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of the
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7-th
SEVENTH INTERNATIONAL CONGRESS
OF LINGUISTICS

PROCEEDINGS
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PROCEEDINGS OF PREVIOUS CONGRESSES

- I. *Actes du Premier Congrès international de Linguistes, à La Haye du 10-15 Avril 1928.* A. W. Sijthoff's Uitgeversmaatschappij. N.V. — Leiden [1930]. viii + 198 pp.

- II. *Actes du Deuxième Congrès international de Linguistes, Genève 25-29 Août 1931.* Librairie d'Amérique et d'Orient. Adrien-Maisonneuve, 5 rue de Tournon, Paris (6^e), 1933. 254 pp.

- III. *Atti del III Congresso internazionale dei Linguisti, (Roma, 19-26 Settembre, 1933).* A cura di Bruno Migliorini e Vittore Pisani. Firenze, Felice Leimonnier, 1935. xv + 449 pp.
 The following French title also appears on p. ii: *Actes du IIIème Congrès international de Linguistes . . .*
 Rédigés par Bruno Migliorini et Vittore Pisani.

- IV. *Actes du Quatrième Congrès international de Linguistes, tenu à Copenhague du 27 Août au 1^{er} Septembre 1936.* Einar Munksgaard, Copenhague, 1938. 305 pp.

- V. Owing to the deterioration in the international situation the Vth Congress was not held. The following preliminary material appeared:

Vme Congrès International des Linguistes, 28 Août - 2 Septembre 1939.

1. *Première publication Réponses au Questionnaire*. Imprimerie Sainte Catherine, Tempelhof 51, Bruges [1939] 104 pp.
2. *Première publication Supplément Réponses au Questionnaire (Suite)*. [1939] 53 pp.
3. *Deuxième publication Rapports*. [1939] 147 pp.
4. *Résumés des Communications*. [1939] 66 pp.

[The following document was prepared by a Committee appointed at the IVth Congress, and it was to be submitted at the Vth Congress:

Dokumente zur Interpunktion europäischer Sprachen. Elanders Boktryckeri Aktiebolag, Göteborg, 1939. xlii + 57 pp.

It is not a document sponsored by a Congress though it should be included for the sake of bibliographical completeness.]

- VI. *Actes du Sixième Congrès international des Linguistes*, publiés sous le patronage du C.I.P.L. avec le concours de l'U.N.E.S.C.O. par les soins de M. Michel Lejeune. Paris, Librairie C. Klincksieck, 11, rue de Lille (VIIe), 1949. lxxi + 608 pp.

The following English title also appears on p. ix: *Proceedings of the Sixth International Congress of Linguists*, published under the auspices of C.I.P.L. with the assistance of U.N.E.S.C.O. Edited by Professor Michel Lejeune.

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OPENING SESSION
HELD IN
THE SENATE HOUSE
UNIVERSITY OF LONDON
SEPTEMBER 1 1952

THE STATE OF NEW YORK

IN SENATE

January 11, 1892

REPORT OF THE

COMMISSIONER OF THE LAND OFFICE

ADDRESS BY THE PRESIDENT,
PROFESSOR SIR RALPH TURNER

Your Excellencies, Distinguished Colleagues, Ladies and Gentlemen,

This is the seventh of the series of congresses that was inaugurated at the Hague in 1928. That first International Congress of Linguists was followed by others at Geneva in 1931, at Rome in 1933, and at Copenhagen in 1936. The Congress organised to be held at Brussels in 1939 scarcely came to fruition in the shadow of the tragic events impending in the autumn of that year which were destined to bring such miseries upon mankind. Peace of a sort returned to the world in 1945, and after a gap of nine years the Sixth Congress was held in Paris in 1948, when many linguists had the opportunity of meeting again for the first time after the break caused by the long years of war. Four years have gone by since the Congress in Paris and now we meet in London where we British linguists are greatly honoured to see in our midst so many distinguished colleagues from other countries.

The importance which is attributed here to the meeting of our Congress is attested in no uncertain manner by the fact that Her Majesty the Queen has graciously consented to be Patron of the Congress, an honour not often accorded to meetings of this kind in our country. Moreover it appears to me not inappropriate that it should be the Philological Society which invited the Congress to meet for the first time in Great Britain and that the University of London should be our meeting-place. It was one of the greatest linguists of our age, Antoine Meillet, who said: "*Les anglais sont bons lexicographes*," and I need scarcely remind you that it was the Philological Society which nearly one hundred years ago inaugurated and for long controlled the immense undertaking which was at first known as the New English Dictionary and now is named the Oxford English Dictionary, and which, completed with its supplement in 1933, is an invaluable and indispensable instrument for research of every kind into the history of our English language. In the Minutes of the Philological Society, when mention of this work is made, it is still referred to as "*The Society's Dictionary*."

The revival of the Philological Society, which had fallen on evil days during the First World War, coincided with, and indeed was largely instrumental in stimulating, a notable increase in linguistic studies in the University of London. Not only was London the first university in this country to establish a Chair of General Linguistics, held by our colleague Professor J. R. Firth, but to the institutions, notably University College, the original home of the Philological Society and of the first university department of Phonetics in this country, and King's College as well as other Colleges of the University, where the study of the classical languages and the modern languages of Europe had been long pursued, was added another institution devoted to the languages and civilizations of Asia and Africa.

The foundation of the School of Oriental and African Studies, and particularly its great growth during the past five years, and the many young scholars, who are now studying the more widely documented classical languages of the Orient as well as recording and analysing many of the languages both of Asia and Africa which are without literary history, have notably augmented the resources of this University for linguistic studies. This and other developments within the University of London and other British Universities have placed this country in a more favourable position for the pursuit of our subject and have made us happy that the Permanent International Committee of Linguists was able to accept our invitation to hold this Seventh Congress in London.

Many of you, no doubt, are fully aware of the normal pattern on which these congresses of linguists are organized, but I think it is not inappropriate that I should remind this assembly that the linguists were in fact pioneers in working out a form of international congress that has come to be accepted by the U.N.E.S.C.O. Co-ordinating Committee as a standard pattern.

You will all have received in preparation for the Congress the preliminary brochure which was circulated in order to place in your hands a copy, both of the contributions made by individual members and of the reports drawn up by scholars invited to undertake this work. In this way we have all been given the opportunity to familiarize ourselves with topics that are to be dealt with during the Congress, and for this

reason we shall be able to devote the whole of the time available, in both the plenary sessions and the meetings of the sections, to the discussion of the material presented in the brochure. This means that, following the normal practice of the Congresses of Linguists, there are to be no papers read either at the plenary sessions or during the meetings of the sections. The Organizing Committee of the Congress, however, felt it right that members should not be deprived of the opportunity of hearing short papers read by specialists, who were desirous of presenting to the Congress the results of some of their work not immediately bearing on the questions to be discussed. For this reason a special period has been set aside at 9.30 each morning for the delivery of such short papers to interested groups of members, who, it is hoped, will avail themselves of the opportunity to hear and to discuss these special contributions.

With these preliminary remarks I now have the honour to declare the Seventh International Congress of Linguists to be open.

ADDRESS BY MR. PREM KIRPAL,

DEPUTY DIRECTOR, DEPARTMENT OF CULTURAL ACTIVITIES,
U.N.E.S.C.O.

Mr. President,

May I thank you and your committee for having been so good as to invite a representative of U.N.E.S.C.O. to attend the proceedings of the Seventh International Congress of Linguists. It has not been possible for the Director-General, Dr. Torres-Bodet, and the Director of the Department of Cultural Activities, Professor Jean Thomas, to join you today, which they would have very much liked to; they have, however, asked me to convey to you their greetings and best wishes for the success of your work, in which U.N.E.S.C.O. is greatly interested.

Ever since the establishment of U.N.E.S.C.O. there has been very close co-operation between it and the linguists, and among this distinguished gathering of scholars U.N.E.S.C.O. can count many old and esteemed friends who have given much of their valuable time and experience to the cause of international understanding. The President of the Permanent

International Committee of Linguists, Professor Alf Sommerfelt is one of the oldest members of our Executive Board and to him U.N.E.S.C.O. owes a deep debt of gratitude for his ceaseless interest, wise guidance, and unsparing efforts in the implementation of our projects. There are also many others present here without whose help the collaboration between U.N.E.S.C.O. and the various " disciplines " of Human Sciences could not be as happy and fruitful as it is today.

The Seventh International Congress of Linguists opens under the most favourable auspices. There could not be a more fitting place to meet than the great city of London which, through its long and glorious history, has contributed immensely to the enrichment of the human spirit. Strong and assured in its great achievements, and always tolerant and generous towards peoples and causes, London has never been second to any town in the world in defending and promoting truth and freedom — and all the human values, which give meaning and significance to life and are the common heritage of mankind. The spirit of man which this great city has nourished with tenderness and strength is the same spirit which the science of Linguistics aims to understand and serve through the patient and toilsome study of languages. Here in London the International Congress of Linguists will, indeed, deliberate in the midst of inspiring surroundings. The University of London, where we meet this morning, is well-known for its special emphasis on Linguistics, and its well-established schools for the study of Oriental, African and Slavonic languages enjoy the highest reputation.

It was more than 24 years ago that your first congress met at the Hague in 1928. Since then you have met in Geneva, in Rome, in Copenhagen, in Brussels, in Paris, and now in London. The names of these cities mark the various stages of your activities, which have constantly developed; even the interruption caused by the last war did not prove to be a very serious set-back to your work; on the other hand, it appears to have opened new horizons and led to unforeseen developments, resulting from the urgent need for international co-operation in all fields of knowledge and action.

During the years immediately following the war it was of course, difficult to renew contacts between scholars scattered all over the world, to organize meetings, and to ensure publica-

tion of valuable studies. Thanks to the efficiency and resourcefulness of the Secretariat of the Permanent Committee personal contacts between the linguists were soon re-established and rapid progress was made in spite of financial difficulties.

U.N.E.S.C.O. can look back with pride and satisfaction to the beginning of its collaboration with the Permanent International Committee of Linguists. The assistance which U.N.E.S.C.O. could afford was small and limited but your Committee made the best use of it. The most striking example is the preparation of the Linguistic Bibliography, which has become absolutely indispensable to scholars and specialists. U.N.E.S.C.O. has also been able to facilitate the meetings of your three specialized Commissions on Linguistic Inquiry, Statistics and Terminology. Through the work of these Commissions and by effectively organizing documentation and information services you have made notable contribution to the execution of U.N.E.S.C.O.'s programme and to the development of international co-operation in your field of activities.

With its excellent organisation and remarkable achievements, it was natural that the Permanent International Committee of Linguists should have played a leading role in the preparatory work which led to the formation of the International Council for Philosophy and Humanistic Studies. Along with the International Academic Union, the International Federation of Societies for Philosophy, the International Commission on Folk Arts and Folk-Lore, and the International Committee for Historical Sciences, your Committee was one of the five International organisations invited by U.N.E.S.C.O. to participate in the activities which resulted in the creation of the International Council for Philosophy and Humanistic Studies in 1948. Ever since you have given your fullest co-operation to the Council and have successfully promoted active collaboration with the organisations representing other disciplines in the field of Philosophy and Humanistic Studies.

Like its sister-organisations your Committee now contributes to the work of U.N.E.S.C.O. through the International Council for Philosophy and Humanistic Studies. In particular cases, however, the Director-General may request direct assistance. For instance, in compliance with a resolution of the General Conference, the Director-General has requested your Committee to make a study of the structure of languages spoken in certain

limited areas of Africa, Oceania, India, Ceylon and America. This research, which will be conducted by qualified scholars in Linguistics, will serve as a basis for the study of cultural differences and modes of thinking and will form an important part of U.N.E.S.C.O.'s programme in the fields of Human Rights and racial problems. In view of the tensions of the present times, which threaten the inner peace of man as well as the social order around him, it is of great importance to undertake a really scientific study of cultural differences between peoples in their modes of thought, and such a study can be most appropriately made on the basis of language, the structure of which is so relevant to logic. These studies, will, undoubtedly, be of far reaching importance to U.N.E.S.C.O. and to all those who are engaged upon the task of building firm foundations for the "intellectual and moral solidarity of mankind" by the slow but sure way of understanding the nature of man and the different manifestations of his creative spirit.

In the implementation of this project and other similar activities U.N.E.S.C.O. relies upon the co-operation of the linguists. I have no doubt that this collaboration will increase in the future. Next November in Paris the General Conference of U.N.E.S.C.O. will approve a programme of activities for the years 1953 and 1954. It will decide upon the assistance which U.N.E.S.C.O. would be able to give by way of subventions and otherwise to organisations, like the International Council for Philosophy and Humanistic Studies, of which your Committee is a member. Already, the representatives of your Committee have indicated the projects for which they would like to receive financial assistance, particularly for the preparation of the bibliography, for certain publications, and for holding meetings of various commissions and committees. Perhaps these proposals and other new projects and suggestions will be discussed by the Congress in this session. May I assure you that U.N.E.S.C.O. will study with the greatest attention any recommendations formulated by your Congress, which includes eminent specialists from all regions of the world and, consequently, exercises the greatest authority in the field of linguistics.

I wish all success to your deliberations, and I am sure that great good will come out of these.

ADDRESS BY PROFESSOR A. TOVAR,
OFFICIAL SPANISH DELEGATE

M. le Président,

Permettez-moi de remercier publiquement la Présidence du Congrès, qui m'a fait l'honneur de cette adresse dans cette session solennelle. J'ose interpréter cela comme un témoignage de confiance à la science espagnole qui a réussi à conquérir sa place dans le vaste champ de la linguistique. C'est maintenant la première fois, me semble-t-il, que les Universités et le Conseil Supérieur de la Recherche Scientifique sont représentés officiellement à un Congrès International de Linguistique, quoique d'illustres représentants aient pris part à des réunions antérieures. Qu'il me soit permis de rappeler la présence au Congrès tenu à La Haye de M. de Urquijo, savant basquissant, disparu depuis. Je voudrais que ma présence ici signifîât pour tous les savants réunis ici la preuve de notre volonté de collaboration et de notre résolution de travailler dans un champ chaque jour plus élargi avec les savants de tous les pays.

Il est vrai que la linguistique espagnole n'est pas cultivée avec la dévotion et l'ambition qu'aurait laissé espérer le passé. C'est l'Espagne qui a découvert le continent américain à la linguistique, et également l'immense champ du chinois, du japonais et des langues malayo-polynésiennes. On pourrait écrire une histoire de ce grand travail couronné par le *Catalogue des Langues* de Hervas qui débouche comme un fleuve dans la mer de la science moderne à travers les écrits de Guillaume de Humboldt.

Mais, comme l'on sait, l'histoire de mon pays a toujours subi de grandes crises, et tout ce passé de trois siècles fut balayé. Et, fait symptomatique, le premier grand nom de la nouvelle période est le philologue colombien Rufino José Cuervo (1844-1911), qui dans une époque difficile et employant les méthodes les plus rigoureuses, a inauguré une nouvelle tradition. Il resta longtemps le maître incomparable de notre linguistique auquel succède Menéndez Pidal.

Les travaux d'un savant comme Don Ramon Menéndez Pidal ont réussi à former une véritable école hispanique de linguistique. A Menéndez Pidal l'on doit l'emploi des méthodes les plus exactes, combinant la sagesse avec la fantaisie. Grâce

à lui les origines de l'espagnol ont été étudié d'une façon parfaite, et le nom des élèves de Menéndez Pidal ferait le catalogue de presque tous les romanistes de l'Espagne et l'Amérique de notre langue. Amado Alonso, disparu depuis, a fondé une véritable école à Buenos Aires et plus tard ses disciples se répandent au Vénézuéla, aux Etats Unis et surtout au Mexique. Navarro Tomás, le phonéticien de l'école, après son long enseignement à Madrid, a continué aux Etats Unis, en publiant aussi des travaux sur l'espagnol américain. A Madrid Dámaso Alonso, aussi fin dans la critique littéraire que dans l'étymologie ou la dialectologie, a continué le travail de l'école, et l'on pourrait citer aussi d'autres anciens amis et élèves de Menéndez Pidal, ainsi que des représentants des nouvelles générations: García de Diego, Lapesa, Zamora Vicente, E. Lorenzo, F. Lázaro, Mme. Canellada, Mlle. Casado et d'autres. Une quantité toujours croissante de thèses et monographies complète chaque jour la carte de la dialectologie hispanique des deux côtés de l'Atlantique. Dans le champ de la lexicographie il faut citer un maître comme J. Casares, le Secrétaire de la Royale Académie Espagnole, le professeur Gili Gaya, et nous aurons bientôt un dictionnaire étymologique monumental de notre langue, dû au savant catalan J. Corominas.

Mais il ne suffit pas de continuer ces travaux dont Cuervo fut le précurseur et Menéndez Pidal le maître. Les nouvelles générations s'intéressent à un champ plus large et à des problèmes modernes. La phonologie, le structuralisme ont eu déjà des échos considérables dans la linguistique espagnole, et c'est à un des plus jeunes professeurs, Alarcos Llorach, que l'on doit deux ouvrages importants dans ce nouveau champ d'investigation: *Fonología española* 1950, *Gramática estructural (según la escuela de Copenhague y con atención especial a la lengua española)* 1951. Il faut citer spécialement le livre de S. Fernández Ramírez, la première grande grammaire d'une langue moderne basée sur des méthodes descriptives. D'autre part un de mes collègues, M. Sánchez Ruipérez, prépare un ouvrage étendu sur l'aspect dans le verbe grec.

Un Atlas linguistique de la Péninsule, avec inclusion du portugais et du catalan, avait été envisagé il y a vingt-cinq ans. Après notre guerre civile le travail a été continué par des membres de l'école de Menéndez Pidal, et le jour semble proche où paraîtront les premiers fascicules.

L'école linguistique catalane, représentée surtout par Monseigneur Grier, peut présenter la moitié d'un *Atlas linguistic de Catalunya* (la deuxième moitié ayant été détruite par les milices rouges) et plusieurs autres travaux importants. Le professeur Badia continue à Barcelonne maintenant cette activité. La toponymie et l'onomastique ont été spécialement cultivées par l'école catalane, et il existe aussi dans cette spécialité des publications importantes de Menéndez Pidal, García Blanco, Alvar et d'autres.

Mais il fallait, après le réveil des études de linguistique ne pas se limiter à l'étude des langues de notre péninsule. En 1939 par l'initiative de Menéndez Pidal, l'on créa à Madrid une nouvelle section du Centro de Estudios Históricos, avec une bibliothèque spécialisée et une revue, dont le but était le développement des études classiques. Le linguiste italien, M. Bonfante fut appelé à collaborer à aider cette entreprise. Sa présence signifia une orientation plus linguistique de notre philologie. Depuis lors l'étude des langues indo-européennes se développa parmi nous. Il suffit de mentionner la revue *Emerita* pour apprécier l'orientation linguistique de nos études classiques. Nous avons lancé une série de petites grammaires des langues indo-européennes qui montre l'intérêt croissant de ces études non seulement en Espagne, mais aussi en Amérique du Sud. On pourrait citer le nom de plusieurs savants dans les langues classiques qui sont en même temps des linguistes, dont MM. Bassols, Rodríguez, Arados, Vallejo, Pariente etc.

Le basque, qui a eu naturellement une belle tradition chez nous, dont le porte-parole était la *Revue Internationale des Etudes Basques*, a traversé une crise difficile, due au manque de spécialistes capables de continuer les travaux des Azkue et d'Urquijo. Mais une nouvelle génération travaille, groupée autour de la nouvelle chaire de basque à mon Université de Salamance, et plusieurs publications, dont l'une périodique, le *Boletín de la Real sociedad Vascongada* de San Sebastien, attestent la vitalité de ces études. On a même commencé la préparation d'un dictionnaire étymologique de cette langue.

Dans les études de la période pré-latine de notre péninsule, j'oserai dire, qu'on a beaucoup avancé. Le déchiffrement des écritures indigènes hispaniques par M. Gómez-Moreno, et l'emploi de nouvelles techniques dans ces études ont permis

de déchiffrer des textes relativement étendus en céltibérique, et de signaler de nouveaux points de vue sur la langue ibérique et sur la pluralité des substrats linguistiques de notre péninsule. Plusieurs savants étrangers, dont quelques-uns ici présents: M. Lejeune, Prof. Foster, Prof. Pokorny, M. Lafon, pourraient témoigner de ces brillants résultats.

En m'excusant du nombre de détails je signalerai encore que les langues américaines retrouvent un nouveau intérêt parmi nous, dont témoignent plusieurs articles parus récemment.

Je voudrais justifier cette énumération, peut-être trop longue, par le désir de vous présenter un compte-rendu des activités linguistiques espagnoles. Nous voulons occuper notre place modeste dans la science mondiale, et c'est là toute notre ambition. Ouverts au progrès scientifique, participant à toutes les écoles modernes, sans distinction ni parti-pris nationaux, les linguistes espagnols travaillent à la recherche de la vérité.

Personnellement je suis heureux, Messieurs, d'être leur interprète en cette occasion solennelle, et d'avoir le privilège et l'honneur de pouvoir contempler toute la gamme des intérêts des linguistes anglais et d'assister à cette réunion générale des savants du monde.

ADDRESS BY DR. S. M. KATRE,
OFFICIAL INDIAN DELEGATE

Mr. President,

I am grateful to the Organising Committee for giving me this opportunity of associating myself with the deliberations of this Congress as an official representative of my country. Though I have been a full Member of these International Congresses since the Bruxelles session this is my first personal visit to the Congress. It would not be inappropriate, therefore, in my view, especially at this Session, presided over by Sir Ralph Turner, whose distinguished work in the field of Indo-Aryan Linguistics has been a great land-mark, to give you a brief account of some of the principal activities to which we have given our attention in India in the field of Linguistics and related studies.

In his opening remarks our President referred to the work of the Philological Society's English Dictionary. Since 1948 we in India have been organising at the Deccan College the work of compiling a Dictionary of Sanskrit on historical principles. It is a pan-Indian and international project and a minimum programme of extracting material in full from over 2,000 representative texts from the earliest Vedic period to 1800 A.D. representing all the different branches of arts and science has been issued to participating scholars. International collaboration has been secured and centres created in England, France, Germany and Holland in addition to those in India. The project, when completed, will be of the greatest significance to Indic studies and to Indian linguistics and replace the great Petersburg Dictionary of Roth and Böhtlingk. Apart from the published and unpublished texts this Dictionary will fully utilize the material found in inscriptions and coin legends.

The second major project, which is also being directed from the same college, is primarily connected with the deliberations of this Congress. Since the great work initiated by Sir George Grierson in the Linguistic Survey of India was purely exploratory in nature, a vast field has been opened up for fuller investigation and I am glad to note that trained scholars from England, France, the United States, Norway and Denmark have been visiting India to collect material from the least studied dialects and languages. But a systematic effort is required in the country itself to record the fast disappearing material constituting more than 7 families of languages and over 1,000 dialects and languages. A new project has been envisaged with a wider term of reference in an Ethno-Linguistic Survey of India which, in addition to recording fully all the available linguistic material, will also undertake a study of dialect geography intensively in certain selected areas.

With the development of the regional languages as State languages and of Hindi as the national language of India a number of interesting problems in the educational field will require the competent assistance of linguists to solve the possible linguistic conflict arising from such a situation. This opens up a new field where linguistics — so far a specialised study — will have a direct bearing and interest for the common man, and will bring together the linguists, the pedagogue and the psychologist and synchronise their different approaches.

The third and the last major project is that undertaken by the International Academy of Indian Culture at Nagpur whose Director is present among us here. The aim of that project is to publish not only the entire Sanskrit literature but also the translations and adaptations of Indian texts from Far-, Central- and Near-Eastern Asia. The project is appropriately named the Satapitaka in which the collaboration from scholars all over the world is being sought.

It would not be out of place to mention here that the impulse and direction for these projects come from scholars who had the advantage of being trained in this University by our worthy President and should be considered as a silent tribute to his own work on the Nepali Dictionary and the hitherto unpublished Comparative Etymological Dictionary of Indo-Aryan. In my country such a tribute is appropriately called Guru-Dakṣiṇā.

In conclusion it is my privilege to move a hearty vote of thanks to the Organising Committee of this Congress for their selfless work in arranging the Congress here and seeing to its successful conclusion. We have had an interesting and profitable session in which members coming from all the continents of the world have participated. The Organising Committee have not only made very satisfactory arrangements, but have also secured for the Congress the royal patronage of H.M. the Queen, which is indeed a signal honour. On behalf of us all I would like to express our grateful thanks to the Organising Committee for their unstinted labour in making a great success of this Session.

REPORT BY SECRETARY GENERAL OF C.I.P.L.

Mr. Chairman, Ladies and Gentlemen,

In presenting this report I must begin by giving an account of the present composition of C.I.P.L. Since our last congress we have had to deplore the death of one of the greatest linguists of our time, Leonard Bloomfield. And we have received with great regret the resignations of Professor Corvan, Professor Daniel Jones, one of the founder members of our committee, and Prof. Rosetti. In their stead we have co-opted Professor Deeters (Germany), Professor Norman (United Kingdom),

Professor Twaddell (U.S.A.) and Professor Whatmough (U.S.A.).

