nachvollziehende Neubildung* des betr. Inhalts im lernenden Gesprächspartner oder überhaupt Handlungspartner. Solche Neubildung kann in einem einzelnen Akt (Übernahmeakt, Verstehensakt) erfolgen oder in einer Folge von Akten."

In my opinion, however, the *Wortkörper* which is associated, as Glinz states, with the *Wortinhalt* is not a mere string of sounds, i.e. vibrations of air molecules, but a *geistige Grösse* just like the *Wortinhalt*, and cannot be directly transmitted from a person to another. This point will be clearly understood when one sees how difficult it is to learn the sounds of words of a foreign language. One may imitate the sounds of a foreign word successfully once, but may fail next time unless one has grasped correctly their distinctive features. The foreign sound may sound like some of the sounds of one's native tongue at one time, but they may sound like different sounds the next

time. However, once one has succeeded in grasping the distinctive features after repeated imitations, one will be able to imitate the sounds freely and with constant success. This means that the *Wortkörper* have been established in one's mind, or, in other words, internalized.

In the same way, the *Wortinhalt* of a foreign word can be learned only by repeated imitations. One has to learn how to use the word, i.e. in which contexts and in which situations to use it.

Just as the *Wortkörper* of a foreign word can be scientifically studied by investigating their acoustical and articulatory manifestations, their oppositions with the acoustical and articulatory manifestations of other *Wortkörper* instances in the same language, and the responses of the native speakers including their verbal reports, so can the *Wortinhalt* of a foreign word be scientifically studied by investigating the common features of the things or events to which the word can refer, those of the situations in which it can be used, the various contexts in which it can occur, the oppositions in which it stands to the other words of the same language, and the responses of the native speakers, including their verbal reports.

I think that some linguists are wrong in stating that the sounds of a foreign language can be objectively observed, but the meaning cannot. Assuming this, they often forget the fact that their weapons for the "objective" observation are their ears which are biased to the sounds of their native tongue, and that they come to wrong conclusions for that reason.

I believe that not only the meaning but also the sounds of a foreign language are very difficult to observe objectively, but, nevertheless, both of them can be scientifically studied if we are careful enough.

LEBRUN:

Deux points dans l'exposé de Mr Glinz me paraissent critiquables :

1. Le *Wortinhalt* nous est présenté comme une unité sémantique minimale, mais l'auteur ne dit pas quels critères permettent la détermination de cette unité. En d'autres termes: qu'est-ce qui prouve que "sinnlich-geistiges Aufnehmen und Festhalten", qui nous est donné comme le *Wortinhalt* de *bemerken*, soit "eine kleinste semantische Einheit"?

2. L'auteur oppose *Wortinhalt* à *Bedeutung*, l'un étant "eine geistige Grösse ... von einem Wortkörper getragen", et l'autre un concept désigné par un mot. Quelle différence peut-il bien y avoir entre les deux? Et en quoi la lexicographie qui s'est attachée à la définition des *Bedeutungen* a-t-elle failli?

HORÁLEK:

Ich halte den ganzen Begriff der inhaltbezogenen Grammatik and sogar der inhaltbezogenen Sprachwissenschaft für sehr unklar und sogar widersprüchig. Es ist schwer zu sprechen z. B. von einer inhaltbezogenen Lautlehre, obschon in den poetischen Texten die formal-akustische Seite eng mit dem Inhalt verbunden sein kann. In der

Grammatik haben wir meiner Meinung nach nur mit den Bedeutungen und nicht mit den Inhalten zu tun. Ich empfehle das Fachwort Inhalt auf die Texteinheiten zu beschränken. Man kann auch nicht die Hypothese von dem Zusammenhang der grammatischen Struktur und Denkweise der betreffenden Nation annehmen, die grammatische Struktur ist auch in dieser Hinsicht unmotiviert.

SEMANTIC VALUE

WILLIAM HAAS

1. Statements about meaning may be graded. Firstly, at the lowest, a given form (some segment or feature of speech) may merely be said to *be significant*, to “have semantic value”; we then only say *that* it has meaning (never mind *what* meaning). Secondly, still without troubling to say what meaning it has, we may assert about a significant form (a “sign”) that it has *the same meaning, or not*, as some other form has, or that it has the same meaning, or not, on different occasions of its use. Finally, we may say about a sign, *what meaning* it has. Of the three kinds of statement, the first is clearly basic. The fundamental task of Semantics, then, appears to be to provide criteria of semantic value. Putting it in another way, we seek general criteria for “identifying” signs.

Our aim in searching for general criteria is of course *not* to “discover the signs”. We know from the start that *table* is a sign in *this table*, and not a sign in *a stable*, that *talk* has semantic value in *your talk* but not in *a beanstalk*. What we do *not* know, and are trying to discover, is *general characteristics* of such knowledge. As long as in every particular case an appeal is made to some specific intuition no systematic knowledge is forthcoming, and semantics remains, as Hjelmslev once complained, anecdotal. The outlines of a *semantic system* begin to appear, when many and various independent intuitions are replaced by one regular and uniform procedure of derivation. The rational coherence of our knowledge of meanings – the density of the encompassing system – will be proportionate to the paucity of presupposed and the wealth of derived knowledge. What we are looking for is not mere classification, not a “filing system” (such as is commonly found in traditional semantic studies), but necessary connexion, some statements about meaning being basic, others derived.

In an empirical science (as distinct from an abstract logical scheme), it is important to decide what to presuppose and what to derive. Generally, we presuppose what is more reliably known, easier to agree about. For example, if *some* kind of semantic information needs to be presupposed, and it can be either about whole sentences or about constituent signs, the former will certainly be preferable. In fact, this is on the whole prevailing practice, though not always acknowledged. Any attempt to do the reverse, to derive semantic information about sentences from a presupposed knowledge of their constituent morphemes, would fail to be objectively controllable.

Statements about the meaning of a form are verified by reference to its contexts, its "privileges of occurrence" – occurrence in sentences and situations. Investigation of the distribution of sentences in social situations belongs to non-linguistic disciplines; but the relations between sentences and their constituent signs are a subject of linguistic inquiry.

2. One necessary criterion of semantic value in a form (morph or word or phrase) is found in what has been variously described as "distinctive value" (Truketzkoy), "commutability" (Hjelmslev), or "distributional independence" (Harris). A form is said to be distinctive or commutable, if substitution of others for it within some utterance-frame (generally a "sentential function") correlates with an exchange of utterances of different meanings. The problem how to supplement this necessary but not sufficient criterion of semantic value has been tackled in ways that, however different, are interestingly convergent.

The least explicit but perhaps most boldly suggestive idea for supplementation is Hjelmslev's. He suggests simply that the distinctive value of a sign is *recurrent*.¹ It is possible to state Hjelmslev's condition in such a way as to make the identification of a constituent sign derivable from semantic contrasts between the sentences in which it occurs. We should then say that a constituent form is significant, if substitution of some other form for it entails the *same* semantic contrast in different sentential functions. (This is clearly not the case in an exchange of non-significant forms, e.g. of /e/ and /æ/ in *pet* : *pat*, *mess* : *mass*, *pen* : *pan*.)

Such a statement would require both elaboration and qualification. But there is one feature which this "discovery procedure" shares with any other. Every attempt to identify constituent signs, even a procedure apparently so different from Hjelmslev's as Zellig S. Harris's, seems to fasten upon the recurrence of "paradigms" (sets of mutually substitutable forms) in a variety of utterance-frames. Harris speaks of "distributional patterning", and the difference is that he relies on overall grammatical significance of paradigm and sentence-frames, rather than on a recurrence of sentential semantic contrasts.

3. The difficulty of Hjelmslev's criterion is that the kind of irreducible intuitive knowledge on which it relies is in excess of what can be safely presupposed. In the absence of further tests, can we be sure to agree that the semantic difference between "A good soup" and "A bad soup" is the same or not the same, as that between "A good book" and "A bad book"? And are we not in danger of being swamped by a great many dubious "signs"? (We might say, for instance, as Chomsky has pointed out, that there is "corresponding variation of meaning" in *flip-flop* and *drip-drop*, yielding the signs *fl-*, *dr-*, *-ip*, *-op*.²)

¹ *Prolegomena to a Theory of Language*, pp. 41 f. J. H. Greenberg's method of "squares", relying on an intuition of "corresponding variation of meaning", is essentially on the same lines (*Essays in Linguistics*, 1957, 20 ff.).

² Review of Greenberg's *Essays in Linguistics*. – *Word*, 15, 208 f.

The difficulty, on the other hand, with Harris's alternative criterion of grammatical significance is that we cannot know whether a paradigm and the set of its characteristic environments are grammatically significant, before we have completed the grammatical analysis of the language as a whole; more than that, we must have compared a number of alternative analyses, each based on a different set of tentatively "identified" morphemes. Thus, there would seem to be no *prima facie* reason, why, for instance, *hammer*, *badger*, *ledger*, *matter*, etc, should not be analysed each into two morphemes. Without comparing meanings in Hjelmslev's or Greenberg's way, precarious though it is, it would take time, and even then not be easy, to query a grammatical morpheme-class /hæm/, /bædz/, /ledʒ/, /mæt/, etc. and a special morpheme *-er*, in these cases. Another procedure, suggested by Harris for establishing morpheme-boundaries – his "successor-variety count"³ – would seem to permit the same kind of dubious analysis.

It is always possible, of course, simply to accept the less plausible analyses, and be content to say that there is no reliable correspondence between morphemes, on the one hand, and semantic intuitions, on the other. This has actually been suggested in some cases: e.g. no distinction as to morphemic status is made between the *re-* of *rewrite*, *relive*, *regroup*, etc. on the one hand, and the *re-* of *receive*, *refuse*, *reserve*, etc., on the other. However, an unexplained disparity between grammar and semantics does not seem to be satisfactory. In view of the obvious and very close agreement between the two in the overwhelming majority of cases, we ought to be able to account for both agreement and divergency.

If the techniques, so far, are not quite satisfactory for the purpose of identifying meaningful forms, we should yet acknowledge that they have already taught us something new and important about semantic as well as grammatical value. We have learned that for a form to "have meaning" it is not enough that it be *distinctive* (chosen) in *different* environments; it must be distinctive with regard to a *recurrent range* of choice and in a *recurrent type* of environment. We have also learnt (from Harris's second procedure) that a form, if it is significant, will *enter* an utterance at points of maximum range of choice. These seem to be necessary, though not sufficient, conditions of semantic value. It is reasonable to expect that in trying either to supplement or to replace them we may gain further insight into what it is to "have meaning".

4. There seems to be no doubt that for assigning meanings to forms an essential part of what we require is a more precise and objective technique for assessing "recurrent distinctive value".

In trying to lay the foundations of a systematic semantics, we shall have to presuppose *some* semantic information. This, preferably, will be about sentences, and only about sentences, never about mere citations culled from a dictionary. – In the following outline of a possible technique, it will be assumed at first that we know about any two sentences which are partially similar in form not only (i) whether they *differ in*

³ "From Phoneme to Morpheme", *Language*, 1955.

meaning, but also (ii) whether one is, in an obvious way, *less normal* than the other. We shall not regard these two kinds of presupposition as irreducible: though even if they were, neither would seem to strain our intuitive powers. ("My cat had kittens" is easily recognized as different in meaning from, and more normal than, "My mat had kittens" or "My dog had kittens".) In fact, (i) is reducible to (ii) – see below under (b); and (ii) might well be statable, partly at any rate, in terms of frequency of occurrence.

On the basis of such information about sentences, it seems to be possible to ascertain semantic values of constituent forms as follows:

(a) Some sentential occurrences of a distinctive form (say, *cat*) may be less normal than others. ("My cat had puppies" is less normal than "My cat had kittens".) In such a case, referring to the more normal occurrences, let us speak of the "focal" sentential selection of a form, and also of the correlated "focal" paradigm of it (i.e. the set of different forms which share some part of its focal selection). Both selection and paradigm may be referred to as the "focal sentential distribution" of the form.

It is one of the essential characteristics of a meaningful form (a "sign") that its sentential distribution has a focal organisation. (The sentential distribution of the /k/ which is substitutable by /m/ in "My -at had kittens" has no such organisation, but the distribution of *cat* has.) An ambiguous form has more than one focal distribution.

(b) Signs which differ in meaning differ with regard to the organisation of their sentential distribution, more especially, their focal distributions. (They need not differ with regard to "totals of environments"; and no illusory comparison of such totals is required.)

It follows that the semantic contrast between two sentences (sentential signs) is capable of exposition and confirmation in terms of their respective focal distributions. Thus, in the case of two partially similar sentences $A(x)$ and $B(x)$, expansions or substitutions or transformations which are normal for $A(x)$ may be less normal or even impossible for $B(x)$, and *vice versa*. ("Here's our cat", is expanded more normally by *Siamese, Tom, with her kittens*, etc. than "Here's our dog".)

A semantic contrast between two such sentences as $A(x)$ and $B(x)$ may then be said to *recur* between two other sentences containing the forms A and B respectively, $A(y)$ and $B(y)$, if some differences in focal distribution are the same in both cases – the non-recurring differences being accounted for by the substitution of (y) for (x). This will be the case, if A and B are signs. A significant form contracts recurrent semantic contrasts (between sentences) in different sentential functions. [It is clear – tho take a negative case – that of the semantic contrasts we might obtain by replacing *out* of *outlaw* ("A couple of outlaws/in-laws/by-laws"), none can be found, in this sense, to recur in any other sentential function which would admit the same formal contrasts ("A good outcome/income", "An appreciable output/input, etc.") *Out* in *outlaw* is no sign. Neither is *law*, here; nor, in the required sense, are *ham* in *hammer*, or *re* and *ceive* in *receive*, or *up* in "Give up!"]

(c) Thirdly, and lastly, it is an essential characteristic of a constituent sign, that the

complementary rest of a sentence containing it, should be capable of exhaustive analysis into other constituent signs, plus a possible residue of "grammatical markers". (An example of the latter is the verbal-phrase marker *to* of "wish to", "try to", "going to", etc. – as against the sign *to* of, say, "Come to me!")

This is the bare outline of a possible procedure for deriving semantic values. We need not expect that, in every case, the results of it will agree with our ordinary intuitions. Every systematic discipline must be allowed to some extent to redraw the map of knowledge, so long as the differences are explained. Again, as in every other descriptive study, we shall find border-line cases. There can be no objection to this so long as (i) such cases are comparatively few – much fewer than the determinate, and (ii) we can always show precisely, where the border lies.

A procedure of the kind here outlined would appear to be capable of providing an anchorage for semantics in the study of linguistic structure, and a basis which can support more than a mere assessment of "morphemic boundaries". Having established a general system of semantic links between sentences and constituent forms, we should find it possible to go on and deal, in a systematic manner, with all those more specific semantic problems – of idiom and free construction, synonymy, homonymy, translation, metaphor, neologism, semantic change –, which so far have received only anecdotal or classificatory treatment.

INTRODUCTION TO THE DISCUSSION

1. In attempting to "establish" the meaningful units of utterances, what are we trying to do? – In most cases, we know the units. We know, for example, that *pot* and *-s* have meaning in *pots*, and that neither has meaning in *spot*. We know this intuitively. Our aim cannot be to "discover" it again. As optics, in trying to establish the colours of the spectrum, is not concerned to discover the colours, so linguistics, in trying to establish the constituent signs of utterances, is not concerned to discover the signs. If we are not satisfied with our intuitive acquaintance with the facts, the reason is simply that such acquaintance is piecemeal, disconnected – a mass of mutually independent data, a new intuition in every case. What we are looking for is *general criteria* – of "having meaning", as, in optics, of "being a colour". The point of establishing the facts by an analytic procedure is that this yields *systematic knowledge*, to parallel our disconnected intuitions. It is only to a uniform procedure of establishing semantic values that the uniformities of a semantic system can become apparent.

In the present climate of linguistic discussion, it might be important to add a further disclaimer. Our procedure need not be mechanical. We shall be satisfied, if the proposed operations of analysis are capable of reliable performance by human beings. We need not aim at replacing the linguist by a machine.

2. In the first part of my paper, I have reviewed some previous attempts to answer our question – mainly those of Hjelmslev, Greenberg, and Harris. These attempts,

while they do not appear to me to have been entirely satisfactory, do yet seem to have succeeded in making some general statements about elements which have semantic value. Such elements, it seems, have *recurrent distinctive value*. They are not merely chosen for their positions in various utterances; they are chosen *from recurrent ranges of choice*.

The proposed procedures seem to be open to criticism on three points:

(a) The techniques for assessing recurrent distinctive value are to some extent impractical; either because they refer to "total distributions" (Harris) or because they rely on a kind of intuition which we cannot easily agree on (Hjelmslev, Greenberg).

(b) The proposed criteria do not seem to be stringent enough. While netting all we want, they also include much we do not want.

(c) Even if the analytic techniques and criteria were adequate for assessing semantic value, they would fail to provide a basis for further semantic inquiries: they would be no help in answering questions about synonymy, homonymy, polysemy, idioms, metaphors, translation, semantic change, etc.

3. The alternative or improved version of semantic analysis which I have ventured to offer does not seem to have these defects. In dealing with semantic questions concerning individual signs, I presuppose no knowledge of their individual meanings, but derive such knowledge from some minimal and, as it seems to me, reliable information about sentences – *the same kind* of information in every case.

What I find I have to presuppose as known about sentences is (i) that they have some meaning or other (never mind what meaning) and (ii) that there are some paired sentences such that one of the pair may reliably be judged to be less normal than the other (e.g. "My dog had kittens" to be less normal than "My cat had kittens"). The notion "more normal than" would require some further comment, but, on the whole, what is here intended can be explained sufficiently by exemplification.

"Recurrent distinctive value", i.e. the same semantic contrast recurring in different pairs of sentences, can then be expressed in terms of the "comparative normality" of paired sentences. To take a trivial example: We might ask, whether the semantic contrast between

1a *Here's our cat,* and

1b *Here's our dog*

is the same as the semantic contrast between

2a *Do you like that cat?* and

2b *Do you like that dog?*

If it is, then *cat* and *dog* "have meaning". We do not wish to answer such a question by referring to simple and incomparable intuitions – a new one in every case. We subject the paired sentences to certain test-operations. We may look for distinctive expansions, i.e. such as will be more normal for one sentence of the pair. E.g. expanding **1a** to *Here's our Tom cat* or *Here's our Siamese cat* is more normal than

expanding **1b**, to *Here's our Tom / Siamese dog*. On the other hand, *Here's our watch-dog* is more normal than *Here's our watch-cat*. Again, *Here's our cat on the roof/purring* is more normal than *Here's our dog on the roof/purring*; while *Here's our dog wagging his tail/barking* is more normal than *Here's our cat wagging his tail/barking*. We find that this assessment of the comparative normality of the proposed expansions holds equally for the pair **2a** and **2b**. We conclude that *cat* and *dog* have recurrent distinctive value. It appears that, in this way, we are able to state general criteria of "having meaning".

Out and *law* of *outlaw*, *give* and *up* of *give up* do not pass the test, nor do *re-* or *per-* of *receive*, *perceive*, *revert*, *pervert*. But *re* of *relive*, *rewrite* do pass.

We might note here that, for distinguishing established semantic values, nothing so difficult is required as comparing the "total distributions" of forms. One pair of sentences can be sufficient. If *help* is more normal as an exclamation than *assist* would be, this will establish the two as different in meaning. No definition or paraphrase is required for making the distinction (it would be difficult to paraphrase the two words differently), and no specific reference to extralingual facts.

Furthermore, by assembling different sentences containing the same meaningful form, and grading them according to their relative normality (see p. 1069), we shall arrive at a manageable and verifiable notion of "semantic field". We shall not look for it in physical space, nor in spiritual space ("Geistesraum"), but find it in easily statable relations among sentences. This, in turn, will enable us to derive further information about the semantic system of a language, without in any way adding to our presuppositions. However, I must not go too far in seeking support for the present argument by offering what may strike you as mere promise of its further development.

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THE STRUCTURAL ANALYSIS OF KINSHIP SEMANTICS*

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The set of KIN-TYPE designations – such as *father*, *father's brother*, *mother's brother*, *father's sister's son*, etc. – specifying the genealogical positions of one's known kin in relation to himself, can be regarded as constituting a semantic field. Linguistic usage, in any given community, groups these kin types into a smaller number of labeled KIN CLASSES, such as “father”, “uncle”, “cousin”, etc. The set of linguistic forms employed to designate such kin classes in a speech community constitutes its KINSHIP VOCABULARY. Any one of the forms is a KIN TERM. The classificatory structure imposed on this semantic field by conventional usage of kinship vocabulary varies greatly from society to society. We shall consider one single instance of such usage – that of the Seneca Indians, an Iroquois tribe of western New York State, as documented by Lewis Henry Morgan in the middle of the nineteenth century.

A kinship vocabulary can be regarded as constituting a paradigm. It can be subjected to a kind of analysis similar to that given other paradigmatic sets in a language. The Seneca data will be analysed in this manner. The application of the method yields results which are not common knowledge and which run counter to a classic but erroneous anthropological view concerning the nature of the “Iroquois type” of kinship system. Our interest in this paper, however, is not in correcting an anthropological error, but in illustrating a method of semantic analysis.

PRELIMINARY NOTIONS

PARADIGM. – We shall regard as a paradigm any set of linguistic forms wherein. (a) the meaning of every form has a feature in common with the meanings of all other forms of the set, and (b) the meaning of every form differs from that of every other form of the set by one or more additional features. The common feature will be said to be the ROOT MEANING of the paradigm. It defines the semantic field which the forms

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of the paradigm partition. The variable features define the SEMANTIC DIMENSIONS of the paradigm.

DIMENSION; FEATURE. A dimension of a paradigm is a set of mutually exclusive (i.e., non-cooccurrent) features which share some or all of the same privileges of combination (“bundling”) with features not of this dimension. – A feature is an ultimate term of characterization in a set of descriptive terms appropriate for the analysis of a particular given paradigm. A dimension is thus an “opposition”, and the features of a dimension are the terms of the opposition. Reduction to dichotomous oppositions is always possible, but is normally carried out only when a resulting increase in clarity and simplicity warrants it.

MEANING. – In §1 above, where we have written “meaning”, one may read “meaning and/or distribution” without departing from the sense intended. The term is meant to be interpreted broadly, covering both (a) objects and conditions of reference, and (b) restrictions and special privileges of context. In the instance of the kinship paradigm given below, however, we have only to deal with reference.

COMPONENTIAL DEFINITIONS. – A term belonging to a paradigm can be defined componentially in terms of its coordinates in the paradigm. The definition represents a bundle of features: one from each of several, or of all, of the dimensions of the paradigm. This bundle of features states the necessary and sufficient conditions which an object must satisfy if it is to be a DENOTATUM of the term so defined. Terms having single denotata are the exception; multiple denotation is more generally the case. The class of all possible denotata of a term constitutes its DESIGNATUM. The defining features of this class – i.e. the necessary & sufficient conditions for membership in it – are its SIGNIFICATUM.¹ The componential definition of a term is the expression of its significatum.