Following the decisions taken by C.I.P.L. at its meeting in Paris in 1948, we have taken part in the setting up of a Council for Philosophy and Humanistic Studies, affiliated to U.N.E.S.C.O. At the constituent assembly of this Council, which opened its debates in Brussels on the 18th January, 1949, your Secretary General was elected vice-president for 1949-51, and in 1952 he was re-elected for the period 1952-54.

As a member of the Bureau of the Council, he cannot vote and Dr. Mohrmann has taken his place in the assembly. C.I.P.L. is thus a member of the great federation of international organisations devoted to the study of Man, but it has retained its complete autonomy and independence. Thanks to a financial aid from U.N.E.S.C.O., C.I.P.L. has been able to undertake several important tasks. But where we have been able to undertake these, the credit goes also to our secretary Dr. Mohrmann. I am happy to pay a warm tribute to the devotion and efficiency of Dr. Mohrmann. Without the real spirit of sacrifice which she has shown, it would have been impossible for C.I.P.L. to accomplish its tasks. These have been:

- (1) Publication of the *Actes* of the Paris Congress, edited by M. Lejeune.
- (2) Publication of Linguistic Bibliography. The retrospective bibliography for the years 1939-1949 appeared in two volumes 1949 and 1950. The volume for 1948 came out in 1951, that for 1949 in the same year. The volume for 1950 will appear in a few weeks and that for 1951 is in preparation and will be published in 1953.

I should like to say here that the reason why these volumes have had to be published so quickly is entirely due to the very strict financial regulations of U.N.E.S.C.O. If we had had more time at our disposal, we should have been able to adopt a more detailed system of bibliographical sections.

As far as the sub-commissions are concerned, the technical committee of C.E.L. met on the 2nd December, 1949, in Paris, thanks to the financial aid of U.N.E.S.C.O. Professor Sever Pop was charged with the preparation of the bibliography of linguistic atlases, published or in preparation. The result of his enquiry was published in his great work — "*La*

dialectologie " (1951). Professor Pop too has shown a real spirit of sacrifice in accomplishing this task for C.I.P.L. It was further decided to publish a new edition of the *questionnaire* of Professor Marcel Cohen with translations into English and Spanish. The edition was published in 1951. Finally it was decided to apply for a subvention enabling us to publish a volume *Die tasmanischen Sprachen*, by Pater W. Schmidt.

I think it will be important to have in one volume the material which remains of the Tasmanian languages and we are delighted that we have been able in this way to help an old and distinguished linguist to publish a book which has been ready in manuscript for some 30 years.

The Commission for Terminology met on the 29th May in Paris. It was decided to re-edit the *Lexique* of M. Marouzeau with the addition of the Italian linguistic terminology. Thanks to a subvention from U.N.E.S.C.O. the volume appeared towards the end of 1951. It was also decided to start a *Terminological Bulletin* with supplements dealing with the terminology of the different schools. The first of these supplements containing the terminology of Ascoli will appear in a couple of months.

The Commission for Linguistic Statistics published a Tentative Bibliography of Linguistic Statistics, prepared by Professor Marcel Cohen and Professor Trnka. The manuscript of a more complete bibliography comprising some 2,000 items has been compiled by M. P. Gieraud. This will be published with the addition of American publications.

At its General Conference in Florence in 1950 U.N.E.S.C.O. decided to take up the study of "primitive" mentality in connection with its efforts to counteract racial prejudice. It was found that the best way of conducting an enquiry would be to start from Language. It was resolved to select a certain number of so-called "primitive" languages spoken by people who had lived in comparative isolation and for whom there exists sufficient ethnographic material. As U.N.E.S.C.O. could not finance field-work, it would be necessary to entrust young linguists who either were in the field already or who had access to extensive texts with the description of the languages chosen according to a certain set of directives. This method would ensure parallel and consequently comparable descriptions. When we have these descriptions, which

we hope will be ready some time in 1954, we will call a meeting of linguists, anthropologists and philosophers to see whether any definite conclusions can be drawn. Personally, I think it will be possible to draw such conclusions, but it should be stressed that the scholars who will be called upon will have complete liberty and will not be influenced by any preconceived ideas of what the result ought to be. The work is already under way. Mr. J. Berry has undertaken to describe Twi, Mr. F. Carnachan Igbo, Mr. G. B. Milner Fijian and Professor Kurt Briggs Eskimo. I have just been in America where specialists on American Indian languages have promised their collaboration. Prof. Voegelin will charge one of his students with one of the language groups he is working on, probably an Argentine language, Prof. Lounsbury will do the same with Oneida and Professor Harry Hoijer has promised to co-operate as far as a western Indian language of the United States is concerned. The work will be carried out by young linguists under the supervision of these three professors. The directives to be followed in the descriptions have been published in the latest volume of the *Transactions of the Philological Society* (1951) and in the *Norsk Tidsskrift for Sprogvidenskap*, vol. XVI.

Finally I ought to add that the *Comité International des Sciences Onomastiques* is affiliated to C.I.P.L. That means that it obtains its subventions through the intermediary of our organisation.

I mentioned that C.I.P.L. is now a member of the Council for Philosophy and Humanistic Sciences, a federation of international associations devoted to the study of Man. Through this Council C.I.P.L. obtains its subventions from U.N.E.S.C.O. and the Council is represented at U.N.E.S.C.O.'s conferences. The Council itself and the different associations making up this federation, the historians, the philosophers, etc. are organised on a representative basis. They consist generally of an assembly of delegates and an elected executive body. When C.I.P.L. was constituted in 1928 by men such as Schrijnen, Meillet and Jespersen, it was constituted differently. The first members were elected and as soon as members died or resigned the remaining members co-opted new ones. I think that at the time this system was a wise one — it was the period of the rise of rabid nationalism in Europe in the form of fascism and nazism. Now, however, our

constitution carried the risk of appearing in a false light when compared with those of the other members of the Council for Philosophy and Humanistic Sciences. And our constitution makes it extremely difficult to find cover for the expenses of the office of C.I.P.L. — U.N.E.S.C.O.'s subventions cannot be used for purely administrative purposes. If we have been able to carry on it is due solely to Dr. Mohrmann's spirit of sacrifice. At a meeting of C.I.P.L. on the 10th January 1951, therefore, a proposal to re-organise C.I.P.L. was discussed and a committee of three was appointed, consisting of M. Vendryès, Dr. Mohrmann and your Secretary-General. The proposals for new statutes and a set of rules of procedure have been discussed in meetings of C.I.P.L. at this congress. It is proposed to send an appeal to the countries which are members of U.N.E.S.C.O. and to those which have members either on C.I.P.L. itself or on its specialised commissions to be prepared to send delegates to the next congress in order to found an association on a representative basis. This appeal will be accompanied by proposals for statutes and rules of procedure. It will be the task of the members of C.I.P.L. in the countries where there are such and in other countries by linguists who are willing to co-operate with C.I.P.L. to consult with their linguistic societies and/or other individual linguists about the matter and to decide, if they are willing to co-operate, how their delegates are to be elected. It would serve no purpose to discuss here the proposed statutes and other details of the proposed organisation. You will all of you have time and leisure to study them when they are sent to the responsible representatives in your country and you will have the opportunity to give your advice through your delegates.

Let me say in conclusion that in taking this step for its own re-organisation C.I.P.L. has acted under no pressure of any kind. We have received no hint from any organ of U.N.E.S.C.O. that our constitution ought to be changed. The initiative comes solely from ourselves. We feel that although the constitution of 1928 was excellent at the time the moment has come now to conform to the general pattern on non-governmental international organisations. We know that such a proposal will meet with difficulties and it may be feared that some linguists may be reluctant to agree to what has been accepted by the research workers in the other fields

of the sciences of Man. But we feel that the present constitution of C.I.P.L. may one day prove detrimental to its work and we cannot accept the responsibility for this. We think, therefore, that it ought to be transformed into an international organisation on a representative basis.

ADDRESS BY PROFESSOR N. B. JOPSON

Mr. President, Distinguished Delegates, Ladies and Gentlemen,

After the lucid expositions of the gentlemen famous for scholarship, experience in public business and renown in diplomacy who have just addressed you I feel a diffidence which you will surely understand.

It is an honour of which I am keenly sensible to have been invited to make a few remarks before the Reception of this afternoon and before the lavish philological fare to be set before you this week.

Since I am an official delegate to the Congress by virtue of being this year the President of the Philological Society — that organization which, so far as the United Kingdom is concerned, has been principally responsible for initiating the preparations for the Congress — it would seem proper that I should say something about the work of the Society during the last hundred years and about its distinguished past and its present members.

The Philological Society was founded "in its present form" in 1842 and it consisted (here I quote) "partly of members of a Society of the same name established at the University of London in 1830 to investigate and to promote the study and knowledge of the structure, the affinities and the history of languages."

I would single out two points in this quotation: first, the honoured mention of the University of London — that same great University which today opens its hospitable doors to us and which has trained or signally recognized so many British philologists, including the President of this Congress, Sir Ralph Turner. Second, the final words of the sentence. It is "study and knowledge of the structure, the affinities and the history of languages" that the word philology means to the average Briton, though some scholars and associations of

this country exist which prefer to invoke the meaning of the Greek *φιλολογία* i.e. "a love of discourse, literature and language," as Skeat has it.

For myself I think it advisable to use the word in the Society's sense (but you will perhaps be thrashing this out at one of the Sessions) in spite of the absence of agreement even in English-speaking countries and of the difference between philology and, say, German *Philologie* or French *philologie*.

May I say, by the by, that continental scholars do not exactly agree either in their usage. Thus, the *Volksbrockhaus* in a recent issue defines *Philologie* as "die Wissenschaft von der Sprache und dem Schrifttum, überhaupt der gesamten geistigen Hinterlassenschaft eines Kulturvolks" while the *Petit Larousse illustré* gives under *philologie* "Science qui envisage les oeuvres littéraires et les langues sous le rapport de l'érudition, de la critique des textes et de la grammaire." After further mention of the intellectual, social and artistic life of one or more peoples an example is given: "la *Philologie classique*."

But whatever meaning of the English word may be finally decided upon we shall probably all agree that justice is done to one at least of its aspects by the lines in Cowper's *Retirement* which speak of

. . . . philologists, who chase
A panting syllable through time and space,
Start it at home, and hunt it in the dark,
To Gaul, to Greece, and into Noah's Ark.

The Philological Society cannot boast of a *Grundriss* rivalling Brugmann's, and few of the scholars who have contributed to its *Proceedings* or *Transactions* or its *Independent Volumes* have the scope of a Bopp or a Meillet or a Meyer-Lübke or a Menéndez Pidal, yet a wealth of valuable publications has seen the light under the Society's auspices and a host of scholars, native and foreign, have shed lustre on its name.

One only of the works sponsored by the Society shall I mention: *The New English Dictionary on Historical Principles*. This was completed in 1928 and the authors are Murray, Bradley, Craigie (to whom the Society will do honour in October) and Onions. This dictionary was then, and remains now, a publication of the first magnitude; it has been an inspiration, and often a model, for many foreign dictionaries.

The earlier writers for the Society tended to concentrate on English philology — the names of Ellis, Skeat, Sweet, Chambers, Mawer and Weekley come at once to mind — but there was always an awareness that the philology, historical and comparative, of other languages also was meet and due for exploration. Whitley, Stokes, Dawkins, Magnus and Fraser are a few of the British scholars of what I may call the late Middle Period.

The generation still living is represented by many notable names. Distinction is invidious, so I place them in alphabetical order. Allen, Bailey, Brough, Burrows, Campbell, Collinson, Crossland, Dillon, Ellis, Foster, Guthrie, Jones (D. M.), Lorimer, Mann, Matthews, Master, Nandriş, Noble, Norman, Palmer, Ross, Tucker, Turner, Wakes, Wrenn. And I have not mentioned our Altmeister, Professor Thomas — but is it not written "He who is last shall be first." His name, like good wine, needs no bush.

The ladies too have had their share — particularly Miss Woodward, for long our enthusiastic and valiant secretary, and Miss Daunt.

Apart from these scholars, whom I call philologists in the traditional sense there are others, of somewhat different interests, whose contributions will certainly never be forgotten. Daniel Jones and Professor Firth here have pride of place.

Alas that I have to omit from my list of living scholars two names: Boyanus, tireless Russian phonetician and Entwistle, equally at home in Iberian, Scandinavian and Slavonic studies.

The list of foreign or foreign-born philologists is similarly impressive even if I count only recent contributors. Between Baudiš and Sommerfelt there lie names we can all acclaim: Benveniste, Birnbaum, Gershevich, Henning, Mirambel, Simon and Szemerényi — with subjects ranging from those of Cowper's philologist to Yiddish.

It only remains now to thank you for the courteous attention you have given me, to ask for your indulgence if I have exceeded my allotted time, and to invite your deep attention to the views of the scholars who will be expounding their theses.

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CLOSING SESSION

HELD IN

THE SENATE HOUSE

UNIVERSITY OF LONDON

SEPTEMBER 6 1952

LECTURING SEASONS
OF THE
THE GRAYSON HOUSE
UNIVERSITY OF TORONTO
SEPTEMBER 2-1913

FINAL ADDRESS BY PROFESSOR A. SOMMERFELT

Mr. Chairman, Ladies and Gentlemen,

As Secretary-General of C.I.P.L. it is my duty to try to point out what may be learnt from this Congress and to indicate what consequences this experience ought to have for future congresses. It is not my purpose to try to sum up the result of the debates; that has already been done by the different rapporteurs.

An international congress is always a hazardous undertaking, and the organizers have reason to congratulate themselves if it turns out to be a success. I think we have reason to assert that this congress has been a success, and those who have taken part in it will have gained experience and inspiration. Our first congresses were not so much centred round special problems; many of us had the feeling that they fell apart with discussions of often peripheral problems. Then in 1948, in Paris, it was decided to have no special sessions at all. In theory this was excellent, but quite a number of congressists were disappointed that they could not find occasion to discuss what they were more especially interested in. This is the reason why the organizers of this Congress — the organizers are always the scholars of the inviting country — found that they ought to have some special sessions, mainly in the morning. I think this is to be retained, but I should wish that another time there could be less clashes between important items. This time some important items had to be placed in special sessions. We might avoid this by putting up a smaller number of matters for discussion. This is no criticism of the organizing committee who selected the important and interesting questions, some of them fundamental, which we have been dealing with. As far as the discussion itself is concerned, I think there is a general feeling that often it fell too much apart into interventions prepared at home and therefore not related to the other interventions in the debate. The organizers of the next congress will have to try to avoid this tendency. It will not be easy and I do not know if they will succeed. To get a real discussion going in a congress of some 400 to 500 people would demand much more time.

A congress is particularly important by giving scholars the opportunity of meeting each other and of enabling them to see

the trend of the development in our science. In 1928 the discussion on phonology inaugurated a movement towards structural linguistics which since has become more and more pronounced. What was most striking in this respect in this congress was the constant reference to American structural linguistics. And that is quite natural. At present linguistics in America develop in a way which will be of paramount importance to linguistics everywhere. Another trait has been the interest taken in the work of the communication engineers. Here, I am sure, we are only at the beginning of a great movement. If I am to risk a prediction I think that future congresses will be centred round these items: structural theory, perhaps especially now the structural viewpoint will have to be adopted also in the study of the historical development of language, and communication theory.

Now I must say a word about the next congress. The situation is that at present nothing definite can be said. But certain developments are going on and I hope that there will be an invitation in a not too distant future. You will then be informed through C.I.P.L.

It is not my task to thank the organizing committee for all that they have done for us, but as a representative of C.I.P.L., I cannot refrain from paying a warm tribute to their efficiency and devotion. They have the gift of making things run smoothly. We are happy to have met in the proud and battered city of London. Tomorrow it is twelve years since London's greatest ordeal began. If this country had not stood fast at that moment I am afraid there would have been few linguists left in the world outside of America.

FINAL ADDRESS BY PROFESSOR J. WHATMOUGH

Sir Ralph Turner,

It is in the name of the members of the Congress collectively that I have the honour to speak. I address myself not only to you, Mr. President, as the centre, as it were, of being of the Congress, together with the Chairman of the Executive Committee, Professor Guthrie; and through you to the galaxy of Vice-Presidents of this Seventh International Congress of Linguists here in London, but also to the constellations,

both of them shining with stars of great magnitude, of the Organizing Committee and of the Executive Committee; and not least to the two poles around which all this universe has turned so smoothly and unerringly, its treasurer and its secretary.

But having now reached interstellar space, I forebear from pursuing this metaphor further, lest I be thought to share that sort of nonsense (or worse) that talks of stratospheric linguistics, or supposes that mere talk can kill; we keep our feet on the ground — all of us. And that applies, if I may be allowed to say so, as I think I may, being in some sense particeps of both, that applies to transatlantic as well as to the cisatlantic regions.

We join together, then, in thanking our hosts the Philological Society, famous for its long line of great scholars; the University of London, above all its world-renowned schools of language-study; and indeed all those from whatever part of these islands, who have contributed by the warmth of their welcome and hospitality, by their active interest in our comfort and well-being, to the success of the Congress; and we do indeed feel that it has succeeded in all the purposes which it serves of fostering friendships, old and new, as well as encouraging discussion and stimulating progress in our studies.

If the magic number of seven may be at all invoked by us, it is not astonishing to see at this seventh Congress, the subject of Philology and Linguistics once more flourishing here. When I left this country over a quarter of a century ago, except for a small group in London who still tended the plant carefully, it was all but moribund. But now it is full of new vigour — its seeds are taking or have taken root once more up and down the land, and branches are being sent out even to those two Universities that are said to need no reputance. I think in particular of the recent election of Professor Palmer to the Corpus Chair at Oxford, a matter on which both he and Oxford are to be congratulated.

But it is not only Linguistics. This great city of London, this age-old people of the British Isles also are arising new like the phoenix. This renewal cannot but gladden all men of good will, no matter from where they came; for they also must hope, that, as the years go by, still more lands will send their representatives, from East and West, North and South, to successive congresses.

On that note I end, only saying, as I did at the beginning, Sir Ralph, "Thank you" to you, and to all who have worked with you, — "Thank you" from the bottom of our hearts.

FINAL ADDRESS BY THE PRESIDENT

Ladies and Gentlemen,

First I should like on behalf of the Organising Committee, the Executive Committee and all your British Colleagues, to thank the speakers for the very kind sentiments to which they have given expression and all of you for the way in which you have received them. For myself I feel I have done little as your President to deserve what has been said. The whole brunt of the work of making the manifold arrangements for the Congress has fallen upon the Executive Committee and in particular upon its Chairman, Professor Guthrie, the Treasurer, Professor Norman, and its two indefatigable secretaries, Mr. Jones and Dr. Kane. Whatever success has been achieved in making your stay in London smooth and pleasant has been due to them. I think it has been generally agreed that we could not have had a more convenient locality in which to meet than the Senate House of the University. As Professor Sommerfelt has said, much of the advantage to be derived from our Congresses consists in the opportunities of meeting outside the lecture hall and conference room, and I shall have much pleasure in conveying to the Vice-Chancellor the thanks of the Congress for placing its resources at our disposal.

In conclusion allow me to express the hope that you who have come from overseas will carry away pleasant memories of your stay here. If the pleasure and profit which it may have brought you is in any degree comparable with the pleasure and profit we have had from the opportunity of meeting old friends and making new ones among you, then the Congress has indeed been successful.

It only remains for me now to declare the Seventh International Congress of Linguists to be ended.

PROGRAMME

SEPTEMBER 1st

9.30—11.30 Standing Committee: ENQUÊTE LINGUISTIQUE

11.30—12.30 OPENING SESSION

Addresses:

The President
Mr. P. N. Kirpal
Professor A. Tovar
Dr. S. M. Katre
Professor A. Sommerfelt
Professor N. B. Jopson
Professor M. Guthrie

2.00—4.00 Standing Committee: TERMINOLOGY

4.00—5.30 RECEPTION

BY THE VICE-CHANCELLOR OF THE UNIVERSITY OF
LONDON

Professor H. Hale Bellot

5.30—7.00 Standing Committee: STATISTICS

SEPTEMBER 2nd-6th

EXHIBITION of books and periodicals published since
the VIth Congress, July 1948

SEPTEMBER 2nd

LECTURES

- 9.30—10.30
- i Professor A. Johannesson: The third stage in the development of human speech
 - ii Professor F. Schubel: The finer shades of Old English words with Christian meaning
 - iii Dr. L. Kaiser, Dr. G. L. Meinsma: Electronics and Audition in the research of speech melody
 - iv Professor Zong-In-Sob: The far-eastern mono-syllabic characters and the Korean language

10.30—12.30 PLENARY SESSION I (1st meeting)

LINGUISTICS AND THE PROBLEM OF MEANING

Chairman: Professor A. SOMMERFELT

Rapporteur: Professor J. R. FIRTH

Recorders: Mr. K. Northcott

Mr. A. E. Sharp

SECTIONS

2.30—4.00 GENERAL LINGUISTICS (combined meeting)

A 3 What are the central problems of form and meaning in negation?

A 4 What questions of general linguistic theory are raised by the study of numeration and of classifiers? What bearing have these questions on the categories of numerals and determinatives?

Chairman: Professor G. DEETERS

Rapporteur: Professor W. E. COLLINSON

Recorders: Mr. I. Richardson

Mr. P. B. Salmon

COMPARATIVE LINGUISTICS

B 2 What contributions to comparative linguistics are to be sought from the two groups of disciplines and techniques broadly termed (a) phonetics, (b) semantics?

Chairman: Professor G. DEVOTO

Rapporteur: Professor J. FOURQUET

Recorders: Mr. D. N. Mackenzie

Miss R. Wallbank

COMPARATIVE INDO-EUROPEAN PHILOLOGY
(combined meeting)

C 1 The laryngeal hypothesis and the theory of phonemes in Indo-European

C 2 Is there still a root-determinative problem in Indo-European?

Chairman: Professor L. R. PALMER

Rapporteur: Professor J. WHATMOUGH

Recorders: Dr. P. F. Ganz

Mr. M. A. Tanner

SEPTEMBER 3rd

LECTURES

- 9.30—10.30 *i* Professor F. Owen: The Indo-Europeanization of Northern Europe
- ii* Dr. A. Safrastian: The unbroken continuity of the Hurrian-Khaldian-Urartian and Armenian language
- iii* Dr. L. Homburger: L'indo-africain
- iv* Professor A. Belić: About conceptual and realised words; a contribution to the relationship of thought and language

Standing Committee: ENQUÊTE LINGUISTIQUE

10.30—12.30 PLENARY SESSION 1 (2nd meeting)

Recorders: Mr. K. Northcott
 Mr. A. E. Sharp

SECTIONS

2.30—4.00 GENERAL LINGUISTICS

- A 2 Can a purely formal grammatical analysis be carried out on languages such as Chinese, in which all or nearly all the words are invariable, and if so, on what principles?

Chairman: Professor W. SIMON
Rapporteur: Professor H. FREI
Recorders: Mr. P. B. Salmon
 Miss R. Wallbank

COMPARATIVE LINGUISTICS

- B 1 What special problems arise in the comparative study of languages without a history? What special methods are applicable in such fields?

Chairman: Professor B. BLOCH
Rapporteur: Professor M. GUTHRIE
Recorders: Mr. D. N. Mackenzie
 Mr. I. Richardson

COMPARATIVE INDO-EUROPEAN PHILOLOGY

- C 3 How far can the methods and principles of linguistic geography be successfully applied to the study of Indo-European?

Chairman: Professor W. PÉE
Rapporteur: M. LEJEUNE
Recorders: Dr. P. F. Ganz
 Mr. P. D. Soskice

SEPTEMBER 4th

LECTURES

- 9.30—10.30 *i* Professor C. C. Fries: The use of meaning in linguistic analysis
- ii* Informal discussion for members interested in foreign language teaching
- iii* Professor A. Tovar: The decadence of language
- iv* Professor B. Collinder: Linguistic Affinity

10.30—12.30 PLENARY SESSION 2 (a)

Can a logical calculus be devised that shall be structurally independent of the grammatical patterns of any known language?

Chairman: Dr. J. VENDRYES

Rapporteur: Professor E. J. L. BUYSENS

Recorders: Mr. M. A. Tanner

Miss R. Wallbank

1.30—2.30

Dr. Y. Bar-Hillel: Discussion of problems connected with machine translation.

SECTIONS

2.30—4.00

GENERAL LINGUISTICS

A 5 Conceptual dictionaries and dictionaries of usage; their nature and use

Chairman: Professor W. VON WARTBURG

Rapporteur: Professor F. MEZGER

(Report read by Professor J. Whatmough)

Recorders: Mr. D. N. Mackenzie

Mr. W. E. Skillend

COMPARATIVE LINGUISTICS

B 3 In what sense and to what extent is comparative grammar of value in descriptive linguistics? Can a more comprehensive method be developed in traditional comparative linguistics?

Chairman: Professor W. K. MATTHEWS

Rapporteur: Professor L. HJELMSLEV

(Report read by Professor H. J. Uldall)

Recorders: Mr. D. N. Mackenzie

Mr. W. E. Skillend

COMPARATIVE INDO-EUROPEAN PHILOLOGY

- C 4 How far can conclusions about cultural and social systems be drawn from purely linguistic evidence in the Indo-European languages?