CONJUNCTIVE DEFINITIONS. – A componential definition represents a Boolean class product, and is thus a “unitary” or “conjunctive” definition. It is assumed that the meaning of any term belonging to a properly defined paradigm – one whose semantic field is itself unitary – will be susceptible to such a definition. This is perhaps a stronger item of faith than we have a right to hold at this moment; but it furnishes the motivation for the analysis of kinship systems at least. We proceed from extensional definitions (definitions by listing of denotata) to intensional definitions (definitions by specification of distinctive features). We feel that we have failed if we cannot achieve conjunctive definitions for every terminological class in the system. Were we to compromise on this point and admit disjunctive definitions (class sums, alternative criteria for membership) as on a par with conjunctive definitions (class products, uniform criteria for membership), there would be no motivation for analysis in the first place, for definitions of kin classes by the summing of discrete members – as in the table of Seneca data given below – are disjunctive definitions par excellence.

¹ The use of these terms derives from C. W. Morris, *Foundations of the Theory of Signs* (= *International Encyclopedia of Unified Science*, vol. 1, no. 2) (Chicago, University of Chicago Press, 1938), and *Signs, Language and Behavior* (New York, Prentice-Hall, 1946).

SENECA KINSHIP DATA

Following is a list of the Iroquois kinship terms, given in the language of the Seneca. Each term designates a class of one's kinsmen. The reference of each term is defined by naming all of the more closely related types of kinsmen, as well as a small sample of the more distant ones, to which the term is applied. We restrict our discussion here to consanguineal types.

ABBREVIATIONS. – Primary kin types are abbreviated as follows: F = *father*; M = *mother*; B = *brother*; S = *sister*; s = *son*; d = *daughter*.² Higher-order kin types are abbreviated with compound symbols, e.g.: Bd = *brother's daughter*; FSs = *father's sister's son*; MMBsd = *mother's mother's brother's son's daughter*; etc. Since we shall deal here only with the consanguineal system, we shall not need to employ the additional symbols H (= *husband*) and W (= *wife*) which are necessary in the writing of affinal and step types.

SEX OF PROPOSITUS. – All kin types listed after any given kinship term are assumed to be possible referents of that term in relation to a propositus *of either sex*, except when otherwise indicated. Such indication is either written out (unabbreviated), as in the list of data at the close of this section, or is indicated by the prefixed signs ♂ and ♀, as in some of the later discussion. Thus. ♂s = *a man's son*; ♀s = *a woman's son*; ♂Ss = *a man's sister's son*; etc.

TRANSLATION LABELS. – English labels are also given for the Seneca terms. In each case the label is the word which we would use in English, in our kinship usage, to refer to the pivotal member or members of the class – that one which is (or those which are) the most closely related to the propositus. It should be borne in mind that these English labels are *not* proper English translations, for they do not cover the same areas of denotation. English translations can be achieved only by descriptive circumlocution after the classificatory features defining the Iroquois kin classes have been discovered. The purpose of the English labels is merely to save the reader the task of learning an Iroquois vocabulary, and also to identify the pivotal member or members, i.e., the focus or foci, of each class. Translation labels will always appear in double quotation marks (e.g., “my father”) to mark them as Iroquois concepts and to distinguish them from the normal English meanings of the same words.

<i>hakso:t</i> , “my grandfather”	FF, MF; FFB, FMB, MFB, MMB, FFFBs, etc.; also FFF, MMF, etc.
<i>akso:t</i> , “my grandmother”	FM, MM; FFS, FMS, MFS, MMS; FFFBd, etc.; also FFM, MMM, etc.
<i>haʔnih</i> , “my father”	F; FB; FMSs, FFBs, FMBs, FFSs; FFFBss, etc.
<i>noʔyěh</i> , “my mother”	M; MS; MMSd, MFBd, MMBd, MFSd, MMMSdd, etc.

² As a mnemonic device one can remember that lower case means lower generation.

<i>hakhnoʔsēh</i> , “my uncle”	MB; MMSs, MFBS, MMBs, MFSs; MMMSds, etc.
<i>ake:hak</i> , “my aunt”	FS; FMSd, FFBd, FMBd, FFSd; FFFBsd, etc.
<i>hahtsiʔ</i> , “my elder brother”	B; MSs, FBs; MMSds, FFBss, MFBds, FMSss, MMBds, FFSss, MFSds, FMBss; MMMSddd, etc., <i>when older than Ego</i> .
<i>heʔkē:ʔ</i> , “my younger brother” . . .	Same, <i>when younger than Ego</i> .
<i>ahtsiʔ</i> , “my elder sister”	S; MSd, FBd; MMSdd, FFBsd, MFBdd, FMSsd, MMBdd, FFSsd, MFSdd, FMBsd; MMMSddd, etc., <i>when older than Ego</i> .
<i>kheʔkē:ʔ</i> , “my younger sister” . . .	Same, <i>when younger than Ego</i> .
<i>akyä:ʔse:ʔ</i> , “my cousin”	MBs, FSs; MMSss, FFBds, MFBss, FMSds, MMBss, FFSds, MFSss, FMBds; MMMSdss etc.; <i>and</i> MBd, FSd; MMSsd, FFBdd, MFBsd, FMSdd, MMBsd, FFSdd, MFSsd, FMBdd; MMMSdsd, etc.
<i>he:awak</i> , “my son”	s; Bs; MSss, FBss, MBss, FSss; MMSdss, etc., <i>of a man; but:</i> s; Ss; MSds, FBds, MBds, FSds; MMSdds, etc., <i>of a woman</i> .
<i>khe:awak</i> , “my daughter”	d; Bd; MSsd, FBsd, MBsd, FSsd; MMSdsd, etc., <i>of a man; but:</i> d; Sd; MSdd, FBdd, MBdd, FSdd; MMSddd, etc., <i>of a woman</i> .
<i>heyē:wō:tēʔ</i> , “my nephew”	Ss; MSds, FBds, MBds, FSds; MMSdds, etc., <i>of a man</i> .
<i>hehsōʔneh</i> , “my nephew”	Bs; MSss, FBss, MBss, FSss; MMSdss, etc., <i>of a woman</i> .
<i>kheyē:wō:tēʔ</i> , “my niece”	Sd; MSdd, FBdd, MBdd, FSdd; MMSddd, etc., <i>of a man</i> .
<i>khehsōʔneh</i> , “my niece”	Bd; MSsd, FBsd, MBsd, FSsd; MMSdsd, etc., <i>of a woman</i> .
<i>heya:teʔ</i> , “my grandson”	ss, ds; Bss, Bds, Sss, Sds; FBsss, etc.; <i>also</i> sss, dds, etc.
<i>kheya:teʔ</i> , “my granddaughter” . . .	sd, dd; Bsd, Bdd, Ssd, Sdd; FBssd, etc.; <i>also</i> ssd, ddd, etc. ³

³ The data are from Lewis Henry Morgan’s *Systems of Consanguinity and Affinity of the Human Family* (= *Smithsonian Contributions to Knowledge*, no. 218) (Washington, D. C., 1871). The items listed here are attested there either directly or by reciprocal, or both. The spelling of the kinship terms is after Wallace Chafe, *Handbook of the Seneca Language* (= *New York State Museum and Science Service, Bull. no. 388*) (Albany, 1963), with but minor modifications.

ANALYSIS

THE ROOT OF THE PARADIGM. – An individual related to a propositus in any of the ways specified by the various kin-type designations given under the kin terms of the above list is also *akyatēnōhk*, “my kinsman”, and can be referred to as such. This general term subsumes all of the special terms of the kinship vocabulary, each of which in turn subsumes all of the kin types listed after it. It thus defines the common feature of meaning required of a set of forms if they are to be regarded as constituting a paradigm. It represents, therefore, the root of the paradigm. This feature will be written as K (for *kinsman*) in the kin-class definitions below.

THE DIMENSION OF GENERATION. – Inspection of the data shows that one of the dimensions of the system is obviously GENERATION. This presents a set of five features, which represent obligatory categories in the system. These are: *second-or-higher ascending generation*; *first ascending generation*; *the generation of the propositus*; *first descending generation*; and *second-or-lower descending generation*. In the kin-class definitions below, these will be written as G^2 , G^1 , G^0 , G^{-1} , G^{-2} , respectively.

The categories of generation in Seneca, unlike those in our own system of kinship terminology, are overriding categories. Seneca kin-classes do not cross generation lines, whereas some of ours do (e.g., our classes *cousin*, *uncle*, *aunt*, etc.). Seneca kin classes, on the other hand, cross degrees of collaterality, whereas none of our English classes transgress the boundaries of the three DEGREES OF COLLATERALITY obligatorily distinguished in our system, viz.: the *zero degree* (i.e., lineal kin); *first degree* (*brother*, *sister*, *uncle*, *aunt*, *nephew*, *niece*); and *second-or-higher degree* (*cousin*).

THE DIMENSION OF SEX. – Another obvious dimension of the system is that of SEX. Its features are *male* and *female*. In the kin-class definitions below, these will be written as ♂ and ♀ respectively.

The features from the dimensions of generation and sex are sufficient to distinguish and define four of the kin classes of the list:

<i>hakso:t</i> , “grandfather”	♂· G^2 ·K.
<i>akso:t</i> , “grandmother”	♀· G^2 ·K.
<i>heya:teʔ</i> , “grandson”	♂· G^{-2} ·K.
<i>kheya:teʔ</i> , “granddaughter”	♀· G^{-2} ·K.

Note that, unlike the analogous terms in English, these four Seneca terms include known collateral kin of all degrees, as well as lineal kin. The componential definitions given here recognize this fact, inasmuch as they do not incorporate any features drawn from a dimension of collaterality distinctions, as definitions for our English terms must.

THE CLASSIFICATION IN THE FIRST ASCENDING GENERATION. – Four kin classes are distinguished in the first ascending generation: *haʔnih* (“father”), *noʔyēh* (“mother”), *hakhnoʔsēh* (“uncle”), and *ake:hak* (“aunt”). Assuming that we may be dealing with two dimensions of dichotomizing features, we may try pairing the terms.

Given any four terms, there are three possible ways of pairing them. In the present case, we might first pair "father" with "uncle" and oppose these to the remaining pair consisting of "mother" and "aunt". Inspection of the data will show that, if we do this, the opposition is in the dimension of sex. (It should be noted that we *must* inspect the data to determine this; we cannot simply assume it as natural, or infer it from the translation labels. Anthropological literature furnishes many examples of systems that have both females and males in the "mother" class, and both males and females in the "father" class.)

Inspection of the data also suggests another plausible pairing: that of "father" with "aunt", as opposed to "mother" and "uncle". In this case the dimension can be characterized as *SIDE*, and the opposed features which constitute it as *patrilateral* and *matrilateral*. These we may write as π and μ , resp. As can be seen from the table, the types of kinsmen who are called by the "father" term, *haʔnih*, in Seneca are male first-ascending-generation kinsmen related to the propositus on his father's side (patrilateral), while those called "uncle", *hakhnoʔsēh*, are all on his mother's side (matrilateral). Similarly, all of those who are "aunt", *ake:hak*, are female kin of that generation on the father's side, while those that are "mother", *noʔyēh*, are on the mother's side. These features from the dimension of side, together with those from the dimension of sex, suffice to differentiate the kin classes of the first ascending generation. The definitions are as follows:

<i>haʔnih</i> , "father"	. . .	$\delta \cdot \pi \cdot G^1 \cdot K$.
<i>ake:hak</i> , "aunt."	. . .	$\text{♀} \cdot \pi \cdot G^1 \cdot K$.
<i>hakhnoʔsēh</i> , "uncle"	. . .	$\delta \cdot \mu \cdot G^1 \cdot K$.
<i>noʔyēh</i> , "mother"	. . .	$\text{♀} \cdot \mu \cdot G^1 \cdot K$.

There is however a third possible way of pairing these terms, which, because it may appeal to us as a more or less reasonable and natural kind of pairing, at least should not be overlooked. This is to pair "father" with "mother", and to oppose them to the pair consisting of "uncle" and "aunt". Study of the data, with the aim of discovering a feature shared by all of the members of both the "father" and the "mother" classes, and some other opposed feature common to the members of the "uncle" and "aunt" classes, shows that, from the standpoint of the data, this is a less obvious manner of pairing and will require a more contrived set of features. It is possible, nonetheless, to define such features. And this fact suffices to show that this is indeed a natural pairing; for arbitrary and unnatural pairings never allow the discovery of common features.

Accordingly we may define a feature, $L^=$, which will be said to inhere in any kin type in which *the sex of the designated kin is the SAME as that of the first link*; and an opposed feature, L^{\neq} , which will be said to inhere in any kin type in which *the sex of the designated kin is OPPOSITE to that of the first link*. Now, the second of these features is common to all of the members of both the "uncle" and the "aunt" classes, while the first is common to all of the members of both the "father" and "mother" classes.

Thus, in the types MB, MMSs, MFBs, MMBs, MFSs, etc. ("uncles"), and in the types FS, FMSd, FFBd, FMBd, FFSd, etc. ("aunts"), it is true that the sex of the designated kinsman or kinswoman (given by the last term in any kin-type abbreviation) is in every case opposite to that of the first link to the propositus (given by the first term in the abbreviation). *The sexes of intervening links, when present, are irrelevant to the reckoning.*⁴ Similarly, in the types F, FB, FMSs, FFBs, FMBs, FFSs, etc. ("fathers"), and in the types M, MS, MMSd, MFBd, MMBd, MFSd, etc. ("mothers"), the sex of the designated kinsman or kinswoman is in every case the same as that of the first link. [Note: F and M are the limiting cases, where "designated kin" (last

⁴ The italicization, for emphasis, is to call this point to the attention of anthropologists. The "classic but erroneous anthropological view concerning the nature of the 'Iroquois type' of kinship systems", to which reference was made at the beginning of this paper, is that this kind of system classifies kin by membership in unilineal descent groups. Thus, given exogamous matrilineal moieties (as the Seneca are said to have had), all of one's "fathers" should be found in one's father's moiety, and all of one's "maternal uncles" should be found in one's own moiety. A glance at the data shows that this theory of the Iroquois system gives about fifty percent right predictions and nearly fifty percent wrong predictions. Of the "maternal uncles", for example, MB, MMSs, and MFBs would indeed be found in one's own exogamous matrilineal moiety, but MMBs and MFSs would be in the opposite moiety. Similarly one has classificatory "fathers" in *both* moieties, his own as well as his father's. — Another version of the theory, thought to be applicable where matrilineal clans (sibs) exist but moieties do not, or where kinship is reckoned to clan limits but not to moiety limits, is that the "father" term refers to men of one's father's clan in his generation or age grade, and that the "maternal uncle" term refers to men of one's own clan in the generation or age grade of one's mother's brother. The facts, however, correspond as little to the predictions of this clan theory of Iroquois kinship as they do to those of the moiety theory; for one may have "fathers" in any clan, and one may also have "maternal uncles" in any clan. The predictions of these theories are as far off for the other kin-classes of the system as they are for these. — These facts are true not only of the Iroquois Indians themselves, but also of every "Iroquois-type" system included in Morgan's tables in *Systems of Consanguinity and Affinity*. While I was becoming acquainted with this for the first time in 1954–55 by reading these tables, my colleague Leopold Pospisil was finding out the same thing for the Kapauku Papuans while engaged in field work in the highlands of the (then) Netherlands New Guinea. (Cf. L. Pospisil, "The Kapauku Papuans and their Kinship Organization", *Oceania* 30 Sidney, 1960, 188–205.) My astonishment at discovering the real principle operative in the reckoning of bifurcation in an Iroquois-type kinship system was matched by his. It was contrary to all of the expectations to which we had been led by the anthropological theoretical writings on the subject. It is surprising that the essential data pertinent to a subject about which so much has been written should have been in print and available to all for nearly a century without anyone's having taken account of the classification of any but the closest collateral kin-types. The classic theory predicts correctly only to the immediate (closest) uncles and aunts [FB, MB, FS, MS] and first cousins. Beyond this its predictions are half right and half wrong. Morgan himself, already under the influence of a clan theory of kinship (of his own making), is partly responsible for this error. Statements in Chapter IV, Book I, of his *League of the Ho-de-no-sau-nee or Iroquois* (New York, 1851) can be derived from a metaphoric use of "sibling" terms and from the ignoring of all but the closest kin-types included in his own tables of data that he published in *Systems*.

There *do* exist systems which classify kin-types in the way that the Iroquois type was imagined to. These are the "Dravidian" type of systems. Interestingly, they are *not* generally founded on clan or moiety reckoning, but on a mode of reckoning of bifurcation that, unlike the Iroquois, takes account of the sexes of all intervening links. The Dravidian and Iroquois types are rarely distinguished in anthropological literature, all passing under the label 'Iroquois type'. Actually, they are systems premised on very different principles of reckoning, and deriving from social structures that are fundamentally unlike.

term) and ‘first link’ (first term) coincide. In the case of coincidence, the condition of equality of sex can of course be said to be satisfied.]

With three possible pairings of the four G^1 kinship terms, we are now in possession of one more dimension than is necessary for uniquely characterizing them. The features from any two of these dimensions might be chosen as defining features, and those of the third dimension regarded as “redundant”. (“Redundant” in a logical sense, not merely in an empirical sense, since the features of any one of the dimensions can be defined in terms of those of the other two.) It may be objected that in the case of the third pair of features we are attempting to impose on the Seneca system a pairing which is natural and reasonable from our point of view, as members of our society, but which, because of its contrived nature, may be inappropriate to the Seneca system. We can leave the judgment on that point until later. For the time being, we recognize that there are three possible alternative definitions for each of the first-ascending-generation kin classes:

<i>haʔnih</i> , “father”	$\delta \cdot L = G^1 \cdot K$,	or	$\delta \cdot \pi \cdot G^1 \cdot K$,	or	$\pi \cdot L = G^1 \cdot K$.
<i>noʔyēh</i> , “mother”	$\text{♀} \cdot L = G^1 \cdot K$,	or	$\text{♀} \cdot \mu \cdot G^1 \cdot K$,	or	$\mu \cdot L = G^1 \cdot K$.
<i>hakhnoʔsēh</i> , “uncle”	$\delta \cdot L \neq G^1 \cdot K$,	or	$\delta \cdot \mu \cdot G^1 \cdot K$,	or	$\mu \cdot L \neq G^1 \cdot K$.
<i>ake:hak</i> , “aunt”	$\text{♀} \cdot L \neq G^1 \cdot K$,	or	$\text{♀} \cdot \pi \cdot G^1 \cdot K$,	or	$\pi \cdot L \neq G^1 \cdot K$.

THE CLASSIFICATION IN THE FIRST DESCENDING GENERATION. – Six kin terms are given in the list for kin types of the first descending generation, but only four of them are available to any given *propositus*.

First let us consider the four terms for the kin of a male. As before, we seek all possible ways of pairing the terms. We may begin with the sex pairing, of “son” (*he:awak*) with “nephew” (*heyē:wō:tēʔ*), as opposed to “daughter” (*khe:awak*) and “niece” (*kheyē:wō:tēʔ*).

A second possible pairing is that of “son” with “daughter”, these being opposed to “nephew” and “niece”. This pairing, as can be seen from the list, opposes a class consisting of the children of a male *propositus* and of all of his male generation-mates,⁵ to a second class consisting of the children of his female generation-mates.⁶ The features of this opposition we may symbolize with the letters σ and σ (suggested by the partially – though not completely – descriptive terms “fratrilial” and “sororifilial”).

There should be a third manner of pairing these four kin terms. This can only be to set “son” with “niece”, and to oppose these to “daughter” and “nephew”. While this might not appeal to us (or to the Iroquois either) as a natural pairing, it is nonetheless possible to define a feature which would unite the “son” and the “niece” classes (*viz.*, sameness of sex of designated kin and last link), and an opposing feature

⁵ I.e., the children of his brother and of *all* of his male cousins, regardless of whether the latter be classificatory “brothers” to him (e.g., MSs, FBs) or “cousins” to him (e.g., MBs, FSs).

⁶ I.e., the children of his sister and of *all* of his female cousins, regardless of whether the latter be classificatory “sisters” to him (e.g., MSd, FBd) or “cousins” to him (e.g., MBd, FSd).

which would unite the “daughter” and “nephew” classes (viz., oppositeness of sex of designated kin and last link). We might symbolize these features as $P=$ and $P\neq$, respectively (suggested by: *kinsman's PARENT of SAME sex as kinsman*, and *kinsman's PARENT of OPPOSITE sex to kinsman*).

Thus, one may write three alternative definitions of each of the four G^{-1} kin classes for a *male propositus*:

<i>he:awak</i> , “son”	$\delta \cdot \emptyset \cdot G^{-1} \cdot K$,	or $\delta \cdot P= \cdot G^{-1} \cdot K$,	or $\emptyset \cdot P= \cdot G^{-1} \cdot K$.
<i>khe:awak</i> , “daughter”	$\text{♀} \cdot \emptyset \cdot G^{-1} \cdot K$,	or $\text{♀} \cdot P\neq \cdot G^{-1} \cdot K$,	or $\emptyset \cdot P\neq \cdot G^{-1} \cdot K$.
<i>heyɔ̃:wō:tēɔ̃</i> , “nephew”	$\delta \cdot \sigma \cdot G^{-1} \cdot K$,	or $\delta \cdot P\neq \cdot G^{-1} \cdot K$,	or $\sigma \cdot P\neq \cdot G^{-1} \cdot K$.
<i>kheyē:wō:tēɔ̃</i> , “niece”	$\text{♀} \cdot \sigma \cdot G^{-1} \cdot K$,	or $\text{♀} \cdot P= \cdot G^{-1} \cdot K$,	or $\sigma \cdot P= \cdot G^{-1} \cdot K$.

If we consider now the classification of kin in relation to a *female propositus*, we find we can write the following definitions:

<i>he:awak</i> , “son”	$\delta \cdot \sigma \cdot G^{-1} \cdot K$,	or $\delta \cdot P\neq \cdot G^{-1} \cdot K$,	or $\sigma \cdot P\neq \cdot G^{-1} \cdot K$.
<i>khe:awak</i> , “daughter”	$\text{♀} \cdot \sigma \cdot G^{-1} \cdot K$,	or $\text{♀} \cdot P= \cdot G^{-1} \cdot K$,	or $\sigma \cdot P= \cdot G^{-1} \cdot K$.
<i>hehsōɔ̃neh</i> , “nephew”	$\delta \cdot \emptyset \cdot G^{-1} \cdot K$,	or $\delta \cdot P= \cdot G^{-1} \cdot K$,	or $\emptyset \cdot P= \cdot G^{-1} \cdot K$.
<i>khehsōɔ̃neh</i> , “niece”	$\text{♀} \cdot \emptyset \cdot G^{-1} \cdot K$,	or $\text{♀} \cdot P\neq \cdot G^{-1} \cdot K$,	or $\emptyset \cdot P\neq \cdot G^{-1} \cdot K$.

It will be seen that none of these definitions are invariant to the sex of the propositus. In fact, the definitions of “nephew” (*hehsōɔ̃neh*) of a female are identical to those of “son” (*he:awak*) of a male, while those of “son” (*he:awak*) of a female are identical to those of “nephew” (*heyē:wō:tēɔ̃*) of a male. Preferable, surely, would be definitions invariant to the sex of the propositus – at least where the same linguistic forms are involved (*he:awak*, *khe:awak*).