Chairman: Professor A. DEBRUNNER

Rapporteur: Professor A. SCHERER

Recorders: Dr. P. F. Ganz

Mr. P. D. Soskice

4.30—6.00

RECEPTION BY HER MAJESTY'S GOVERNMENT,
represented by Dr. K. Pickthorn, Parliamentary
Secretary to the Ministry of Education

SEPTEMBER 5th

LECTURES

9.30—10.30

- i Professor Ernst Fraenkel: Some appellations of the hand in the Indo-European languages

- ii Dr. Y. Bar-Hillel: A quasi-arithmetical notation for syntactical description

- iii Statement by the Directors of the English Dialect Survey Planning Committee

- iv Professor J. Holt, Mr. J. Ellis, Mr. M. L. Samuels: Tense, Aspect and Aktionsart

- v Professor Raghn Vira: Developing modern Indian languages to meet modern needs of administration, law, science and commerce

10.30—12.30

PLENARY SESSION 2 (b)

On what principles are word-classes to be defined and named?

Chairman: Professor A. BELIĆ

Rapporteur: Professor O. FUNKE

Recorders: Dr. P. F. Ganz

Mr. K. Northcott

1.30—2.30

Professor W. N. Locke: Progress towards the identification of speech-sounds by machine

1.45—2.30

Professor B. Collinder, Professor A. Schmitt: Discussion on the accentuation of Greek and Latin verse

SECTIONS

2.30—4.00

COMPARATIVE LINGUISTICS

B 4 Are there areas of *affinité grammaticale* as well as of *affinité phonologique* cutting across genetic language families?

Chairman: Professor C. REGAMEY

Rapporteur: Professor A. MARTINET

Recorders: Mr. P. B. Salmon

Mr. W. E. Skillend

COMPARATIVE INDO-EUROPEAN PHILOLOGY

C 5 What inferences can be drawn from the evidence of morphology and word-formation about the development of proto-Indo-European?

Chairman: Professor A. ERNOUT

Rapporteur: Dr. O. SZEMERÉNYI

Recorders: Dr. P. F. Ganz

Mr. M. A. Tanner

(*In lieu of Section meeting*). A demonstration of Synthetic Speech as a means of linguistic research
Prepared by Professor P. DELATTRE, Dr. F. S. COOPER and Dr. A. M. LIBERMAN

Presented by Professor P. DELATTRE

4.15—5.00

Professor Zong In-Sob: Further discussion of his paper

5.00

Sir Richard Paget, Bart.: A New Systematic and Grammatical Sign-Language for the early education of the Born-deaf

(*Illustrated by a film published by the Wellcome Foundation*)

Chairman: Professor J. WHATMOUGH

SEPTEMBER 6th

11.30—12.30 CLOSING SESSION

Addresses: Professor A. Sommerfelt
Professor J. Whatmough
The President

PREFACE

The publication of the *Proceedings* of the Seventh International Congress of Linguists has been intolerably delayed, and a few words of explanation are due to the members of the Congress.

The Congress was got well under way at a preliminary discussion which took place a little more than the customary two years before the meeting was due to be held, i.e. in the early summer of 1950. Thereafter, progress was for a long time slow, for some unforeseen difficulties developed. As a result, preparations were severely hampered, and the real preliminary work started late. It was, however, possible to despatch the *Preliminary Reports* to members well before the Congress began. The Congress is most grateful to U.N.E.S.C.O. for so liberally providing the necessary funds for printing the *Preliminary Reports*. Without this generous support it would not have been possible to proceed at all.

Some time after the Congress had dispersed an Editorial Committee was appointed. This Committee met several times to settle preliminary problems and it was only after the Committee had approved the general shape and lay-out that detailed work on the preparation of manuscripts could begin. It had been laid down by the Editorial Board that it was no part of its responsibility to edit the contributions of members except in very minor details. The *Proceedings* were to be a factual record of what was said, and no more.

After the General Editor had received his instructions, detailed work began. Whilst the Committee was always prepared to help and advise, the entire work of the preparation of manuscripts, arrangements with the printer, reading and correcting of proofs, were the business of the General Editor. This proved to be so irksome a task that the Committee in the autumn of 1953, authorised the appointment of an Assistant Editor, Dr. Peter Ganz.

The fact that, after long delays, the *Proceedings* have at last appeared is in large measure due to his tireless efforts. Dr. Ganz is, however, in no way responsible for any shortcomings and errors which may have crept in, nor can the Editorial Committee be held in any way answerable either for the long

delay or for any inadequacies. These are to be laid entirely at the feet of the General Editor.

An attempt has been made to prepare as accurate a bibliography as possible of the publications of previous Congresses. This proved to be particularly troublesome for the Fifth Congress, which was ultimately not held on account of the Second World War. It is hoped that all the details have now been collected; this could not have been done without the most courteous assistance of Professor R. Fohalle of the University of Liège who not only took great trouble in answering all queries but actually supplied a complete set of the very rare documents which had been published in preparation. The Congress owes him a great debt for his ready help.

It remains to thank the Recorders, Dr. P. F. Ganz, Mr. K. Northcott, Mr. D. N. Mackenzie, Mr. I. Richardson, Mr. P. B. Salmon, Mr. A. E. Sharp, Mr. W. E. Skillend, Mr. P. D. Soskice, Mr. M. A. Tanner and Miss Rosemary Wallbank without whose efficiency and energy, and persuasive powers, a great deal of the material gathered could not have existed in print at all, Mr. F. M. Guercio for reading some of the Italian material and Mr. W. N. Jeeves, who performed a like service for French, Dr. Herbert Thoma who read a complete set of page-proofs and whose care and vigilance are alike responsible for much of the material collected in the *Corrigenda* on p. 563 ff., Mrs. Marion Rosenberg, secretary to the Department of German, King's College from 1952-1954, who typed a great deal of difficult material written in sometimes very difficult hands, and the present secretary to the Department, Mrs. Martha Willis, who has seen the job through.

Editorial Board, Assistant Editor, Recorders, secretaries: they one and all deserved that their labours should have been rewarded by a far earlier appearance of these *Proceedings*. That in this one respect only they laboured in vain is regretted by no one more than by the General Editor. He can claim that he has many other duties and that he has simply not had the necessary time and energy to deal quickly and expeditiously with the ceaseless difficulties that arose. He knows, however, that whilst this is an explanation it is no excuse, and he would only plead that nobody can be as content and relieved that this book should at last appear in print as himself.

F. NORMAN

SEVENTH INTERNATIONAL CONGRESS OF LINGUISTS

London, 1-6 September, 1952

PRELIMINARY REPORTS

CONTRIBUTIONS IN RESPONSE TO
THE QUESTIONNAIRE

NOTE—*Some Rapporteurs have found it impossible to send their Preliminary Reports in time for them to be included in this Brochure. These reports will, if possible, be circulated to Members at a later date.*

THESE REPORTS HAVE BEEN PREPARED FOR AND SEEN THROUGH THE PRESS
BY F. NORMAN



PLENARY SESSION I

LINGUISTICS AND THE PROBLEM OF MEANING

REPORT BY J. R. FIRTH

Contributions from

- | | |
|----------------------|---------------|
| 1. E. BUYSENS | 5. W. HAAS |
| 2. J. B. CARROLL | 6. G. KANDLER |
| 3. MARCEL COHEN | 7. B. POTTIER |
| 4. A. J. J. DE WITTE | 8. S. ULLMANN |
| 9. J. WHATMOUGH | |



REPORT

by

J. R. FIRTH

To discuss "the meaning of meaning" in general terms would be unprofitable in a congress of linguists. The phrase is English and translation into the languages of the congress for the purposes of an interlingual discussion might at this stage make confusion worse confounded.

It is also necessary to point out that the distribution in collocation of the English word 'Semantic' and all its derivatives, both in general language and in linguistics, indicates such wide scatter of usage that they have ceased to have any precision as technical terms.

Nevertheless the main features of the discussion of the problem of meaning may be recapitulated under eight headings:

1. The dualist-mentalist view:

(a) Speech is sign-behaviour by means of which we "express" or "communicate" our thoughts and the "meaning" of language is to be stated by reference to inner mental states. This approach steadily declines in favour. It is perhaps at its worst when "sentences" are regarded as the linguistic "expressions" of "complete thoughts." Is this view anywhere seriously held or does it survive only in pedagogical manuals?

Even more perilous is the setting-up of words as isolates corresponding to concepts or ideas, often endeavouring to combine a constant core with a field of indeterminacy.

But both are open to the gravest objections, especially when combined with a rigidly mechanistic set of procedures for dealing with the overt signs. Some so-called mechanists, by confining their attention to the phonological structure of language material or to quasi-mathematical phonemics, recognise the mentalist nature of the study of meaning by specifically excluding it, and may be described as one-sided mentalist-mechanists.

(b) de Saussure's theory of the sign, psychological or "mentalist" in both its aspects, may be used to set up descriptive systems which are quasi-mathematical sets of interior relations. These interior relations of the elements constitute functional values. But "meaning," conventional and arbitrary, is still to be dealt with as a function of "the mind" and considered to be beyond the linguist's reach.

(c) The logical mentalists also regard the sentence as the primary datum, every sentence being regarded as a judgement expressed in words, i.e. a proposition with subject and predicate. According to this view every linguistic expression is "limited to the written image and is thus reduced to a combination of a finite number of different graphic signs" (von Mises). Such a logical approach neglects the *implication of utterance* which is a main postulate for descriptive linguistics.

2. Semanteme and Morpheme

The notions associated with these two terms have been common since the days of Henry Sweet, if not since Aristotle, and came into use in their French forms thirty years ago in association with further advances in scientific linguistics.

There is no agreed international usage based on the expression '*morpheme*.' For some it has phonological shape, for others it is indistinguishable from "grammatical category." *Morphemes* may be segmental, discontinuous, cumulate, or they may be non-linear, non-segmental grammatical abstractions from the phrase or sentence.

They may be so many different things that *semantemes* are likewise without basis in general principles, and must necessarily be in similar disarray.

Are there any clear indications that the distinction *semanteme/morpheme* is helpful in the study of meaning?

3. The context of instance

There are two distinct but perhaps related concepts of "context of situation." The earlier concept favoured by Malinowski and Gardiner was a sort of picture of a bit of social life in which speech is the creative force. The expression "context of situation" was used to refer to a scene or to an actual piece of behaviour, which might be reproduced, say by talking film in colour. The instances of the language texts

are attested in instances of events. This may be termed the context of instance. It is, if you like, to be associated with *parole*, with *discours*, with stimulus-response experiments, with particular speakers and listeners and particular effects. It is the method of ethnography and social anthropology.

4. Context of Situation

The expression "context of situation" is, however, more often used to refer to a scheme of general categories in which the linguistic text with its characteristics finds a place and function in relation to other categories such as the participants, relevant non-verbal behaviour, relevant objects and effect or result. Such an analysis, abstract though it is, does indeed "humanize and personalize" utterance, the implication of which in connection with any text, is fundamental. Such a context of situation is multi-evental and as generalized as the normal linguistic text which forms an element in this interior system of relations. Generalisations state "general facts," and are related to instances. A generalisation asserts the interconnectedness of instances.

5. Collocation

The concept of meaning by collocation is an essential part of the contextual approach which is wrongly regarded by many as a sociological classification. In England it is regarded as a linguistic classification largely because the focus is on normal expressions in a statistical sense. The text may be regarded as consisting of words or other elements in habitual company. Collocations are attested in contexts of situation, and description begins at these levels before further structural analysis. *Meaning by collocation* and distribution in collocation can be directly applied to words and other pieces. The association of preceding and following textual material, or *extended collocation*, adds further determinants of function. To adapt Professor Whatmough's words, such statements of meaning are "totally different from a verbal or referential definition and much more exact." Two different sets of words (a word and its definition; or a sentence and its explanation) cannot really be said to be exactly equivalent.

The statement of meanings in terms of collocation, extended collocation, and situational context is not limited to present-day

speech but can for example be applied to Anglo-Saxon words or texts, or to any ancient text of sufficient length to provide materials for collocational and structural studies.

6. Collocations and Citations in Lexicography

The study of words and other elements in their habitual collocations is to be distinguished from the collection of lexicographical citations to exemplify a more or less arbitrary series of "different meanings," defined verbally in shifted terms, or to illustrate historical changes of meaning. Descriptive lexicography based on distribution in normal collocations and attested by usage in situational context has scarcely begun.

Linguistics is not concerned with personal intentions and must neglect instantial peculiarities which change continuously with time and place.

7. Operationalism

The contextual approach to meaning has something in common with Bridgman's "operationalism" in discussing scientific concepts, "which," he says "must be definable in terms of operations rather than of properties." "The true meaning of a term is to be found by observing what a man does with it, not by what he says about it."

Operationalism gives the meaning of a word in terms of a finite number of operations it serves. The generalisation of these operations constitutes the meaning.

For everyday language such descriptive lexicography could perhaps be attempted by linguists. For scientific and all metalanguages, the specialists themselves would have to be their own lexicographers or encyclopaedists.

8. The Expressions of Mathematics and Symbolic logic

The expressions of mathematics and symbolic logic and other similar systems of signs have implications of expansion in some sort of utterance when the symbols are in use, i.e. in context. Have they been developed in a background of Indo-European and Semitic language structure and are they to be ultimately referred to experience verbalized in normal language? Are they in fact collocated with general language and, if so, to what extent are they determined by it? It would

seem that attestation of collocations with implication of utterance in context of situation as a pre-requisite of structural analysis would render it improbable that the "grammar" of a logical calculus would be similar or parallel to the grammar of the general language with which it was associated in use.

By definition a logical calculus would not be possible as a technique of structural linguistics, though of course a logical calculus could be applied to the metalanguages of linguistics.

Conclusion

For my own part, I would emphasize that meaning is decisive for all branches of linguistics, and that the strictly linguistic interpretation of this fundamental concept is of high importance for the development of the science of language through the co-operation of all the so-called schools of linguistics. [see also *Modes of Meaning*, English Association "Essays and Studies," 1951, and *General Linguistics and Descriptive Grammar*, Transactions of the Philological Society, 1951].

TEXT OF CONTRIBUTIONS

1. Il faudrait constituer des dictionnaires de phrases (cf. H. Fréi, *Cahiers Ferd. de Saussure*, I); mais au lieu de classer les phrases uniquement d'après les concepts auxquels elles font allusion, il faudrait d'abord considérer leur but social; car tout acte linguistique est un comportement destiné à influencer l'interlocuteur: la politesse — en particulier à la fin des lettres — nous fait dire bien des choses qui, prises littéralement, n'ont guère de sens (cf. L. A. G. Strong: "A tongue in your head," ch. II).

Le classement sociologique des actes linguistiques a été jusqu'à présent abandonné aux modestes Guides de la Conversation ou de la Correspondance, aux Phrase-books: c'est à eux qu'on s'adresse pour savoir par exemple la différence entre "How do you do?" et "How are you?," ou celle entre "Veuillez croire à mes meilleurs sentiments" et "Cordialement vôtre": les mots qui composent ces phrases ne nous permettent pas de deviner dans quelles circonstances sociales elles s'emploient.

Quand un homme désire se documenter, par exemple, sur les procédés linguistiques dont on disposait au 18^e siècle pour parler du génie, il ne trouve rien: il doit consulter tous les ouvrages de critique artistique écrits au 18^e siècle; il en serait de même si l'on étudiait la même question en ce qui concerne notre époque.

L'étude des interjections nous ramène aux fondements de l'acte de communication; mais cette étude est complètement à faire.

ERIC BUYSENS

2. My purpose is to place before the attention of the Congress a number of directions which the study of meaning has taken in America, particularly in psychology. "Meanings" can be generated in a variety of psychological experiments; hence,

the properties of meaning and changes in meaning can be studied in the laboratory. The items of linguistic codes may be said to have meaning by virtue of the common elements in the contexts in which they have been conditioned — largely by what is called “operant” conditioning, through the reinforcement of voluntary behaviour. Verbal contexts may be described by mathematical means; non-verbal contexts may be distinguished along a finite number of dimensions of experience. By use of concepts of stimulus-generalization and stimulus-discrimination, the theory makes possible a rigorous separation of denotative and connotative aspects of meaning. Work in the measurement and indexing of meaning is described. Implications for linguistics and lexicography are discussed. For example, semantic change must be described not by classification into a series of *ad hoc* categories, but with reference to the changes in stimulus contexts involved in each case.

JOHN B. CARROLL

3. Comme chaque état de langue comporte une structure, les éléments audibles en doivent être étudiés avec leurs relations réciproques, de manière à faire apparaître les équilibres imparfaits, instables et cependant suffisants du système.

Pour les phonèmes, en petit nombre, on doit considérer non seulement le tableau plus ou moins symétrique mais aussi les emplois dans la constitution des mots: chevauchements de phonèmes, limitations des suites et des combinaisons (longueurs de radicaux, incompatibilités), non-emploi de certains phonèmes dans les affixes: au total aspect sémantique de la phonologie.

Pour les morphèmes, de nombre sensiblement plus grand, on doit considérer comme normaux au cours de l'évolution les emplois d'une même marque à plusieurs usages et les emplois de différents morphèmes avec la même valeur, ainsi que les réemplois des disponibles pour de nouvelles fonctions.

Pour les mots, dont le nombre très grand est maintenu dans certaines limites, on considérera toutes les synonymies, y compris les métaphores complexes.

Les phrases, théoriquement en nombre indéfini, seront examinées dans leurs cadres plus ou moins fixes et dans leurs variations, en pratique restreintes.

Pour des essais d'explication, la non-connaissance des fonctionnements nerveux, surtout centraux, est un inconvénient majeur. Mais on doit prendre conscience des possibilités et des commodités, des manques et des suppléances de l'attention, du discernement, de la mémoire, de l'invention, etc.

Ainsi, et non en évoquant de soi-disant concepts innés ou des schèmes idéaux, on pourra viser à se rendre compte du fonctionnement des instrumentations complexes qui satisfont aux besoins de communication dans les cadres sociaux.

MARCEL COHEN

4. The concept of context of situation can be most fruitfully applied to the semantic analysis of utterances by regarding the factors of Humanizing and Personizing of these utterances.

A. J. J. DE WITTE

5. The meaning of a linguistic symbol is a function of it, in linguistic or in extralingual environment. While fully recognizing the fundamental importance of the latter ('situation'), linguistics proper only studies the former ('linguistic context'). Thus, the meaning of a word is derived from the meanings of the sentences in which it occurs. Two words are semantically similar to the extent to which both satisfy the same sentential functions; they are syntactically related in so far as they fill complementary 'positions.'

In so examining their relations to one another, the linguist is not analysing symbols (into signs and meanings, or signs and situations, or signs, meanings, and situations): he is analysing complex symbols into simpler symbols. This

requires ('postulates') at some point in the process of analysis, utterances of underived meaning; and it is to the sentences (unit-utterances) of a language that we assign the part of semantic data.

Here contact is made with the psychological, anthropological, and historical ways of analysing sentences, namely, by reference to extralingual context. Such contact is no more injurious to the autonomy of Linguistics than epistemological postulates are to the autonomy of Physics, provided only that (a) all reference to extralingual fact is made at one single point (all of it being vested in the sentential data), so as not to defeat the essential uniformity of systematic linguistic description, and (b) the adopted notion of symbol-meaning admits of a rational explanation both of the functional relations of symbols to one another and of their organic change.

W. HAAS

6. Die Frage gehört zu dem äusserst fruchtbaren, noch kaum behandelten Problemkreis, welche Rolle die Bedeutungen des Sprachsystems in der Sinnvermittlung aktueller Rede spielen, d.h. wie überhaupt das tradierte Sprachgut in der menschlichen Verständigung funktioniert. Man meint gewöhnlich, der Redesinn baue sich aus den Bedeutungen der geltenden Sprachmittel auf, die in der Situation nur auf bestimmte Wirklichkeitsdinge bezogen würden. Andererseits lehrt die Alltagserfahrung, dass Aussprüchen vielfach ein "Zweitsinn" beiwohnt (Es zieht = Schliess das Fenster!). Diese Tatsache ist sprachwissenschaftlich bisher überhaupt noch nicht recht gewürdigt worden, obwohl man manche verstreute Beobachtungen dazu findet. In meiner ungedruckten Untersuchung: "Zweitsinn" — Vorstudien zu einer Theorie der sprachlichen Andeutung — von der ich nächstens eine Kurzfassung zu publizieren hoffe, habe ich zu dem Paradoxon, dass mit Sprachäusserungen oftmals Unausgesprochenes erfolgreich zum Verständnis gebracht wird, das Wichtigste zusammenzubringen versucht. Hier sind nur wenige Hinweise möglich: das "Unausgesprochene" wird durch allerlei aussersprachliche und sprachverquickte

“Verständnishilfen” kommunizierbar, die den Literalsinn modifizieren; Andeutungen (= absichtlicher Zweitsinn) stehen im Dienste verschiedener Leistungen, die nur die indirekte Ausdrucksweise hergibt; die Andeutung nutzt die konventionellen Sprachmittel (u.a. die “Bedeutungspotenzen” des Wortes) aus, jedoch nicht im üblichen Sinne, sodass sie als Wachstumsschicht und Tiefendimension der Sprache gelten kann. Auf die Begriffe “Affekt, Tabu, Metapher, Ellipse, langue-parole, Sprachfunktionen, Sprachursprung und-entwicklung usw.” wirft die Andeutungstheorie ein neues Licht. Geschichtlich wichtig sind besonders die Theorien allegorisch-pneumatischer Schriftauslegung, am wertvollsten die bei uns praktisch unbekannte Dhvanilehre der Inder, wonach Wesen der Dichtung das Unausgesprochene und Andeutefähigkeit (vyañjanā) eine der drei Sprachfunktionen, neben Aussagefähigkeit und Übertragung sei.

GÜNTHER KANDLER

7. On ne peut établir en langue un système rigoureux des sémantèmes. Le propre de ceux-ci est de se déterminer dans le discours; leur caractérisation dépend du locuteur et de la situation de locution. Cependant, pour qu'un certain nombre d'individus se comprennent régulièrement, il est nécessaire qu'il y ait des éléments sémantiques communs dans chaque forme d'expression. Le contenu total peut se représenter par une base primaire de contenu S , commune à tel groupe d'individus, à laquelle s'ajoutent, suivant le locuteur, les caractéristiques secondaires s^1 , s^2 , s^3 . . .

L'évolution sémantique dépend de la nature des caractérisateurs. Voir par ex. l'histoire de la synonymie de *fleuve* et *rivière* (Rom. Ph., II, 285). Au début, *fleuve* et *rivière* ont S = “cours d'eau plus important qu'un ruisseau”; puis, peu à peu, un s^1 “qui se jette dans la mer” s'attache au S de *fleuve* dont le contenu sémantique, par suite d'une association de discours, est d'abord $S+s^1$ (possibilité de synonymie avec *rivière*) puis S' (somme de $S+s^1$); à partir de ce moment, *fleuve* et *rivière* ne sont plus synonymes.

La métaphore utilise également ces indices. Ex. *cochon*:

$S = \text{"animal," } s^1 = \text{"rose," } s^2 = \text{"sale," } s^3 = \text{"comestible"}$ etc. . . . (selon le locuteur et la situation). La sélection d'un de ces caractérisateurs aboutit à un emploi métaphorique; ex. le s^2 "sale" donne "tu es un petit cochon," "tu es cochon."

Un "contraire" est essentiellement un terme complémentaire dans une association d'habitude. Le contraire de *long* semble logiquement être *court*; les écoliers répondent d'ordinaire *large* car pour eux, *long* et *large* sont complémentaires. De même l'emphase n'est pertinente que si le mot qui en est le support est opposable dans une association née en telle circonstance de discours.

D'une manière générale, la régularité des emplois de discours (association $a+b$) crée à la longue de nouvelles unités (fusion $a+b = N$) qui s'intègrent dans l'inventaire sémantique de la langue.

BERNARD POTTIER

8. In the past, work in semantics has been predominantly historical. There is need now for more studies in descriptive semantics and for systematic comparisons between the semantic structure of different languages. Such comparisons, developing a method first applied by Bally, may enable us to establish the characteristic features and tendencies of the semantic structure of a given language. The following are some of the problems which might lend themselves to comparative treatment:

(a) The dosage of arbitrary and motivated, opaque and transparent words (onomatopoeia, analyzable compounds and derivatives, "popular" etymology etc.).

(b) The dosage of particular and generic, concrete and abstract terms.

(c) Special devices employed to convey emotive nuances of meaning (stress, intonation, word-order etc.).

(d) The wealth, distribution and organization of synonymic resources (e.g. Latin and French *versus* native in English, learned *versus* native in French).

(e) The relative frequency of polysemy; its favourite types;

ambiguities due to this factor, and the safeguards available against them.

(f) The relative frequency of homonymy and its causes (monosyllabism etc.); homonymic clashes; special safeguards; the part played by spelling in averting conflicts.

(g) The relative independence of the word as a semantic unit, and the role of context (verbal context and context of situation) in determining meaning (especially in the light of points (b), (e) and (f) above).

Such enquiries may perhaps form the basis of a semantic typology and may furnish criteria for the establishment of affinities and discrepancies between the semantic structure of different languages.

S. ULLMANN

9. (a) There appears to be some prospect of applying statistical method and probability calculus to the problem of meaning (see Shannon and Weaver, *Mathematical Theory of Communication*, 1949, p. 117. "The idea of utilizing the powerful body of theory concerning Markoff processes seems particularly promising for semantic studies, since this theory is specifically adapted to handle one of the most significant but difficult aspects of meaning, namely, the influence of context"). All linguistic structure is such that all except extremely short phonematic sequences may be described in terms of higher-order units, i.e. morphemes, phrases, sentences. These latter may also be defined either in acoustic (or formal) terms and in functional terms. The functional definition doubtless must attempt to give a statement of the environments in which the unit in question occurs. But meaning is also involved. For example, contexts such as

(i) O, my offence is X, it smells to heaven.

(ii) The X is but the guinea stamp, a man's a man for o' that.

determine the occurrence of the English morpheme-word *rank*; that is, they define it by contrast with other English words or morphemes of which the occurrence in both of these same contexts is altogether unlikely, e.g. *gross*, *standard*.

Such a functional definition is totally different from a verbal or referential definition, and much more exact. The quantitative criterion presumably could be established by some electrical apparatus, such as an autocorrelator, or some scanning device.

(b) There is less novelty about what has been called the "semantic differential," an objective measure of connotative meaning aimed at by psychologists, and discussed in a valuable report on psycholinguistic method (issued at Ithaca, N.Y. in August, 1951, still in mimeographed form) by John B. Carroll and others. The method rests chiefly on the assumption that polar opposites delimit an experiential continuum within which a particular concept is allocated. The problem here is to devise a means whereby the direction and position of a given semantic unit can be measured.

JOSHUA WHATMOUGH

PLENARY SESSION II

THE POSITION OF LANGUAGE IN PHILOSOPHY, LOGIC, AND SOCIAL ANTHROPOLOGY

- a — Can a logical calculus be devised that shall be structurally independent of the grammatical patterns of any language?

REPORT BY E. BUYSENS

Contributions from

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| 1. E. BUYSENS | 3. E. GLÄSSER |
| 2. A. J. J. DE WITTE | 4. W. HAAS |
| 5. J. WHATMOUGH | |



REPORT

by

E. BUYSENS

En général, les opinions émises concordent ou se complètent: logique et langue ne sont pas d'accord sur beaucoup de points; il doit y avoir moyen d'inventer un langage plus logique que les langues. Seul M. HAAS est radicalement opposé à la logique: il examine un type de langage logique et déclare que si l'on imposait cette structure aux langues, on les réduirait à être des machines. C'est exact; mais il n'est pas question de modifier les langues existantes: il s'agit d'instituer à leur côté un langage artificiel — probablement visuel — dépourvu de ce qu'on appelle métaphoriquement la vie du langage, et pourvu par contre de la rigueur logique des sciences exactes. Ce langage ne servirait qu'aux logiciens et aux savants.

Il reste d'ailleurs à prouver que la solution combattue par M. HAAS a une valeur réellement logique: l'auteur du présent rapport a précisément mis en question la valeur logique des notions de prédicat et de sujet.

La logique occidentale est explicitement considérée par M. WHATMOUGH comme issue des langues occidentales et différente, pour cette raison, des logiques impliquées dans les langues d'Afrique, d'Asie et d'ailleurs. Les autres linguistes semblent considérer implicitement que la logique est universelle comme la science, ou tend à l'être. M. WHATMOUGH les rejoint en préconisant l'étude des divers groupes de langues: en effet, cela aurait pour résultat de déceler les divergences, et permettrait d'aboutir à une logique universelle.

Pour M. GLAESSER, le fait qu'une vérité scientifique exprimée dans une langue donnée peut toujours être traduite dans une autre langue prouve que la pensée est indépendante de la langue. Cette thèse implique que l'on a procédé à cette vaste comparaison entre les langues que préconise M. WHATMOUGH et avec lui MM. DE WITTE et BUYSENS.

Pour M. DE WITTE, les éléments communs à toutes les langues pourraient ou devraient servir de base à la logique. Cette thèse implique qu'une valeur logique est reconnue aux éléments communs précisément parce que l'accord des esprits est réalisé sur ces éléments. Mais ce raisonnement n'a de

valeur que si l'on croit à la polygénèse des langues: si les hommes sont arrivés indépendamment aux mêmes conclusions dans le monde entier, il est très probable que ces conclusions sont vraies. Mais si l'on croit à la monogénèse des langues, les éléments communs remontent à une même source historique, peut-être à un fait fortuit, et l'accord des esprits provient simplement d'une convention commode.

Le rapporteur recommande de ne pas oublier l'étude des langues artificielles, qui ont généralement tendu vers plus de logique, en particulier l'Interglossa de L. Hogben.

Le rapporteur fait en outre remarquer que nous connaissons déjà deux langages artificiels, internationaux, dont la structure s'écarte nettement de celle d'une langue: il s'agit d'abord du langage des mathématiciens, physiciens, chimistes et astronomes, langage dont la rigueur scientifique est bien connue. Ensuite il y a la signalisation routière instaurée par la Société des Nations: cette signalisation constitue certes un langage très limité, mais sa structure offre le plus grand intérêt parce qu'elle s'écarte encore davantage de celle des langues. L'humanité a donc prouvé expérimentalement que l'esprit n'éprouve pas grande difficulté à se libérer de la structure linguistique.

En conclusion, le rapporteur propose que les congressistes apportent leur contribution à la session plénière sous forme de réponses aux questions suivantes:

1. Comment peut-on justifier l'idée que des caractéristiques grammaticales peuvent ou doivent servir de base à un langage logique?

2. Quelles sont éventuellement les caractéristiques grammaticales qui pourraient servir de base à un langage logique?

3. Quels faits peut-on alléguer pour ou contre la thèse que les notions de sujet et de prédicat ont un fondement logique?

4. Y a-t-il des faits prouvant que l'expression d'une vérité scientifique donnée est impossible dans telle ou telle langue uniquement à cause de sa structure grammaticale?

5. Quels faits peut-on alléguer pour prouver que la conception logique de la signification est différente de la conception linguistique?

6. Quels faits peut-on alléguer pour prouver que la logique de l'Occident n'a pas le caractère d'universalité que l'on reconnaît aux sciences exactes?

TEXT OF CONTRIBUTIONS

1. Les sciences exactes ont élaboré un langage logique pour leurs besoins; bien que ce langage soit issu des langues, il s'en est libéré structurellement dans une mesure qui ne fait que grandir. Cette invention a été possible parce qu'elle concernait des idées sur lesquelles tous les mathématiciens étaient d'accord.

La signalisation routière en usage sur le continent constitue un autre langage, encore plus indépendant des traditions linguistiques; lui aussi a été possible parce que l'on était d'accord sur certaines idées. Il a ceci de remarquable qu'il ne place pas les signes dans une succession semblable à celle des mots dans la phrase; il tire parti des deux dimensions du plan dans lequel il s'inscrit.

La preuve a donc été faite qu'il nous est très facile de nous affranchir de la structure linguistique. Le jour où les logiciens se débarrasseront de l'idée fausse que toute phrase comprend un sujet et un prédicat, et de l'idée encore plus fausse que le sujet est l'élément primordial, ils sortiront de l'impasse.

Il serait utile de comparer les structures linguistiques les plus différentes, et d'étudier les langues artificielles (Interglossa, de Hogben). Mais le plus urgent est de connaître objectivement les faits sur lesquels doit porter la communication: c'est ce qu'ont fait les inventeurs des deux langages cités ci-dessus.

ERIC BUYSENS

2. It is such a pity that Symbolic Logic in its investigations works regardless of the results of General and Comparative Linguistics. Comparative Linguistics in combination with General Linguistics should be given the following chance: to try to fix (after difficult analyses and syntheses) the principles of grammar, occurring in many variations in every language.

This must be of importance for the composition of a logical

grammar or logical syntax. It is noticeable that symbolic logicians know almost nothing of the results (however imperfect they may be) of Comparative Linguistics. Therefore an intense co-operation between the two groups is desirable. Then the logicians will pay more attention to the form (which e.g. must be the principle for the definition of the word-classes). Then a future grammar from the symbolic logicians will not be forced, but lean on values that humanity supplies in the domain of grammar (to be found everywhere by Comparative Linguistics in combination with General Linguistics). This co-operation is not incompatible with the principles of Symbolic Logic.

A. J. J. DE WITTE

3. Die Frage kann nicht mit Ja oder Nein beantwortet werden. Die notwendige Voraussetzung ist vielmehr die Erkenntnis einer Relativitätstheorie der Sprache.

Wie ist das zu verstehen? :—

(a) Es geht um die Grenzen von Sprach-Kritik und Erkenntnis-Kritik — die schon bei Aristoteles verwischt sind. Um sie ringen Kant, Hegel, Trendelenburg; bleiben Brentano, Kraus, Marty, Funke bemüht; auch Heidegger — Duns Scotus 1916 — phänomenologisch-teleologisch beeinflusst von Husserl, Rickert; Paula Matthes 1926.

Dieses Ringen wird elastischer, wenn M. Regula die Grazer Gegenstands- und Erfassungs-Theorie (Alexius Meinong von Handschuhsheim; A. Mally) für die Syntax-Forschung mobilisiert; die Struktur-Linguistik der skandinavischen Sprachtheoretiker sublimiert das kategoriale Moment denkpsychologisch. Subtil interpretiert L. L. Hammerich (Indledning til tysk Grammatik, København 1935, §§ 60 ss.!) Quidditas, Annitas etc. für historisch-moderne Syntax.

Bühler definiert die Leistung der Sprachfunktionen; Dempe, Emil Winkler, Alan H. Gardiner u.a. erarbeiten einen hohen Kritizismus der Bedeutungslehre.

(b) Inzwischen erscheint das ganze Kategorien-Schema und Grammatik-System der abendländischen Sprach-Logik und -Psychologie ins Wanken gebracht, denn Levy-Brühl

u.a. haben die "Pluralität der Kulturen," Leisegang eine Typologie der Denkformen aufgestellt.

(c) Die Pilatus-Frage erhebt sich: Was ist Wahrheit? Ist jede Darstellung "Wahrheit," ist alle "Wahrheit" nur Darstellung? Glässer, *Einführung in die rassenkundliche Sprachforschung* 1939 (Index s.v. Kategorien, Relativität, Verstehen) diskutiert das Problem: dass zwar Verschiedenheit der Denkformen, aber Einheit der Wahrheit besteht.

Diskussionsvorschlag:

Verschiedenheit der "Denkformen," der "Sprachen" besteht. Doch auch Möglichkeit des Verstehens, der "Übersetzung." Es gibt also "Spielarten" der "Darstellung." Was ist an der Darstellung "frei," was "gebunden"? Die Innere Form ist gebunden; die "Meinung" ist frei. Der gemeinte "Sinn" jedes "dargestellten" Sinnverhalts ist (von der Inneren Form der grammatischen Struktur, der sprachbaulichen Eigenwelt) frei, (vom So-Sein der sprachlichen Darstellung) unabhängig, (für anders-sprachliche Darstellung) übertrag-, versteh- und beurteilbar.

EDGAR GLÄSSER

4. From Aristotle to the present day, the analysis of logical language has tended to be confused with the logical analysis of language.

Language differs from a calculus as an organism from a machine.

Logical grammar guarantees logical structure to speech, and this by restriction of (a) sentences to two types of syntactical structure: predicative and relational, and (b) words to three parts of speech: subject, predicate, and connective. Every sentence expresses an analysis (i.e. a selection of a number of properties or segments from a segment), and every word indicates a segment, property, or relation.

If these restrictions were universally binding no language could be a living one. Syntax, if confined to merely putting together what is given in words, would fail in its primary task, which is to produce from a limited number of simple symbols

(words) an unlimited variety of complex symbols (sentences). Speech is a creative process capable of presenting what is not present in any one of its parts nor distributed over all. Sentences (the unit-phases of speech) disclose regular patterns of interaction amongst their constituent symbols: constructions, and constructions within constructions, like 'subject — predicate,' or '(adj. — nom. subst.) — predicate,' or '[(nom. subst. — prep. — subst.) — predicate] — adv.,' etc. (Cp. analysis into 'Immediate Constituents.'). A symbol's aptitude for a 'part' in a construction assigns it to a 'part of speech.' Constructions are not limited to the two types, and 'parts' in them not to the three of Logical Grammar. And the logical notion of meaning does not fit the efficient relations of syntactical interaction. (Cp. *On Speaking a Language*, Proc. Aristotelian Society, 1950/51.).

W. HAAS

5. The question whether a logical calculus can be devised that will be structurally independent of the grammatical patterns of any language calls not for a theoretical answer but for experiment; e.g. in addition to the structure of "Standard Average European" (i.e. largely western European languages), in the framework of which Symbolic Logic was created, the structure of each of the other broadly defined groups of languages (e.g. African, Amerindian, Eastern Asiatic), so far as it should appear *a priori* that each group does implicate a common logistic, must first be examined by experts and their answers to this question awaited. My own conjecture, in the interim, is neither (as some assume) that "syntax" creates logic, nor (as others assert) that "logic" is syntax, but that the truth lies somewhere between these extreme positions, and that language and "thought" re-act upon and re-shape one another. Whorf was wrong, for example, in declaring that Newtonian physics is a mere receipt from language, as if (notwithstanding the fact of nuclear fission) modern physics were a linguistic invention! But it is also a fallacy to suppose that your language habits have nothing to do with your logic.

JOSHUA WHATMOUGH

PLENARY SESSION II

THE POSITION OF LANGUAGE IN PHILOSOPHY, LOGIC, AND SOCIAL ANTHROPOLOGY

b—On what principle are word-classes to be defined and named?

REPORT BY O. FUNKE

Contributions from

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| 5. W. HAAS | 10. K. TOGEBY |

REPORT

by

O. FUNKE

Some preliminary remarks will not be out of place. Difficulty in discussing linguistic problems because of the ambiguity of our linguistic terminology which is usually double-faced (either referring to form or meaning; examples: 'sentence,' 'substantive,' 'oppositions,' 'function'). What is meant by 'word' whose existence is taken for granted when speaking of word-classes? Are there pseudo-words? [Ullmann, *Principles of Semantics* (1951), Porzig, *Das Wunder der Sprache* (1950)]. Why such wide-spread horror of psychological considerations about linguistic facts? Why are they often called 'extra-linguistic'? Again blurred terminology when using such terms as 'psychological,' 'logical,' 'ontological'? Is 'structural linguistics' only concerned with word-forms and group-forms and not also with meanings (lexical, relational)?

Our problem of 'word-classes,' *partes orationis*, is more or less bound up with all these questions.

It is burdened by a tradition of over two thousand years. This classic tradition with its well-known 8 parts of speech, starting with the Greek philosophers and grammarians, continued by the Romans and later grammarians, is still alive and more or less silently, perhaps somewhat reluctantly used and accepted by modern linguists. In spite of its obvious inconsistencies it has served its turn and seems to be universally understood. It goes without saying that it is unsatisfactory for general use as it was deduced from the structures of old Greek and Latin, the Indo-European inflexional type, and therefore not of universal validity. Concerning the classification of these 8 parts of speech it shows a mixture of various aspects by taking into account word-forms, group-forms (syntactical features) and meanings. I do not consider it my task to enter into the history and criticism of this classic terminology: such surveys have been given by Brøndal (*Les parties du discours*, French version 1948), and long ago by

H. Paul (*Prinzipien*) where this scholar declared it to be impossible to establish a strictly logical system of word-classes.

As there is a tendency in modern linguistics to base all classifications of words exclusively on form (morphologically and syntactically) a historical remark will perhaps be not out of place. Semantics found its way into grammatical terminology on a twofold line: the grammarians used to add semantic definitions to the chief parts of speech (e.g. Priscian: *proprium nominis est substantiam et qualitatem significare*) and such definitions are still to be found with modern linguists (e.g. G. Curme). A second semantic current entered grammar in the sixteenth century, a semantic distinction going back to Aristotle himself, who opposed *φωναὶ σημαντικαὶ* to *φ. ἄσημοι*, especially with regard to assertions where S and P, the chief semantic elements, were called 'categorematics' and the other words, serving as a kind of links and having no self-contained lexical meaning 'syncategorematics'; this general semantic classification was well-known throughout the middle ages (cf. Shyreswood's 'Synopsis': *principales* [subst., verb], *secundariae* or *syncategoremata* [adj., adv., conj., prepos.]), but not introduced into grammar. We find it grammatically employed with Linacre in England, with the Spaniard Sanctius (who classifies words into *nomen*, *verbum* and *particula*) and with Petrus Ramus who, however, gives it a completely new interpretation by refusing any semantic aspect and by trying to arrange the word-classes according to purely formal characteristics, the head-group falling into *voces numeri* (*nomen*, *verbum*) and *voces sine numero* (*adverbium*, *conjunctio*). If I am not greatly mistaken, we might call him the first 'structuralist.' The semantic distinction continued to live; Harris (18th c.) speaks of 'principal' and 'accessory' words, Mill of categorematics and syncategorematics, Marty at last of 'autosemantic' and 'synsemantic' expressions and recently Ullmann of 'pseudo-words' for the latter synsemantic group. In any case — I do not want to dwell longer on this problem — it is important to be aware of the undeniable fact that these are words which are lexically not self-sufficient; it is still a wide-spread error to think that all words denote concepts or something like 'thing-meant'! Language, by way of thought, partly unifies and simplifies 'reality,' partly splits it up into real or fictitious parts and relations.

After these introductory remarks I am going to give a general survey of the various attempts by which the classification of word-classes has been approached in recent times. I must expressly state that I cannot aim at completeness.

The chief dividing line is the classification according to word-sense or meaning (semantic aspect) or according to formal characteristics (morphological and syntactical aspects).

(I) THE SEMANTIC ASPECT

Philosophers of speech, psychologists and linguists have tried to find out a certain parallelism between word-classes and word-contents, i.e. categories of meaning.

Wundt, E. Otto, Ed. Hermann: nouns (substantives) denote substances (or beings), adjectives qualities, verbs processes (actions, states); adverbs are partly placed with the adjectives, partly given a separate function (adv. of place, time: 'Umstandswort'), prepositions signify relations. Conjunctions are usually classified with the prepositions. As far as I can understand, Otto and Hermann combine psychological and ontological aspects; they speak of 'Eigenbedeutung' (lexical meaning) and 'Beziehungsbedeutung' and seem to mean by this latter term (Beziehungsbedeutung) the reference of word-meaning to outward realities. It would indeed be a very welcome coincidence if such a parallelism existed; yet it is an illusion, as is not difficult to show. Take only the so-called 'abstract' nouns which may also denote processes, actions, states. Or consider the adjectives; do they really denote only qualities? What about 'the late king,' 'the beautiful picture,' 'the probable winner'? Hermann noticed the fact that such word-classes, the substantives, adjectives, verbs, have a peculiar semantic colour, a subsidiary meaning (an 'inner form') which, as it were, adheres to the substantival, adjectival, verbal word-form, but he did not clearly discern the importance of this problem. There is some truth in it when other linguists (Gardiner, Larochette, *Grammaire et Psychologie*, 1950, p. 109) emphasize that the so-called parts of speech, as to their meaning, are not primarily based upon the nature of the 'objects' to which they refer, but upon the mode of their presentation; e.g. 'the name of anything presented as a thing is a 'noun'.'

Other attempts at semantic classification are by Noreen

(*Vårt Språk*) who distinguishes 'substantial glosses' (nouns) and 'accidental glosses' (adj., part., verb) with further sub-groups, and by Bühler (*Sprachtheorie*) who speaks of two main groups: indicating words (pers. pron., demonstr.) [*Zeigwörter*] and all the others, naming words [*Nennwörter*], a general classification which seems to me to be untenable.

A far-reaching attempt at basing the word-classes on four categorical concepts has recently been made by Brøndal (1928, 1948 *Les Parties du Discours*); for the moment I should only like to refer to Jespersen's short review in his *System of Grammar* (1933, p. 12f.).

My own view is that the word classes as transmitted to us by the classic tradition cannot be classified according to strictly delimited semantic categories.

(II) THE FORMAL ASPECT (MORPHOLOGICAL, SYNTACTICAL = GROUP FORM)

Modern linguists speak of 'structural linguistic' formulation of the problem, and they refer to de Saussure (*Cours de Linguistique* p. 150-2, 190) where this scholar touches the question whether word-classes are to be classified according to logical principles (which he calls 'extra-linguistic') or according to their place in the language system (morphologically, syntactically). It is this consideration from which the modern problems derive, and it finds a certain parallel in the science of phonematics. Thus a pure formalism seems to prevail; yet as far as the syntactic or functional aspect is concerned, we nevertheless find a fluctuation between form and (relational) meaning.

The purely formal aspect is advocated by the Bloomfield school. B. Bloch and G. L. Trager (*Outline of Linguistic Analysis* §§ 4.6, 4.11): 'all our classifications must be based exclusively on form—on differences and similarities in the phonemic structure of bases and affixes, or on the occurrence of words in particular types of phrases and sentences. In making our classification there must be no appeal to meaning, to abstract logic, or to philosophy'. Concerning syntactic use they understand by it pure group-form: some words are used as the kernel of a group, others as attributes, and each in a different way; every language has categories of words with different syntactic 'function' (i.e. group-form).

In a recent publication (by various contributors) *Grammaire et Psychologie* (Paris, 1950) four scholars [Buyssens, Fourquet, Martinet, Larochette] have dealt with the problem of word-classes, especially with the nominal and verbal categories. They agree in their central points of view: word-classes differ in different languages; they are not strictly delimited sense-classes. They must be defined according to their word-forms and group-forms (morphologically and syntactically), the basis is their use in discourse. Yet when Buyssens characterises the 'verb' as the word functioning as 'predicate' and Fourquet as the element indicating 'time,' 'mood' and 'actualization' (i.e. making the phrase self-sufficient) we at once observe the vacillation of the term 'function' because such characteristics are semantic and not purely formal ones.

The contributions which have come in lately for discussion at the plenary session take similar views:

Word-classes are first of all formal classes. Buyssens (Brussels) and W. K. Matthews (London) accentuate syntactic function, W. Haas (Cardiff) and R. H. Robins (London) morphological and syntactic features; Togeby (Paris) considers the purely linguistic relations between words in the constructions of speech as the basis of a classification, yet allows of a semantic description afterwards. H. Glinz (Zurich) states that word-classes cannot be delimited for themselves alone, but only in connexion with word-forms, members of the sentence and the whole sentence-structure. He refers to his forthcoming book *Die innere Form des Deutschen. Eine neue deutsche Grammatik* (Bern 1952). P. Eringa (Helmstedt) makes some general remarks on the different kinds of linguistic and 'glottic' definitions of classes of minimal meanings. E. Glässer thinks that the classic grammatical categories are a 'hypostasis' of a late stage in linguistic development. He asks for a linguistic theory of relativity. He probably means that word-classes have to be considered as component elements in a synchronic language system.

(III) SEMANTIC AND FORMAL ASPECTS

O. Jespersen (*Philosophy of Grammar*, p. 58ff.) devotes a long and interesting discussion to the problem of the parts of speech, and he comes to the conclusion that all the three aspects (meaning, morphology, syntactic use) have to be taken into

consideration, that it is most advisable to start with the formal problems, but not to neglect meaning altogether.

I must confess that, in principle, I share Jespersen's point of view. In another way De Groot has lately taken up the whole problem again in a long article 'Structural Linguistics and Word-Classes' (*Lingua* I 427-501; 1948). His intention is to unite the formal and the semantic aspects and to establish for three languages (Chinese, Latin, Dutch) first the word-classes on morphological and syntactic principles and then to arrange them in word-class systems by introducing Bühler's aspects of *Ausdruck* (expression) and *Darstellung* (representation) as well as his distinction of *Zeigwörter* (indicating words) and *Nennwörter* (naming words referring to sth.). It is an ingenious undertaking which, however, breaks down, according to my view, because De Groot's main thesis, i.e. the identification of stem-categories with semantic categories, cannot be proved. It is certainly meritorious to pay attention to semantics, but to overarch the formal groups by a strictly delimited semantic structure seems to me a questionable procedure.

This, in short, is the present situation of the problem of word-classes.

(IV) NOMENCLATURE

Nearly all linguists seem to prefer the retention of the classic terminology because of its old standing, its international currency and its practical usefulness. When used with certain provisos and the necessary warnings the old terms will still serve their turn until something better has been devised and generally acknowledged.

TEXT OF CONTRIBUTIONS

1. Les conjonctions et prépositions ont toujours été définies par leur fonction syntaxique; les adverbes et les adjectifs le sont aussi depuis quelque temps; il reste à faire de même pour les noms, pronoms et verbes.

Définir le nom comme le mot qui désigne un être ou une chose, et le verbe comme un mot qui désigne un état ou un procès, c'est établir une coïncidence entre la classification des mots et la classification des idées. Cette méthode échoue, car on définit le verbe au moyen de deux noms: état, procès. En réalité le nom et le verbe, tout comme les autres parties du discours, se définissent par l'ensemble des fonctions syntaxiques qu'ils peuvent remplir.