These can be obtained by employing a pair of features which are the reciprocals of those used to obtain the pairing of “mother” with “father”, and “uncle” with “aunt” in G^1 , as follows. Let us define a feature $L=$, which will be said to inhere in any kin type in which *the sex of the last link is the SAME as that of the propositus*; and an opposed feature, $L\neq$, which will be said to inhere in any kin type in which *the sex of the last link is OPPOSITE to that of the propositus*. The first of these features is common to all the members of both the “son” (*he:awak*) and the “daughter” (*khe:awak*) classes, regardless of whether this be in relation to a male or to a female. Inspection of the data will verify that this is so. E.g., δBs , $\text{♀} Ss$, $\delta MBss$, $\text{♀} MBds$, etc. [Note: s (i.e., δs and $\text{♀} s$) and d (i.e., δd and $\text{♀} d$) are the limiting cases, where last link (second-last term) coincides with the propositus. In such a case it can of course be said that their sexes are the same.]

We may redefine the G^{-1} terms as follows:

<i>he:awak</i> , “son”	$\delta \cdot L= \cdot G^{-1} \cdot K$.
<i>khe:awak</i> , “daughter”	$\text{♀} \cdot L= \cdot G^{-1} \cdot K$.
<i>heyē:wō:tēɔ̃</i> / <i>hehsōɔ̃neh</i> , “nephew”	$\delta \cdot L\neq \cdot G^{-1} \cdot K$.
<i>kheyē:wō:tēɔ̃</i> / <i>khehsōɔ̃neh</i> , “niece”	$\text{♀} \cdot L\neq \cdot G^{-1} \cdot K$.

Now, instead of having *he:awak* a being a pair of homonymous words, it is just one word with one signification; and similarly with the *khe:awak*. And we get a bonus out of it besides: the two “nephew” terms end up being synonyms of a sort, differing only by an additional component specifying the sex of the propositus; and similarly the two “niece” terms. [This is a useful bonus, especially in a related language such as Tuscarora, which lacks the extra synonym and has but one term (Tusc. *kheyēhwaṛ-nēṛ*) undifferentiated for either sex of propositus or sex of kin, for the meaning $L^{\neq} \cdot G^{-1} \cdot K$.]

THE CLASSIFICATION IN THE ‘ZERO’ GENERATION. – Five kin terms are employed in G° . Four of them, the “sibling” terms *hahtsiṛ*, *heṛkē:ṛ*, *ahtsiṛ*, *kheṛkē:ṛ*, form a readily analysable set based on the differentiations of RELATIVE AGE and SEX. The fifth term, *akyä:ṛse:ṛ*, “cousin”, has a range of denotation comparable in magnitude to that of the four “sibling” terms conjointly. Within this range, no distinctions are made either for sex or for relative age.

We wish now to find out the dimension of difference that opposes the combined “sibling” class (the sum of the four special “sibling” classes) to the “cousin” class. Study of the data reveals one, and only one, possibility. Let us accordingly define a feature, $\Lambda^=$, which will be said to inhere in any kin type in which *the sex of the last link is the SAME as that of the first link*; and an opposed feature, Λ^{\neq} , which will be said to inhere in any kin type in which *the sex of the last link is OPPOSITE to that of the first link*. The second of these features is common to all of the members of the “cousin” class (e.g., MBs, FSs, FFSds, FMSds, etc.), while the first is common to all of the members of the four “sibling” classes (e.g., MSs, FBs, FFSss, FMSss, etc.). [Note: B and S are the limiting cases, where the last link and the first link coincide – a fact that is not obvious simply from the writings B and S but that can be readily seen when it is remembered that B is Fs and/or Ms, and that S is Fd and/or Md. (There are empirical reasons why B and S are admitted as “primary” kin types in kinship reckoning, and why the ambiguity inherent in them can be tolerated.)]

Now we may write the definitions for the G° terms. (A^+ and A^- are for the features of RELATIVE AGE.)

<i>hahtsiṛ</i> , “elder brother”	$A^+ \cdot \delta \cdot \Lambda^= \cdot G^{\circ} \cdot K$.
<i>heṛkē:ṛ</i> , “younger brother”	$A^- \cdot \delta \cdot \Lambda^= \cdot G^{\circ} \cdot K$.
<i>ahtsiṛ</i> , “elder sister”	$A^+ \cdot \text{♀} \cdot \Lambda^= \cdot G^{\circ} \cdot K$.
<i>kheṛkē:ṛ</i> , “younger sister”	$A^- \cdot \text{♀} \cdot \Lambda^= \cdot G^{\circ} \cdot K$.
<i>akyä:ṛse:ṛ</i> , “cousin”	$\Lambda^{\neq} \cdot G^{\circ} \cdot K$.

THE DIMENSION OF BIFURCATION. – Reviewing the definitions given in preceding paragraphs for kin classes in G^1 , G^{-1} , and G° , it is seen (a) that the features $L^=$ and L^{\neq} occur only in the context G^1 ; (b) that the features $L_=$ and L_{\neq} occur only in the context G^{-1} , and (c) that the features $\Lambda^=$ and Λ^{\neq} occur only in the context G° . They are thus in complementary distribution. This may suggest that they may be but conditioned variants of one basic pair of features; and that they may, if the similarity

condition can be met, be grouped into a single pair of units in the metalanguage which we use to spell out the semantic content of the Seneca kin terms. They may thus be reduced to one opposition of wider applicability in the system, instead of three oppositions of more limited applicability.

The condition of similarity can indeed be met (all three contrasts involve comparisons in the generation just above the lowest represented by *propositus* and/or *kin*), and we may take $L^=$, $L_=$, and $\Lambda^=$, as defined previously, to be conditioned variants of one basic feature. Similarly, L^\neq , L_\neq , and Λ^\neq can be taken as conditioned variants of the opposed feature. These features we can call by the traditional names of *parallel* and *cross*, respectively, although the real meanings of these terms in their application to Iroquois-type kinship systems have been rather poorly understood in the past. And the dimension which these features constitute can similarly be called by the traditional name of BIFURCATION. The symbols \parallel and \times will be used to represent the features in the writing of definitions.

THE STRUCTURE OF THE FIELD. – The definitions of the kin classes in the middle three generations may now be rewritten; and the entire paradigm may be presented, so as to show the structure of its semantic field, in a four-dimensional diagram, a four-column matrix, or a four-margin outline.

The field dealt with up to this point has been that of the consanguineal kin-types. The step-kin and in-law types, and the terms that classify them, can be dealt with in a similar fashion. Step and in-law categories are obligatorily distinguished from the consanguineal ones, as well as from each other, and their classification is peculiar to the Iroquois system.

Also not dealt with yet are the many forms designating the superclasses which are obtained by neutralizing the oppositions of sex, relative age, and generation “direction” – i.e., *ascending* vs *descending*. These neutralizations are accomplished by grammatical devices provided in the Iroquoian inflectional and derivational systems. The existence of these does not invalidate the claim to “obligatoriness” that was made for the distinctions drawn in the sections above, for the neutralizing forms are cover-terms that are appropriate only to rather particular contexts, having a status in usage (though not in grammar) somewhat comparable to our cover-terms “parent”, “child”, “parent-and-child”, “sibling”, and such artificial ones like “grandkin” – as anthropologists occasionally employ.

These various aspects of the Iroquois kinship system cannot be treated here. To do so would expand this article to a length inappropriate to the present occasion. One matter of some general interest deserves comment however. It will be noted that, of the four dimensions employed in the analysis of the consanguineal system, three of them – sex, bifurcation, and relative age – were dimensions representing a *dichotomous opposition* of just two features; but one of them – generation – was a dimension whose variable could assume five values. Two questions may be raised. One of these is whether the five-valued dimension is reducible in fact to a larger number of dimensions of dichotomous oppositions. The other is whether, in dichotomous oppositions,

one member of the opposition can be said to be the marked member (a positive feature), and the other the unmarked member (the absence, or negation, of the positive).

In regard to the first question it may be remarked that, since kinship terms come in reciprocal sets, it is always possible to analyse out the polarity between the reciprocals as a separate dimension of opposition. Thus, in place of the five-valued dimension of GENERATION ($G^2, G^1, G^0, G^{-1}, G^{-2}$), we may have a three-valued dimension of GENERATION DISTANCE (consisting of the absolute values G^2, G^1, G^0) and a dimension of POLARITY (*senior* vs. *junior*) or GENERATION DIRECTION (*plus* vs. *minus*). This is especially appropriate in Iroquoian where, for example, a set such as that consisting of the two “parent” terms, together with their reciprocal “child” terms, is covered by a single cover-term that neutralizes the generation direction (or polarity), as well as the sex, of the basic terms. Thus:

$$\{[ha^2nih + no^2y\tilde{e}h] + [he:awak + khe:awak]\} = \{akyatathawak\},$$

i.e., $\{[(\mathcal{J}\cdot\|\cdot G^1\cdot K) + (\mathcal{Q}\cdot\|\cdot G^1\cdot K)] + [(\mathcal{J}\cdot\|\cdot G^{-1}\cdot K) + (\mathcal{Q}\cdot\|\cdot G^{-1}\cdot K)]\} = \{\|\cdot G^{\pm 1}\cdot K\}$; and similarly with the other reciprocal sets of the system.

This new dimension can be equated with that already set up for RELATIVE AGE, for the polarity relation between the “parent” and “child” terms is similar to that between the “elder sibling” and “younger sibling” terms. Thus also:

$$\{[hahtsi^2 + ahtsi^2] + [he^2k\tilde{e}:^2 + khe^2k\tilde{e}:^2]\} = \{akyatate^2k\tilde{e}:^2\},$$

i.e., $\{[(\mathcal{J}\cdot\|\cdot A^+\cdot G^0\cdot K) + (\mathcal{Q}\cdot\|\cdot A^+\cdot G^0\cdot K)] + [(\mathcal{J}\cdot\|\cdot A^-\cdot G^0\cdot K) + (\mathcal{Q}\cdot\|\cdot A^-\cdot G^0\cdot K)]\} = \{\|\cdot G^0\cdot K\}$.

Thus the analytic simplification of the dimension of generation can be accomplished at no cost to economy in the total number of dimensions.

As for the possibility of reducing the remaining three-valued dimension of GENERATION DISTANCE still further, I know of no *good* natural basis for doing this; though it can, of course, always be done by fiat. One might cut it in either of two places: between G^2 and all else, or between G^0 and all else. More-or-less plausible arguments might be adduced for either of these, but it can be done only at the expense of adding a dimension to the system. This *is* an ‘expense’, for it would take two dimensions of dichotomous opposition to account for only three values.

As for the second of the questions posed above, viz. whether a distinction can be made between a ‘marked’ and an ‘unmarked’ member of every opposition, it may be stated that there are good reasons – primarily semantic-structural, but with strong linguistic as well as social correlates – for regarding the *first* term of each of the following oppositions as the *marked* member:

POLARITY: *senior*, vs. *junior*

SEX: *male*, vs. *female*

BIFURCATION: *cross*, vs. *parallel*.

The fourth dimension of the system, GENERATION DISTANCE, remains a three-valued one unless reduced by fiat. Just as I have not yet found any good basis, linguistic or social, for dividing this into two taxonomic dichotomies, so I am also without any basis for determining which features might best be regarded as marked and which as unmarked if this were done.

To justify the above choices of "marked" members (*senior, male, cross*) would require an extended treatment of the transitive pronominal prefix system, the gender system, and the stem-derivational system of Iroquois grammar, together with a "Whorfian" exegesis of the same, and an additional discussion of the typology of so-called 'Iroquois-type' kinship systems. It must suffice here to say that in each case the marked member is a "special" one in some sense, that is opposed to a "general" or "common" one. In positions or contexts of contrast, the unmarked member is specific. In positions or contexts of no contrast, it is general.⁷ Thus, the "common gender" of Iroquois is the feminine (not the masculine as in English); the general root for the parent-child relation is *-hawak*; and the extension of bifurcation into G⁰ (making the system "Iroquois type" as opposed to "Cheyenne type") in fact rests rather lightly on the Iroquois.

INTRODUCTION TO THE DISCUSSION

This paper is presented as an example of the structural analysis of a lexical set which covers and partitions a semantic field. It was noted that this particular kind of lexical set can be regarded as constituting a paradigm, and that it can be subjected to a kind of analysis similar to that given other paradigmatic sets in a language. Certain common linguistic notions basic to this treatment were also defined, or briefly discussed, with special reference to their use in semantic analysis. These included the notions of semantic field, paradigm, root, dimension, feature, componential definition, the route from extensional to intensional definitions, the possibility of dichotomous dimensions of contrast, and the identification of the marked feature of an opposition. Also, something of the reason for the desideratum of conjunctive definitions was indicated.

A rather frequent response of linguists to such kinship exercises, I have found, is that they are of limited interest so far as the general problems of semantic analysis are concerned because, it is said, kinship vocabularies and their meanings are something special in lexicology, permitting as they do, the specification and analysis of reference

⁷ "... a marked category states the presence of a certain (whether positive or negative) property A; ... the corresponding unmarked category states nothing about the presence of A, and is used chiefly, but not exclusively, to indicate the absence of A. On the level of general meaning the opposition ... may be interpreted as 'statement of A' vs. 'no statement of A', whereas on the level of 'narrowed', nuclear meanings, we encounter the opposition 'statement of A' vs. 'statement of non-A'." — Roman Jakobson, *Shifters, Verbal Categories, and the Russian Verb* (Harvard University: Russian Language Project, 1957), p. 5.

with a satisfactory degree of rigor; but it is felt that they are, for this very reason, unrepresentative of linguistic-semantic, or lexicological, problems in general. I would not care to make any exaggerated claims for the particular methods that are of utility in the analysis of systems of kinship terminology, though I do think that their potentialities are rather generally underestimated. In any case, I would like to comment on a few further points of general relevance that arise out of the exercise presented in the preprint paper.

The first of these is the question of whether there are other content fields represented in language that are susceptible to this kind of analysis. On this point I will only say that anthropologists have applied this or similar sorts of analysis to the vocabularies representing a number of lexical and cultural domains of special interest to them. Among them are color vocabularies, native ethnobotanical terminologies, vocabularies of disease taxonomy in primitive societies, those of primitive cosmologies, systems of religious concepts, etc. The work is still new, and much needs to be done yet in the development of the method. A review of this work and a bibliography of some relevant items are contained in a recent paper by Harold Conklin ("Lexicographical Treatment of Folk Taxonomies", in *Problems in Lexicography*, ed. by F. W. Householder and S. Saporta [=Indiana Univ. Res. Center in Anthropol., Folkl., and Ling., Publ. 21], 1962).

The second point has to do with the formal characteristics of the structure of semantic fields. There is something a bit special about the structure of kinship systems, viz., that their structure is in large part that of the "paradigm". While there are numerous sets of this sort in lexicon, this is by no means the general case. More typical, perhaps, is the "taxonomy". In the perfect paradigm, the features of any dimension combine with *all* of those of any other dimension. In the perfect taxonomy on the other hand, they never do; they combine with *only one* feature from any other dimension. In the perfect paradigm there is no hierarchical ordering of dimensions that is not arbitrary; all orders are possible. In the perfect taxonomy there is but one

F							
a ₁				a ₂			
b ₁		b ₂		b ₁		b ₂	
c ₁	c ₂						

Fig. 1.

F							
a ₁				a ₂			
b ₁		b ₂		c ₁		c ₂	
d ₁	d ₂	e ₁	e ₂	f ₁	f ₂	g ₁	g ₂

Fig. 2.

possible hierarchy. To illustrate the difference we may consider a set of eight elements constituting a field *F*. If these represent a paradigm, it takes but three dimensions of dichotomous opposition to fully characterize them (Fig. 1). If they represent a taxonomy, it takes seven (Fig. 2). Kinship terminologies usually represent something intermediate between these, the imperfect or asymmetrical paradigm, which combines

principles of both kinds. In the analysis of content fields other than kinship, one must be prepared to find both kinds of structures. Anthropological work on folk taxonomies reckons with both.

A third point has to do with the question of metaphor, the delimitation of a semantic field, and the possibility of conjunctive definitions. I should confess at once that I have not included *all* of the meanings of the Iroquois kinship terms in the tabulation of data given in the paper. Not included, for example, are *the moon* in the list of denotata of the "grandmother" term, or *the thunderers* amongst the "grandfathers", or *the earth* as our "mother", or *the sun* as our "elder brother". Nor have I included the metaphoric uses of the "brother" and "cousin", "father" and "son", "elder brother" and "younger brother" terms, in ceremonial discourse, for divisions of the Longhouse and of the political confederacy of the Six Nations; or that of the "uncle" term for the Bigheads (certain masked dancers at Midwinter ceremonies) or, formerly, for prisoners at the stake. There is no difficulty here in identifying these as "marginal" or "transferred" meanings, to use Bloomfield's terms. (Cf. Prof. Jakobson's comments after Dr. Strang's paper on Tuesday.) Metaphoric extensions can be expected for any lexical item. In the structural analysis of a semantic field, however, they are excluded. We have not intended to deal with all of the meanings of the Iroquois kinship terms here, but only with those that fall within the field defined as *genealogical kin*. All of these have one common feature of meaning which is lacking from the metaphoric extensions. Determining the criteria for the delimitation of fields is the first important step in semantic analysis. Determining the bases for metaphoric extensions beyond the field is one of the last, and sometimes one of the most interesting. Normally it is not possible to subsume *all* of the meanings of a lexical item under one *conjunctive* definition. We expect that it should be possible to do this, however, for all of those meanings of an item *that lie within a properly defined field*.

A fourth point has to do with the way of entry into a problem of meaning. Bloomfield was of the opinion that "signals can be analyzed, but not the things signalled about," and that "this reinforces the principle that linguistic study must always start from the phonetic form and not from meaning" (*Language*, p. 162). The entry into phonology for Bloomfield was the same-or-different test applied to the meanings (*Language*, Chapter 5, esp. pp. 74-78). I have suggested elsewhere that the entry into *semantics* could be a same-or-different test *applied to forms*, and that this also offered a possible starting place ("A Semantic Analysis of the Pawnee Kinship Usage", *Lang.*, 32, 1956, 158-194, esp. pp. 190-192). C. M. Ebeling has an important and interesting discussion of this possibility, and of the symmetry, or parallelism, between the analytic constructs of semantics and those of phonology (*Linguistic Units*, The Hague, 1960, Chapter III). And a comment by Jakobson, that "meaning can and must be stated in terms of linguistic discriminations and identifications, just as, on the other hand, linguistic discriminations are always made with regard to their semantic value," is fundamental to this view ("Boas' View of Grammatical Meaning," *Memoir* 89, Amer. Anthrop. Assn., 1959, pp. 139-145; quote from p. 143). For purposes of

the analysis of the Iroquois kinship semantics that was made in the paper submitted to the Congress, it was not really necessary for us to know anything about the Seneca forms other than whether any two responses of Morgan's informants were *the same* or *different*. Even Morgan, poor phonetician that he was, was able to tell that, and he has given us the necessary information. I have used spellings based on Chafe's analysis of Seneca phonology only because Morgan's clumsy and inaccurate ones offend me, not because more precise information about the forms is necessary. It suffices only know that they are different. They could as well be called alpha, beta, gamma, etc., or assigned random numbers.

A fifth and final point has to do with the analysis of multiple denotation *within a field*. It should be noted that dimensional analysis of a field, and componential definitions of the elements that constitute it, are as applicable to sets of forms having only single denotata as to sets of forms having multiple denotata. Only in the latter case – of which the Seneca kinship vocabulary has furnished an example – does one face the typical “*allo-unit*” problem. I want to point out that there are two ways of handling this, at least in kinship analysis. One is by the method of *total class definitions*; the other is by a method of *basic member definitions* and supplementary *rules of extension*. Much of linguistic method as we have known it in the recent past is based on the former method. The first attempts at componential analysis of kinship terminologies,¹ as well as the present paper, take an analogous approach in the handling of multiple denotata. Differences of degree within the class of denotata of a term are of course recognized, but these are treated as “nondistinctive”.

There is, as I have mentioned, another way of handling this. It is to regard one, or sometimes two, members of a terminological kinclass as the basic members and to fit the definition of the kinship term to these. The other members of the class are then treated as extensions (“metaphoric” in relation to the narrow field covered by the basic types and their definitions, but yet not “metaphoric” when considered in relation to the wide field which is the subject of analysis). These extensions are then accounted for by rules. The rules may be written either as expansion rules or as reduction rules. In the former case they derive distant members of the class from the basic member or members; in the latter they reduce the distant ones to the basic ones.

To illustrate this method let me take an example of a type of system somewhat more complicated than that of the Iroquois, for it will allow perhaps a more convincing demonstration of the potentialities of the method. There are kinship systems called “Crow” type (after the Crow Indians, whose system was one of the first of these to receive notice) which are found in many parts of the world. Actually, the Crow type is not one, but many. I shall speak of one particular subvariety which we might as well call the Choctaw subtype. I shall not give you another whole kinship system, but will mention only the classification of two particular kin-types (cousins to us),

¹ Ward H. Goodenough, *Property, Kin., and Community on Truk* (= *Yale Univ. Publ. in Anthropol.*, no. 46) (1951); “Componential Analysis and the Study of Meaning”, *Lang.*, 32: (1956), 195–216; F. G. Lounsbury, “A Semantic Analysis of the Pawnee Kinship Usage”, *Lang.*, 32: (1956), 158–194.

viz., *father's sister's son* [FSs] which goes by the "father" term in these systems, and *father's sister's daughter* [FSd] which goes by the "grandmother" term. These two can usually be taken as quick diagnostics of this particular variety of kinship system.

This system can be generated by a set of three rules, which I shall write here as reduction rules. These account not only for FSs and FSd, but for the whole system. They are:

(1) Skewing Rule: *Let any woman's brother, as linking relative, be regarded as equivalent to that woman's son, as linking relative.*

$$\text{♀B...} \rightarrow \text{♀s...}$$

From this follows a corollary stating the consequent relationship of the reciprocals: *Any male linking relative's sister will then be equivalent to that male linking relative's mother.*

$$\text{... ♂S} \rightarrow \text{... ♂M}$$

(2) Merging Rule: *Let any person's sibling of same sex, as linking relative, be equivalent to that person himself directly linked.*

$$\text{♂B...} \rightarrow \text{♂...}; \text{♀S...} \rightarrow \text{♀...}$$

From this follows the corollary pertaining to the reciprocals: *Any linking relative's sibling of same sex as himself (or herself) will then be equivalent to that relative himself (or herself) as an object of reference.*

$$\text{... ♂B} \rightarrow \text{... ♂}; \text{... ♀S} \rightarrow \text{... ♀}$$

(3) Half-sibling Rule: *Let any child of one of one's parents be regarded as one's sibling.*

$$\text{Fs} \rightarrow \text{B}; \text{Fd} \rightarrow \text{S}; \text{Ms} \rightarrow \text{B}; \text{Md} \rightarrow \text{S}$$

This rule contains its own reciprocal corollary.

Of these three rules, the third one is – so far as I know – universal in kinship systems; the second is widespread, applying to many systems besides the one now under consideration, but is by no means universal; while the first of these is the one of most restricted occurrence, being peculiar to this particular subvariety of so-called "Crow" systems, but being found in quite a number of unrelated systems in many parts of the world nonetheless.

The rules constitute an unordered set. When we scan the rules for applicability in reducing a kin-type, if any is applicable, there is never more than one that is applicable at any particular step in the reduction. And if we write them as expansion rules rather than as reduction rules (which can be done by merely reversing the arrows), all possible orders of application of the rules must be exploited in generating a system. Since the rules cannot come into conflict, there is no basis for ordering.

We may apply them now to the kin-types *father's sister's son* [FSs] and *father's sister's daughter* [FSd].

FSs	→ FMs	(by skewing rule corollary),
	→ FB	(by half-sibling rule),
	→ F	(by merging rule),
	→ "father"	(by definition).
FSd	→ FMd	(by skewing rule corollary),
	→ FS	(by half-sibling rule)
	→ FM	(by skewing rule corollary),
	→ "grandmother"	(by definition).

Now you can see by what logic one's father's sister's son may be called "father", and one's father's sister's daughter be called "grandmother".²

I have mentioned this alternative method not just to exhibit a bit of the variety in systems of kinship semantics, or the methodological resources of their devotees, but to raise also a more general point concerning the possible nature of relationships *between* the various denotata of a form. I am not prepared at this point to show that there are other semantic fields where a few generative rules may account for all instances of multiple denotation for all of the forms of an entire lexical set; but I think it might be suggested that the derivation of denotatum from denotatum, and the formulation of the principles involved, is a rather general problem in structural semantics.

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DISCUSSION

GALTON:

Where exactly does the contribution of structural linguistics to the treatment of kinship terminology lie, as against Delbrück's work (*Die indogerm. Verwandtschaftsnamen*, 1890) or my own ("The Indo-European Kinship Terminology", *Zeitschrift für Ethnologie*, Braunschweig, 1957)? We are indeed dealing here with an interrelated structure, but the sole reason is to be sought in referential reality, in which the degrees of kinship form a closely knit and coherent pattern.

Just one example of a semantic shift, not due to any structural pressure within the terminological system, but purely to a relationship in the underlying reality. In some Slavic and other languages, the original term for "son-in-law" is also applied to the "brother-in-law", because the man who was son-in-law to the patriarch, the head of the clan, the grandfather, was also brother-in-law to the son of the patriarch who (the son) therefore transferred the term to denote his own relationship to the entrant into the clan. The semantic shift within the system of the kinship terminology goes back to an external factor, and it seems to me that the arguments Lounsbury adduces against my point of view are all extralinguistic.

² A full presentation of the method of reduction rules is given in my paper "A Formal Account of the Crow and Omaha-type Kinship Terminologies", in *Explorations in Cultural Anthropology: Essays Presented to George Peter Murdock*, ed. by W. H. Goodenough (New York, McGraw-Hill, in press).

LOUNSBURY:

Professor Galton has raised a crucial question, and one that I think many social anthropologists would also want to raise, viz., what does a "linguistic" or "structural-semantic" approach contribute to the treatment of kinship terms that is not adequately taken care of by a direct "social-structural" approach to their explanation? The answer that I would give to this question is that a prior formal analysis of the semantic structure will shift, and I believe simplify, the problem of relating the phenomena of linguistic usage to the social and cultural realities that lie behind them. It will do this by revealing a few underlying *principles of classification* whose effects are far-reaching. Both componential definitions and rules of extension describe such "principles of classification".

The usage of the "son-in-law" term in Slavic, that Professor Galton has referred to, finds a parallel in Latin. I believe that the extension of this term to include a man's sister's husband is an old trait of at least some of the branches of the Indo-European family, and that it is only one of many details that were the automatic consequences of one deep-lying structural principle. The example is an especially appropriate one in the context of the present discussion, because the principle involved is the precise mirror image of the "Choctaw" type of skewing rule that I have just mentioned and illustrated. If you reverse the sex of every term in that rule and its corollary, you will have formulas that account not only for the inclusion of a man's sister's husband in the son-in-law class (L. *gener*), but also for the inclusion of a man's sister's children in the grandchild classes (L. *nepos* and *neptis*), the characterization of one's mother's brother by an essentially grandparental term (L. *avunculus*, diminutive of *avus*, the diminutive distinguishing collateral members from lineal members of the original class), etc. In this skewing rule, which generates one of the so-called "Omaha" types of kinship systems, a man's sister as linking relative is made equivalent to his daughter as linking relative, and by the corollary, a female linking relative's brother becomes equivalent to her father. A *raison d'être* for all this is not too difficult to discern in the structure of the early Roman gens, the filial position of women in relation to adult male members of their natal gens, their attainment of an adult status only in their husband's gens, etc. When these early conditions changed however, and the laws pertaining to them, then the nature of the skewing rule and the principles of kin classification also changed. The "Omaha" type of asymmetrical skewing of terminological generation gave way to a bilaterally symmetrical form of skewing such as persists in Italian and Roumanian to the present day. In French, Spanish, and Portuguese this in turn also gave way, and was replaced by principles that emphasize a pervasive contrast between lineal and collateral relatives. Analogs to these developments can be shown for Germanic as well. I am not prepared to say anything specific about the Slavic case, but I am told that there is some evidence that at least the early stage is similar to what I have posited for Latin and Germanic. With structural analysis, it becomes possible not only to trace the details of semantic change of particular words through time, but

also to trace structural mutations, i.e., changes in underlying principles of classification that affect a whole system.

FISCHER:

It is interesting to note that Dr. Lounsbury has presented two forms or types of structural analysis of kinship semantics. The preprinted analysis of the Seneca Iroquois system might be termed a "taxonomic" analysis, while the analysis of the Crow system presented here orally, which uses a limited set of "focal" or "core" kin-types and provides a few simple rules of transformation for extending these to more remote kin, might be termed a "generative" analysis, with intentional reference here to Chomsky's paper, presented elsewhere at the Congress, "The Logical Basis of Linguistic Theory." The taxonomic type of analysis (Seneca Iroquois example) has been more popular among anthropologists recently, but in this type of analysis the definitions of terms often apply most elegantly to the least important kin, i.e., the more remote kin on the edge of the system. When applied to focal relatives, the taxonomic analysis may imply somewhat forced statements, such as, "When a Seneca calls his father *ha²nih*, he does so, for one thing, because the fathers' sex is the same as that of the relative who is the first link in the kin chain from Ego (propositus, to use Lounsbury's useful innovation) to father, because in this special case the first linking relative is the same person as the distal end of the chain, so one accordingly compares the father with the father himself as to sex, and finds happily that his sex from both these viewpoints is the same." This example suggests that a completely taxonomic type of analysis can seem at times intuitively dissatisfying even when it can be shown to be logically applicable. I therefore welcome Dr. Lounsbury's statement that he too personally favors use of the currently unfashionable but long-established idea of the extension of the meaning of kinship terms from a focal kin type. This idea is explicitly recognized in his Crow system analysis, and could as easily be applied to the Iroquois system, he tells us. I believe that the transformational, extensionist, or generative (all these terms are intended to be synonymous here) approach which Dr. Lounsbury has demonstrated is both more elegant logically and more accurately representative of indigenous thought processes than the taxonomic approach, even though the latter is logically adequate for handling the data in this case. But I would further suggest that a transformational or generative approach will prove a necessity for handling other more complex and less clearly delimited types of semantic data than kinship terminology.

GRIMES:

The delimitation of a field cannot actually be considered definite until the semantic distinctions by which the field is organized have been recognized. The boundaries of the field are then the area within which these semantic distinctions apply. Initial "roughing out" of a field does not, therefore, constitute the definition of that field in semantic terms; in the process of analysis items may be brought into or excluded from the field.

HATTORI:

Lounsbury has come to a beautiful conclusion without using contextual tests, which are Haas' main weapon. Do we have here two kinds of structural semantics? I do not think so. For me, these are just two different approaches, and, for that matter, not the only two. In some cases of semantic research, we have to investigate the relation between the speaker and the hearer(s). In many cases of research in structural semantics, we have to combine these two or even three, in order to obtain better results.

SENTENCE SEQUENCE IN DISCOURSE

JOHN NEWTON WINBURNE

This paper will consider discourse – a set of continuous sentences – and describe certain of its structural patterns as these evidence themselves in English exposition: how sentences attach themselves to their predecessors and successors.

The research data thus far collected indicate four major types of discourse: a) exposition (discourse containing no poetry, dialog, or song); b) poetry (discourse containing phonological patterns, or metrical patterns, and/or refrain patterns); c) dialog (discourse consisting of conversation); and d) song (discourse set to music). Other discourse – narration, fiction, opera, description, argumentation, persuasion, etc. – is one or a combination of two or more of the four major types and may be analyzed according to each of the types composing it. The basis of discourse structure is verbal, phonological, grammatical, or musical repetition.

This paper will concern itself with exposition and one aspect of its structure: sentence attachment. Exposition, discourse containing no poetry, dialog, or song, will be analyzed using Lincoln's *Gettysburg Address*, complete discourse in ten sentences of about 270 words.

In sentence one appear:

fourscore	our	nation	brought	dedicated
and seven			forth	
years ago			conceived	

Sentence two contains:

now	we	nation	conceived	dedicated
		nation		

Sentence three:

	we			battlefield
--	----	--	--	-------------

Sentence four:

	we	nation	dedicate	field
				here

Etc.

Table I illustrates semantic repetitions from sentence to sentence in the *Gettysburg Address*.

Table I, however, leaves several questions unanswered: 1. On what basis are the words placed in categories? 2. What structural patterns are evidenced in the table?

3. Is this table of categories valid for other discourse? 4. How does discourse progress from the first sentence to the last one in discourse?

1. SENSEMES OF DISCOURSE: MEANING CATEGORIES. Each sentence of exposition contains words appearing: a) in the immediately preceding sentence; and b) in the discourse prior to the immediately preceding sentence. These repeated words: a) may be identical; b) may vary in stem ending (*dedicate, dedicated*); c) may be synonyms (*battlefield, field*); or d) may be semantic substitutes though not necessarily grammatical substitutes (*battlefield, here*).

In the *Gettysburg Address* [we], [our], and [us] constitute one class of meanings distinct from and in contrast to each of the other classes of meaning in that particular discourse. Similarly, [dedicate], [hallow], [say], [do this], constitute another discrete class of meanings, as do [conceived], [brought forth], and [birth]; and [endure], [live], [living], [died], [dead], [perish], [last full measure of devotion].

Discourse, then, contains classes of meaning each distinct from, and in contrast to, other classes of meaning in the same discourse. Such a discrete class of meanings is designated a *senseme*, and each member of the class is an *allosens*.

Further analysis of Table I reveals two types of sensemes: a) one whose allosenses appear regularly throughout the discourse; and b) one whose allosenses appear irregularly. The distinction between regularity and irregularity of recurrence is made by the comparison: a) of the total number of occurrences of allosenses by senseme; and b) by the total number of sentences in which the allosenses of each senseme appear.

TABLE II
Senseme Pattern

Sent. No.	Sensemes A			Sensemes B											
	w	x	y	a	b	c	d	e	f	g	h	j	k	l	m
1.	w	x		a	2b	c	d	2e							
2.	w	x		a	b	2c			f	g					
3.	w		y						f		h				
4.	w	x	2y			c				2g	h	2j			
5.	w	x													
6.	3w	3x	y												
7.	w	3x	2y						f	2g		j			
8.	w	x	2y						f			j	2k	3l	
9.	w	x	2y	a					2f	g		j			
10.	4w	x	2y		b	2c	3d	e	2f	5g		j			2m
Total	15	13	12	3	4	6	4	3	8	11	2	6	2	3	2
Factor	150	117	84	9	12	24	8	6	48	55	4	30	2	3	2

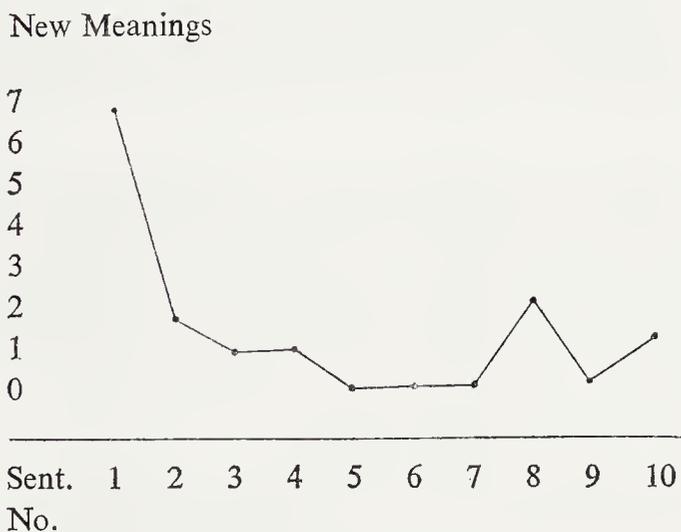
Table II makes this distinction between regularity and irregularity by coding the allosenses and showing the number of occurrences of each allosens in each sentence.

Thus, Table II shows that Senseme *X* contains 15 allosenses which appear in 10 sentences, giving a factor of 150 whereas Senseme *a* has a factor of 9. Table II further reveals a marked difference in the factors: /W/, /X/, /Y/, having larger factors than /a/, /b/, /c/, /d/, etc. /W/, /X/, /Y/ are one type of sensemes and /a/, /b/, /c/, /d/, etc. are another. The senseme whose allosenses appear most often and with regularity according to the factor are Senseme *A*; and those allosenses recurring irregularly and less often are Senseme *B*.

2. STRUCTURAL PATTERNS OF SENSEMES. Sensemes *A* of the *Gettysburg Address* (Table I) when each is arbitrarily assigned a word (it must be understood that a class of meanings can no more be represented by one word than a phoneme can be represented by one allophone) may be transcribed: [We dedicate a battlefield], a summary out of context for the *Gettysburg Address*. Sensemes *A* of any exposition appear to be the principal meanings of that discourse. And it may be deduced that Sensemes *A* produce the effect in discourse commonly called unity or that they provide cohesion for discourse.

Sensemes *B* of the *Gettysburg Address* appear also to provide unity for discourse. But Sensemes *B* are introduced into discourse throughout, not in the first sentences as are Sensemes *A*. Sensemes *B* being introduced as the discourse progresses also appear to cause it to progress; that is, the introduction of sensemes after discourse is initiated causes the discourse to advance.

TABLE III
Wave Curve – Introduction of New Meanings



INTRODUCTION OF SENSEMES. Sensemes are introduced into discourse, as shown in Table III, in a wave curve with harmonics. Or in the first sentence of the *Gettysburg Address* seven sensemes are introduced; in sentence 2, two; in 3, two; in 4, one; in 5, 6, 7, zero each; in 8, two; in 9, zero; and in 10, one. Thus, a harmonic, or large wave, of introductions of new sensemes appears in the first sentences of discourse followed by lesser waves of introductions.

TABLE IV
Attachment

Sent. No.	Sentence Attachments	Discourse Attachments	Total Attachments
	Total		
2.	w-x-a-b-2c 6		6
3.	w-f 2	y 1	3
4.	w-2y-h 4	x-2g-c 4	8
5.	w-x 2		2
6.	3w-3x 6	y 1	7
7.	w-3x-2y 6	f-2g-j 4	8
8.	w-x-2y-f-j 6		6
9.	w-x-2y-2f-j 7	a-g 2	9
10.	4w-x-2y-2f-5g-j 15	b-2c-3d-e 7	22
Total	54	19	71

SENTENCE ATTACHMENT. Each sentence is attached to its predecessor and successor by common sensemes. The pattern of attachment (Table IV) is the appearance of at least one Senseme A and one or more Sensemes A and/or B which have occurred in the immediately preceding sentence. Each sentence must have in common with its predecessor at least one senseme which recurs regularly throughout the discourse and most often one or more sensemes which recur irregularly. In the *Gettysburg Address* (Table IV) the number of sentence attachments varies from 2 to 15 with a median and average of 5. For example, sentence 3 is attached to 2 by [we], [war], but 9 is attached to 10 by [we], [birth], [nation], [dedicated], [freedom], [task], [dead], [here], [they]. In most English exposition, however, the median of sentence attachments per sentence is two.

A sentence is attached to discourse in yet another fashion: a sentence has one or more sensemes in common with discourse prior to the immediately preceding sentence. In the *Gettysburg Address* the number of discourse attachments per sentence ranges from zero to 7 – the average being 2 and the median 1. For example, the discourse attachments in sentence 4 are /nation/ and /gave lives/, neither occurring in sentence 3 but both occurring in 1 or 2 or both. Most English exposition, however, has a median of 1 discourse attachment per sentence.

Total attachment, both sentence and discourse, in the *Gettysburg Address* ranges from 2 to 22 with a median of 6.5 and an average of 7.8. Most English exposition, on the other hand, has a median of 3 attachments per sentence.

Sentence attachment is only one part of the structure of expository discourse. But the principle of repetition or recurrence of substitutes, whether grammatical, phonological, metrical, or musical, is illustrated in the patterns of sentence sequence.

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DISCUSSION

QUIRK:

While agreeing with Mr. Gleason that "the sentence" as a unit poses thorny problems in general for the investigation of cohesion and progression, we must acknowledge that Mr. Winburne did not allow his results to be undermined by these difficulties since he restricted himself to a consideration of a printed English text. In such material, no problem need arise about the delimitation of a basic sentence unit: it is orthographically defined. Mr. Winburne's attempt to identify still higher units is very interesting, but more could be done in plotting and categorising the overt grammatical sequence items before letting oneself get involved in rather slippery judgments of 'semantic substitutes' and the like. The use of pronouns, articles, demonstratives and conjunctive elements should be regarded as the grammatical anatomy of unit sequence, whether the linguistic material is neatly set out in printed sentences or forms a stream of spoken discourse.

BI-POLARITY IN ARABIC KINSHIP TERMS

MILLICENT R. AYOUB

The concept of polarity is one of some nine basic principles which anthropologists use to classify systems of kinship terminology. It has been defined as that "criterion, the linguistic recognition of which produces two terms for each kin relationship, one by which each participant can denote the other".¹ In the father/son relationship, for example, the father calls the son "Son" and is called "Father" in return. If, however, the same term should be applied to *both* poles of the kin relationship then we would recognize it in two directions, that is, the same term would denote both the speaker and the one spoken to. Again, in the father/son relationship, a term of this sort (hereinafter called a *bi*-polar term) is one which serves for both parties, the father calling his son "Father" and the son calling him so in return, "Father". The present paper will discuss this concept of bi-polarity as it operates within a sub-system of Arabic terminology. It will attempt to give, first, a formal statement of the content and occurrence of these bi-polar terms; and, second, to offer an interpretation of the sociological significance of the phenomenon.

The particular usages I shall be concerned with come from the speech of a Druze Arab village in Lebanon; however it may safely be claimed that similar usages are current among Arabic speakers in the populations of what is sometimes known as Greater Syria, that is, the present national states of Lebanon, Jordan, Israel and Syria. The particular village which supplies the terminology has a social organization which I will briefly characterize as having patrilineal descent, patri-lineage segmentation, preferred kin group endogamy and usual residence among the paternal kin. (I recognize, of course, that this cursory characterization hardly does justice to the finer points of the actual village, but space does not allow for the fuller presentation it deserves.)

The vocabulary of Arabic kinship needs some explication now so that the place within it of the bi-polar system may be more readily appreciated. As a type of kinship terminology, the Arabic system is heavily descriptive, which is to say that it contains a predominance of forms that "combine(s) two or more elementary terms to denote a specific relative" (Murdock, 98). First and second degree consanguineal kin-types are denoted by elementary terms (i.e., minimal free forms), twelve in number. Paternal side and maternal side are discriminated except in the case of grand-

¹ Murdock, G. P., 1949, *Social Structure*, p. 104.

parent terms where they are merged, that is they are distinguished only by sex of person denoted, not by sex of linking relative. For example, the Arabic form for "grandmother", *sitti*, applies both to father's mother and mother's mother.

Additional consanguineal kin-types are signified by complex (descriptive) constructions made from these twelve elementary terms. For example, as the kin-type father's brother is expressed by a term which is different from the kin-type mother's brother, *'ammi* and *xeyli*, so the eight terms for cousins are also each unique. Father's brother's son is stated *'ibn 'ammi*, ("Son" + "Father's Brother"), or mother's brother's daughter is stated *bint xeyli* ("Daughter" + "Mother's Brother"). Kinsmen who are not blood relatives are denoted by terms tailored on the same pattern, e.g., Daughter-in-law, *mart 'ibni*, consists of the terms "Wife", *mart*, and the term "Son", *'ibni*. The first person possessive pronominal suffix is affixed to the final term in an utterance and as this is constant it will not be mentioned here again.

All these terms are used in reference situations; they all occur when the speaker is speaking about a present or absent third party. Some, but not all the terms are also appropriate to the context of face-to-face speech, that is, the set of direct address terms. This latter set differs from the former in several important respects, and it is there we see the beginning shape of what we are calling "bi-polarity".

First and foremost, the set of direct address terms violates the scheme of descriptive terminology ("Sudanese", Murdock, 239) which the reference terms present and incline instead toward a format which is more nearly generation (or "Hawaiian", Murdock, 228) in type. Elementary terms appropriate in age and sex are extended in solidary form to distant kinsmen rather than in combination as descriptive expressions. A second cousin who should be referred to as "Father's Father's Brother's Son's Son", *'ibn 'ibn 'amm bayyi* will be addressed simply "Brother", and a maternal second cousin could be called the same thing. Second, direct address terms may also ignore difference in sex of linking relative. Third, with one consistent class of exceptions, only elementary terms occur in direct address. Terms in the form "Wife of (a kinsman)", e.g. *mart xayyi* "Brother's Wife", do occur in direct address. Whether this exceptional class is a function of extended patri-family residence or not is a problem which cannot be treated here.

Fourth, it is among direct address terms that we find forms having a dual antecedent, that is to say, the bi-polar terms. For convenience of reference, the terms are given below. Their meanings are defined by listing all the kinship types which they may denote. The reader should note that the English given for these types is intended as useful labels, not as literal translations of the Arabic. The types listed under each term are classed by sex of speaker and by generations from him. Positive numbers are used for ascending generations, negative numbers for descending generations. We may now attempt to induce the rules which govern the empirical possibilities of occurrence of the usage.²

² This listing is influenced by Floyd Lounsbury in his "A Semantic Analysis of the Pawnee Kinship Usage", *Language*, 32 (1956), 158-194.

We begin by noting that where the vector of the kinship term is bi-polar, or in other words, where it has more than one kin-type as referent, it is always the *senior* of the pair of kin-types which is the one chosen to designate the junior and never the opposite. Bi-polarity is communicated in speech by the senior, only received by the junior. The senior is addressing his junior by one of the terms which the latter will normally use in addressing him. The senior borrows from the vocabulary of the junior, but the parallel loan does not occur. For instance, a father calls his son "Father", *bayyi* and is called "Father" in return, or a father calls his daughter "Father", and again is called the same in return. A grandmother calls her son's son "Grandmother", *sitti* but her grandson addresses her as usual, "Grandmother". A man calls his brother's son or daughter "Father's Brother", and is called that in response but he uses another term when addressing his other sort of nephew or niece, his sister's children. Then he must call "Mother's Brother", *xeyli*, what they will call him.