On objecte parfois qu'un même mot peut être indifféremment verbe, nom, etc.; par exemple en anglais "hand." C'est aussi vrai que de dire que le mot anglais "fie" se retrouve dans "defy, defiled, fighter," etc. Nous ne confondons pas "fie" avec les sons correspondants des autres mots parce que nous interprétons le contexte; de même, c'est l'interprétation du contexte qui nous dit qu'il y a un nom "hand" et un verbe "hand." Un mot n'est pas une entité, mais un fragment isolé artificiellement dans notre comportement linguistique; il ne doit pas être classé d'après des critères historiques, mais d'après des critères fonctionnels.

Quant à la façon d'appeler les différentes classes de mots, elle n'a guère d'importance: qui se soucie de l'étymologie de "verbe, grammaire," etc.?

E. BUYSENS

2. Any analysis of a mental complex into an element which it has in common with other mental complexes and one or more elements which distinguish it from the other mental complexes is a definition. That also applies to our definition-of-definition itself. What a definition has in common

with other descriptions is its being an analysis of a mental complex, but that which distinguishes it from the other descriptions is that the analysis is one into a common element and one or more different elements, in other words its being an analysis *per genus et differentiam*.

In every science, whether physical, biological or mental, we may distinguish already two classes of definitions:

(i) Definitions of categories — a category being a scientific generic notion — in other words scientific definitions.

(ii) Definitions of terms — a term being a scientific generic sense of a minimal meaning or group of minimal meanings — in other words terminological definitions.

Thus we may distinguish in the field of logic:

(i) Definitions of logical categories, e.g. one of the category notion.

(ii) Definitions of logical terms.

And in the field of linguistics we may distinguish:

(i) Definitions of forms of language, in other words linguistic scientific definitions, e.g. one of the (inner) form of a class of minimal meanings. Thus one might define the pronouns as minimal meanings without specific lexical features. And the particles as minimal meanings without flexional features.

(ii) Definitions of linguistic terms such as *langue* not in the sense of *tongue* but of *language*.

Besides we may distinguish:

(iii) Definitions of the matter of glottic forms, in other words glottic interpretative definitions, e.g. definitions of the matter of classes of minimal meanings, in other words material definitions of what the questionnaire seems to call for convenience "word"-classes, e.g. a definition of the interjections as designing subjective treatment, or a definition of *maybe*, *no*, *yes* as modal interjections.

Also in the last case our definition-of-definition seems to hold good for the principle of defining.

PIER ERINGA

3. Nomenklatur und (Real-) Definition der grammatischen Kategorien sind abhängig von den (deduktiven) Systemen

der klassisch-(logisch) = scholastikomorph-philosophischen Grammatik.

Brugmann Germ.—Rom. Monatsschr. I, 1909, 209.

Pohlenz Göttingen 1939.

“Kasus” etwa: schon bei Priscian, Humanisten wie Scaliger; Sittig (Tübinger Beiträge . . . 13, Stuttgart 1931); Hjelmslev La catégorie des cas 1935; Glässer Einführung . . . 1939, 81.

Le Parallélisme logico-grammatical, Serrus, Paris 1933.

Besonders die Schulpraxis lernt “Sprache aus der Grammatik” (Rerum cognitio potior, verborum prior).

Linguistik lehrt — (mit Cato: Rem tene, verba sequentur) — “Grammatik aus der Sprache.”

Linguistisch (-genetisch) gefragt:

Lässt sich — mit historischen Kriterien — der “gordische Knoten” der Syntax-Theorie entflechten, den Apollonios Dyskolos mit seiner Lehre von der Kongruenz geknüpft hat? Cf.: Delbrück Syntax I, 8/9.

Eine “kopernikanische Wendung”:

Das idg. Verbum ist — proethnisch — medial=impersonales “Verbalabstraktum” (idg. *bhĕrĕ/tī, *sneighueti) (— cf. Infinitivus historicus: *hostes apparere*); Verbum finitum, Genera verbi etc. durch “Verschiebung der verbalen Auffassung” im Sinne einer “Übertreibung der Kongruenz” (synkretistische) “Grammatikalisierungen.”

Lindsay Lat. Sprache §§ 62 ss.; Edgar Glässer Wörter und Sachen 1938, 94; 1940, 120; Gamillscheg Ausgewählte Aufsätze 1938, 185; Ettmayer, Syntax II, 807; Kurt Stegmann von Pritzwald Handbuch für Deutschunterricht II², 652; Deutsche Literaturzeitung 1939, Sp. 1235; Finck, Zeitschr. vergl. Sprachforschung 41, 219; Hirt Handb. Urgerman. III, 1934, 111 (*signifer* etc.); Meillet bei Damourette-Pichon Essai de Grammaire III, § 872.

Also (u.a.): Mythologisch-animistische Personifikation (*Ignis* — *Aqua*, vedischer Göttername *Agni*-), Maskulin — Feminin, älter (— so etwa Meillet —) als Neutrum πῦρ — ὕδωρ (Cf. Bartoli Studi albanesi II, 33, 35; Arch. Glott. Ital. XXX, 64) ? Oder gerade umgekehrt?

Wer — mit Hirt, Idg. Gramm. III, § 240 — das Letztere glaubt, kann allenfalls das “Aktions-Schema” Ζεύς ὕει mit K. Stegmann von Pritzwald Wörter und Sachen 1938,

166 für "besseres" Indogermanisch gelten lassen, keineswegs aber für "ursprünglicheres"!

Ergebnis: Das indo-europ. "Aktions-Schema" ist eine (sekundäre) "Grammatikalisierung," das Verbum war proethnisch "neutral" (nicht "subjekt" = bezogen); die klassischen grammatischen Kategorien sind eine Hypostase eines Spätstandes der sprachlichen Entwicklung.

Vorschlag: Eine "Relativitätstheorie der Sprache." Im genetisch-linguistischen Lichte sehen wir, dass die Grammatik seit der Stoa auf der Spitze einer Pyramide aufgebaut hat. Trotzdem ist der Praxis Verbleiben bei den traditionellen Termini der grammatischen Nomenklatur zu empfehlen, weil sie eine seltene Homogenität internationaler Übereinkunft, zureichende praktische Klarheit, durch nichts zu ersetzende Bequemlichkeit und einen hohen formalen abendländischen Bildungswert besitzen. Für die Linguistik aber ist die Real-Bestimmung genetisch wichtig; die Einsicht in die Relativität von Theorie und Praxis genügt, die Nomenklatur als solche ist mehr oder minder irrelevant.

EDGAR GLÄSSER

4. (a) Die Wortarten können nicht für sich allein abgegrenzt werden, sondern nur im Zusammenhang mit den Wortformen, Satzgliedern und dem gesamten Satzbau.

(b) Nach dem Grundsatz des willkürlichen Zeichens (Saussure) und der blossen Schlüsselnatur aller Wörter (Gardiner) müssen auch die Kategorien als willkürlich anerkannt werden. Sie können daher nicht im Sinne einer "allgemeinen Grammatik" aus irgendwelchen Grundbegriffen abgeleitet, sondern müssen für jede einzelne Sprache aus deren konkretem Material gewonnen werden.

(c) Als "Einzelsprache" ist dabei ein "synchronisches System" zu verstehen, dh. dasjenige Zeichengesamt, das für eine bestimmte Sprachgemeinschaft zu einer bestimmten Zeit wirklich gilt.

(d) Die Forschungsmethode muss von den lebendigen Sprachteilhabern und ihrem Verständnis ihrer eigenen Literatur ausgehen. Sie darf keine vorgegebenen Einheiten

und Begriffe zugrunde legen, sondern muss alle Einheiten erst gewinnen durch geeignetes Experimentieren (verschiedenes Lesen eines Textes, Umstellen, Weglassen, Ersatz und Formveränderung von Stücken, unter ständiger Beobachtung der auftretenden Inhaltsveränderungen).

(e) Erst wenn auf diese Weise Einheiten und Gesetzmässigkeiten objektiv festgestellt sind, kann sie der Forscher aus seinem eigenen Sprachverständnis heraus interpretieren. Dabei muss er mit aller Art von Unvollkommenheit der zu bildenden Begriffe rechnen. Eine systematische Anwendung dieser Grundsätze auf das heutige Deutsch findet sich in der Schrift des Verfassers "Die innere Form des Deutschen, Eine neue deutsche Grammatik," 504 S., Bern 1952.

(f) Bei jeder Aufstellung einer Kategorie müssen vorgenommene Experimente und für die Interpretation massgebende Erwägungen klar ersichtlich sein.

(g) Wo Kategorien umgedeutet oder neu geschaffen werden müssen, soll man nicht die herkömmlichen Namen dehnen und umdeuten, sondern neue Namen bilden.

(h) Diese Namen sollen der betreffenden Sprache selbst angehören und einfach, unauffällig, aber sinnhaltig sein ("relativement motivés"). Bei jedem Namen soll angegeben werden, auf Grund welcher Erwägungen man ihn gebildet hat.

(i) Die Namen sollen jederzeit ersetzbar sein, wenn vollkommeneren Experimente und Interpretationen es erfordern.

HANS GLINZ

5. Of a method of classification we require (a) as few (and hence comprehensive) fundamental classes as possible, (b) the highest possible degree of differentiation within such classes, and (c) exhaustiveness.

The method of classifying linguistic units is determined by the method of analysis which obtains and defines them. Analysis defining a unit W may be either (1) of W itself, defining it by internal features: as construct of a specific kind of constituents, or (2) of an inclusive unit, defining W by function: as constituent of some specific kind of construct.

Classification of words will then be either (1) by their morphological construction, or (2) by their syntactic function.

The first method (referring to common 'bound features' of inflection, derivation, etc.) cannot be exhaustive for a language some words of which are morphologically simple. A purely conceptual analysis of words (referring to mere meanings such as 'substance,' 'process,' etc.) being excluded as linguistically irrelevant, the most general method of classifying words is the second, i.e. by syntactic function ('environment'). This method will consist mainly in examining reciprocal selections of words, yielding at the same time (a) complementary substitution-classes and (b) definitions of syntactic structures.

Since different words have some, but generally not all of their functions in common (the most general common function being that of being a word), we are assured of both sufficient comprehensiveness and sufficient differentiation of classes so established. Wherever possible, there will be further differentiation by reference to morphological construction. — The term 'part of speech' might conveniently be reserved to classes distinguished by syntactic function.

W. HAAS

6. La classe (ou partie du discours) à laquelle appartient un mot ne peut être définie scientifiquement que par les possibilités qu'a ce mot de se combiner avec d'autres mots (ou affixes pour les langues à mots variables) dans les phrases correctes de la langue. Le nombre des parties du discours, leur nature et leur rapport est différent pour chaque langue. Certaines de ces parties du discours sont très riches en mots et se partagent la quasi totalité du vocabulaire de la langue (ex.: verbe, nom, adjectif), d'autres ne sont formé que d'un petit nombre de mots mais fréquemment employés dans le discours (ex.: pronoms, nombres, prépositions), ce sont celle-ci "particules" ou outils grammaticaux, auxquels on pourrait réserver le nom de morphème, par opposition aux autres: les sémantèmes.

Dans les langues à mots quasi-invariables comme le vietnamien ou le tahitien le classement des sémantèmes en

deux catégories que l'on peut appeler noms et verbes ne fait aucune difficulté, par exemple on classera comme verbe tout sémantème qui peut être précédé du morphème de l'accompli (vietn. 'dā, tahit. *ua*) de sorte que le mot qui traduit "midi" dans ces deux langues (vietn. *tru'a*, tahit. *avatea*) est en réalité un verbe "être le milieu du jour" puisqu'il peut être précédé du morphème en question pour traduire "il est midi" (vietn. 'dā *tru'a*, tahit. *ua avatea*). Dans ces langues un certain nombre de mots sont à la fois nom et verbe, mais cela ne met pas en cause la distinction de ces deux parties du discours, puisqu'en anglais, où cette distinction est reconnue, une grande partie des monosyllabes est à la fois nom et verbe.

A l'intérieur de la catégorie du verbe on peut distinguer dans ces deux langues une catégorie de verbe d'état ("être grand," . . .) qui joue le rôle de nos adjectifs.

Le classement des morphèmes au contraire diffère de langue à langue. On trouve des catégories tel que les "numéraux" ou "spécificatifs" qui ne correspondent à aucune catégorie des langues européennes; il serait utile que le Congrès s'accorde pour définir et nommer ces catégories.

A. G. HAUDRICOURT

7. The question takes it for granted that word-classes must be universal categories; in my opinion they are not. Is *wish* in *my wish* and in *I wish* one and the same word? It is generally regarded in the former case as a noun, in the latter as a verb. However, the only way we have to distinguish between two different linguistic units is the commutation test, which yields a negative result in this case, because the expression (le signifiant) is the same, and then, it is unimportant how different the content (le signifié) seems to be; in *my gift* and *I give*, on the other hand, the commutation test proves that *gift* and *give* are different linguistic units, so that we can look upon *give* and *gift* as two words, and in the same way *wish*, *wished*, *wishes*; these three words are to be analyzed as *wish-O*, *wish-ed*, *wish-es*, and it is shown that the difference is to be found in the characters (-O, -ed, -es), but the theme *wish-* is always the same. If *wish*, *wished* and *wishes* are to be regarded as different words — and we must, for the sake of clearness,

distinguish between word and theme (Cf. Actes du Sixième Congrès International des Linguistes, pp. 282-283) — we cannot, in inflecting languages as English, establish word-classes but only theme-classes; it is only possible in isolating languages, where there is no difference between theme and word. We can also put it in the following way: the isolating languages possess only themes with zero character. Therefore, the establishing of theme-classes will be more important than the classification of words.

In languages of the inflexional type the classification must start from the categories of characters and register the dependences which exist between the parts of the characters (morphemes) and the parts of the themes (pleremes), since this classification is given by the structure itself. In the former, we can speak about CGANCp's, i.e. themes inflecting for case, gender, articles, number and comparison, and here belong the greater part of the Danish adjectives; in English, owing to lack of all inflection except comparison and case (*happy* - *happi-ly*), they must be named CCp's. For it is to be supposed that there exist in the theme certain pleremes corresponding to the morphemes with which the themes can be connected. In the same way the relations between derivatives and pleremes give us a new series of pleremes, and further on, the nexual relations can give fresh series, and the relations between different nexias, and so on. The final task will be to establish a pleremic alphabet, so that we may name the content of a given theme in the same way as we now can write its expression in a phonemic alphabet. This task will be an enormous one. Since the verbal character is a nexual character, special verb-classes will be sub-classes of the other classes.

As to the isolating languages the analysis will be the same; the only difference is that we must start from the junctional and nexual relations.

JENS HOLT

8. In setting up linguistic categories we should be guided by the data of language rather than by those of logic, philosophy,

or any other extralinguistic body of doctrine. This conceded, the vista becomes rather clearer.

Study of diverse types of languages shows that their significant elements are lexical — words or “*semantemes*” — and that these are mutually defined in syntagmata by the functions they exercise in such semantic groupings. The definition of word-classes then should be syntagmatic or functional rather than paradigmatic or structural, though where paradigms exist, these can (and do) contribute to definition by submitting morphological evidence. Where the morphemes are invariable and therefore belong to a unique formal class the onus of definition falls on syntagmatic relationships or “(syn)taxemes.”

The naming of word-classes should proceed from the known and the tested. There is accordingly no valid reason for abandoning the parts of speech although they may be subordinated, in the first place, to the dichotomy of part and particle.

As the sum-total of extant information on language structures is still far from complete, our linguistic categories must necessarily be empirical and tentative. When we shall have at our disposal what we now lack, we shall be in a position to know whether our classical terminology must be completely abandoned. As things are, we are merely confusing issues by multiplying the vocabulary of an unilluminating and unimaginative jargon.

W. K. MATTHEWS

9. It is now recognized that grammatical categories serviceable to linguistics can only be established by purely formal techniques of analysis, whatever analytical and classificatory methods may be useful to other disciplines. But consistently formal procedures are still rare in the definition of word-classes.

The word as a grammatical unit must be isolated by formal criteria. In some languages this can be achieved in phonological terms alone (fixed tone, stress etc.). In others this is not possible, and the criteria must be potentiality of

occurrence as a minimal free form and syntactic substitutability for such free forms.

Once isolated, words are classified formally. Appeal to meaning at levels other than those of morphology and syntax must be confined to the contextual attestation of the utterances under examination. Actor and Action, being non-formal, are, as universal categories, no less objectionable than Subject and Predicate. Word-classes can be established by reference to phonological structure (where grammatically significant), positional occurrence, collocational possibilities with other words, and membership of morphological paradigms.

The class-meanings of word-classes so established are defined in terms of their morphological "scatters" and syntactical functions. When established, word-classes may be designated "nominals," "verbals" etc., if these names seem applicable, but they add nothing to the meanings and definitions of the word-classes, which must have been established first by formal analysis. No preconceptions of a minimum number of classes or of the universality of any category must be allowed to influence the classification of words in any language.

R. H. ROBINS

10. La langue est en principe une structure indépendante de la logique, quoi qu'en disent les logiciens modernes. La logique, comme les mathématiques, opère avec des symboles internationaux où la même expression accompagne toujours le même contenu, tous les deux étant irréductibles, par exemple + et —. La langue est composée de signes dont tant l'expression que le contenu sont analysables: *moins* contient les phonèmes (muæns) et les morphèmes (petit + comparatif + cas adverbial). L'expression et le contenu changent d'une langue à l'autre: en français on n'a que 3 conjonctions de coordination: *et, ou, ni*, en anglais il y en a 6, qui partagent le même domaine: *both- and, either-or, neither-nor*.

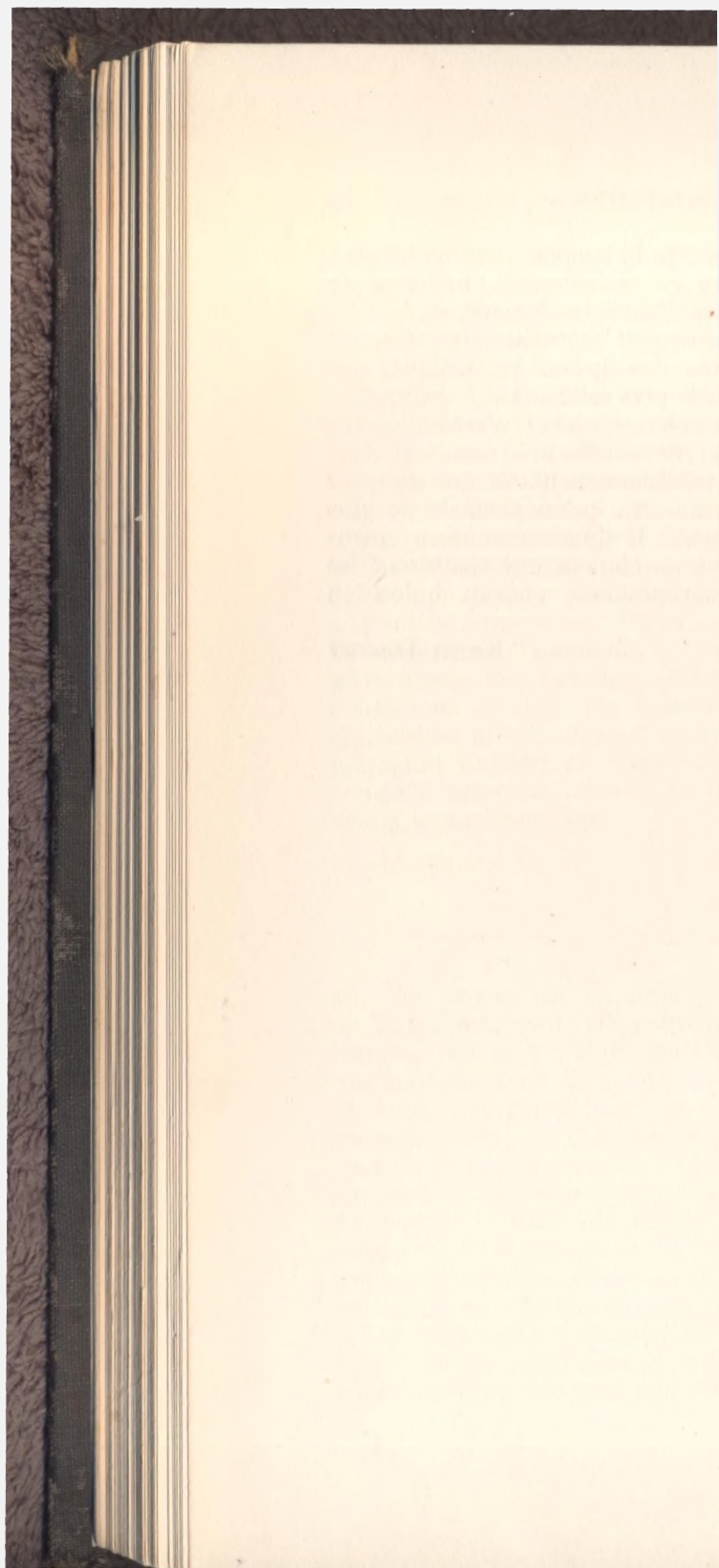
Les définitions fondamentales de tous les éléments de la langue, et par conséquent aussi des parties du discours, doivent reposer sur les rapports purement linguistiques des éléments

entre eux dans les constructions de la langue. On distinguera par exemple mots invariables et variables, à l'intérieur de ces derniers le classement se fera d'après les flexions, etc.

Une fois les définitions purement formelles données, on peut donner des éléments une description sémantique, qui trouvera probablement sa base la plus solide dans la logique.

Les définitions doivent être précises, mais il n'est nullement nécessaire que la terminologie reflète cette précision. Il vaut mieux s'en tenir aux termes traditionnels plutôt que de créer une nomenclature neuve qui ne sera qu'un obstacle de plus à la compréhension d'une théorie. Il n'y a aucun inconvénient à appeler adjectifs les suffixes esquimaux qui traduisent les adjectifs des langues indo-européennes, pourvu qu'on en donne une définition exacte.

KNUD TOGEBY



SECTION A

GENERAL LINGUISTICS

- (1) What are the relations between prosodic analysis and grammatical statement?

(Owing to the small response to this question, the Executive Committee decided to withdraw it from the programme of the Congress).

Contributions from

1. P. ERINGA

2. W. HAAS

3. W. K. MATTHEWS

TEXT OF CONTRIBUTIONS

1. All glottic elements common to the users of one given language constitute its grammar. A number of grammars may have some elements in common, called intergrammatical. It is a problem, which may be solved by intergrammatical sociography, whether and in which languages — e.g. English? French? German? Italian? — pitch accent / no pitch accent and interrogation, i.e. modal appeal / no interrogation are interdependent oppositions. It should be clear then that in such languages interrogation may not be a function of dubitative inversion, e.g. *are you?* (/ *you are?* / *you are.*) —, that emphasis must be a function not of pitch accent but e.g. of stress or accent d'insistance, stress being absence-of-absence of syllabic accent, i.e. of increased intensity — and that some sentences may be indifferent to interrogative pitch accent / not-interrogative absence of pitch accent, e.g. *Sir!* The term non-interrogation, not-interrogative function seems preferable to any logical term such as statement involving logical subject and predicate, just as interrogation to problem or question in its logical sense.

Users of such intergrammatical elements as interrogative pitch accent / not-interrogative absence of pitch accent, and passive deixis, e.g. *which* / active deixis, e.g. *the*, seem inclined to believe in their universal nature. Thus pitch accent in Attic *τί*, *πῶς*, *πότε*, *ποῦ*, *ποῖ*, *πόθεν*, *πῇ* and Latin *quid*, *quod* etc. seems to be re-interpreted as sentence accent with interrogative function.

However, the only syllabic mora of *τι* and *quid*, the first of *πῶς* and *πότε* shows pitch intensity as word accent / *τι* and *quid* etc. the opposite, a mora being an extensive unit, a syllable an intensive unit, a word a phonic unit. — But in an Attic or Latin sentence an additional, not distinctive, group accent starts absence-of-word-accent featuring such an enclitica as *τι* or *quid*, if the preceding word has no accent of its own on any mora of its ultima, respectively its only syllable, or on the mora immediately before that syllable — a word group and a sentence being a phonic unit as well as a

word, and an enclitica an atonic word following at any rate another word. If a dissyllabic enclitica follows a word with accent of its own on the mora before its ultima, a not distinctive group accent stops the enclitica's absence-of-word-accent in Attic. Compare (not distinctive) high pitch starting not-interrogative absence of pitch accent / not-high pitch starting interrogative pitch accent, and also high pitch stopping a non-final word group, in the above-mentioned modern languages.

The function of pitch accent in *σε, σου, σοί, αὐτό* etc. is emphatic (denying a negation), whereas absence of pitch accent in *σι, σου, σοι, αὐτο* etc. has no function at all. And nothing distinguishes any interrogative or any passive-deictic function from the emphatic function of *τί* or *quid*. So, why interpret emphatic function of Attic and Latin indefinita with pitch accent of their own as passive-deictic or interrogative function of sentence accent?

PIER ERINGA

2. If Linguistic Analysis is pushed beyond segments — to a discrimination of distinctive features, prosodic features will rank naturally among the phonological and morphological units of a language: as non-segmental units, tied either (a) to one other unit, or (b) to more than one.