DRUZE ARABIC BI-POLAR KINSHIP TERMS

<i>židdi</i> "my grandfather"	
(male or female speaker)	2: father's father; mother's father
(male speaker)	-2: son's son; son's daughter; daughter's son; daughter's daughter
<i>židdu</i> "my grandfather"	
(male speaker)	-2: son's son; son's daughter; daughter's son; daughter's daughter
<i>sitti</i> "my grandmother"	
(male or female speaker)	2: father's mother; mother's mother
(female speaker)	-2: son's son; son's daughter; daughter's son; daughter's daughter
<i>sittu</i> "my grandmother"	
(female speaker)	-2: son's son; son's daughter; daughter's son; daughter's daughter
<i>bayyi</i> "my father"	
(male or female speaker)	1: father
(male speaker)	-1: son; daughter
<i>baba</i> "my father" (diminutive)	
(male or female speaker)	1: father
(male speaker)	-1: son; daughter
<i>'immi</i> "my mother"	
(male or female speaker)	1: mother
(female speaker)	-1: son; daughter

<i>mama</i> "my mother" (diminutive)	
(male or female speaker)	1: mother
(female speaker)	-1: son; daughter
<i>'ammi</i> "my father's brother"	
(male or female speaker)	1: father's brother; male consanguine of father
(male speaker)	-1: brother's son; brother's daughter
<i>'ammti</i> "my father's sister"	
(male or female speaker)	1: father's sister; female consanguine of father
(female speaker)	-1: brother's son; brother's daughter
<i>xeyli</i> "my mother's brother"	
(male or female speaker)	1: mother's brother; male consanguine of mother
(male speaker)	-1: sister's son; sister's daughter
<i>xeylti</i> "my mother's sister"	
(male or female speaker)	1: mother's sister; female consanguine of mother
(female speaker)	-1: sister's son; sister's daughter
<i>mart 'ammi</i> "my father's brother's wife"	
(male or female speaker)	1: father's brother's wife; spouse's mother
(female speaker)	-1: husband's brother's son's wife; son's wife
<i>mart xeyli</i> "my mother's brother's wife"	
(male or female speaker)	1: mother's brother's wife
(female speaker)	-1: husband's sister's son; husband's sister's daughter

The foregoing is what I would call the prime characteristic of bi-polar terminology, a generational asymmetry corrected for by the loan of the senior term to denote the junior. I will even belabor the point to say that this asymmetry could have been corrected in two ways: the senior, (*vid.* the father) could have addressed his son "Son" and be called "Son" in return, but the Arabic case takes the other alternative and balances the two on the higher generational level. Equals are made by promoting the junior rather than by down-grading the senior.

We may give this now as the first and second defining conditions of Arabic bi-polar terminology. Only terms implying an inequivalence of generation may be so used; and it is the senior referent which is extended to the junior and not the opposite.

The third condition concerns the specification of *sex*. In the listing above, we note that it is always the sex of the senior which is communicated by the bi-polar term, not that of the addressee. A man calls his daughter "Father" as a woman calls her

son "Mother". The junior's sex is irrelevant for the form of the term (as it is whether a boy or a girl is addressing his or her mother's brother). The speaker who is junior signals by his choice of terms whether he is addressing his mother's or his father's brother, but is silent as to whether he, himself, is male or female.

The fourth condition concerns the principle of *descent*. Generation and sex lines may be confounded between speaker and one spoken to, but descent lines are everywhere preserved, better preserved even than in polar usages or perhaps appropriate to the greater system of Arab kinship. A speaker may trespass over generation and sex boundaries and call his brother's daughter "Father's Brother", but he must use a wholly different term for addressing his other niece, his sister's child. Both are bi-polar usages, but they are not interchangeable. (This principle is a corollary of the preceding.)

The kin-types brother and sister have so far not been mentioned. They do not appear in the listing of bi-polarity although they would seem by rights to be candidates for the class as they occur in direct address and are in elementary form. But a brother does not address his sister, "Brother," nor is he called anything but "Brother" in return. There may be two possible ways of accounting for this aberrance, I think, one more linguistic, one more social. First, we note that in Arabic the same discontinuous consonantal morpheme provides the root for terms designating any pair of kin-types which differ only on sex. For example, the same root [b-n] appears in "son" as in "daughter" (*'ibn* and *bint*), in "brother" as in "sister" (*xayyi* and *'uxti*). Etymologically speaking then, the terms for siblings are already bi-polar in their very composition. Second, by definition, sibling terms differ in that they are inherently equivalent, or put another way, there is no generational asymmetry to be righted by the device of bi-polarity. Sibling terms need no planeing down, they connote a generational peerage whatever be the cultural overlay of preference for one sex.

The structure and distribution of bi-polarity in hand now, the non-linguistic aspects of its occurrence can be considered. In effect, I turn to the meaning of this usage as I suggest the social context in which it is most likely to appear.

The situation to be described is one in which, for example, a father wants his child to act upon his wishes but does not want to issue a command to do so. He makes the request in a conciliatory manner using a cajoling or placating tone. In this situation, if he addresses his child, it will be with the usage described above. Even when the term has been used in address with angry overtones, the situation is essentially conciliatory rather than threatening. "Baba, what is this?" a father might say irritated by a son's mischief. He is expressing displeasure surely, and implicitly requesting the child to stop behaving as he has been, but he has not put the force of his authority explicitly behind his words. The use of the bi-polar term affirms this. Should the father become exasperated beyond endurance, he is more likely to call his son by his name or utter the expletive, "wleh!" which has, perhaps, the flavor of "jerk" or "stupid". The father when he urges rather than commands resorts to using the same

technique his son might use in making a request of him. Addressing a son as "Father" codifies that temporary and unilateral reversal of relationship. This is not a reversal of roles. The son is expected to fulfill the request *as a son*, not to deny it as a father. The father has not abdicated his authority, only the expression of that authority embodied in his right to command.

Hierarchical notions which were implicit in standard polar terms are soft-pedaled. No one is deceived by the loan of the "father" term to the son, but its use alone may suffice to disguise the command as perhaps a petition for favor. The speaker, senior always, is here denying his higher status and by the same token denying the discrepancy between himself and son. He is taking away the son's "son-ness" in order to replace it with the higher rank – a rank from which the son should be all the more ready to grant the father's request without loss of face on either side. Moreover, the son's title to the new rank is not wholly clear, he has been made more than an equal, he has been promoted over his father's head, so to speak. The term "father" is still inherently hierarchical no matter who says it, only in the speech of the father it suggests a ladder minus its lower rungs. The son is "senior" but not senior to anyone. The same label stamped on both parties means that a bi-polar or Golden Rule style of action may be expected of both. Father and son now "father" each other; uncle and nephew are both avuncular.

The question may be raised whether there is something in Arab culture which can be pointed to to support this line of reasoning. I suggest that such a theme does exist, and it is the theme of extreme permissiveness in child training, especially the training of boys. It is believed that a boy ought not to be compelled, he should be persuaded, cajoled into doing whatever his older relative wishes him to do. This is the ideal and from it comes the notion that cajoling will be even more productive (and palatable to the adult) if it is couched as a favor between equals. We see the man who addresses his brother's son, "Father's Brother" conjuring up a metaphoric kin group, a group of persons who can lay claim to standing in the same relationship to either of them - the members of it are all "father's brothers" to each other, age or genealogical level even notwithstanding. Or we may look outside the family and see the usage occurring in the plea of a woman to her speeding taxi-driver. "Please, *Mama*", she says, "go more slowly." What we are seeing is her attempt to bolster her command by creating a superordinate of the driver so that he will perhaps play the nurturant role that goes with it. I am not asking the reader to imagine a million tiny kin groups existing for a fraction of time, I am rather suggesting that in the confines of a single verbal exchange of this sort they may be so summoned to camouflage an injunction as an indulgence.

I wish to interpose now some relevant field observations. My husband, Victor Ayoub, an anthropologist, recently spent some six months in the Lebanese village referred to above. While there, he lived with a family and took systematic note of the occurrence of bi-polarity in the interaction of this family. He reports that during this time, he was unable to record an example of its use in an unequivocally com-

mand environment. When it did occur, it was used in the implicit situation presented as the model above, or in one consistent with it.

Whether these observations would be confirmed in more extensive work we cannot say, but I believe that there is evidence enough to suggest an hypothesis for further testing: although when they are very young, girls as well as boys will be addressed with the bi-polar term, its use as directed to them will decline in frequency faster and more permanently than it will as applied to their brothers.

As a final point, I suggest that the viability of Arabic bi-polar terminology may be seen in its translation into other languages by native speakers of Arabic. Two illustrations are offered from the kinship lexicon of Arab immigrants to the United States. A father asks his son to do something and addresses him as "Dad"; a woman entreats her brother's son by calling him "Auntie". The structure and meaning of bi-polarity is carried over into their English; whether the son and nephew so addressed will sustain the usage for another generation is questionable.

To summarize now, I have been arguing that one means the name by which he addresses another, and the form of the name he decides upon is determined by the meaning intended. If hierarchical considerations are inherent in Arabic kinship terminology, then this will be reflected in the election of the senior term over the junior one for bi-polar usage. Hierarchy is only escaped by being egalitarian about who is in the high position.³

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³ I am grateful to David Schneider, Dell Hymes and Victor Ayoub for their reading and comments on earlier versions of this paper.

SOCIOLINGUISTIC VARIATION AND LANGUAGE CHANGE

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1. *Introduction.* It seems probable that no language is as monolithic as our descriptive grammars sometimes suggest; wherever sufficient data are available, we find diversity within languages on all levels – phonological, grammatical, and lexical. Such diversity can be studied along three synchronic dimensions – geographical, social, and stylistic. The geographical dimension is, of course, the main one which has occupied the attention of dialectologists and which has been presented in dialect atlases. Other types of variation within languages, however, have received less attention. What is here termed the social dimension of linguistic variation is correlated with the socially established identity of the speaker and/or the person addressed or mentioned. Examples are the special linguistic forms used in Nootka to speak to or about children, fat people, dwarfs, hunchbacks, etc. (Sapir, 1915); cases of separate men's and women's speech, as in Koasati (Haas, 1944); and the cases, familiar from our own society, where speech differences are correlated with the speaker's social status. The term "sociolinguistic variation" may be applied to cases such as these, and in addition to those where linguistic variation is correlated not with the identity of persons, but with other factors in the social context. These are the factors we have called stylistic. Linguistic styles determined by such factors range from the special war-path speech of the Chiricahua Apache (Opler and Hoijer, 1940) to the written styles appropriate to particular literary contexts in societies like our own. Included here also are differences between formal and informal styles of speaking. Although these occur, perhaps, in most languages of the world, some speech communities such as those of Arabic and Modern Greek show such a marked difference between formal and informal style as to produce a kind of bidialectism which Ferguson (1959) has named DIGLOSSIA.

The study of all these varieties of sociolinguistic variation has proved especially fruitful in the South Asian area (India, Pakistan, Ceylon), and a volume recently published (Ferguson and Gumperz, 1960) has dealt with several aspects of the subject. On the one hand, clear-cut social dialects are found to be associated with the caste system of Hindu society, and these "caste dialects" constitute one important field for investigation. On the other hand, many Indian languages have formal and informal styles which are differentiated to the point of diglossia. However, since most published works on South Asian languages concentrate on high-caste dialects

or formal style, adequate data on differences of caste dialect and on diglossia, as well as on relationships between the two phenomena, are still lacking.

In the Dravidian languages of South India, we find sociolinguistic factors organized into at least two contrasting patterns. In Tamil and Kanarese (and probably also in Telugu and Malayalam), there are classic cases of diglossia. The formal or literary style is used by educated persons in writing and in public address; it varies only slightly with the social class or place of origin of the person using it. Contrasting with this is an informal or colloquial style, showing much greater internal diversity. Differences correlated with the regional and caste background come to the fore in this informal style, although the speech of the educated may be somewhat more uniform than that of the uneducated. An entirely different pattern is found in the Tulu speech community, occupying a small area on the western coast of South India, and probably also in the area of the Kodagu or Coorg language, farther inland. Here we find Hindu societies comparable to those in the rest of South India, but lacking a tradition of written literature in the native tongue. The social functions which are elsewhere served by a formal style of the local languages are here served by the formal variety of Kanarese. Tulu is, to be sure, sometimes written in Kanarese script for informal purposes, but the language is not the customary medium either for education or for a literary tradition. Dialect divisions corresponding to regional differences and caste differences do occur in Tulu, however, just as in the informal styles of Kanarese or Tamil.

The question then arises: What processes have operated to bring about the differences that exist between modern caste dialects? If forms of the present-day dialects are compared with earlier forms of Dravidian speech, it is apparent that some modern forms represent retentions of earlier ones, while others represent innovations. It has been claimed that linguistic innovation in general comes from the lower social levels; thus a recent paper speaks of "la langue populaire, riche en innovations, qui a pour elle le grand nombre, et la langue des classes aisées, qui est plus conservatrice" (Schogt, 1961, 91). On the other hand, it has also been argued that phonetic change, and perhaps linguistic change in general, are initiated by the upper social strata, in order to "maintain a prestige-marking difference" from the lower strata (Joos, 1952, 229). The lower class is said to narrow the gap again by imitation, forcing the upper class to innovate still more. Thus language change is viewed as a "protracted pursuit of an elite by an envious mass, and consequent 'flight' of the elite" (Fischer, 1958, 52). The information available on Indian caste dialects can be used to test such views. Two years ago, an investigation of material from Kanarese, and to a lesser extent from Tulu (Bright, 1960a, 1960b) reached the following conclusions: 1) It is inadequate to operate simply in terms of "change"; changes must be classified as phonological, grammatical, or lexical, and as involving loan materials or native materials. 2) In a comparison of a Brahmin dialect of Kanarese with a middle-caste Non-Brahmin dialect (the abbreviations B and NB will be used hereafter), the B dialect showed innovation on the more conscious levels of phonological and lexical

borrowing and of semantic change, while the NB dialect showed changes on the less conscious levels of native phonology and morphology. 3) However, in a similar study of Tulu, B and NB dialects showed phonological change in similar degrees; the data then at hand were insufficient for the study of other types of change in Tulu.

In an effort to account for the difference between the Kanarese case and the Tulu case, it was hypothesized that it might be due to the existence of a separate formal style in Kanarese, especially as actualized in the written language. That is, the greater literacy of Kanarese Brahmins was seen as a force counteracting tendencies to change in their dialect – the “frozen” phonology and grammar of the literary language serving to retard the unconscious processes of change to which speech is normally subject. Tulu Brahmin speech, on the other hand, having no written Tulu tradition to affect it, has been subject to changes of the same type that have operated in the NB dialects of Tulu. In more general terms, it is suggested that literacy, wherever it is present in human societies, acts as a brake on processes of linguistic change. This suggestion has recently been supported by a study of Latin legal terminology over a 2000-year period. This study finds an unusually high retention rate in legal vocabulary, and concludes that “since these materials have been selected within an area where total literacy is a primary and integral necessity in the communicative process, it seems reasonable to conclude that it is to be reckoned with in language change through time and may be expected to retard the rate of vocabulary change” (Zengel, 1962, 138-39).

It is clear that further study of South Asian caste dialects is desirable in order to establish more clearly the role of literacy in linguistic change. To this end, we have now examined data on caste dialects of Tamil, a language with an exceptionally long literary tradition; at the same time, an expanded body of Tulu data has been taken into consideration. The following sections present our findings on these two language communities.

2. *Tamil*. The majority of publications on Tamil deal exclusively with the formal style of the language, as manifested in the writing system. Colloquial Tamil, in its various geographical and social dialects, has received attention in publications of Vinson (1895), Matthews (1942), and Jothimutthu (1956); but these works suffer from lack of organization, and they fail to give clear geographical and social identifications of their data. More systematic discussions have been presented by Bloch (1910), Shanmugam Pillai (1960), Zvelebil (1959, 1960, 1961), and the present authors (1962). The work done to date, however, has barely scratched the surface of the subject, and generalizations about Tamil dialectology are still risky.

With these qualifications in mind, we have nevertheless attempted to find general features distinguishing B from NB dialects of Tamil, and to ascertain which social group plays the innovating role in each case. B data have been obtained from Ayyangar and Ayyar informants; NB data have been obtained from members of Vellala, Nadar, Chettiar, and Christian communities. The historical perspective is provided by con-

sidering the Literary Tamil form (which is usually, though not always, historically prior to the colloquial form), the cognates in other Dravidian languages (by reference to Burrow and Emeneau, 1961), and the forms which loanwords have in their source languages. The comparisons made are divided into those involving 1) vocabulary, 2) phonology, and 3) morphology; syntactic comparisons are yet to be carried out.¹

2.1. Caste differences in Ta. vocabulary may be classified into two types. In the first type, one caste has a loanword and the other has a native word, e.g. B *jalō* "water" (Skt. *jala-*), *tīrtō* "drinking water" (Skt. *tīrtha-*), *taṇṇi* "water not for drinking" (native), as against NB *taṇṇi* "water in general". In most of the cases noted, it is B which has innovated by introducing the loanword; a contrary case occurs, however, in B *āmbaḍeyā* "husband", NB *puruṣē* (Skt. *puruṣa-*). In a second type of vocabulary, both castes have native terms, e.g. B *tūngu*, NB *orangu* "sleep". The B form also has the meaning "hang" (intransitive), which is apparently the original sense; cf. the corresponding transitive *tūkku* "lift", and Ka. *tūgu* "weigh". The NB form reflects LTa. *uraṅku* and other Dravidian forms meaning "sleep". Here B has innovated through semantic shift where NB has not; our sample contains no cases of the opposite possibility. There are, however, cases where the two dialects differ without evidence that one has innovated more than the other, e.g. B *alambu*, NB *kaḷuvu* "wash", both apparently descended without change of meaning from PDr. stems.

2.2. Phonological comparisons of B and NB again may be classified into two types. The first type is that of loanwords, in which B frequently preserves non-native phonology, while NB assimilates them to the native pattern, e.g. B *svāmi*, NB *sāmi*, *cāmi* (Skt. *svāmin-*). At the same time, B is prone to hypercorrections in loanwords, such as *jīni* "sugar" (NB *cīni*, from Hindi *cīni*), and *krāfu* "haircut" (NB *krāppu*, from English "crop"), where the foreign sounds /j/ and /f/ are erroneously introduced. The second type of phonological comparison involves native words, where the differences found between caste dialects are most clearly typified by the cases where B has /r/ while NB has /r/ inconsistently varying with /y/ (in northern areas) or /l/ (in southern areas); e.g. B *vāṅṅeppaṅḍō* "banana" as against NB forms like *vāṅṅeppaḷō*, *vāḷeppaṅḍō*, and *vāḷeppaḷō*. The overall picture thus shows B as innovator in the introduction of foreign phonemes, sometimes in etymologically unpredictable places. NB, on the other hand, innovates in native material, although the result (at least for educated speakers) is often free variation between older and newer forms, rather than complete replacement of the older.

2.3. Morphological differences between B and NB mostly involve varying shapes of morphemes, not all of which can be explained by the regular phonemic correspondences. An example is B *-du*, NB *-ccu* "it" (subject of verb), as in B *vandudu*, NB *vanduccu* "it came" (LTa. *vantatu*). In this case it appears that the NB form repre-

¹ Abbreviations used below are Ta. for Tamil, LTa. for Literary Tamil, Ka. for Kanarese, PDr. for Proto-Dravidian, and Skt. for Sanskrit.

sents an analogic extension of the ending found in both B and NB *pōccu* "it went", *āccu* "it became" (LTa. *pōyirru*, *āyirru*). In this, as in other examples, NB plays the innovating role. In some other examples, to be sure, B and NB seem to have innovated equally, but in different directions, as when the present tense marker (LTa. *-kir*) becomes *-h* in some NB dialects, but *-r* in B; e.g. B *paṇṛā*, NB *paṇṇuhā* "he does" (LTa. *paṇṇukirāṇ*). But no clear case has been noted in which B has innovated while NB remains conservative.

2.4. The examination of Tamil materials which has been carried out so far shows a situation similar to that previously noted for Kanarese. Neither dialect has a monopoly of innovations in any part of the structure, and yet tendencies are discernible: on the part of B, toward greater use of foreign vocabulary, foreign phonology, and semantic shifts; on the part of NB, toward shifts in native phonology and in morphology.

3. *Tulu*. Published data on Tulu are found in Brigel (1872), Ramaswamy Aiyar (1932a, 1932b, 1936), and Krishnamurti (1958). These sources do not, unfortunately, distinguish regional dialects, so that there is difficulty in separating regional variations from social variations. This problem has been solved in part by checking with three Tulu speakers.

3.1. The comparisons between B and NB dialects of Tulu can be classified as were those of Tamil. Thus we have: 1) vocabulary differences involving loanwords, such as B *puruṣe* "husband" (Skt. *puruṣa-*), NB *kaṇḍane* (cognate with Ta. *kaṇṭaṇ*, Ka. *gaṇḍa*); 2) vocabulary differences involving native words, such as B *jōvu*, *jēvu* "girl", NB *poṇṇu*. The B form means "child" in some NB dialects, and can be compared with Parji *cēpal*, Ollari *sēpal* "boy"; the NB form is cognate with Ta. *peṇ* "woman, girl". A semantic shift is evident in the B usage. In both these types of correspondence only the B dialect is found to innovate, either by loans from Sanskrit, Hindi, or Kannada, or by semantic shifts of native terms.

3.2. Phonological correspondences are also of two types. 1) Some cases involve loan phonology, as when B aspirated stop corresponds to NB unaspirated stop. Some of these cases are loans from Indo-Aryan, e.g. B *gandha*, NB *ganda* "fragrance" (Skt. *gandha-*). In other cases, however, B forms with aspiration may be traced to PDr., which had no distinctive aspiration: e.g. B *chaḷi*, NB *caḷi* "cold" (cf. Ta. *caḷi*). The B aspiration in such cases presumably originates as a hypercorrect pronunciation. 2) Other cases involve native phonology, such as B /s/, NB /t/ from PDr. *c, as in B *sikk-*, NB *tikk-* "be obtained" (cf. Ta. *cikku*). The B form may be regarded as the more conservative, especially since PDr. *c probably included sibilant allophones (as in many modern Ta. dialects). Five other sound correspondences have been noted in which NB shows greater innovation. But we also have a smaller number of cases where the opposite is true, such as the correspondence of B /ē/ to NB /yā/ where PDr. appears to have had *yā, as in B *ēnu*, NB *yānu* "I" (cf. Ta. *yāṇ*). It thus appears that both B and NB have innovated in phonology, with the NB dialect showing the greater number of innovations. The B dialect, however, shows one

special kind of innovation, the introduction of the foreign element of aspiration.

3.3. Morphological correspondences between B and NB Tulu are more difficult to deal with historically, since we have no writing system to reflect older forms, and no full reconstruction of PDr. morphology has yet been made. Certain correspondences do yield to investigation, however, such as the one between B -no, NB -da, Genitive suffix with "rational" nouns; thus we find B *āṇu-no*, NB *āṇu-da* "of the boy" (cf. Ta. *āḷ-iṇ*, with cognate stem). With "irrational" nouns Tulu has B -nte, NB -da; apparently NB has generalized the dental suffix so as to apply to all types of noun. On the other hand, we find a correspondence between B -i, NB -a, Present Participle marker, as in B *barpi*, NB *barpa* "coming"; the NB form agrees with other Dravidian languages, as in Ta. *varu-kiṇṇ-a*, Ka. *bar-uv-a* "coming".

3.4. In the morphological comparisons, as in the phonological ones, both B and NB are found to innovate. In summary, the Tulu evidence shows the Brahmins as chief innovators in the more conscious varieties of change – semantic shift, lexical borrowing, and phonological borrowing. In the less conscious processes of phonological and morphological change involving native materials, both B and NB dialects innovate.