Examples of (a) ('Prosodic Attributes') are:—

(i) on the phonological level: distinctive stress to German *ein* 'one' vs. *ein* 'a,' or distinctive syllabic length to *Bann* 'ban' vs. *Bahn* 'course'; or (ii) on the morphological level: English sentential intonation, 'falling' or 'rising,' contrasting 'statement' and 'question' (*He's in.* vs. *He's in?*)

Examples of (b) ('Prosodic Junctions') are:—

(i) on the phonological level: distribution of stress-intensities over two syllables, as in English '*im*port vs. *im*'port, '*con*tent vs. *con*'tent, or German '*wieder*holen vs. *wieder*'holen; or

(ii) on the morphological level: highest stress among different syntactical 'positions,' marking 'focus of attention' (*He's* 'in. vs. '*He's in.*') 'Order' is amenable to similar treatment, e.g.

'transitive order' (*John hits Bill.* vs. *Bill hits John.*) as a junctional feature tied to three units, one (a transitive verb) being constantly 'medial,' while 'initial' and 'final' positions indicate 'subject' and 'object' respectively.

Features (i), (a) or (b), are tied phonemic features, much like 'voicing' and 'unvoicing' in *bin* vs. *pin*, or 'glottalisation' and 'close juncture' in German *ver^hweist* vs. *verreist*; (ii), (a) or (b), are tied morphemic features, much like English -s (pl.), or *and*.

Prosodic, as many other tied features, have wide selections, and are, therefore, eminently suited to define comprehensive classes of linguistic units and important types of construction.

W. HAAS

3. "Prosodic analysis" belongs to the field of phonology in its wider sense, being concerned with some of the "raw material" of language. Its purpose cannot be divorced from that of phonematic analysis, for "prosodies" are secondary to phonemes and associated with them. By exclusive concentration on "linear" phonemes phonological study has tended to neglect the "supralinear" phonemes, and this has given "prosodic analysis" its opportunity to emphasise these unduly.

"Prosodies" become evident in the semantic synthesis or grouping of phonemes (sounds), i.e. in syllable, word, and sentence. They are mainly attributes of sounds (duration, stress, tone, etc.), but they may also be linear phonemes in certain phonological systems (e.g. *ə*, *ʔ*, etc.). Once isolated by analysis, "prosodies" are seen to mark off the edges of syllable, word, and sentence. In so doing they enter into relations with the grammatical or semantic aspect of language, of which, like the associated linear phonemes, they are the phonetic materialisation.

As "prosodies," like most phonemes, are "inarticulate" or semantically "insignificant," their formal and secondary nature requires no emphasis. They merely serve to aid and confirm the semantic analysis of grammatical "statement."

The patent defects of the technique of "prosodic analysis" are inherent in all techniques which are still in their initial phase of development. There is likely to be much trial and error before its limits and value are discovered. At the present time there is an understandable tendency in some quarters to exaggerate the importance of "prosodies" at the expense of other phonemes.

W. K. MATTHEWS

SECTION A

GENERAL LINGUISTICS

(2) Can a purely formal grammatical analysis be carried out on languages such as Chinese, in which all or nearly all the words are invariable, and if so, on what principles?

REPORT BY H. FREI

Contributions from

- | | |
|----------------------|-----------------------------------|
| 1. E. BUYSENS | 3. J. ELLIS and M. A. K. HALLIDAY |
| 2. A. J. J. DE WITTE | 4. T. KAMEI |
| | 5. K. TOGEY |

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THE HISTORY OF THE UNITED STATES OF AMERICA
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REPORT

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H. FREI

I. La réponse à cette question dépend de l'interprétation que l'on donne à l'expression *a purely formal grammatical analysis* et en particulier au mot *formal*.

I.1. Si l'on admet:

- (a) que *formal* se rapporte au signifiant;
- (b) que le signifiant est constitué par un ensemble de procédés;
- (c) que les procédés tactiques (c'est-à-dire l'ordre des éléments, par exemple *Pierre bat Paul/Paul bat Pierre* font partie de ces procédés,

une analyse grammaticale purement formelle (*sématique*, dans ma terminologie) peut être appliquée à des langues dont tous les mots ou presque tous les mots sont invariables, et cela par l'étude des procédés tactiques au moyen desquels les catégories grammaticales y sont exprimées.

I.1.1. Ainsi, selon la contribution de J. Ellis (Nottingham) et M. A. K. Halliday (Cambridge), le mot chinois, en tant que signifiant ("outer word-form"), comporte non seulement des terminaisons et autres éléments auxiliaires qui rappellent les mots variables de nos langues indo-européennes, mais encore des procédés tactiques ("the ordering of elements").

I.1.2. De même, A. J. J. de Witte (Nimègue), pour qui le chinois moderne n'est pas invariable, pense qu'une analyse grammaticale purement formelle peut être appliquée même au chinois traditionnel si le terme de *formal* comporte des "formes" telles que l'ordre des éléments, l'accent, la vitesse du débit, la mélodie et le groupement des mots.

I.2. Inversement, il faut supposer que chez ceux, comme E. Buyssens (Bruxelles), pour qui cette analyse "est exclue du domaine des mots invariables" et qui ne l'admettent, en chinois, que dans la mesure où cette langue possède des mots composés et dérivés (catégorie intermédiaire, selon Buyssens, entre celle des mots variables et celle des mots invariables),

les procédés tactiques ne font pas partie d'une analyse purement formelle.

1.3. T. Kamei (Cambridge) répond affirmativement à la question posée, mais en partant de prémisses différentes de celles du § 1.1. Dans une langue dont tous les mots seraient invariables, on ne saurait faire de différence, selon lui, au point de vue morphologique, entre sémantèmes et morphèmes, et seule l'analyse syntaxique permettrait d'établir cette distinction: l'expression des catégories grammaticales y serait *formelle* (dans le sens de "not materially expressed") et leur analyse, basée sur le principe de la syntaxe, le serait également ("formal grammatical analysis in the narrow sense of the word").

2. L'introduction, dans ce débat (par E. Buyssens et T. Kamei), de l'opposition traditionnelle entre morphologie et syntaxe ne me paraît pas heureuse, parce qu'elle implique les équivoques habituelles.

L'étude du chinois, comme celle de toute autre langue, comporte une *sématrique* ou théorie des signifiants (considérés comme des ensembles de procédés, y compris les procédés tactiques) et une *sémantique* ou théorie des signifiés, — une *monématique* ou théorie des monèmes (signes dont le signifiant est indivis) et une *syntagmatique* ou théorie des syntagmes (signes dont le signifiant est complexe).

3. Il est dangereux, même en matière de structure générale, de parler du chinois tout court. Les linguistes qui s'occupent de la structure des langues indo-européennes n'ont pas idée de mettre sur le même plan le russe et l'espagnol, ni le latin de Tacite et le parisien d'aujourd'hui. Il serait prudent, quand on cite des "languages such as Chinese, in which all or nearly all the words are invariable," de préciser de quel chinois il s'agit. Est-ce le chinois écrit, et lequel (car il y en a plus d'un)? Ou bien est-ce le chinois parlé, et, là encore, lequel?

Une analyse grammaticale purement formelle, pratiquée sur 2,000 phrases relevées chez un Pékinois, m'a révélé 3 sortes de procédés sématriques:

- (a) des différences bi-explicites (formule a/b);
- (b) des différences zéros (formule ab/a);
- (c) des différences tactiques (formule ab/ba).

Ces 3 procédés se rencontrent, avec un dosage variable, dans toutes les langues du monde.

TEXT OF CONTRIBUTIONS

1. Cette analyse connaît son plein développement à propos des mots variables, mais elle est exclue du domaine des mots invariables; entre les deux domaines, il y a la catégorie des mots composés et dérivés, et cette catégorie semble bien exister dans toutes les langues, en particulier en chinois. Les principes de l'analyse grammaticale appliquée au chinois seront donc les mêmes que ceux que l'on applique aux mots composés et dérivés de nos langues.

Il n'y a pas de fossé entre syntaxe et morphologie: la morphologie n'est qu'une partie de la syntaxe. En effet, la syntaxe étudie comment les signes se groupent en phrases, tandis que la morphologie étudie comment les mêmes signes se groupent en mots, c'est-à-dire en parties de phrases. Les méthodes de la morphologie ne constituent que l'application à un cas particulier des méthodes de la syntaxe. Quand on écrit le chinois en caractères alphabétiques, on est étonné de voir combien ses méthodes se rapprochent des nôtres.

E. BUYSENS

2. Modern Chinese is not invariable. But even on Traditional Chinese (purely) formal grammatical analysis can be carried out, if that "formal" includes "forms" as: placing, accent, tempo, melody and the grouping of words. But, indeed, the Chinese, like Englishmen, Dutchmen and other Western civilized people, express or expressed themselves partly without noticeable binding of the words by forms (prefixes, suffixes), placing, grouping, accent, tempo and melody, because with the help of feeling, intelligence and convention the producing of the concepts (words) is sufficiently precise in meaning for good understanding.

A. J. J. DE WITTE

3. Grammar is concerned with the forms in which words, the units of vocabulary, are put to connect them into sentences. Form includes, in Gardiner's terms, inner and outer form, word-form and sentence-form. While there is no question that all languages have expression of inner sentence-form through outer sentence-form (e.g. in the Mandarin sentence *tamn iding sh kangianle zamn le*, "they've seen us, definitely" (Chao) the order of the words indicates the subject, object, etc., and the presence of the words *iding* and *sh* indicates a particular kind of statement), there is a view that languages like Chinese have not only no outer word-form but no inner word-form.

But an account of Chinese grammar confined to sentence-forms will not tell us all we need to know, in addition to Chinese vocabulary, in order to construct (or even to interpret) Chinese sentences. For example, in the Mandarin sentence cited above, *-mn* (and possibly *za-* and even *ta*), the combining of *kan* and *gian*, and the two *le*'s (and the fact that any of these may occur in one type of word—"part of speech"—(and not in others) are included neither in sentence-form (though the second *le* may be associated with it) nor in vocabulary (though they may be given in dictionaries). In fact, the combining of the words *kan* and *gian* with inner form of 'Aktionsart' (see our 'Temporal Categories in the Modern Chinese Verb.') has outer form of order of main and auxiliary verb just as inner sentence-form has outer form of word-order, and *-mn* and *-le* indicating inner form of number and of tense are "endings" just as are the inflexions of say Sanskrit (the differences between inflectional and other endings being formal) and the final *le* indicating inner form of aspect is an outer form just as is the auxiliary *haben* in *haben uns gesehen* (the differences being again formal, including difference of position).

Thus Chinese outer word-form consists in endings or other auxiliary elements and the ordering of elements (and the inner word-form thus manifested includes parts of speech). These auxiliary elements do not occur except in this function; though some are written with characters having also an independent meaning (e.g. *le*, *liao*, "finish"), others are not (e.g. *mn*), so that a graphic analysis would not be essentially different.

J. ELLIS

M. A. K. HALLIDAY

4. If we assume an imaginary type of language in which all the words are completely invariable, we may examine how grammatical function would operate therein. In a morphological analysis, units corresponding to semantemes and morphemes must all be expressed as free semantic entities, independent and self-contained. In other words, "semanteme-words" and "morpheme-words" would be equally individual words, having the significance of semantemes and morphemes respectively. Thus no distinction can be made between "semanteme-words" and "morpheme-words" from the morphological point of view. In a syntactical analysis, however, it is possible to make such a distinction. The fewer "morpheme-words" there are, the more ought grammatical procedure to be formal (and not materially expressed). Formal grammatical analysis in the narrow sense of the word is carried out most exclusively in languages dependent entirely on syntactical procedure for their grammar. Since morphology is naturally not taken into consideration, syntax alone must become the principle of grammatical analysis. To sum up then, it is possible to carry out a formal grammatical analysis of a language in which even all the words are invariable: and the principle to be employed must be to base the analysis on syntax, however different the results of this process may be from the result of the analysis of inflexional languages.

It is, of course, a different problem to compile a practical handbook, from a utilitarian point of view, of a language in which all, or nearly all, of the words are invariable.

T. KAMEI

5. De même qu'on peut classer les particules d'une langue flexionnelle d'après leurs possibilités de combinaison avec les autres éléments de la langue, on devra pouvoir caractériser les mots invariables d'une langue isolante. Une tentative dans cette direction a été faite par M. Lê-Van-Ly dans *Le parler vietnamien* (Paris 1948).

KNUD TOGEBY

SECTION A

GENERAL LINGUISTICS

- (3) What are the central problems of form and meaning in negation?

and

- (4) What questions of general linguistic theory are raised by the study of numeration and of classifiers? What bearing have these questions on the categories of numerals and determinatives?

(Combined meeting)

REPORT BY W. E. COLLINSON

Contributions from

(3)

- | | |
|--------------|---------------|
| 1. J. ELLIS | 3. M. GUTHRIE |
| 2. P. ERINGA | 4. B. POTTIER |

(4)

- | | |
|-----------------------------------|--------------|
| 1. J. ELLIS and M. A. K. HALLIDAY | 2. P. ERINGA |
| 3. L. L. HAMMERICH | |

CONFIDENTIAL

What are the general principles of law and equity in relation to the subject?

(1) What questions of general principle have been raised by the facts of the case and the circumstances? What points have been raised on the facts of the case and the circumstances?

(Continued next page)

Report of Mr. H. G. G. G.

Continued on page

(2) What questions of general principle have been raised by the facts of the case and the circumstances? What points have been raised on the facts of the case and the circumstances?

(3) What questions of general principle have been raised by the facts of the case and the circumstances? What points have been raised on the facts of the case and the circumstances?

REPORT

by

W. E. COLLINSON

What are the central problems of form and meaning in negation?

Of the contributions subjoined that of Mr. Ellis points out in regard to the formal problem the distinction between a negation as a property of a sentence (including a verb) and a negation of one of many possible lexical elements. Some languages like Finnish have specific negative verbs. In some, e.g. Greek, the specific form of the negative particle is determined by other categories. He further discusses the spread of negation over the sentence including its effect upon the case of the object and refers to the problem of double negation. Mr. Eringa is concerned with 'oppositions,' the more comprehensive genus under which he subsumes negations. By his analysis he shows clearly the distinction between the English interjectional and pronominal *no*. The topic of 'oppositions' is discussed also by M. Pottier who by his example of the relation between *inobstruée*, *obstruée* and *desobstruée* brings up the distinction between the 'contradictory' and the 'contrary.'

To the rapporteur there appear to be three main situations in which negation is employed: (1) the noting of the absence of an object, quality, relation etc.; (2) the reluctance or refusal to accept a proposition; (3) the avoidance of a future event. All three situations find linguistic expression in childhood, cf. A. Sechehaye's examples of (1) *A pu lai lai* 'il n'y a plus de lait' and (3) *pa mena* to avoid going for a walk. The expressions under (1) tie up with the problems of the 'contradictory' raised by M. Pottier, which suggest the advisability of an investigation of the following points of actual usage: (a) the history and range of the modern tendency to form new words of the type *non-stop* [train], *non-utility* [goods], *non-examination* [subject], *Lust/Unlust* > *pleasure/unpleasure*, (b) the means

adopted by languages using a negative prefix for the 'contrary,' e.g. Lettish *nebags* 'poor', to distinguish between 'contrary' and 'contradictory,' and (c) the extent to which the Finnish distinction between the 'contradictory' prefix "*epä*," e.g. *epäkohtelias* 'impolite' and the 'privative' suffix in *hyödytön* 'useless' (Fr. 'inutile') has been fostered by Germanic contacts. Comparative studies would also be welcome on the use by some languages of a negative where others leave the implicit negation unexpressed (e.g. Fr. 'avant que . . . ne ! . . . etc.) and the complementary absence of a negative in some where it is normal in others (e.g. MHG use of *ie* for *nie* etc. in subordinate sentences). Finally, the use of terms like *the devil* as equivalent to a strong negative would repay study.

As to the spread of negation over a whole sentence mentioned by Mr. Ellis spoken North Welsh affords a telling example in *welis i m ono vo* (cf. Fynes-Clinton, *Vocabulary of the Bangor dialect*, s.v. *dim*) where there is no longer a specific negation as in *ni welais i ddim* 'I did not see anything' and where the hearer gathers that a negative is intended by the addition of the prepositional phrase *ohono* [*fo*] 'of him' after [*ddi*] *m*, lit. 'I did [not] see anything of him'.

What questions of general linguistic theory are raised by the study of numeration and of classifiers? What bearing have these questions on the categories of numerals and determinatives?

Of the contributions given below Professor Hammerich clarifies the relation between cardinals and ordinals in linguistics and mathematics and confirms the view expounded by Messrs Ellis and Halliday that the classifier or concretizer is historically prior to simple numeration. The joint contribution uses Chinese to exhibit the role of the classifier in the origin and development of numeration. Mr. Eringa analyses the structure of the Latin system of cardinals in their various groupings.

The use of 'classifiers' lies within the province of the Bantu or Chinese specialist rather than in that of the European

languages, where there are but a few relicts like *a hundred head of cattle*. With the growth of mathematics abstract numeration has become of paramount importance. In some instances cardinals stand outside the flexional system, but in some languages e.g. Finnish they may be fully declined. Not all European languages have developed an indefinite article out of 'one.' A thorough comparative survey of this development might bring out the stages and indicate the influences of one language on another. Some rather unspecific classifiers like 'pair' and 'couple' persist in spite of the loss of the dual number and 'dozen' is used in many languages in spite of the decimal system. Pro-numerals like 'some,' 'some few,' 'quite a few' (Norw. *atskillige*), 'several' represent various totals according to context — it would be of interest to compare the sort of numbers represented by 'several,' 'plusieurs,' 'flere,' 'mehrere' and the like. In contemporary English there is a vogue of reviving old assembly-names like *a pride* [*of peacocks*] often with humorous intent, perhaps as a reaction against the dullness of the usual terms, otherwise they are relicts of hunters' and fishermen's terms as in *a covey* [*of partridges*] etc.

TEXT OF CONTRIBUTIONS

(3)

1. The central formal problem in negation may be said to be the movement between negation being formally a property of the sentence (including of the verb), i.e. constituting a type of sentence, and being one among many possible adverbial attributes, i.e. lexical elements. Historically speaking there appears in general to be development from the first to the second, which is part of the general development of degrammaticalization of more concrete concepts (e.g. the dual) that proceeds side by side with the delexicalization of the more abstract "grammatical" concepts (e.g. "tense" and "aspect"). (Whether this development specifically indicates a central semantic problem depends on how negation is a concrete concept in the sense just mentioned.) But temporary development in the other direction does occur.

Negation may be said to be in some degree a property of the sentence when it is cumulated with other categories in one form, notably in the verb, as in Finnish or to a lesser extent Turkish or Japanese, (or, adjective/adverb, in English *little*, MHG *lützel*, beside *a little*, *ein lützel*,) or when the negative indicator, though a separate word, has its form determined by other categories, already fully expressed or not, (e.g. Latin *non*, *ne*, Greek οὐ, μή; or Chinese *bu* and *mei*, including tense-use (Cantonese *mou*), converse of determination of tense/aspect by negative mentioned below,) or when it is spread over the sentence, other categories being determined by it; when, in short, rules for forming "negative" sentences are required more complex than simple addition of negative adverb.

Negation is spread over the sentence, i.e. affects more than one constituent of it, in three ways: by original semantic determination (e.g. Russian genitive for object with negative, Imperfective aspect with negative); by the consequence of purely formal development, as phonetic "decay"; and the intermediate case, by concord ("double negative," e.g. in Greek), sometimes in association with phonetic weakening, e.g. French *ne . . . jamais rien* — MHG double negative is a more complex case, since double negative was known in OHG before weakening of *ni*.

J. ELLIS

2. The form of any language, whether inner or outer form, is the object of linguistics as a science. In the field of form we may distinguish two categories of direct oppositions with respect to the quantity or quality of their terms.

- (i) Formally equipollent oppositions.
- (ii) Formally unequipollent oppositions.

The terms of the former are equally specific, equally positive. But the terms of the latter stand in the relation of genus and species — a species being more positive than the genus involved. The matter of any inner or outer form, however, concerns glottic interpretation and reflexion as an act. Thus an opposition of category (i) may be interpreted as: a) one of positive terms, b) one of negative terms, c) one of negative and positive terms. And an opposition of category (ii) may be interpreted as: a) one of terms with and without a positive element, b) one of terms with and without a negative element.

Examples:

- (i) (a) the inner opposition *father/mother*, the outer opposition *p/t/k*;
- (b) the inner opposition *le/la* in French distinguished by non-femininity/non-masculinity;
- (c) the inner opposition *no/yes*, and the outer opposition *ð/ð̄, p/b*.
- (ii) (a) the inner opposition *man/woman*, and the outer opposition *ð/ð̄* in Dutch;
- (b) the inner opposition *one/none*.

The interjection *no* and the pronoun *no* are not identical. The former is featured by the inner elements: (1) subjectivity, (2) modal answer, (3) certainty, (4) denial. The latter is featured by (1) objectivity, (2) quantity, (3) absence of quantity. The opposition subjectivity/objectivity distinguishes the two main classes of minimal meanings: interjections/pronouns etc. The opposition modal-answer/appeal distinguishes *no/yes/maybe* from the other interjections, e.g.

pst. The opposition certainty/doubt = *no/yes/maybe*. The opposition denial/assent = *no/yes*. The opposition quantity/quality distinguishes *no/some* = negative/positive. The opposition absence of quantity/presence of quantity distinguishes *no/some*. The interjections constitute in a maximal meaning by themselves independent clauses or co-clauses, e.g. *No?/Oh, no./No, Sir!* But *no/some* determines another minimal meaning or group of minimal meanings in a group, e.g. *no more; no sugar, no eggs; no smoking allowed*. The interjection *no* and the pronoun *no* may be determined by emphasis, i.e. negation-of-negation, "not-not," and the former directly, the latter indirectly by non-interrogation/interrogation, i.e. modal appeal, in a maximal meaning (meaning of a sentence). Irony, however, is not a matter of meaning but of sense opposite to a meaning.

The outer oppositions *no/yes* and *no/some* are indirect : *no/note/not/net/yet/yes*, and *no/so/see/seem/some*. However, the interjections *no* etc. are meanings of morpheme-words, being a category of outer forms which are neither words nor morphemes. Cf. with so-called words in Chinese. But the pronoun *no* is a meaning of a monomorphemic word, *nowhere* one of a dimorphemic word — the microphoneme *n-* being no morpheme, e.g. *one/none* = *ice/nice*. The morpheme-words constitute by themselves phrases, but the monomorphemic word *no* does not. In a sentence with absence or presence of interrogative pitch-accent *no* and *no* may have (emphatic) stress, i.e. absence-of-absence of increased intensity, i.e. of syllabic accent.

Any formal connexion between the negative fact that such morpheme-words as *no* are neither words nor morphemes and the positive fact that their meanings belong to one of the two main classes seems undeniable.

PIER ERINGA

3. An example of the difference in the result obtained by the use of the criteria of form and meaning respectively in the study of negation is provided by what occurs in some Bantu languages.

In closely related languages there may occur any one of the following varieties.

- (a) The negation may be characteristic of the sentence associated with the presence of a special "negative word." In this case it is immaterial whether the sentence contains a verbal word or not. The negative word most commonly occurs at the end of the sentence.
- (b) In other languages a negative sentence contains a negative element in close relationship with the verbal word. In some languages such an element is prefixed or suffixed, whereas in others it occurs in an infixed position. In these circumstances there is an identical range of negative and affirmative sentences.
- (c) More commonly in Bantu languages negation is an integral part of the verbal conjugation. The effect of this is that there are special negative tenses which are normally fewer in number than the affirmative tenses.

M. GUTHRIE

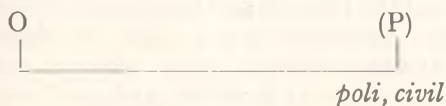
4. Distinguer deux catégories de négations:

(i) La négation par contraste lexical (*poli-grossier, civil-rude . . .*).

Elle dépend du contexte lexico-sémantique mobilisé à un moment donné chez un individu donné. Elle aboutit à la notion momentanée de "contraire." Le concept opposé ne dépend que d'une association d'habitude (le "contraire" de *long* peut être *court* ou *large*). Il s'agit d'un fait de discours, non de langue, et doit être rejeté d'une étude de linguistique générale, comme n'étant pas systématique.

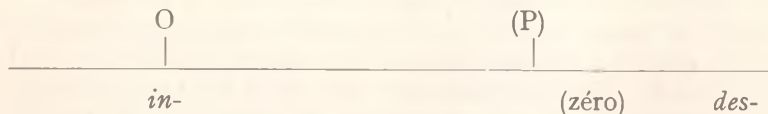
(ii) La négation par modificateur (*poli-impoli, civil-uncivil, to be-not to be . . .*).

Toute notion se développe à partir d'un zéro de contenu jusqu'à une plénitude de contenu (P), caractérisée formellement par l'absence de morphème:



Le rôle de tout morphème modificateur est de saisir ou porter la visée à un point différent de (P):

(a) Ex. *une rue obstruée*. Par saisie minimum contre le point O, on a *in-obstruée*; par report de la limite à un point au-delà de (P) (car telle est la caractéristique du préfixe *des-*), on a *des-obstruée*.



(b) Ex. *informe* "qui n'a pas encore de forme" (saisie antérieure à (P)), *difforme* "qui s'écarte d'une forme donnée (saisie postérieure à (P))".