4. *Conclusion.* We feel that the evidence so far examined supports the hypothesis that upper and lower class dialects innovate independently of one another, and in two ways, here labelled conscious and unconscious. Of these types of change, the more conscious variety is regularly the mark of the upper class dialect. The less conscious changes apparently may affect both upper and lower dialects, as seen in the Tulu case; but in Kanarese and Tamil, where there is widespread literacy among Brahmins, the formal written style seems to have retarded the less conscious processes of innovation. A study of the Kodagu language, which like Tulu lacks a literary tradition, would be extremely valuable for the further testing of this hypothesis.

The important of sociolinguistic factors in language history has recently been pointed up by Hoenigswald (1960:55) and by Schogt (1961). We feel that further investigation of social dialects in the South Asian context can contribute much to understanding the mechanisms of linguistic change.

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DISCUSSION

HAUGEN:

(1) The terms "informal" or "colloquial" style have recently been replaced by "casual" (vs. "non-casual" for "formal") by Voegelin. In my recent studies on this subject I have arrived at the point of suggesting that "private" style might be a better term when looking at the problem from a social point of view. The opposite would then be "public" style, the style used when one person speaks to a public. These would be the two ends of a continuum. (2) The conclusion that literacy inhibits linguistic change is a familiar one in traditional histories of language. But of course it has not been properly tested, and evidence in its behalf is always welcome. A recent straw in the wind was the extraordinarily conservative showing of Icelandic in a recent critique of glottochronology published in *Current Anthropology* by Bergsland and Vogt. Icelanders have been literate during most of their thousand-year history, and this may be part of the explanation. (3) The terms "conscious" and "unconscious"

as applied to innovation are somewhat slippery, unless carefully defined. Perhaps they could be associated with the terms used by some of "surface" vs. "depth" grammar; the B changes are superficial, resulting from borrowing, while the NB changes are deep, resulting from language drift.

FISCHER :

The Tulu data of Dr. Bright may be of special value in determining the social position of the originators of linguistic change, since the complicating factor of the literacy of the Brahmans as opposed to the non-Brahmans is absent or weak in this case. Evidence of the sequence of innovations in the language which are shared by both Brahman and non-Brahman dialects would be of great interest.

One word of caution, however: it is my opinion that the actual dynamics of linguistic change are to be found within communities of face-to-face speakers. If the barriers to social contact and communication between Brahmans and non-Brahmans in this part of India are firm and strict enough, then the elite *within* each lower caste or group of communicating castes may be more important as a sociolinguistic model than the next higher caste. I think some sort of elite is always involved as a "pursued and fleeing model" in linguistic change, but I do not think it is always a formally recognized political, economic or religious elite. If in fact *intra*-caste elites are dynamically the most important as models in this part of India, then imitation of higher caste speech by the lower would be much weakened, though I would not expect it to be entirely absent. I suspect that in investigating caste barriers to communication the earlier years of life, through adolescence, may prove most critical.

SJOBERG :

The patterns you have described for Tamil and Kannada certainly hold for Telegu as well. Highly educated Telegu speakers use both a formal and an informal style, depending on the social situation. But the pattern is most prominent among Brahmans, excluding strongly Westernized ones in recent years. From your discussion now, I have the impression that you believe there are two separate systems – Brahman ~ non-Brahman dialects and formal ~ informal styles. But with Telegu speakers the "Brahman dialect" includes at least two styles, formal and informal. Speakers of non-Brahman dialects would include a few having a formal and informal style, but many with just a single style, the informal or colloquial. The chart does not make this overlapping of the two systems clear. Furthermore, the diagram does not include *uneducated* persons, whose speech would diverge considerably from the most informal style of Brahmans and even middle-class persons.

HINDI-PUNJABI CODE-SWITCHING IN DELHI

JOHN J. GUMPERZ

This paper deals with a linguistic phenomenon which is characteristic of urban agglomerates in the so-called "plural societies" of the East. In these societies, groups of widely different regional and cultural background live together in close geographical proximity. They trade, exchange services and mingle freely in public places. They are subject to the same government and often attend the same schools. Yet regular and frequent interaction has not obliterated the most important cultural and linguistic differences among them. Each group continues to preserve its own separate traditions, values, and loyalties, often evident in differences in dress and diet. The result is a range and variety of behavioral norms which are considerably greater than those found in the more highly industrialized urban societies of the modern West.

Home, family and kindred continue to be at the center of the individual's life. Marriage alliances and the relaxation of informal friendships rarely transcend these boundaries. Contact with outsiders is a necessity of daily life, but behavior in these public situations is sharply distinct from the intimacy of the family circle or peer group. An individual may mingle freely with others of different background and even strive to imitate them in business, at public gatherings, or in school. But at home he is expected to revert to the pattern of his own group. The separation of private from public behavior thus serves to insulate the group by limiting the flow of innovations filtering in from the outside.

The presence in private life of many exclusive kin groups parallels the concentration of certain types of public activities in the hands of particular communities. Wholesale and retail trades and crafts are parcelled out among many relatively exclusive groups. Religious practitioners, lawyers, and administrators are drawn from yet other groups. The number of public activities in which the individual engages determines how many modes of public behavior he must learn. While these inter-group barriers are now slowly but steadily breaking down, we still have a society which tolerates and keeps distinct a wide variety of diverse modes of behavior. Interaction is characterized by a high degree of what anthropologists have called "role specificity" (Bruner, 1956) – that is, the round of daily activity is segmented into a series of separate spheres governed by distinct and often conflicting norms.

Given a population with highly diverse linguistic backgrounds, the above social

environment is one which would tend to preserve pre-existing language and dialect differences to an extent rarely found in Western societies. In fact, throughout India and other parts of Asia we find immigrant groups who maintain their linguistic identity for many centuries, even in relatively small communities. The number and kind of linguistic codes employed in a community and their genetic origin are matters of historical accident; once a code is established it tends to become associated with the behavior characteristic of the group that most frequently employs it. The group's language then becomes the symbol of group identity. But this does not necessarily mean that it is monolithic; far from it. Special, formal styles of the group language may be used for religious and/or professional activities peculiar to the group. Other styles influenced by surrounding codes are used by those members of the group whose activities bring them into daily contact with members of surrounding groups. These conditions insure that to the extent that an individual participates in different aspects of community life, he must control the codes associated with those aspects of community life.

In contrast to Western society, therefore, where one linguistic code or a set of closely related styles of what is popularly considered to be the same language serves all requirements of the daily routine, code diversity characterizes the plural societies of the urban East. Multilingualism is an integral component of social interaction and a requirement for full participation in community life.

There is, however, a fundamental difference between community multilingualism of this sort and the isolated bi- or multilingualism which occurs when individuals of exceptional educational background living in an essentially monolingual society control more than one language. The distinction is one which is not ordinarily made in the literature on the subject. Psycholinguists distinguish between "compound bilinguals" who alternately employ two different languages, not always with equal competence but presumably in the same social context, and "coordinate bilinguals" who have near-native control of two languages which they employ in distinct social settings (Ervin and Osgood, 1954). They have also presented some evidence for the fact that the latter condition is a more stable one, but their interest so far has concentrated on the bilingual as an individual and not on the group.

Both isolated and community bilingualism may be coordinate. They differ in the nature of the linguistic norms followed. Isolated bilinguals follow the norms of pronunciation and grammar prevalent among native speakers of both languages in question. Thus an American bilingual in French will attempt to follow Parisian norms. Multilingual societies, on the other hand, tend to create their own norms which are often quite different from those prevailing in the respective monolingual societies. An Indian may speak English with near-native control; he may read it, write it and lecture in it with great success. But when he uses English in India his speech will share many of the features of the other Indian codes with which English alternates in the daily round of activities. Indian English will thus deviate considerably from the norms current among native speakers of English in the American Midwest.

This kind of deviation represents not a failure to control English, but a natural consequence of the social conditions in the immediate environment in which Indian English is spoken.

The social conditions prevailing in multilingual societies create a number of often conflicting tendencies. The need for frequent code-switching on the part of a large number of individuals tends to reduce the language distance between codes. Linguistic overlap is greatest in those situations which favor intergroup contact. Here, as we will show later in this paper, borrowings may reach proportions hitherto associated only with pidgin languages. But, on the other hand, the need for maintenance of at least some symbols of role specificity acts as a deterrent to excessive borrowing and thus prevents complete merger of codes. Interference will be considerably less in those situations which are specific to a single group. The linguistic picture thus shows a range of situationally determined styles of what is popularly considered the same language. These styles may vary greatly in pronunciation, grammar and lexicon, but the linguistic differences among them are rarely reflected in the popular conception of the language – and hence not in the terminology popularly applied to the language.

The linguist studying this problem may take one of two approaches: he may, as in most existing literature on the subject, concentrate on those styles or codes which are popularly known by the same name. He will then analyze all forms of the language regardless of when and where they are spoken. In the case of Punjabi, the linguist's statement would cover varieties used in those regions where Punjabi is the dominant language as well as in Delhi and those other parts of India where it is a minority language.

Or the linguist may approach the problem by defining the bounds of his study in terms of the "speech economy" (Hymes, 1962) of a single community. In this case, he would study all the codes used there regardless of language names and genetic affiliation. He would, however, exclude those varieties which are spoken elsewhere and are not "functional" (Nadel, 1957) in the community. This approach, seldom adopted to date, seems a promising one, since it relates speech behavior to the extra-linguistic environment in which it operates and may thus provide an insight into the relationship between social factors and language change. For these reasons, this approach is followed in this paper.

We use the term "code matrix" to designate the totality of functionally important codes in a specific community. The components of such a code matrix may be dialects or styles of the same language or genetically related or even unrelated languages. In India, urban societies differ considerably in the components of the code matrix. In industrial centers established in former tribal areas, such as Jamshedpur in Bihar, the code matrix includes tribal languages of the Munda family, the local dialects of Bihari, standard Hindi and the English of the educated. It is not unusual even for a relatively uneducated tribal to control at least some styles of all these languages. In Delhi, the Hindi and Punjabi components of the matrix are more closely related,

but regardless of actual language distance between codes, Delhi and Jamshedpur remain multilingual societies exhibiting similar social characteristics.

The present paper is a preliminary attempt to illustrate some aspects of community multilingualism as it occurs among speakers of Hindi and Punjabi in Delhi. Speakers of Punjabi, the largest of Delhi's linguistic minority groups, trace their origin to a broad region extending from the districts of Hisar and Patiala about 150 miles west of Delhi to Rawalpindi in the present West Pakistan. Their native idiom, although genetically a close relative of Hindi, is recognized as a distinct language in the Indian Constitution. It has its own literary tradition and grammatical norms and is considered by many to be mutually unintelligible with Hindi. Although most Punjabi families have come to Delhi since the turn of the century, the two languages have coexisted within the same linguistic area as part of the same cultural complex for several hundred years. Varieties of Hindi are commonly employed as trade media in most urban bazaars within the Punjab. They are also used as superposed literary codes by Muslims and Hindus and thus coexist with literary Punjabi, cultivated primarily by Sikhs. Residence in Delhi has not brought about any radical change in linguistic environment for most Punjabis. It has merely increased the number of bilinguals and has also increased the uses to which Hindi is put, so that both idioms now serve important functions in daily social interaction. Punjabi is thus spoken both inside and outside the home. The varieties of Delhi Punjabi, however, are by no means homogeneous. Reflections of many regional dialects occur alongside special urban styles showing the influence of the predominant Hindi. Natives recognize these distinctions and use the term *thet* to refer to styles with divergent local color.

The present study concentrates on those situations in which code-switching is normal. The principal informant is a college student who speaks Hindi, Punjabi and English. Although his family came to Delhi from Peshawar, he has spent most of his life in Delhi. In the interviews he was confronted with certain well-defined social situations and was asked to imagine himself conversing in these situations, first with a Punjabi-speaking fellow student and then with a speaker of Hindi. At a later time the informant was requested to give the *thet* equivalents for his urban Punjabi utterances. The data was compared with field observations made during a three-month stay in Delhi and with data from auxiliary informants and information from a standard grammar of Punjabi (Bailey, 1961). The information thus obtained is, of course, preliminary and will require checking in more carefully controlled field observations, but the results seem significant enough to merit a report at this time.

The prospects for structural comparison of linguistically diverse materials have been considerably improved by recent advances in theoretical linguistics. Theoretical models of language structure have been proposed which provide analytical categories general enough to apply to all types of linguistic systems. A grammar is not considered as specific to a language, but is conceived as a set of rules assigning a structural

description to observed data at the various levels or strata of language (Chomsky, 1961). These grammatical rules are expressed in terms of formulaic symbols related one to another by closely defined quasi-mathematical procedures, a fact which lends the statement an amount of rigor and comparability difficult to achieve in a traditional prose description. Since grammatical rules may be made arbitrarily abstract, there is no longer any *a priori* reason why the same statement may not be made to apply to different sets of data, even though these data may be popularly considered parts of separate languages. We simply distinguish between shared rules and those which are particular to a sub-system within the same overall structure. The latter rules will then form a measure of the language distance between the substructures involved.

These characteristics apply to a number of recently proposed models of language structure. The present comparative analysis will follow the system recently proposed by Sidney M. Lamb (Lamb, 1962). Lamb's method consists of an elaboration and a refinement of the so-called "item and arrangement" approach of Hockett and others. Language patterns are viewed as describable by code-like systems patterning on each of a series of strata – phonemic, morphophonemic, morphemic, sememic – which are recognized as basic properties of language. The model recognizes three kinds of relationships: that of a class to its members, that of a combination to its components and that of an *eme* to its *allos*. These relationships are symbolized by a set of class symbols specific to each stratum, a set of morphotactic rules describing the combination of units within a stratum and a set of representation rules which serve to convert the units of one stratum into those of another. A grammatical statement is complete if it assigns to an utterance a description on all strata of linguistic structure.

Comparative analysis of the linguistic texts collected from the principal informant reveals that the differences among them are almost entirely grammatical. More than 90% of the lexical items in the urban code-switching style are also Hindi words. Many of those items that differ do so by regular phonological correspondences such as that between long *-aa* in Hindi *kaam* (work) and short *-a* in Punjabi *kam* (work).

We begin our comparative grammatical analysis at the morphemic stratum. Tactical rules on this stratum are expressed in terms of general class symbols representing categories such as form-class, case, gender, etc. These symbols are given specific lexical content only at the lower, morphophonemic level. The greater proportion of tactical morphemic rules are common to both codes. Some illustrative examples are given below.¹

¹ Symbols are used with the following meanings unless otherwise defined in the text:

S = substantive phrase,	V = verb phrase,	Pr = pronoun;
Nc = noun construction,	A = adjective,	stN = noun stem;
stA = adjective stem,	cn = case-number suffix,	pp = postposition;
Aux = auxiliary,	stAux = auxiliary stem,	D = demonstrative pronoun;

A	B	C	D	Aux
stV	ûû	T	cn ₁	
stAux	iye	n	aa	

Morpheme combinations possible in various types of verb construction may be symbolized by the rule given below, from which the individual possible sentences may be derived by multiplying out as in an algebraic formula.

$$9) \quad A(1 + B^1(1 + C^1D^1)) + B^2(1 + C^1D^2) + D^1(1 + C^1 + C^2)$$

While the bulk of the morphemic rules are shared, differences between codes appear in the representational rules translating morphemic class-symbols to the morphophonemic stratum and in the representational rules converting the morphophonemic into phonemic symbols. These will be illustrated by concrete examples from the data, which for brevity's sake are given in phonemic transcription. (Double vowels indicate length.)

<i>Punjabi</i>	<i>Hindi</i>
10) oo na-ii khaa-nd-aa	woo na-ii khaa-t-aa
	He doesn't eat.
11) oo ghar-wic hæ-g-aa	woo ghar-mã hæ
	He is in the house.
12) is-d-i k-ii kimat hæ-g-iii	is-k-ii k-yaa kimat hæ
	What is the price of this?
13) tuhãa-nuu k-ii caa-iidaa	aap-koo k-yaa caa-iye
	What do you require?
14) mã khaa-wãa	mã khaa-ûû
	I should eat.

In the above examples, note the similarity in the case-number suffixes *-aa* and *-ee* in item 10, and in the initial question morpheme *k-* of Punjabi *k-ii* and Hindi *k-yaa* (what), in items 12 and 13. Some important differences appear in the morphophonemic realization of shared morphemes. Thus Punjabi *-d-* and Hindi *-k-* in item 12 represent the morpheme K also shown in item 5 above. Similarly, Punjabi *-nd-* and Hindi *-t-* in item 10 represent the same morpheme. The code differences can be symbolized by representational rules such as those illustrated below.

$$15) \quad K^m // P, \text{ — } /d \qquad \qquad \qquad \hat{u}\hat{u}^m // P, \text{ — } /ãã$$

$$m // H, \text{ — } /k \qquad \qquad \qquad m // H, \text{ — } /ûû$$

P and H in this formula stand for Punjabi and Hindi respectively. The rule states that the morphemes *K* and *ûû* are realized morphophonemically as *k* and *ûû* in Hindi and as *d* and *ãã* in Punjabi. Similar relations hold between Hindi *-koo* and Punjabi *-nuu*; and Hindi *-mã* and Punjabi *-wic* (in).

In other cases the identity of rules on the morphophonemic level is reflected in

differences when these rules are translated to the phonemic level. This is the case for Hindi *woo* and Punjabi *oo* (that, he). Because of their phonological similarity, and since within Hindi *w-* is lost within compounds such as *j-oo* (who) (see item 8 above), the two segments may be assigned the same morphophonemic transcription in both codes. The difference is then expressed in representational rules converting the morphophonemic into phonemic transcription:

- 16) $oo^{mp} // P, \# \text{ — } /oo$
 $^{mp} // H, \# \text{ — } /woo$

This rule states that morphophonemic *oo* after juncture ($\#$) is realized phonemically as *oo* in Punjabi and *woo* in Hindi.

On the phonological stratum, no differences in phonemic inventory were found in the code-switching styles. Punjabi tone, which occurs in items where Hindi equivalents have voiced aspirates, as in *ghar* (house), seems to be lost in code-switching situations, although it does occur in other contexts. Many of the phonetic characteristics of Punjabi are also shared by Delhi Hindi. This is the case with /æ/ and /ɔ/, which appear as monophthongs in Delhi Hindi and diphthongs in Eastern Hindi. Similarly, /ʈ/ and /ɖ/ in Delhi Hindi share the extreme retroflexion of Punjabi, while in Eastern Hindi retroflexion is less pronounced.

When we move from the code-switching situations into others more specific to Punjabi-speaking groups, additional differences appear at all strata resulting in an increase in the Hindi-Punjabi language distance. On the morphemic level, *theṭ* Punjabi adjectives have special endings to show agreement with oblique plural nouns. Furthermore, the present participle allomorph *-d-* changes to *-n-* with first person subjects. A locative suffix is added to the class of number suffixes.

Some additional phonemic and morphophonemic differences are illustrated by the *theṭ* alternates of items 11 and 12 above:

- 11a) *oo kár-wic hæ-g-aa*
 12a) *ed-d-ii k-ii páuu hæ-g-ii*

Here, *kár* (house) and *páuu* (price) show rising tones. Furthermore, in item 12a the Hindi-like pronominal oblique *is-* (this) is replaced by *ed-*.

Other morphophonemic differences are brought in by alternations in postpositions such as those listed below:

<i>Hindi</i>	<i>Punjabi</i> (Code-switching)	<i>Punjabi</i> (<i>theṭ</i>)	
-se	-se	-tõõ	(from)
-saat	-saat	-nal	(with)

Lexical differences between *theṭ* and code-switching styles include a high proportion of common adverbs of time, place and manner and many frequently-used adjectives and nouns. Some examples are:

<i>Hindi</i>	<i>Punjabi</i> (Code-switching)	<i>Punjabi</i> (<i>thet</i>)	
ab	ab	hun	(now)
idhar	idar	edar	(here)
itn-aa	itn-aa	enn-aa	(this much)
æs-e	æs-e	edd-aa	(like this)
acch-aa	acch-aa	cang-aa	(good)
dukaan	dukaan	haṭṭii	(store)

Aside from the *thet* variations, additional alternations are possible in certain extreme situations which reduce rather than increase the Hindi-Punjabi language distance. These may be illustrated by the following alternates for items 11, 13 and 14 above:

11b) oo ghar-mã hæ-g-aa

13b) aap-koo k-ii caaiye

14b) mã khaa-ũũ

Note the substitution of *mã* for *wic* in item 11 and of *aap-koo* and *caaiye* for *tuhãã-muu* and *caa-iidaa* in item 13. In item 14 the substitution of the suffix *-ũũ* for *-ãã* makes the utterance identical with Hindi. This utterance would be identified as Punjabi only if it occurred as part of a larger one with definite Punjabi features.

One striking linguistic aspect of the code-switching situation is the fact that we find few of the strictures against structural borrowing commonly reported in the linguistic literature. Interference extends to all levels of the grammar – morphemic, morphophonemic and phonological – as well as to the lexicon. It almost seems as if the two languages were gradually merging. This situation differs little from what occurs in pidgins. At the extremes of the stylistic continuum, only a few items, such as the Punjabi verbal suffix *-nd-* and the Punjabi question word *k-ii* (what), do not seem subject to borrowing. This evidently suffices to preserve the necessary minimum of symbols of role specificity.

Viewed from the point of view of social prestige, the linguistic status of the code-switching style is uncertain. Our principal informant considers it part of Punjabi. Older natives of Punjab, however, tend to dismiss it, representing it as an attempt of an ignorant person to speak Hindi. This seems unlikely, since as a bilingual our informant could easily have dropped the few markers which separate his Punjabi from his Hindi. We must assume that, deviant though it is, this style fills a definite social function in at least some situations.

Because of its lack of prestige and the disagreement regarding its status, the code-switching style may or may not be included in a descriptive grammar of Punjabi for which data has been collected without control of situation. Its inclusion depends on the choice of informant and on the way in which the interviews are conducted. Standard normative grammars will certainly not include it. Similarly, the informant, when questioned about Punjabi, will respond in terms of those situations which are specific to speakers of Punjabi. This should not be taken to mean that styles

such as the code-switching style are unanalyzable by traditional linguistic methods. Such styles show the same kind of patterning found in other linguistic data and could be recovered if proper situational controls were used.

One other conclusion that emerges from this study concerns the problem of structural borrowings and the differences between pidgins and other languages. It would seem that the occurrence or non-occurrence of structural borrowings is not solely a matter of linguistic fact but is at least in part dependent on the existence of social norms which may act to filter them out of descriptive grammars. Similarly, the high incidence of what has been called "language mixture" in the case of pidgins may be explained by the absence of feelings of group loyalty or fluidity of social norms governing the situations in which pidgins are used.

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CONTINUITÉ DE LANGUE, CONTINUITÉ ETHNOGRAPHIQUE CHEZ LES GRECS DU PONT-EUXIN (ASIE MINEURE)

ODYSSEUS LAMTZIDIS

1. Les termes "Pont" et "Pont hellénique" ont été consacrés par l'usage pour signifier la région au Nord-Ouest de l'Asie Mineure, qui s'étend de l'Ouest à l'Est depuis Sinope et la rivière Alys jusqu'aux abords de la rivière Akampsis et du Nord au Sud depuis les rives méridionales du Pont-Euxin jusqu'au mont Skydisson (en turc Kapan-dag); c'est pourquoi nous les employons tout au long du présent article.