(iii) Conclusions.

(a) Du côté du signifié, seule est systématique la négation par modificateur, dont le rôle essentiel est d'obtenir un point de visée éloigné de la limite (P) de plénitude de contenu (Unité le conditions).

(b) Du côté du signifiant, à priori tout modificateur peut avoir un sens "négatif"; en réalité, il doit ou saisir à l'instant O (+ε) (lat. *in-*, *non*, germ. *un-*, *not*, *nicht*) ou entraîner un mouvement d'éloignement de la limite (P) qui peut ainsi être saisi en son instant terminal (lat. *dis-*, *de-*, *minus*, angl. *-less* etc. . . .). Pour chaque langue, il existe un système de modificateurs à déterminer (Pluralité d'expression).

BERNARD POTTIER

(4)

1. Classifiers occur in two main categories of languages (some languages belonging to both categories): the languages of East Asia and the Islands; and languages of primitive peoples. In the latter, classifiers are a case of obligatory spatial specificness (namely shape, etc.; another case in other grammatical categories is location) and are inseparable (often

formally) from numerals, i.e. numeration is not abstracted from other, concrete qualities. The problems involved include not only the origin and development of numeration but the semantic function of the classifier, including as determinative, once the numeral is abstracted, which may be seen in languages like Chinese.

In Modern Chinese the classifier may be used alone before the noun, meaning in Mandarin "a ——" (in Cantonese "the ——"), contrasting with the rarer order alone after the noun which forms generic compounds, e.g. *zh chuan* 'a boat,' *chuanzh* 'boats.' The latter use is apparently a literary construction, derived from the Ancient Chinese order noun-numeral-classifier; the former is a special case (with ellipsis of qualifier) of the Modern Chinese order qualifier-classifier-noun.

According to the rule that what qualifies precedes what is qualified, the Modern Chinese classifier must be taken as one unit with the qualifier (not with the noun over against the qualifier), as a suffix or "ending" of the qualifier showing a sort of concord with the noun, depending on its semantic type (thus functioning lexico-grammatically).

The classifier may be used without noun (e.g. *iben (shu)* 'one (book)') in a kind of pronominal function, thus corresponding again to Bantu determinative prefixes in qualifiers. This pronominal alternant of certain nouns appears to occur also in some lexical compounds (as in Lao), e.g. *duben* 'reading-book.'

Thus the Modern Chinese classifier functions both with noun and replacing noun, both as word-formative and syntactically.

In this semantic function as concretizer, Průšek compares the classifier, together with the second part of purely lexical (non-classifier) compounds (e.g. *siangshu* 'oak-tree'), with the auxiliary verb following the verb, which makes concrete the generic meaning of the verb.

J. ELLIS

M. A. K. HALLIDAY

2. Synopsis of Latin Cardinals

I. MINIMAL MEANINGS

(a) *mille/millia* contain the primary bundle : (1) objectivity, (2) substance. Apart from the non-lexical determinants (3) number, (4) oneness/more-than-oneness) they contain a lexical (3) number, (4) '1,000.'

(b) The pro-cardinal *quot* contains : (1) objectivity, (2) quality, (3) number, and *quotquot/quotcumque*, *tot/aliquot*, *multa/pauca* also emphasis/ever, positive/negative deixis, many/few. But the cardinals contain the following lexical determinants of number as a quality:

A : 11 simple determinants :

They distinguish : *unum/duo/tria/quattuor/quinque/sex/septem/octo/novem/decem/centum*, a more-dimensional opposition.

B : 7 co-ordinative determinants:

They have one common element but are distinguished by 7 elements distinguishing *undecim/duodecim/tredecim/quattuordecim/quindecim/sedecim/septendecim*, also more-dimensional.

C : 16 subordinative determinants:

The one-dimensional oppositions *viginti/ducenta*, *triginta/trecenta*, *quadraginta/quadringenta*, *quingenta/quingenta*, *sexaginta/sescenta*, *septuaginta/septingenta*, *octoginta/octingenta*, *nonaginta/nongenta* are distinguished by 2 primary elements. But 8 secondary elements distinguish the more-dimensional oppositions *viginti/triginta* etc., *ducenta/trecenta* etc.

D : 16 compound subordinative determinants:

The one-dimensional oppositions *undeviginti/duodeviginti*, *undetriginta/duodetriginta*, etc., *undenonaginta/duodenonaginta* are distinguished by 2 primary elements. But 8 secondary bundles distinguish the more-dimensional oppositions *undeviginti/undetriginta* etc. and *duodeviginti/duodetriginta* etc. The bundles contain one common element *de*, subordinate to the subordination (<) of the elements distinguishing the more-dimensional oppositions *undeviginti/*

undetriginta etc. and *duodeviginti/duodetriginta* etc. to another common element, e.g. *undeviginti* = *un* > [*de* < ('2' < '10')].

II. GROUPS OF MEANING

In *viginti-unum* 2 meanings are co-ordinate. In *duo-millia*, *duo* determines *millia*. In *unum-et-viginti*: *unum* and *et-viginti* are co-ordinate but *et* determines *viginti*: *unum* ~ (*et* < *viginti*).

Conclusion: Logic cannot solve lexicological problems. Only 51 (= 1+11+7+16+16) notions of the logical series 1, 2, 3 etc. correspond to minimal meanings in Latin. Those meanings imply 12 (1+11) elements — apart from *de* — which involve the element number. Those elements form a more-dimensional opposition. But the logical series 1, 2, 3 etc. is one-dimensional, and 2, 3, 4 etc. imply and involve only 'one'.

PIER ERINGA

3. The mathematical problem of numbers and the linguistic problem of numerals are connected in a puzzling way. The linguist has no doubt that the relation between cardinals and ordinals is an important one, nor that ordinals are derived from cardinals. Until Cantor invented the transfinite numbers, the difference between cardinals and ordinals was of little use to mathematicians, who, however, generally like to derive cardinals from ordinals. Sometimes the current definitions of cardinals and ordinals are not intercoherent, even among mathematicians.

The conception of number is no primitive human conception. It is still a difficult one to the child and, indeed, to many adults as well. If, as in some languages, only numerals for '1' and '2' are found, there is no conception of number. This conception presupposes (1) the realisation of the fact that '2' is 1 more than 1, and '3' 1 more than 2, and so on (the numerical order); (2) the invention of the device of marking the numerical order (1 + 1 + 1, and so on): this marking is called counting and is generally done by means of gestures and/or words.

A cardinal is the expression of a quantity which is determined by a given place of counting. An ordinal is the expression of a place of counting which is determined by a given quantity.

To the mathematician the place of counting is simply the place within the numerical order. To the linguist the relevant expression is by means of words.

The importance of the primitive conception of '2' not as $1 + 1$ but as a pair, is clearly seen in languages which preserve the dual number. To be deprived of the dual means a revolution to the mind.

A comparison of quantities of given measures (e.g. "as many as the fingers of my hands") does not presuppose the numerical order; this is seen in languages deriving lower numerals from higher ones, expressing e.g. '9' by "not fully 10" (Eskimo).

LOUIS L. HAMMERICH

SECTION A

GENERAL LINGUISTICS

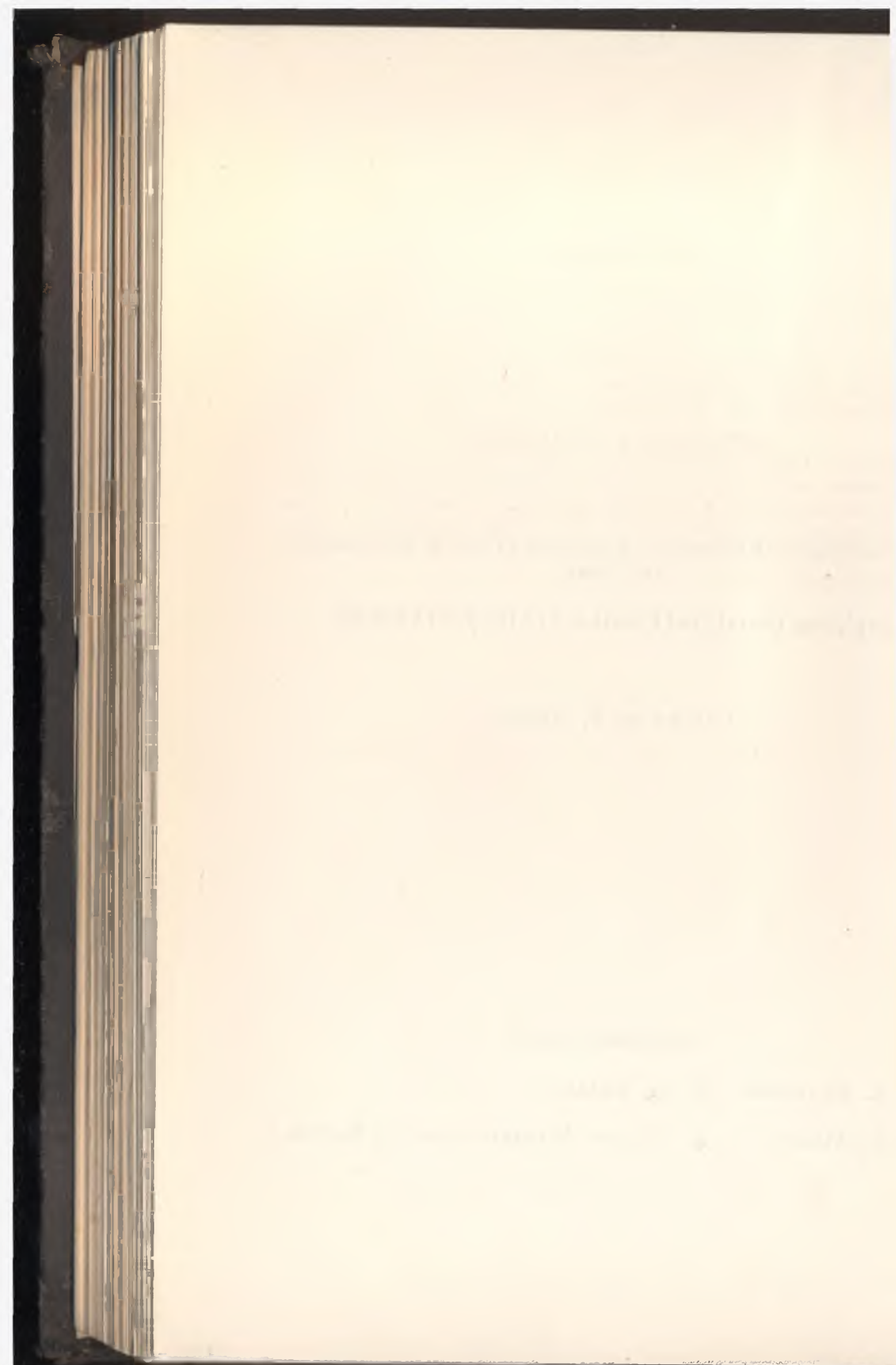
- (5) Conceptual dictionaries, dictionaries of usage: their nature and form.

(replacing the original Question 5 of the First Circular)

REPORT BY F. MEZGER

Contributions from

- | | |
|---------------|----------------------------------|
| 1. E. BUYSENS | 3. G. MELLOR |
| 2. W. HAAS | 4. W. VON WARTBURG and R. HALLIG |



REPORT

by

F. MEZGER

SYSTEMS OF LINGUISTIC EXPRESSION, CONCEPTUAL
DICTIONARIES, AND DICTIONARIES OF USAGE

The conceptual dictionary as well as the dictionary of usage refers to a system of linguistic expression. This system of expression, as a rule, is that of a spoken language. These works embody in their presentation of linguistic material parts of this system of expression. Both types of dictionary aim at practical goals, the dictionary of usage exclusively, and the conceptual dictionary combining with the practical task the theoretical investigation into the nature of linguistic expression and into the thought-content of linguistic expression.

In attempting to realise the practical goal of these two types of dictionary, the compiler must take the standpoint of the man who speaks or writes and tries to find the right expression. Both types serve to provide the reader who consults them with information about linguistic usage which he does not remember, is uncertain about, or in which he is deficient. As to form, the dictionaries are different. The conceptual dictionary, as a rule, arranges the word-material according to ideas or concepts. There are alphabetical conceptual dictionaries, in which, in order to avoid the alphabetical index needed in usual conceptual dictionaries, the compilers assemble clusters of words having a similar meaning around a basic word of the group, and arrange these basic words in alphabetical order. Explanations about usage are scarce or absent. The dictionary of usage is alphabetical, and usually gives explanations, a few synonyms and quotations to illustrate the usage, and often a variety of other useful information. The organization of the dictionary of usage varies a good deal.

Both types of dictionary refer to the language which man has in himself. When he undertakes to speak, certain linguistic evocations, certain linguistic associations take place,

and certain linguistic expressions are formulated; all these are interrelated with certain concepts. Sometimes a human being does not succeed in bringing this process to a good end; the linguistic consciousness fails him. Here such tools as the conceptual dictionary or the dictionary of usage perform the task of stimulating the failing memory and the wanting ability to formulate, in short, of supporting and enhancing the faculty of good linguistic expression.

These dictionaries cannot answer to the individual process going on in the mind of a certain individual at a certain time. They try to furnish references to a kind of superstructure of linguistic expression which is common to a social group speaking the same language. The process of formulating these references varies in difficulty. The task is made easier by the fact that these references to the linguistic superstructure serve at the same time as references to the system of expression of the individual. It is true that the linguistic superstructure of the social group is not identical with the system of expression of the individual. It is by far more comprehensive than the individual's linguistic range of expression. And yet the two systems are correlated to such an extent that for practical purposes the references aim at an almost identical speech-content. The individual may try to establish a contact with any segment of the system in order to further his linguistic expression and to expand his knowledge of the world; he always will be in a position to provide himself with means of expression and an additional knowledge fitting perfectly into his own expressional system and his own image of the world. What are the reasons for the fact that the linguistic systems of the society and of the individual dovetail in their over-all set-up, in the basic lines, and even in most details? It is true that language is a product of convention. At the same time, language is a closely inter-woven fabric. It shows a far-reaching unity and close interrelations. It is an organization which is accessible to differentiation, finer articulation, and yet maintains its identity. Added experience and finer insight into the external world, into the processes of the intellect, will shape the common linguistic superstructure without dislocating the foundations. It is, and will remain a continuous, coherent, unified system. The individual is born into the system of his social group. It provides him with

the means of expression. He adapts himself to it. He strives to come nearer and nearer to it, to this coherent system. He must act in this way, to make himself understood. In learning a language he also acquires the concepts and images, the judgements and discriminations, the predilections and prejudices, the values and prepossessions which are embodied in the language. The common ground for the individual, as well as for the group, is experience. The contents of experience are similar, if not identical. Shared experience makes understanding possible. As we shall see, this fact is of the greatest importance for the setting up of a project of conceptual dictionaries aiming at comparing the world of expression of one system with that of other systems.

A CONCEPTUAL DICTIONARY OF A SPOKEN LANGUAGE

We avoid the name ideological dictionary on account of its political connotations; otherwise we should have preferred it. The term ideological was considered appropriate by several authors to describe their particular intention and work: T. Robertson, *Dictionnaire idéologique. Recueil des mots, des phrases, des idiotismes et des proverbes de la langue française classés selon l'ordre des idées*. Paris 1859; E. Gomez Carrillo y A. de Sola, *Diccionario ideológico, para facilitar el trabajo literario y enriquecer el estilo*. Madrid 1925; J. Casares, *Diccionario ideológico de la lengua española*. Barcelona 1942. The title conceptual dictionary corresponds to the German 'Begriffswörterbuch' and the Danish 'Begrebsordbog'.

How are these dictionaries classified and arranged? *The Thesaurus of English Words and Phrases* by Peter Mark Roget (1852) has 6 classes, 24 sections, 1,000 groups; the Spanish *Diccionario de Ideas afines* by Eduardo Benot (1899) and the French dictionary of Robinson follow the same classification. Schlessing's *Deutscher Wortschatz oder Der Passende Ausdruck*, with its 6 classes, 27 sections, and 1,000 groups changes the number of the sections, whereas Bring's *Svenskt ordförråd ordnat i begreppsklasser* keeps only the number of the groups, omitting the classes and sections. The Dutch dictionary of L. Brouwers, *Het juiste woord. Beteekenis-woordenboek der nederlandsche taal* has 10 classes, 41 sections, 1,000 groups, and Harry Andersen's *Dansk Begrebsordbog* has 10 classes, 20 sections, 892 groups, following in its general set-up Franz

Dornseiff's *Der deutsche Wortschatz nach Sachgruppen*, which is divided into 20 sections and 906 groups. The fact that the groups amount to just one thousand demonstrates the relative arbitrariness of the classification.

The thesaurus proper is generally preceded by a tabular synopsis of categories and followed by an alphabetical index guide, so that the user may more easily find the right expression, a synonym or antonym for any given word, "suitable words to express a given idea," . . . "appropriate words or new ideas on any given subject" (Roget's International Thesaurus, 1940). Dornseiff has a selective index guide, a very valuable comprehensive bibliography of books and articles (partly contributed by V. Grundtvig) representing "conceptual" research, and a discussion on "Wortschatzdarstellung und Bedeutungslehre."

The ideological dictionary of Casares fulfills the wishes of the user more perfectly than any other work of this type. It consists of three parts. Part three is a usual dictionary giving clear definitions of the meanings of the words. The second part, the thesaurus, lists under more than 2,000 headings, which are alphabetically arranged, all the words which are connected with each specific heading. The synoptic part, part one, is very well thought out and organized. The conceptual organization of the work would have been more completely carried through if the three parts had been unified by relating part two and three to the categories of part one (Wartburg). The excellently planned and executed *Lappische Wortschatz* by Eliel Lagercrantz combines an alphabetical dictionary of etymological word-families illustrated by essential quotations and explanations with a conceptual system referring to the dictionary and shedding a clear light on the Laplandish system of expression.

DICTIONARIES OF USAGE

A dictionary of usage can be organized on the same pattern as a dictionary of a language not spoken any more. The linguistic context is to be illustrated for both types. Quotations are the backbone for both of them. An identical system of illustrative examples may be used. There are, however, some differences. The conceptual dictionary must be all-inclusive, since the language being systematized is not

spoken by the user. Each word must be treated in the conceptual dictionary; not so in the dictionary of usage. This work must supply the right word, a word the user is not familiar with. Such words may be fringe words, vague in meaning by their very nature; such as, for example, expressions designating taste or smell, values or sentiment. Or they may be words of which the particular user has no knowledge. Essential quotations and an explanation will as a rule be of help. Selective as a dictionary of usage must be, (Haas), in some cases a word is to be shown in all the functions it can perform, (Buyssens), a requirement which applies to each word of a conceptual dictionary of a dead language. How frequently does a word occur? Is it more customary, or less so, more adequate or less? (Haas). One must discriminate between synonyms, and also between near-synonyms and nearly equivalent constructions, (Haas). Synonyms which are mutually exclusive in their use, such as "alive" and "living" are to be pointed out. The syntactical possibilities (Buyssens), are to be pointed out. The syntactical possibilities (Buyssens), word-order, and stylistic phenomena, (Buyssens), are to be illustrated by ample quotations. A rather detailed pattern is given in Webster's *Dictionary of Synonyms*. For the general set-up one may obtain some suggestions from the project of conceptual dictionaries. In most cases the statement of typical usage will be adequate. Should the dictionary of usage contain information of a different sort, common errors of usage, a detailed system of grammar, word-formation, pronunciation, literary categories, or encyclopedic information? These matters are to be excluded or to be listed in a separate part; they are very useful, but they do not belong to the limited field presented in a dictionary of usage.

The Historical Dictionary of German Figurative Usage by K. Spalding, listing each word with every figurative expression it has occurred in over the last two hundred years, is different. It promises to be an excellent work of reference for all those interested in the history of the metaphor.

A PROJECT OF CONCEPTUAL DICTIONARIES

The project of conceptual dictionaries will comprise dictionaries of the Germanic dialects up to the Reformation, and also Slavic, Celtic, Latin, Greek, Indian, Egyptian,

Sumerian, Hittite, and Semitic conceptual dictionaries, so that on the one hand the patterns of the near Eastern cultures, and on the other hand the possible links between classical culture and modern Western culture, as well as the dependence of the Western world on Christianity, will be shown in exact, easily comparable linguistic documentation. The classification and arrangement of the material should mirror the system of a particular language at a certain time. The world of images, the thought content, the differentiation of expression, the gaps in the intellectual universe of expression of a particular language will be brought to light most completely in an adequate system of the conceptual dictionary. On the other hand, a common or similar systematizing of the word material of different languages makes a reliable comparison of these languages relatively easy. Is there a criterion for the extent to which a uniform system of arrangement may be used for different languages? It is a matter to be worked out by practical experience. Uniform groups are: the Germanic languages up to the Reformation, the Romance languages, (French standing apart from other members), in a loose sense the modern European languages, possibly the ancient languages of the Near East. It is not so much the relationship of certain languages that determines the common content of systems of expression; it is more the identical or similar experience that shapes the similarity of the thought-content of the different systems of expression. It should therefore be possible to compare such segments of a language as reflect similar experience (as the sun and the stars, day and night) for a wide group of languages; segments which reflect experiences different in character, such as Christianity and Buddhism, cannot be compared. The linguistic expression of Christianity and of Buddhism is to be followed up in all the languages in which they find embodiment; and at the same time the religious expression is to be interrelated with the other connected fields of expression of the particular language.

After having established the worlds of expression of individual languages, we may compare these worlds of images with one another. One does not compare the different meanings of one and the same word, but the meaning of a coherent group of words with similar thought content; one compares the state of a language at a certain epoch with the

state of the same language at a different time, or with the state of another language at a certain or preferably at the same time. In this way one may "grasp from one common standpoint the development and differentiation of the (conceptual) world (of a group of languages), and grasp it in its most inward spiritual essence" (von Wartburg).

To reach this objective, the conceptual dictionary of the individual language must be comprehensive, and yet for practical reasons it ought to be as brief as possible. Even the comprehensive conceptual dictionary should be selective as to the material it includes. The best thing would be to have an archive which would contain the complete material. The least desirable possibility would be a brief edition, but such a one may be compiled if the means for a comprehensive dictionary are not available. In this case, further investigations may in the course of time prepare the material for a comprehensive version.

What should be the content and system of a comprehensive dictionary of a language not spoken any more, and also of a modern language, if it is to serve as a tool for comparing languages? The comprehensive dictionary must have sufficient quotations, by all means the essential ones, and for certain categories all quotations, thus avoiding subjective selections. The examples for a word or a word-group should be differentiated as to prose, poetry — (a) drama, (b) epic, and (c) lyric poetry, with further sub-divisions introduced as to literary genre — glosses, professional language, legal, scientific, (the system of plants listed by Dornseiff belongs to scientific language; it is not common property), different layers of language, literary style, colloquial language, slang. Direct speech is to be carefully noted, and, if necessary, to be commented upon. A loanword is to be characterized as to its source and development of meaning, and the original foreign expression is to be listed.

If there are several meanings for a word, for convenience these are to be numbered and placed at the head of all the quotations so that the particular meaning for each quotation can be identified by its number. At the end of a word group an analysis of meaning is to inform the reader of the differentiation of meaning, frequency, dialectal features, etc. A similar analysis is to be carried through for the sub-group, and, on a higher level, for the group.

The quotations for a word or word group may be arranged in the following way: 1. noun (example *house*) (a) Comparative quotations with the same, similar, or opposite meaning (*hut, house, palace*); (b) noun with noun. (*foundation of the house*); (c) adjective (*large- the large house*); (d) verb with noun (*to build a house*); list different kinds of subjects, objects, adverbs, prepositions connected with the verb; name the agent, the instrument, used in the action, the place of action. (e) compounds. The word in question follows (*light-house*); the word in question precedes (*household*); 2. adjectives. List nouns connected with them, also the adverbs. 3. adverbs, together with other adverbs, with nouns, and with verbs linked up with them. 4. interjections. 5. prepositions. 6. nomina agentis and present participles. 7. verbs; compare 1 d.

Since classification should be identical for as many languages as possible, it should be discussed by scholars of different linguistic fields. Where "the context of cultural reality" (Malinowski) is heterogeneous, a different classification is needed; otherwise one would have to give too lengthy explanations of the divergence of meaning of each word. For our world of expression I submit an attempt at a classification which has a number of features in common with that of von Wartburg, and which may be compared with that of Casares.