Nous devons cependant remarquer que, du point de vue géographique, l'étendue en question présente une caractéristique de très grande importance, qui a beaucoup influencé soit l'histoire en général des Hellènes qui l'habitent, soit plus particulièrement leur langue. Toute la région déterminée ci-dessus constitue en quelque sorte un ensemble isolé, du fait qu'elle est presque dépourvue de communications faciles avec les autres régions tant par voie de terre que par voie de mer.

2. Il est indispensable aussi bien qu'opportun de désigner dès le début le sens que renferme d'après nous le mot "nation". Certes nous n'entendons pas ce terme au sens strict de la race – pure ou mélangée – mais à peu près comme Isocrate entendait le mot "Hellène". Le signe distinctif d'une nation c'est la civilisation commune, dont l'expression significative est la langue. Aussi ces deux mots, langue et nation, coïncident-ils par la suite, si bien qu'en parlant de l'Hellénisme du Pont nous entendons la population qui habite la région du Pont et qui parle la langue hellénique (ainsi p. ex. on dit la nation anglosaxonne).

3. Il nous faut aussi expliquer pleinement le terme "dialecte hellénique du Pont". Certes la langue hellénique du Pont fut partagée en plusieurs idiomes, à cause du morcellement géographique et du manque de communication entre les diverses régions du Pont. En tout cas tous les idiomes pontiques sont plus ou moins distingués l'un de l'autre par des caractéristiques générales qui néanmoins les groupent dans un ensemble. Aussi, par le terme "dialecte du Pont" entendons-nous ci-dessus la langue commune théorique des Hellènes qui habitent le Pont. Nous disons "qui habitent le Pont" quoique depuis 1922 presque aucun Grec n'y soit resté car presque tous ont émigré ailleurs, en particulier en Grèce. C'est pourquoi le dialecte que nous examinons ici est celui qu'on parlait dans le Pont avant 1922.

4. Mais quelle est l'origine du dialecte pontique? Certains spécialistes de la koinè hellénistique et du grec moderne soutiennent que, en dehors du dialecte tsakonien, d'origine dorienne, aucun dialecte ne peut remonter à une époque antérieure à celle de la koinè, et que par conséquent le dialecte pontique lui non plus ne peut être considéré comme suite de la langue des premiers colons Hellènes de la région du Pont. Nous pensons que si des spécialistes connaissant le dialecte pontique examinent de nouveau cette question, ils seront amenés à prouver son origine très ancienne, bien antérieure à la koinè, ce qui est notre opinion à nous.

5. Dès le huitième siècle le génie du commerce et la curiosité ont poussé à la fondation de colonies. Milet, la riche ville ionienne, fonde la première cité grecque au sud de l'Euxin, la ville de Sinope. En peu de temps d'autres villes surgissent sur le même littoral: Héraclée, Amisos, Trébizonde etc. Ces villes grecques payaient tribut au roi de Perse, même si elles ne lui étaient pas complètement asservies. Il est certain aussi que ces "emporion" fortifiés, grâce à leur position, avaient des relations continues avec les Grecs de l'Egée, et que leurs richesses leur attiraient quelques embarras de la part des barbares d'alentour. (Voir les renseignements donnés par Xénophon dans l'Anabase). Jusqu'à l'époque d'Alexandre le Grand toutes les villes du littoral sud du Pont-Euxin vécurent en paix. A l'époque des Mithridates et surtout de Mithridate Eupator leur situation était florissante. Toujours est-il qu'à l'époque romaine et jusqu'à la fondation de l'Empire byzantin toutes les villes grecques de cette région vécurent presque autonomes.

Durant la période byzantine (330-1204) la population du Pont, à cause de sa situation géographique, souffrit d'un manque continu de contacts avec le centre, à savoir Constantinople, et par là se trouvait presque isolée. Cet isolement ne fit que s'accroître sous le règne des grands Comnènes (1204-1461), qui vit cet isolement devenir le lot de toute la population hellène et hellénisée du Pont entier. Mais après la conquête de la région par Mahomet II la population grecque a été réduite pour bien des raisons. Cet abandon fut transféré radical en 1922, lorsque dans tout le Pont restèrent seulement quelques musulmans qui connaissaient à peine le dialecte pontique grec. Selon les calculs les plus modérés, la population grecque du Pont dépassait 700.000 personnes.

6. Cette brève récapitulation historique montre clairement que la provenance ionienne des Hellènes du Pont a marqué le dialecte pendant toute son histoire jusqu'à nos jours, et que celui-ci, en raison de son isolement géographique, qui a duré autant que l'histoire des habitants du Pont, n'a jamais perdu son caractère primitif.

Ainsi donc, dans son ensemble, la langue grecque du Pont, considérée ici comme un seul dialecte pour des raisons de méthode, a conservé plusieurs éléments anciens, par opposition à la langue grecque moderne ordinaire.

1. La prononciation du η comme ε . Des exceptions se présentent surtout p. ex.
 - a) aux mots empruntés,

- b) aux substantifs de la première déclinaison (p. ex. τιμή, βροντή, ἀνοιχτής, κολυμπητής).
2. La négation οὐκί au lieu de οὐχί,
 3. Les substantifs abstraits qui rejettent le ζ final changent le η en ε (p. ex. ἀξιότε = ἀξιότης).
 4. Les verbes qui expriment la passion ou le défaut se terminent en -ῶ ou -ῷ (p. ex. φτερῶ = φθειρεῶ).
 5. Terminaisons des verbes:
 - a) Le présent de l'impératif des verbes qui se terminent en -οῦμαι est -οῦ.
 - b) Augment syllabique: ἐτίμανα, ἐγάπενα.
 - c) Aoriste passif: ἐγεννέθα, -θες, -θεν = ἐγεννήθην, -ης, -η.
 - d) Aoriste passif 2e forme: ἐλλάγα, -γες, -γεν = ἠλλάγην, -ης, -η.
 - e) Aoriste actif de l'impératif: βλάψον, ποῖσον = βλάψον, ποίησον.
 - f) Aoriste passif de l'impératif: τιμέθετε = τιμήθητι.
 6. Les verbes en -ομαι se changent en -οῦμαι: φανεροῦμαι.
 7. L'accusatif du pluriel des substantifs de la 1ère et 3e déclinaison est -ας: ἐβδομάδας, ἡμέρας.
 8. L'infinitif actif et passif: ἀνασκάψαι, ἀνασκαφθῆναι.
 9. Plusieurs adjectifs anciens sont restés en trois genres et deux terminaisons: ἄξιος, -ος, -ον.
 10. Une grande richesse lexicologique provient du grec ancien: ἄβρωτος, ἄγω, ἄκλερος /ἄκκληρος/, ἄλιζω /ἄλας/, ἄναλος /ἀν-ἄλας/, ἀνασκάπτω, ἀνώγαλος, ἀποβροτίζω, ἀσινός /ἀσινής/, γράνω /γράω/, δάνος /δάνειον/, ἔξηχος /ἔξηχία/, κοτύλα /κοτύλη/, στόλος, χρᾶ /χροιά/.

7. De cette langue hellénique du Pont, qui succéda à la langue des Ioniens, premiers émigrants du Pont, nous n'avons pas de spécimens datant de l'antiquité. Il y a pourtant des témoignages remontant au Moyen-Age et une quantité importante de textes de l'époque moderne.

Depuis plus de 100 ans les savants ont commencé à s'intéresser à l'investigation du dialecte pontique. Depuis la deuxième moitié du 19e siècle s'en sont occupés plus sérieusement E. Kousis, G. Valavanis, D. Iconomidis, G. Hadjidakis (dernièrement le professeur M. N. Andriotis et D. Vayakakos) et surtout A. A. Papadopoulos (auteur de la Grammaire historique et d'un Dictionnaire historique du dialecte).

8. Parmi les points mis en relief ci-dessus on voudrait en faire ressortir quelques-uns seulement, illustrant les conclusions que propose le présent article.

1. La prononciation du η comme ε
2. La négation οὐκί au lieu de l'ancien attique οὐχί et du grec moderne δέν.
3. L'existence de l'emploi de l'infinitif actif et passif.
4. L'existence des mots de provenance ionienne (que l'on trouve dans les études sur le dialecte et dans le Dictionnaire historique du dialecte, mentionné plus haut).

Tous ces points, provenant directement du dialecte ionien, ont été conservés depuis l'ancienne colonisation jusqu'à nos jours, malgré l'existence de facteurs tout autre que favorables à cette continuité de la langue, à savoir :

- 1) La colonisation de la même région par des Grecs parlant d'autres dialectes.
- 2) La langue κοινή hellénistique et byzantine.
- 3) Les peuplades barbares qui côtoyaient les Grecs du Pont et se mêlaient à eux.
- 4) L'élément turc, très important aux temps modernes (1461-1922).

Quand bien même on excluerait les deux derniers facteurs, resteraient les deux premiers, qui étaient bien à même d'altérer le caractère ionien du dialecte pontique par l'influence que pouvaient avoir en particulier le gouvernement, l'instruction et la langue ecclésiastique.

Toutefois le caractère ionien a été conservé bien qu'il eût particulièrement subi l'influence des facteurs que nous venons de mentionner. Ceci ne s'explique que si nous admettons que la cellule principale de la langue était toujours l'élément ionien et que l'homme porteur de langue était dès le début si puissant qu'il finissait chaque fois par s'imposer aux facteurs défavorables.

En d'autres termes la continuité de la langue fut liée à la continuité ethnographique.

9. Comment ceci est-il arrivé? Nous l'avons remarqué au début de cet article, mais nous le répétons: c'est la situation géographique du Pont qui a sauvé l'Hellénisme dans cette région. Cela lui a permis de conserver son expression linguistique et de sauvegarder une continuité avec les premiers colons ioniens.

NOTE

Pour la bibliographie sur le dialecte pontique, aussi bien en ce qui concerne les textes qu'en ce qui concerne leur recherche, nous renvoyons à notre étude, que nous avons employée même dans le présent article: Od. Lampsidis, "Un dialecte qui se meurt", *Archeion Pontou*, 23 (1959), p. 199-205. Cependant nous ne croyons pas inopportun de mentionner ici l'étude de M.S. Kapsomenos: *Die griechische Sprache zwischen Koine und Neugriechisch*, au XI Congrès International des Byzantinistes à Munich en 1958, où le lecteur trouvera une riche bibliographie.

THE SPLITTING AND COALESCING OF WIDESPREAD LANGUAGES

ALLEN WALKER READ

1. Those of us who work in the interdisciplinary area of "socio-linguistics" may feel that we are here at this Congress on sufferance. Certain influences on language that we believe to be important – technological improvement in communication, the honoring of a classical literature, the centralization of a culture, and so on – do not fall directly within the field of linguistics. Yet factors such as these have a potent effect upon the external history and even the internal history of languages, and we feel impelled to call attention to their ever-present pressure.

2. When the speakers of a language become widely dispersed, socio-linguistic influences operate toward one of three outcomes, or an indecisive combination of them: (1) the splitting into several languages, (2) the retention of unity, often at the risk of a rift between literary and colloquial levels, or (3) the development of a *koiné*. It has been usual to assume that the first of these happens; and the image of the "genealogical tree" remains uppermost in the minds of most linguists. Various forces, however, work toward delaying or preventing such progressive diversities. As Martinet has wisely admonished us: "Linguistic research has so far favored the study of divergence at the expense of convergence. It is time the right balance should be restored. Linguistic convergence may be observed and studied in all places and at all times, but its study becomes particularly rewarding when it results from the contact of two clearly distinct structures."¹ It seems clear that linguistic traits can diffuse across genetic boundaries not only in lexicon but also in phonology and grammar.²

3. It is a shortcoming of glottochronology (or lexicostatistics) that it depends upon only the one process of progressive differentiation, without allowing for coalescence. When the emergence of a *koiné* (to say nothing of the hybridizing or creolizing process) has been well attested in numerous places over the world within the past two hundred years, it is reasonable to assume that such developments may have taken place on occasion even in prehistoric times.

4. Let us first deal briefly with selected socio-linguistic influences that have been at work, and then see how they may apply to the far-flung language that I am at this moment using. In the distant past, when migrations took place over land masses, the

¹ André Martinet, "Preface" in Uriel Weinreich, *Languages in Contact* (New York, 1953), pp. viii-ix.

² The evidence is strong as given by Murray B. Emeneau, "India as a Linguistic Area", *Language*, 32 (1956), 3-16.

separation tended to be sufficiently complete so that languages became different. The interrelation of the American Indian languages provides a good basis for inferences about population movements.³

5. Chief among socio-linguistic influences is *technological improvement in communication*, and it may change the language problem radically. Even the ease of transportation afforded by boats on the Mediterranean made it possible for the Greek language to develop a *koiné*, apparently a convergence from disparate dialects.⁴ The Mediterranean may have been partly responsible for an Arabic *koiné*, such as that recently postulated by Ferguson,⁵ and it certainly helped to hold the unity of Latin for a time. The ease of communication in the modern world, provided by new technologies, introduces an overturn of older language relationships, and the setting for coalescence is established.

6. As the second influence we may give the *honoring of a classical or sacred literature*. The holding of a body of literature in high esteem will delay the splitting of languages, but a concomitant result is often a rift between the literary and colloquial levels. Presumably there is a transitional period when the sacred forms of a liturgy become archaic little by little.⁶ The reading traditions of liturgies have been known to continue with a character of their own for nearly two thousand years, as in Hebrew.⁷ The rift can grow into a severe cultural problem. The Greek-speaking world is now torn in the battle between *katharevusa* and *dhimotiki*. This has been called in English "diglossia" (modeled on the French *diglossie*), and is especially troublesome in the Arabic world.⁸ The difficulties are compounded when the classical form of the language is embodied in special writing, such as the Chinese "lexigraphic" or "logographic" system. An apparent unity would be dissipated if the writing system were made to conform to the spoken language.

7. A third socio-linguistic influence is the *centralization of a culture to a prestige center*. For centuries the name of Rome had an almost magical drawing power. Cultures differ widely in the degree of their centralization. In French culture of today, convergence of language is strongly re-inforced by the prestige of Paris. The relation of what is actually said in Paris to the "standard" language is a complicated one. In a personal letter of 1929, Antoine Meillet described the situation as follows:

³ The bases for such inferences were worked out in 1916 by Edward Sapir in his monograph *Time Perspective in Aboriginal American Culture*, now available in his *Selected Writings* (Berkeley, Cal., 1949), pp. 389-462; and they have been extended into more intricate theory by later scholars: e.g., Isadore Dyen, "Language Distribution and Migration Theory", *Language*, 32, 611-26; Ernst Pulgram, "On Prehistoric Linguistic Expansion", *For Roman Jakobson* (The Hague, 1956), pp. 411-17; Joseph H. Greenberg, "Language, Diffusion, and Migration", in his *Essays in Linguistics* (Chicago, 1957), pp. 66-74.

⁴ This development is summarized by Donald C. Swanson in his *Vocabulary of Modern Spoken Greek* (Athens, 1957), p. 9 ff.

⁵ Charles A. Ferguson, "The Arabic Koine", *Language*, 25 (1959), 616-30.

⁶ Cyril Korolevsky, *Living Languages in Catholic Worship* (London, 1957), p. 169.

⁷ Shelomo Morag, "A Special Type of Evolution: Aspects of Research in Linguistic Tradition", *Proceedings of the Eighth International Congress of Linguists* (Oslo, 1958), pp. 425-28.

⁸ Charles A. Ferguson, "Diglossia", *Word*, 15 (1959), 325-40.

The fact that French is spoken at the present time in Paris by a majority of provincials and descendants of provincials and of foreigners and descendants of foreigners, is, I believe, of great importance. At first sight, the effects are not appreciable, but the fundamental result is that "Parisian" is disappearing, drowned in a sort of Koiné (common speech), just as Attic formerly disappeared drowned in the Greek Koiné. The idiomatic character of "Parisian" is being progressively effaced.⁹

In such centers "academies" often take their establishment and nearly always throw their influence in a conservative direction.

8. A fourth socio-linguistic influence is *nationalistic sentiment*. An emotional fixation may alter the behavior of a people in regard to their language, but it may work in opposite ways. We commonly think of "nationalism" as causing the aggressive spread of a language, but some sophisticated speakers may see a danger in it and attempt to hold back the spread. The following is a rather typical sentiment by a patriotic Englishman: "The universal use of English merely as a commercial jargon would be an unmitigated disaster for the world and not least for the native English-speaking peoples, who would inevitably be influenced by the meanness and poverty of their language in its international currency."¹⁰ Weinreich has rightly pointed out that it is possible to discriminate between "nationalism" and "language loyalty", because there can be emotional adherence to a language even when there are not any political separatist aspirations, as with the Rhaeto-romansh in Switzerland or Yiddish speakers over a wide supra-national area.¹¹

9. A fifth socio-linguistic influence is *tolerance or intolerance of variation*. A deeply-ingrained linguistic attitude, found all over the world, is to have contempt for any departures from the accustomed forms of the language (a natural safeguard for continuity). This commonly expresses itself in ridicule of "foreigners' language". Among Bantu children, for instance, is found the game of "playing missionary" and one of the most fun-raising features of it is to speak the native language with a "missionary accent".¹² In the French-Canadian community, the label *joual* was coined in 1959 (derived from a lax pronunciation of *cheval*, i.e., "horse French") as an epithet to pour scorn on the influx of English into their speech.¹³ Under some conditions of language contact, the barriers against accepting changes are let down. A tolerance of variation sometimes develops in a culture, especially when dialects are rather similar, and this permits convergence of forms to take place. To describe this situation, George Philip Krapp coined a very useful phrase, "area of negligible variation". This arises, he says, "when two people think they understand each other, when they are agreed to assume that the subjective synthesis which each one makes is the same".¹⁴

⁹ Printed in *American Speech*, 4 (1929), 431.

¹⁰ Harold Goad, *Language in History* (Harmondsworth, 1958), p. 236.

¹¹ Uriel Weinreich, *Languages in Contact*, p. 99.

¹² George Philip Krapp, *Modern Philology*, 11 (1913), 57.

¹³ Peter Desbarats, in the *National Observer*, Washington, D. C., April 15, 1962, reviewing Augustin Turenne's pamphlet, *Petit Dictionnaire du Joual* (Montreal, 1962).

¹⁴ George Philip Krapp, *Mod. Phil.*, 11 (1913), 58.

An easy acceptance of diversity, sometimes not even noting or hearing the diversity, forms a social background for the development of a *koiné*.

10. Let us now look at present-day English as a laboratory for the interplay of these influences. Such a wide geographical spread would, in distant times, have brought about a fragmentation like that which overtook the Romance-speaking world. Now, however, the developing linguistic attitude is the notion that English consists of co-ordinate branches, each one being an entity, such as "Indian English", "Ceylon English", "Philippine English", "Canadian English", etc. It is hardest for the matrix area (or "Mother Country") to accept this, for any departures from its practice appear to be "barbarisms". Also England is faced with acknowledgment of the existence of "Briticisms".

11. The early transit of English to the American continent took place when the crossing of the ocean took at least six hazardous weeks. This isolation fostered a nationalism such that Noah Webster declared in 1789: "As a nation, we have a very great interest in opposing the introduction of any plan of uniformity with the British language, even were the plan proposed perfectly unexceptionable."¹⁵ Before his death, however, the steamboat, in the 1830's, put England only eight days away. The technological factor of increased communication swung the balance in the direction of coalescence.

12. Americans continued to regard themselves as partaking in the English literary tradition. Noah Webster in 1797 spoke very naturally of his "examination of our best writers from Chaucer to Gibbon"; but a prickly English traveler attacked him in a newspaper with the question, "Pray Sir, in what age did *your* Chaucer and *your* Gibbon live?"¹⁶ America, having no London, did not develop the cultural centralization of England. Thus it has been easier for faddist fashions to affect pronunciation patterns in England than in America, and England has had the greater amount of phonological innovation.

13. Each branch has indulged in its share of ridicule and girding at the other. Some English travelers were vitriolic.¹⁷ Americans in their turn have found the "English accent" comical. One Englishman who was associating with an oysterman in rural Maryland found that the man chuckled much of the time, and he reported: "When we got to know each other better I ventured to ask what kept him so happy. He was tickled, he explained, by my English accent. 'I know as you cain't help it,' he said, 'but you talk so ammighty funny I just gotta layuff.'"¹⁸ Scorn of the "accent" of immigrants, especially of their children on playgrounds, was a hard school, but served to keep American English very little modified. The label "American language"

¹⁵ *Dissertations on the English Language* (Boston, 1789), p. 171.

¹⁶ Francis Baily, in the *New York Gazette*, Dec. 13, 1797, p. 3/1.

¹⁷ Rich material of that sort is given by H. L. Mencken, *The American Language* 4 (New York, 1936); and some of my own collections appear in my studies, "British Recognition of American Speech in the Eighteenth Century", *Dialect Notes*, 6 (1933), 313-34, and "Amphi-Atlantic English", *English Studies*, 17 (1935), 161-78.

¹⁸ Gerry Neyroud, *Americans Are People* (London, 1958), p. 165.

has served a useful purpose in supporting the independence of American English as a "branch", but now it has a misleading, divisive effect. Its falsity is demonstrated by the fact that an American in speaking another language commits an "Anglicism" and not an "Americanism".

14. A relative "classlessness" in the United States has served to foster an "area of negligible variation", so that the regional dialects along the Atlantic seaboard are tending to merge, west of the Alleghanies, into a "generalized American English".¹⁹ There may come a merging between branches, as when the English actor Herbert Marshall declares: "I try to achieve a sort of Middle-Atlantic accent as a happy medium."²⁰

15. There is constant interplay between the other co-ordinate branches of English. Words from Indian English have been brought back to England.²¹ Unexpected channels of transmission can be discovered. Thus Australian English brought its characteristics to California as part of the "gold rush" after 1849. The process of making "rhyming slang" was transmitted,²² and the mixture was thus described in 1866 by an American observer:

A confusion of tongues . . . constituted one of the most striking features in Californian life in the earlier days of the gold discovery. I have heard veritable Yankees, and backwoods-men too, using words in plenty from the vocabulary of London cracksmen mingled with the bastard Spanish of *rancheros* to garnish their own quaint idioms. How came this about? Quite simply. The reputation of the gold mines brought hosts of quondam Cockneys from Australia, and the "greasers" were on the spot before. Your correspondent [*i.e.*, Richard Grant White] would say that such speech was incredible, and too absurd to merit a moment's attention, but it happens that I have repeatedly heard it.²³

16. A special problem is created by those branches that have a strong "substratum" of non-English influence. It is characteristic of native speakers of English to think that such language is "going to the dogs". But if it is a viable everyday means of communication, it should be accepted as a reputable "branch". This may even apply to the "creolized" forms.²⁴

17. Technological advances in communication have so speeded up human contacts that conscious direction of change in language is more and more a possibility. This is not, of course, a new story, when one thinks of Ivar Aasen's *landsmaal* and the

¹⁹ In my paper "The Labeling of National and Regional Variation in Popular Dictionaries", *Problems in Lexicography* (Bloomington, Ind., 1962), pp. 217-27, I have pointed out that the term *general American* is misleading when applied to any dialect of the past, but the emergent pattern of coalescence is producing what can best be called *generalized American English*.

²⁰ Reported by Paul Denis in the *N.Y. Post*, May 8, 1947, p. 56/2.

²¹ G. Subba Rao, *Indian Words in English* (Oxford, 1954), 139 pp.

²² See D. W. Maurer, "'Australian' Rhyming Argot in the American Underworld", *American Speech*, 19 (1944), 183-95, with special attention to the implications of footnote 26 of his p. 189.

²³ Henry Sedley, *Spectator*, XXXIX (March 10, 1866), 271/1.

²⁴ Einar Haugen, *Bilingualism in the Americas: a Bibliography and Research Guide* (= *PADS*, No. 26) (University, Ala., 1956), pp. 32-38; Frederic G. Cassidy, *Jamaica Talk* (London, 1961), ix, 468 pp.

subsequent controversies that have wracked Norway, or the housecleanings from foreign words that have periodically overtaken various languages. But a new term entered the scene in 1950 – *language engineering*.²⁵ Language change by *fiat* is now far more prevalent than ever before, and in most cases it leads to coalescence rather than splitting. New nationalities over the world must solve complex linguistic problems.²⁶ The development of an “Indonesian language”, building on the basis of Malay, is making use of outright imposed decisions.²⁷

18. Intentional interference of this sort is repugnant to many of us. We tend to feel that in our role as scientific students we should be observers of the culture rather than participants in it. We like to watch changes evolving naturally. But under these new conditions the world will not wait. The pressures of “language engineering” will probably become stronger and stronger. If this produces further coalescence of languages, as it is likely to do, then the figure of speech of the “genealogical tree” will apply only to past history. We may need a new figure, such as rivulets flowing into an enlarging stream. Future Congresses will no doubt make reports on the matter.

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²⁵ George A. Miller, “Language Engineering”, *Journal of the Acoustical Society of America*, 22 (1950), 720–25.

²⁶ A good survey was made by Selig S. Harrison, *The Most Dangerous Decades: an Introduction to the Comparative Study of Language Policy in Multi-Lingual States* (New York, Language and Communication Research Center, Columbia University, 1957), [iii], iii, 102 pp., with valuable contributed bibliographies.

²⁷ S. Takdir Alisjahbana, “Language Engineering Molds Indonesian Language”, *Linguistic Reporter*, III, No. 3 (1961), 1 ff.

RESULTS OF THE CONGRESS

ROMAN JAKOBSON

Mr. President, dear friends, distinguished colleagues:

Among the numerous students gathered here I am one of the few who have taken part in most of the previous linguistic Congresses, beginning with the inaugural assembly of 1928, which only eleven members of the present Congress attended. Hence a comparison of the latest Congress with the earlier ones and especially with the Hague prologue inevitably suggests itself. Of course I have no intention of grading the Congresses; that would be like the bias lampooned in the recent comedy of the Polish philosopher Kołakowski: any successor is better than the predecessor, but the predecessor too was all right! I shall merely try to elicit – using my favorite label – the distinctive features of the Congresses compared.

The circular sent by the famous Dutch initiators of the First Congress emphasized as the chief reason for its convocation the ever greater role of general linguistics, which required international cooperation. There arose in fact vital theoretical discussions in the plenary sessions of that Congress, although originally the plenary meetings were intended to deal with practical problems. Yet from the small number of forty papers read in The Hague, only ten were devoted to questions of general linguistics, whereas the remaining thirty were concerned with the development of single languages or language families, especially with historical studies in the Indo-European field. And even of those ten papers focused on general linguistics, seven were preoccupied with diachronic topics. Against the forty papers delivered in the Hague, The Ninth Congress schedules 158 reports and communications, two-thirds of which discussed problems of general linguistics. While over 90% of the Hague papers treated questions of diachrony, at the present Congress only about one quarter of the speakers dealt with this field. The few data quoted exhibit the enormous shift in the curriculum of linguistics during the recent decades and need no further comment.

May I bring to your attention another salient difference between the First and the Ninth Congresses. The assembly in The Hague, with scant exceptions, was a gathering of West European scholars only. It is characteristic indeed that such outstanding American and Russian linguists as Bloomfield, Sapir, and Ščerba figure in the List of Members but did not appear in The Hague. In contrast, the Congress closing today actually represents the linguistic science of all parts of the world.

This representation would be even more complete, if there had been no annoying mishaps in the input and output of scientists, or, to put it in administrative terms, obstructions in the entry and exit permits. Thus, on the one hand, our Congress has been deprived of such prominent world scholars as Wolfgang Steinitz, member of the CIPL, who was unable to land in this country, and, on the other hand, despite all the requests and efforts of our Congress Committee, outstanding representatives of the world-renowned tradition of the Cercle Linguistique de Prague could not cross the borders of their native country to take part in our discussions, so that our forthcoming Transactions will unfortunately have to repeat this telling sentence from the Oslo Proceedings: "... was prevented from attending the Congress." It seems to me that the international science of language and international science as a whole can give only one dignified, Archimedian retort to blind and oppressive bureaucratism, the cross-world "red tape": *Noli tangere circulos meos*. – I thank you for your approval.

Gaps in my survey of the Congress activities are unavoidable. During these days I have often recalled the satiric lines of the Russian poet Majakovskij: "They are in two meetings at once . . . Willy-nilly one has to bifurcate." To take part in eleven simultaneous tempting group meetings, as one had to do in our Congress, is an even more sophisticated performance.

As starting point for this concluding summary, may I use a beautiful thesis from one of the plenary reports: "Le sens d'une unité linguistique se définit comme sa capacité d'intégrer une unité de niveau supérieur." The concept of INTEGRATION is one of the focal concepts in present-day science and life. The conflict of two polar ideologies – Integration and Segregation – has been thoroughly discussed in Chatterji's socio-linguistic paper. Applying these notions to the Congress itself, one may state that here the spirit of integration has definitely predominated. If we compare, for instance, the current situation of linguistics with its status in 1948, at the time of the Paris Congress, the latter period can be characterized as a stage of relative particularism, a segregation especially manifest during the late forties in the linguistic life of the USA and the USSR. Since that time the picture has changed radically. I should like to bring to your memory the significant statement made in the closing session of the Oslo Congress, in 1957, by B. A. Serebrennikov: "We, linguists of the Soviet Union, are by no means partisans of sectarianism and isolationism in science." Further developments have confirmed the accuracy of this assertion. Particularly, between Russian and American linguistics now we observe not only a careful mutual attention and comprehension, but, moreover, convergent ways of progress. In both hemispheres, diverse regional schools bearing the names of cities or preceptors are losing their exclusiveness and forgetting their recent dissensions. If there are still here and there reservations of regionalism or *esprit de clocher*, they perhaps attract the lovers of antiques but have hardly a vital part in the worldwide scientific search.

The drive toward integration in space is paralleled by a similar process in time. Yesterday linguistics, said to be structural, stood defiantly opposed to the traditional

doctrine. If today we hear slogans calling for the rehabilitation of "traditional grammar", this is neither retreat nor eclecticism. In Hegel's terms, one might say that the antithesis of the traditional tenet yielded to a negation of negation, i.e., to a synthesis between the immediate and the remote past. This rehabilitation of the latter, which has been witnessed at this Congress, must not be mistaken for an imitation or actual restoration of the past invoked. The ancestors would hardly recognize their descendants, even though the latter claim that their "roots are firmly in traditional linguistics".

The integration in time means a substantial widening of horizon. Typical examples could be noted in the section of this Congress, "History of Linguistic Studies". It pleaded for the recognition of the centuries-old continuity in our science, looked particularly for precursors of modern linguistics in the Enlightenment and the Renaissance, and pursued still further this retrospective path back to the ancient and perennial Indic science of language.

If the First Congress was said to be "un acte d'émancipation", at present the autonomy of linguistics is definitely ensured, and the question of bridges linking this autonomous area to other sciences can and must be advanced – a question of cooperation without any capitulation. The contact gradually growing closer between linguistics and logic found its clear-cut expression this week in two portentous papers – Šaumjan's and Chomsky's – each entitled "The Logical Basis of Linguistic Theory", and today we have faced the crucial question, "whether or where one can draw a natural bond between grammar and 'logical grammar' in the sense of Wittgenstein and the Oxford philosophers". It is gratifying that the philosophy of language repeatedly came up for discussion in a Congress of Linguists.

The rapprochement with mathematical logic, the limits and prospects for the use of set-theoretical models in linguistics, but first and foremost the ties of linguistics with mathematics in its statistical aspect, were vividly debated here. Quantification was recommended as an aid to syntactic analysis, as an additional, auxiliary instrument for comparative linguistics, and in general as a useful, supplementary means, always presupposing qualitative analysis, as Spang-Hanssen, expert both in mathematics and in the science of language, has reminded the partisans of quantitative linguistics. The danger of overestimating the statistical criterion as an independent tool in so-called "glottochronology", and misleading discrepancies in this theory were brought up for discussion. The computer was shown to be a new, highly valuable technical support for the analysis of syntactic and morphological structure, for the determination of phonemic isotopy, and for dialectology.

We have been warned against uncritical transposition of mathematical designations and concepts into linguistics, but we must in turn give warning against a hypercritical purism which is prone to reject even such terms as "redundancy" as an alleged loan-word from information theory, although this term and notion have actually been borrowed by mathematicians from the science of language, where they have been enrooted at least since Quintilian. In any case, one could at present hardly envisage

linguistic analysis without the two polar procedures of eliminating redundancy and utilizing redundancy, as was persuasively exhibited in Kenneth Pike's communication.

The use of computation in machine translation involves perplexing linguistic problems. Bilingualism, for a long time underestimated or even disregarded, is gradually becoming one of the main concerns of our science. It is noteworthy that human translation began to attract due analytic attention only when there arose a possibility of comparing this pattern of translation with another, as Chao said, cognate and at the same time essentially different pattern, namely, machine translation. Whatever the practical outlook of this novel, worldwide experimentation, the emergence of machines as agents in verbal output and input, hitherto exclusively inter-human operations, has yielded rich information about coding and recoding processes and about the make-up of the verbal code.

It is fortunate that "Linguistic Aspects of Translation" has been chosen as one of the five basic topics for our plenary sessions. Andreev's report on this subject is full of profound and suggestive observations, hints, and meditations, and the only thing we missed was the presence of the rapporteur, indispensable for a really fruitful discussion. One can only share Hammerich's confidence that the technical experiments with language, speech, and writing which have been displayed during the Congress and which are sometimes scorned today as mere circus tricks, will prove to be of theoretical and practical usefulness. When the piano was invented, nobody believed it could play a role in serious music, and in fact for a long time it was used only for buffoonery in circuses and music halls.

In the forties, with their sectarian parochialism in linguistic life, Giuliano Bonfante had great difficulty in his efforts toward finding an Italo-American *lingua franca* for our science. Yet since that time both he and his American opponents have matured and changed, and there is prospect of a common parlance. His creed, which he presented here, is in perfect agreement with the drift toward integration manifested by this Congress. In particular, as regards his statement that "the Crocean or esthetic theory of language can and must be *integrated* with the structural theory" and that "special attention must be devoted to the 'peripheral' zone of language – slang, jargon, affective and expressive terms, child language, onomatopoeia, interjections", we observe at our meetings how strongly linguists today are absorbed precisely with the structure of all these "peripheral" phenomena. Let us quote just a few topics of the papers presented: "affective linguistic signs", expressive and appellative phonology with particular reference to the manifold function of pitch, the non-intellectual "spheres of communication", "emphasis as a grammatical category", "the emotion in a sentence", sound-symbolism, "the development of grammar in child language". All these problems are being gradually incorporated into the structural analysis of language.

Crocean emphasis on verbal creativity finds striking correspondence in the final report to this Congress, where it was stated, "A theory of language that neglects this 'creative' aspect of language is of only marginal interest." Likewise in one of our

earlier plenary sessions, the sentence was adequately characterized as “*création indéfinie*”. This suddenly spreading emphasis on creativity finds another, even more radical expression in the report on translation, where the attempted creation of an artificial, “intermediary language for machine translation” prompts the rapporteur to the bold conclusion that “till now we linguists dared only *explain* languages”, while now the “time has come when our chief occupation must be *creating* them”. Haugen’s neat remark about HT (human translation) as “re-creation” points in turn to the creative aspect of language, Humboldt’s *energeia*.

A thorough integration of linguistic studies requires an earnest concern with the diverse functions of language. For the first time a special section of a linguistic congress has dealt with stylistics and poetics: the study of poetry has been conceived as inseparable from linguistics and as its pertinent task. A quantitative expression of today’s lively interest in poetics is the eloquent fact that even on the stairs of the large Kresge auditorium, there was no place left vacant during the meeting of this section. “The description of a poem” has become an appropriate and honorable topic; verses of Wallace Stevens, Yeats, Tennyson, and Moses ibn Ezra were analyzed and talked over (by de Groot, Halliday, Schramm), and, according to the spirited conclusion of Benveniste, henceforth any periodicals or societies of linguistics should carry the supplementary words, “and of poetics”.

It was repeatedly pointed out that diversity among and within languages can and must “be studied along three synchronic dimensions – geographical, social, and stylistic”, in Bright’s and Ramanujan’s formulation. These three aspects of variations and their interplay were intently discussed, especially in connection with diasystems, interdialectal and interlingual borders, contacts, borrowings, mutual adjustment, “tolerance or intolerance of variation”, role of bidialectal (and multidialectal) or bilingual (and multilingual) individuals or communities. Several instructive “socio-linguistic” papers (e.g., by Gumperz and Read) disclosed the promising development of this vital field of research, first outlined by Lévy-Bruhl at the plenary session of the Copenhagen Congress of 1936. Yet one can hardly view the socio-linguistic influences on language as merely extrinsic factors. If we approach linguistics as just one among the conjugate sciences of communication, then any difference in the role of communication may evidently have “a potent effect” upon verbal communication. Thus the role assigned to the wider radius of communication by a nomadic society leads both to technological improvements in transportation and to a coalescence of language.

In Martinet’s report, “Structural Variation in Language”, variations throughout space and time have been confronted, and their explicative study “beside a purely descriptive one” has been demanded. In addition to his sound emphasis on the indissolubility of TEMPORAL and SPATIAL variations, now one will have to examine and clarify the inseparableness of TEMPORAL variations from the STYLISTIC ones, and the transition from reversible fluctuations to the irreversible mutations, which is still far from being apprehended in the recent treatises of historical phonology.

The strenuous and continuous advance of synchronic research gave the impulse

for a new discussion of linguistic changes, the degree of their lawfulness, their nature and types. In particular, the pressing need of a syntactical reconstruction for comparative linguistics was convincingly exemplified both with Indo-European (Watkins) and with Eskimo-Aleut material (Bergsland). The present state of general and Indo-European linguistics urgently requires, as Georgiev rightfully claimed, a new *Grundriss* and a new etymological dictionary, up to date both in their methods and factual data, particularly as to the inventory of languages referred to.

Kuryłowicz's report to the plenum, "On the Methods of Internal Reconstruction", offered us a fascinating outlook upon the Indo-European grammatical prehistory. These are, as the rapporteur has clearly shown, "diachronic conclusions that may be drawn from a synchronic analysis of linguistic data".

In the foundations of this inquiry there are three salient features which unite the report in question with the reports on the levels of linguistic analysis and on the logical basis of linguistic theory. One of these cardinal features is the primary concern with the PARADIGMATIC axis, in contradistinction to the exclusive care for the SYNTAGMATIC axis in the distributionalists' approach of the recent past.

A second feature, closely connected with the first one and no less important, is the recognition and investigation of the HIERARCHICAL ORDER within the paradigmatic set, an attitude diametrically opposed to the depreciation of ordering in the dogma of the orthodox distributionalists. Is "hierarchy" not the catchword of this Congress? – There proved to be distinctly hierarchical ties not only between different levels of language but also between correlated units of one and the same level, and it is not by chance that the asymmetrical relation between the marked and non-marked opposites in language again and again emerged in the course of the deliberations. The principle of IRREVERSIBLE PREDICTABILITY has been exhibited as an efficient inference from this hierarchical arrangement.

The third feature unifying the cited reports is a steady and consistent search for the UNIVERSAL, PANCHRONIC foundations of this order. That means a definitive rejection of such paradoxical, defeatist – well, simply antiscientific – slogans such as "Languages can differ from each other without limit and in unpredictable ways". Such an alleged want of predictability would have meant inevitably the ruin of linguistic science. The bankruptcy of this gloomy paradox permits us safely to anticipate further Congresses of linguists. It is to be noted that the search of our days for language universals found expression in our Congress, where, for instance, word order (Greenberg) and intonation (Bolinger) were examined from this point of view.

Benveniste's report devoted to the levels of analysis and splendidly synthesizing decades of his personal and international research gave insight into the hierarchy of all the CODED linguistic units (*le système de la langue*), from the lowest, the distinctive feature – or *merism*, as he proposes to term this ultimate entity – to the highest, the sentence, which at the same time functions as a constituent of the free, no longer coded discourse. Special papers were concerned with some problems of this hierarchy, especially the ranks of submultiples (Buysens) and their immediate frame of

reference (Seiler). The complex question of verbal phenomena transcending the sentence level, i.e., relationships between the sentence and its context, were likewise tackled (Reichling and Uhlenbeck; Winburne), as well as the opposite, correlate problem of "context-free language" (Schützenberger).

If we compare the two reports dealing with the fundamentals of language analysis, we remark that Chomsky's courageous lesson on "The Logical Basis of Linguistic Theory" starts with syntax and descends to the lower levels, by dissociating the higher units into their constituents from the viewpoint and in the terms of the higher level. Conversely, the report about the levels of analysis chooses *une démarche inverse, partant des unités élémentaires, à fixer dans la phrase le niveau ultime*. The former procedure, *les opérations descendantes de l'analyse*, is pointed toward the dependence of the inferior levels upon the superior ones, whereas the latter method, *les opérations ascendantes*, first reveals the specific, autonomous character of each level. It would be quite arbitrary, I dare say, to consider one kind of analytic operation more realistic, more adequate, or more efficient than the other. The two procedures, as Niels Bohr would say, stand in a COMPLEMENTARY RELATIONSHIP to each other. The descendent operation underlies, for instance, the development of morphophonemics, which, as several speakers have disclosed (e.g., Lehiste, Harms, Graur, and Rosén), occupies an ever more important place in the build-up of scientific grammar, both synchronic and diachronic. On the other hand, the thoughtful paper by Malmberg brings new evidence for the autonomous structure and stratification of the phonemic system itself.

One cannot but agree with the statement of the final report, that "as syntactic description becomes deeper, what appears to be semantic questions falls increasingly within its scope". Yet morphological categories likewise request a search for their semantic value, as has been eloquently advocated here in several papers (Ferguson, Haas, Palmer, Barbara M. H. Strang). It has been frankly said that since in our analytic procedures all of us make use "of semantic categories, we are less than honest if we dismiss these as mere 'heuristic guides' ". In the identification of morphemes, "the attempt to do without semantic criteria" has been pronounced philosophically shaky, and impracticable besides. Briefly put, the tautological proposition that linguistics without meaning is meaningless is no longer viewed as a mentalist aberration. It is indicative that no one raised his voice to argue in favor of the former mechanistic distributionalism. However, the reaction against its monopoly in no way denies the experimental value which this working hypothesis and technique have had for linguistic analysis, and the possibility of approaching even semantic problems in a manifestly distributionalist way was touched upon.

The merging of semantics with grammar has been necessarily accompanied in the Congress deliberations by an insistent demand for the integration of lexicology (Glinz, Heilmann, Coates), and of onomastics as its particular section (Utley), into structural linguistic studies and for relating lexicology with grammar. As has been demonstrated by Lounsbury, certain sections of vocabulary "can be subjected to a kind of analysis similar to that given other paradigmatic sets in a language".

As soon as questions of the interrelation between the external and internal sides of the verbal sign were expressly raised, the corollary, an earnest concern with problems of synonymy and homonymy, was evinced by the assembly, and these questions promise to become one of the focal topics of a future Congress. The idea of semantic value furthered contemplation of two conjugate notions: sense and nonsense, and the latter in turn proved to present a whole scale of distinct species (*Unsinn*, *Widersinn*, etc.), which must be carefully discriminated in linguistic analysis, according to the warning repeatedly given during this Congress.

The attention paid here to the different questions of discourse, to speech recognition, to the diverse functions of language in culture and society, and to the vast field of applied linguistics, illustrates once more how far our science is now from its definition, erroneously (as Godel discloses) attributed to Ferdinand de Saussure: "La linguistique a pour unique et véritable objet la langue envisagée en elle-même et pour elle-même." No doubt our science views language "in itself", yet not only "for itself", but also for the sake of language users and molders, because language is a tool, and the autarkic self-sufficiency of a tool would be a contradiction in terms.

Language and discourse (or, in other terms, code and exchange of messages) appeared to be in a relation of MEANS and ENDS, the same relation as that between the *signans* and *signatum* or as the relation between form and sense (in Benveniste's definition, the relation between dissociation and integration). The mode in which opposition stands to identification is likewise a relation between means and ends. It is symptomatic indeed that the pivotal role of IDENTIFICATION, first advanced by de Groot over thirty years ago, has insistently come to light in the meetings of these days.

To everyone who watched the discussions of this Congress, the general tendency to overcome any leveling and flattening of language and linguistics became quite obvious. The focusing upon the hierarchy of levels revealed a superposition of levels, sublevels, and intermediary ranks in language, a strict order in the rules of derivation, and different degrees of abstraction in linguistic analysis. Each unit of language, and language itself, appeared simultaneously and indissolubly as a whole and as a part of a superior, wider context; and even if an individual linguist could still confine himself to some separate ranks of this scale, none of the ranks may be omitted or expunged by linguistic science. It is precisely the unrestricted scope of this multifarious science which has been patently demonstrated by our Ninth Congress.

Harvard/M.I.T.

CONCLUDING REMARKS

EINAR HAUGEN

I have had the privilege of listening to many papers and of talking with many linguists during these days. I am happy for the quality of the papers, for the patience of both speakers and listeners with the limitations which such a congress imposes on everyone. Thank you all for coming, for discussing so freely and incisively the papers presented. The *social* benefits of the Congress have been evident. The *intellectual* benefits will only appear after you return home and have a chance to sort out your new impulses and impressions. I hope that the seeds sown here in the field of linguistics may prove to be fertile and bear many fold in the years to come.

A special vote of thanks is due to the members of the Executive Committee for their enthusiastic cooperation for the success of the Congress. Our secretary, Morris Halle, has borne the burdens of carrying out our instructions; but he had done much more by contributing ideas of his own, yet without departing from the policies laid down by the Committee. Secretary-General Locke has had the special job of organizing the work of the local committee. We have had more members present than at any previous Congress, so that the logistic problems have not been minor. The results have exceeded our fondest hopes. My gratitude is also extended to the members of CIPL, who have supported our efforts to win and retain the confidence of our colleagues, and to the organizations which have given us financial support, the ACLS, the NSF, IBM, and Pan-Am. All of you have contributed to a success which should not have been envisaged a mere year ago. To all of you I say, as bilingually as possible, Bon Voyage!

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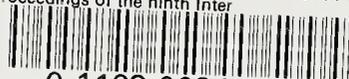
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