A. WORLD, NATURE, AND LIFE

I (a) World, Inorganic matter

(b) Chemical and physical properties of matter, aggregate conditions

II Organic world

Plants

Animals

Man as part of nature — the body, senses and their activities, activity, conditions and needs of the body (sleep, food, motion), bodily defects, diseases, life, reproduction, death, destruction

B. MAN

(a) As a thinking, feeling, and willing being

III Thought and expression, communication (sciences, schools, arts)

- IV Emotions
 - Individual
 - Relations between men
- V Activity and will, character, honour, and morals
- (b) Creative and organizing man
- VI State and people, family and descent
- VII Customs, law, justice
- VIII Property, housing, dwelling and settlement, work, household, family, technique, professions, crafts and service, tools, weapons, military, transport
- IX Religion
- (c) Man and world
- X Existence and relation
 - Time
 - Space, area, position, shape and form ,
 - Quantity, size, degree, number
 - Motion

USE OF CONCEPTUAL DICTIONARIES

Conceptual dictionaries will furnish invaluable tools for research in the humanities. They will provide an exact and complete representation of the system of concepts of a language. These dictionaries map out the realm of expression, the spiritual world of a linguistic community at a given period. By comparing the different conceptual systems as embodied in the different systems of linguistic experience, we create an instrument which will make it possible to write the history of ideas, the change of values, the growth of institutions, the spiritual welding together of the Western nations. In certain respects, such an instrument as furnished by the project of conceptual dictionaries will be all-inclusive as to matters of expression. We shall be in a position to follow the development of expression, the often formula-like nature of our thought and belief, the loans and transfers of ideas, the lacunae in our world of expression. We shall also obtain an insight into the rise of works of art, the literary genres, the continuation of the basic literary form, when transferred from one language into another; in short, we shall be able to ascertain stylistic development, the changing forms, as well as the continuing elements in style.

TEXT OF CONTRIBUTIONS

1. Un pareil dictionnaire devrait nous renseigner sur la fréquence de chaque mot, surtout lorsqu'il a des synonymes. Il devrait nous renseigner davantage sur les points suivants:

(1) Sur les possibilités syntaxiques qu'offre chaque mot. Par exemple, s'il s'agit d'un adjectif, nous devrions trouver une phrase contenant l'adjectif au positif, une autre au comparatif, une autre au superlatif, une dans laquelle l'adjectif est apposé à un nom, une où il ne l'est pas, etc. Un nom devrait nous être montré accompagné de tous les déterminatifs dont il peut être accompagné, il devrait aussi nous être montré dans toutes les fonctions qu'il peut remplir.

(2) Sur les exclusions synonymiques: par exemple, en anglais les adjectifs "alive" et "living" se complètent mutuellement pour remplir toutes les fonctions qu'un adjectif normal peut remplir; de même en français "on" ne peut remplir toutes les fonctions d'un pronom et doit être complété par "vous."

(3) Sur les différentes places qu'un mot, en particulier un adverbe, peut occuper.

(4) Sur l'incidence de divers facteurs comme le style, les circonstances sociales, etc.; par exemple, la différence entre "also" et "too."

Bref, il faudrait combler le fossé existant entre la grammaire et le dictionnaire.

E. BUYSENS

2. There are general patterns of linguistic behaviour ("usage") not covered by the ordinary dictionary or grammar, but, to some extent, amenable to alphabetical charting.

An utterance is completely analysable into (a) constituent formulae and (b) grammatical operations. The formulae (i.e. the constituent expressions which are not so analysable, such as *straw*, *berry*, *strawberry*, *How do you do?*) are given in

the ordinary kind of dictionary, the operations in the ordinary kind of grammar. But an utterance has also other important features: apart from being correct (or incorrect), it is also more, or less, customary and more, or less, adequate. These 'more-or-less' questions (as distinct from the 'yes-or-no' questions of grammar) are the specific questions of 'usage.' Referring to the use of formulae in utterances, they are not concerned with grammatical correctness, but with comparative values of correct uses.

Obviously, there can be no exhaustive treatment of such questions, and to attain some kind of completeness, limitations of one sort or another need to be adopted. Such are e.g.: in a unilingual dictionary, restriction to the task of discriminating near-synonyms or nearly equivalent constructions; or, in a bilingual dictionary, distinguishing words or constructions of one language from nearly equivalent ones of another. Such limitations and especially the latter, allow us to ignore large areas of clear difference or clear agreement in usage, and, in the case of partial agreement, to state only differences.

Exhaustiveness is nevertheless impossible, and selection, being always of typical uses, will be determined by frequency.

W. HAAS

3. The accidents of historical development have influenced dictionary making in the direction of verbal definitions for all entries: (a) because of the difficulty offered in the past by all methods of illustration other than the wood-cut; (b) because 'defining' dictionaries followed 'translating' dictionaries, which naturally were entirely verbal in form; (c) because lexicographers, being themselves interested in words rather than things, automatically use words as their tools. There has consequently been a tendency to look on 'illustration,' since it is mostly found in popular works, as unscientific and even childish.

There seem however two kinds of words best defined by non-verbal means: (1) certain abstract ideas and (2) most concrete objects. Compare the N.E.D. definitions of *long*, *wide*, *broad* with the 'definition' given by appropriately shaped rectangles or the illustrations of *lang*, *breit*, *dick*, *dünn*, etc.

in Sprach-Brockhaus. Similarly a definition of *oleaster* as 'A small S. European shrub with yellow flowers and olive-like fruit' (Wyld) does not really mean anything to a reader who has never seen one, and even a full description produces a picture only in the mind of a reader with good visualizing powers, while the reader without them remains as ignorant as before. A printed illustration would enlighten them both in far less time. There is therefore, for the first group, a very strong case for non-verbal definitions in all types of dictionary, and while not essential for the second group, they are in practice probably the most satisfactory.

G. MELLOR

4. Begriffsverzeichnis als Grundlage für die Lexikographie Versuch eines allgemein gültigen Ordnungsschemas

Da der Wortschatz einer Sprache ein System bildet, in dem jedes Glied durch wechselseitige Bedingtheit mit den andern Gliedern verbunden ist, können Aufbau und Struktur des Wortschatzes nur vom Begrifflichen her erschlossen werden. Der Gliederungsgedanke muss daher auch auf die Darstellung des Wortschatzes angewendet werden. Das bedeutet, dass ihr die Begriffe und nicht die Lautform zugrundegelegt und dass die Begriffe zu einem geordneten Gefüge vereinigt werden müssen. Ein solches Gefüge muss dann auch, da der Wortschatz das dem Durchschnittsmenschen angemessene Mittel zur Gliederung der Welt der Erscheinungen ist, das Bild widerspiegeln, das dieser sich von der Welt macht.

Es ist versucht worden, den Grundriss eines allgemeingültigen und überall verwendbaren Ordnungsschemas, ein Begriffssystem, aufzustellen, das folgende Forderungen erfüllen sollte:

(1) Aus der Gesamtheit der Begriffe ist eine Auswahl so vorzunehmen, dass jeder Seinsbereich vertreten ist, (während die mehr oder weniger eingehende Behandlung eines Seinsbereichs und seine Ausgestaltung je nach dem Wesen der betreffenden Sprache und der Kultur, welcher sie angehört, differieren werden).

(2) Diese Begriffe sind nach einem einheitlichen Gesichtspunkt zu einem Gefüge zusammenzuschliessen, das den innern

Zusammenhang aller Seinsbereiche widerspiegelt. Dieser Gesichtspunkt ist die Anschauungsweise des Durchschnitts-individuums, das mit naivem Realismus Welt und Mensch gegenübertritt.

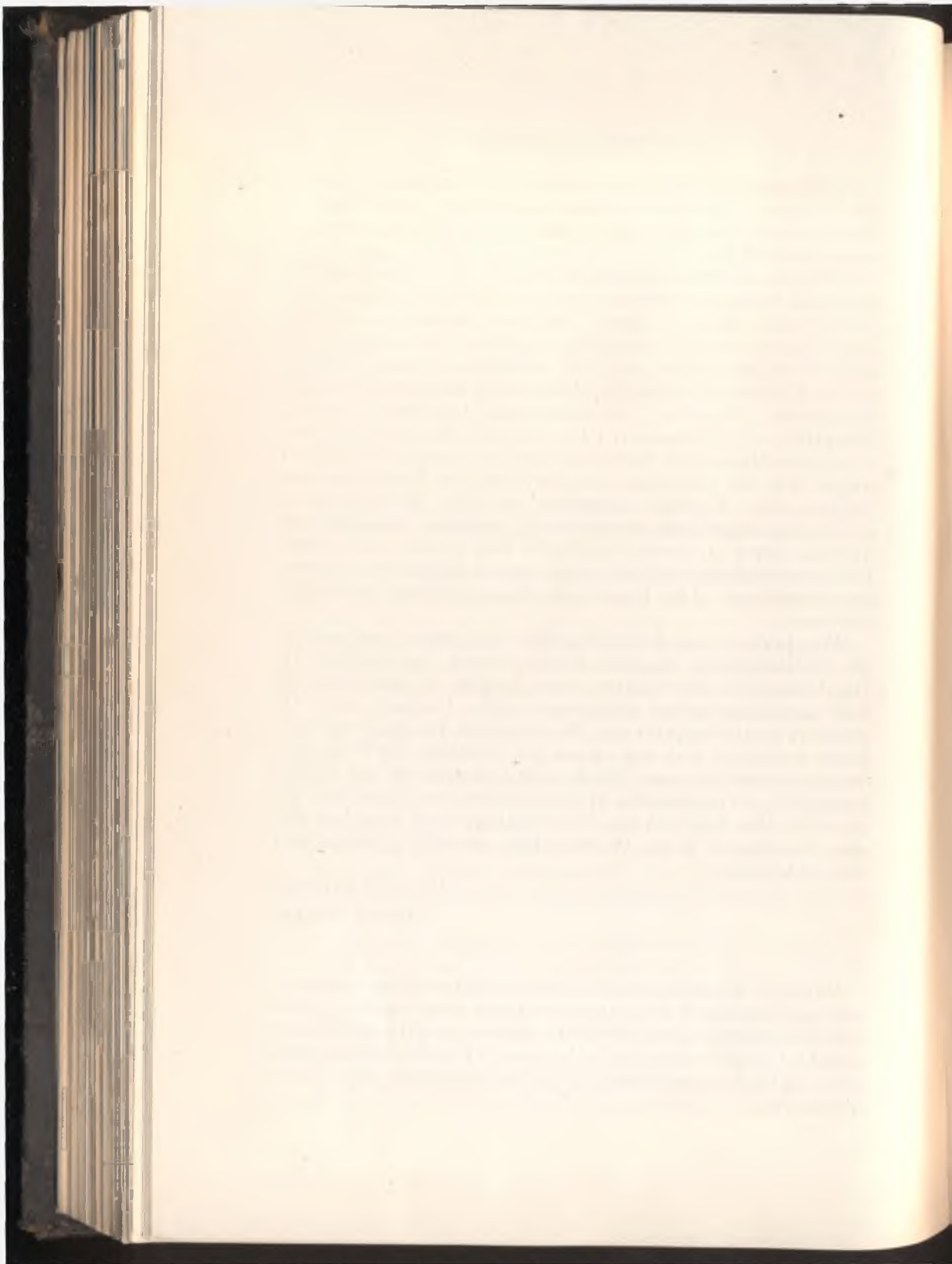
Nach diesen Prinzipien ergab sich für das Begriffssystem zwanglos folgender Rahmen : Die Gegenüberstellung von Natur und Mensch führte zu den beiden Hauptteilen *A. L'Univers*, dem die Begriffe zugeordnet sind, welche sich auf die anorganische und die organische Natur beziehen und *B. L'Homme*, der den Menschen in allen Gegebenheiten erfassenden Begriffen gewidmet ist. In einem dritten Hauptteil : *C. L'Homme et l'Univers* wird der denkende und forschende Mensch der Natur und sich selbst gegenübergestellt, sodass wir die Grundlage erhalten für die Einreihung der Begriffe des *A priori* einerseits und der Wissenschaften andererseits, auf denen wiederum die moderne Industrie und Technik beruhen, deren Begriffsgut das System abschliesst. In diesem Rahmen sind die ausgewählten Begriffe nach ihrem sprachlogischen, d.h. lebendigen Zusammenhang eingereiht worden.

Wir glauben, dass dieses Begriffssystem eine Grundlage für die Lexikographie abgeben könnte, deren Verwendung die Darstellung des Wortschatzes einer Sprache zu einer gewissen Zeit als Gesamtgefüge erleichtern wird. Dadurch wird die Einsicht in die Struktur des Wortschatzes im *synchronischen* Sinne angebahnt und von da aus der Vergleich des Wortgutes verschiedener Sprachen, Epochen oder Mundarten erst richtig ermöglicht. Vornehmlich wird erst auf solcher Grundlage ein *diachronisches* Studium des Wortschatzes einer Sprache, also eine Geschichte ihres Wortschatzes sinnvoll durchgeführt werden können.

W. V. WARTBURG

RUDOLF HALLIG

NOTE: Le Begriffsverzeichnis est actuellement sous presse et sera probablement à la disposition des congressistes. Ceux qui désireraient en prendre connaissance au moins partiellement avant le Congrès sont priés de s'adresser à l'un des deux auteurs, pour qu'ils leur envoient ce qui est disponible sous forme d'épreuves.



SECTION B

COMPARATIVE LINGUISTICS

- (1) What special problems arise in the comparative study of languages without a history? What special methods are applicable in such fields?

REPORT BY M. GUTHRIE

Contributions from

- | | |
|----------------------|---------------|
| 1. E. CROSS | 3. J. HOLT |
| 2. M. A. K. HALLIDAY | 4. G. PICCOLI |

COMPARATIVE LINGUISTICS

(1) What special problems arise in the comparative study of languages without a history? What special methods are applicable in such cases?

Report by M. G. Smith

Contributions from

J. J. Hoff

J. E. Cross

J. E. Hoff

M. A. E. Halliday

REPORT

by

M. GUTHRIE

The difficulties encountered in studying languages with no history on a comparative level fall broadly into two main types. On the one hand there is the question of the fundamental presuppositions involved in this kind of investigation, and on the other, the question of the implications of the procedure ultimately to be adopted.

The most important presupposition is expressed by the use of the term "genetic relationship." It is relatively easy to use a term of this kind without attempting either to define or to demonstrate its significance. It by no means follows that in studying languages with no history the hypothesis of genetic relationship is necessary. On the contrary it may well be that the first aim of the comparative study of such languages is the determining of the nature of the relationship between them, a procedure which may result in the rejection of what is implied by the use of "genetic." Even if the possibility of genetic relationship is adopted as a working hypothesis it by no means follows that an adequate definition is available. Is the term to mean that the languages being investigated have descended from a common ancestor? Is it to mean that there are certain aspects of the languages that justify the theory of common origin? Does the hypothesis include, or alternatively rule out, the possibility that the vocabulary of a given language may have been taken over from an adjacent language of completely different origin?

Supposing that the hypothesis of genetic relationship is confirmed by the evidence, a further question to be considered is whether or not it is possible to estimate the degree of closeness of the observed relationship. There is the problem that arises where the application of a comparative method shows that two languages display relationship to different aspects of a third language; in these circumstances are the first two languages genetically related to one another in virtue

of their being related over different areas of facts to a third language?

In a study of this kind there is also a danger that the presuppositions accepted, either explicitly or implicitly, may in fact falsify the collection and analysis of the data. This particular kind of investigation is particularly open to the risk of assuming the relationship in order to demonstrate it.

The problems involved in the procedure to be adopted in studies of this kind are inherent in the fact that any conclusions reached are based on induction, and are consequently only probably true. It is never possible to achieve any certain results in a subject of this sort, and in fact one of the major difficulties is the working out of a scale of probabilities to be applied to the various conclusions reached.

A further problem arises from the fact that at some stage or other it becomes plausible to introduce non-linguistic considerations in making a choice between possible alternative inferences. A step such as this inevitably brings with it dangers of its own, and it is usually impossible to lay down adequate principles either to determine when it shall be taken or to control the application of such considerations.

In view of the difficulties outlined under the first part of this question, there are certain precautions that have to be taken in developing methods for the conducting of this type of comparative study. The first prerequisite is that the comparisons made shall in the first instance be free from hypotheses of linguistic prehistory. Any method which fails to make this separation will inevitably fall into the danger of arguing in a circle. Broadly there are three stages to be detected in the methodology of comparative study of this kind.

(1) The preliminary stage must involve the application of some method to determine whether or not comparative study of the languages involved is possible. There are cases where the presumption of genetic relationship is very high, but nevertheless comparative study in the strict sense is impossible because the extreme brevity of the items of data available would inevitably have involved the obscuring of any evidence of relationship. One point that has to be considered at this stage is whether or not structural affinity shall be introduced into comparative study. Clearly there is no *a priori* reason why it

should not be, but since it is of a different order from the kind of relationship dealt with by the more usual comparison of items of vocabulary and grammatical apparatus, the two methods cannot readily be synthesized.

(2) Since it is desirable to exclude theory in the initial treatment of the available data, the collecting (and rejecting) of them as well as the assorting and arranging, has to be done on the basis of features displayed by the data themselves. In other words the observance of what could be interpreted as the result of "sound-shift" can well provide the grounds for establishing correspondences of dissimilarity. Nevertheless at this point it is not to be assumed that the observed correspondences are necessarily the result of sound-shift, although clearly that will be a reasonable inference to be made after the data have been fully investigated.

(3) The final stage in the comparative study of languages without recorded history involves the making of inferences that lead into the realm of linguistic prehistory, and a number of points is to be observed. It will be necessary to determine whether the arrayed data imply that the items investigated probably had a single common origin or a multiple one. The details of the excursions into prehistory may also lead to inferences about earlier states of individual languages or groups of languages, provided that the data are adequate for this purpose. Another desideratum is the computing of the linguistic proximity of the presumed descendants of some earlier language or languages. This may be achieved in several ways, some using complicated mathematical calculations, but all having one thing in common with every other branch of this kind of comparative study, that they give results consisting of probabilities only, rather than of real quantities.

TEXT OF CONTRIBUTIONS

1. A procedure will be presented for determining the linguistic proximity of two or more languages. Linguistic proximity connotes geographic or temporal propinquity.

The statistical data obtained from the application of the test to be described can be used to establish a time depth and a space relationship.

The results obtained may serve as a check on empirical observations of linguistic similarity as well as a means of constructing a scale of proximity in time and place.

A key vocabulary is used to find the lexical correspondences of the languages under examination. The vocabulary was not pre-selected to fit cultural acquisitions nor to satisfy long established traditions and prejudices. It is a basic vocabulary for the most part including terms for elementary concepts that have been associated with man for countless centuries. While even the most fundamental term may be discarded and lost, experience has shown that this vocabulary has changed the least.

The vocabulary terms are listed alphabetically. The initial demonstration is with the Romance languages and certain ancient Indo-European languages. Results are tabulated.

EPHRAIM CROSS

2. The absence of any historical record necessitates the establishing of lexical correspondences based purely on synchronic material, which is very difficult unless there are large groups of consistently related words, as there is no possible check on phonetic development.

But many such languages show no possibility of the establishment of lexical correspondences, for two reasons: (1) in monosyllabic languages, as Thai, words are so short that phonetic change soon obscures correspondences; (2) "primitive" languages, which change so rapidly that correspondences are obscured in any case.

Assuming this refers to supposedly related languages, one should still make structural comparisons. This will help to establish relationships.

(1) Structural comparison is valuable for itself as showing how "primitive" languages develop into "civilized" ones. Thus, for instance, in the Malayo-Polynesian group one may find that Malay represents the most "civilized" form, but structural comparison will reveal resemblances with languages of the islands.

(2) If one can show that structural features seem to have developed out of features still found (in what is thus an earlier form) in another language, one can suppose that the languages may be related and look for lexical (phonetic) correspondences, in which the latter language may have preserved a form out of which the other has evolved, or at least representing a stage less changed from the original "parent" form.

It would be useful to compare the study of languages without a history with that of dialects, both those without any history of their own, like some Chinese dialects, and old dialects with only a subsequent history, like the dialects of the ancient Germanic languages.

M. A. K. HALLIDAY

3. The question seems to point to the difficulties which arise in the study of the historical relations between languages without ancient documents. Just because such documents were at hand, the historical study of the Indo-European languages had been particularly favourable to the establishment of sound and clear methods for historical comparative work. These methods consist in the reduction of the sign-differences, both concerning the elements of expression and the elements of contents, and the identification of the signs in a certain group of languages. The study of expression, however, has been more important than the study of content, because the number of examples with only slight differences of content has been sufficient to demonstrate the precise conditions of the distribution of the differences of expression, so that it has been possible to register all the relations which exist between

the phonemic systems of the languages concerned. These relations, or dependences, must be either determinations or interdependencies. In Engl. *three*, *thin*, *night* = Lat. *trēs*, *tenuis*, *noct-em* we have examples of interdependency, Engl. *n* = Lat. *n*, and of determination. The relation Engl. β/t = Lat. *t* is an instance of determination, since the variable (β/t) is found in English and the constant (*t*) in Latin, the English variables β/t being conditioned by the preceding fricative. The case of long and short Skr. *a* = long and short Gr., Lat. *a*, *e*, *o* might make one think that the constant was to be found in Sanskrit and the variables in Greek and Latin, but the distribution of the three vowels in Greek and Latin is unconditioned, so that we must say that the syncretism is conditioned by its appearance in Sanskrit, of which the phonemic system recognizes no commutations in this case, and this point of view is supported by the palatal law of Sanskrit (Skr. *ca* = Lat. *que*). On the other hand, if we must always register free combinations between the phonemic system we are studying, such languages are mutually unrelated: Finn, *puu*, *päivä*, *päytä* = Danish *træ* ('tree'), *dag* ('day'), *bord* ('table'), etc. Loan-words are to be dealt with in their own way; here the relations will differ from the normal ones and they do not concern the whole system (Engl. *three*, *table* = Fr. *trois*, *table*).

If all elements of one system determine all elements of another system, such systems are two stages of the same language; but they belong to different languages if the direction of determination is shifting from one phoneme to another. The system with constants is the older one, and the stage with the variables is the younger one. In this case, too, we have to deal with loan-words.

Other methods are invalid in comparative work. The lack of ancient texts will very often complicate the study. Accurate synchronic descriptions of such languages will be the first step. Then, the comparative methods must be elaborated and refined, and it will be particularly necessary to introduce precise methods for the comparison of the elements of content. Such regular relations have not yet been established. After that, we may hope that all languages bound together by means of such relations will get a history.

JENS HOLT

4. Ritenendo la Linguistica una disciplina essenzialmente storica, ne segue che la risoluzione di problemi linguistici non può fondarsi che su indagini relative ad argomenti cronologici, geografici e filosofici. Infatti, qualsiasi innovazione linguistica rappresenta un fenomeno "lessicale" o "grammaticale" che appare nel tempo e nello spazio e che alla sua volta è soggetto ad una causa per lo più psicologica. Ad esempio, la fase lat. *focus* "fuoco," che vive in idiomi romanzi, è più recente della fase ugualmente lat. *ignis* "fuoco" la quale si riscontra nell'ind. ant. *agni*-š, nel lit. *ugnis* e nel bulg. *ogni* con il significato di "fuoco." Inoltre, *ignis* è anteriore al gr. *πῦρ* "fuoco," che si riscontra in idiomi germanici. Ora, tali rapporti cronologici tra la fase *ignis* più antica di *focus* e di *πῦρ* risultano da documenti come i rapporti geografici di quelle stesse voci risultano dalle diverse località in cui si trovano. Infine, la causa o la ragione per cui in alcuni idiomi indoeuropei sono state preferite le innovazioni del tipo *ignis* alle innovazioni del tipo *focus* e *πῦρ*, recorrenti in altri idiomi ugualmente indoeuropei, è in relazione a quei due indizi ricordati e quindi ai rispettivi influssi di individui creatori e di collettività conservatrice.

Analogamente si può sostenere nei riguardi delle innovazioni delle forme dei segni rappresentanti specialmente le cifre indo-arabiche 4, 5, 6, 7 recorrenti in codici latini antichi nella determinazione della autenticità di quei manoscritti.

GIUSEPPE PICCOLI

SECTION B

COMPARATIVE LINGUISTICS

- (2) What contributions to comparative linguistics are to be sought from the two groups of disciplines and techniques broadly termed (a) phonetics, (b) semantics?

REPORT BY J. FOURQUET

Contributions from

- | | |
|-------------------|---------------|
| 1. J. ELLIS | 4. P. MERIGGI |
| 2. J. ENGELS | 5. E. REIFLER |
| 3. W. K. MATTHEWS | 6. S. ULLMANN |
| 7. S. WURM | |

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REPORT

by

J. FOURQUET

M. W. K. Matthews (Londres) nous donne le préambule à l'étude de la question dans son ensemble: la méthode comparative reste fondée sur la phonétique et la sémantique de la fin du XIX^{ème} siècle. Or ces disciplines ont progressé; la sémantique tend à devenir une science pour elle-même. La méthode comparative doit être mise au niveau des notions plus justes et plus rigoureuses que ces disciplines ont dégagé. Le progrès ne sera pas tant vers plus de subtilité, que vers plus de clarté.

Il y a une remarquable inégalité entre les points (a) et (b): sur le premier une contribution (J. Fourquet); sur le second cinq (S. Ullmann, E. Reifler, J. Ellis, P. Meriggi, J. Engels). Ceci correspond probablement au fait que les rapports entre phonétique et étude comparative sont stabilisés, tandis qu'il n'en est pas de même pour la sémantique, science en train de se constituer.

En fait, notre contribution concerne non la phonétique au sens strict, mais la phonologie (phonemics), dont l'objet et les méthodes sont encore discutés. Cependant nous pensons que dès maintenant, le phonologiste doit reconsidérer les problèmes phonétiques que pose l'étude comparative, parce qu'il voit les éléments sur lequel se fait la comparaison sous un éclairage nouveau: un *ü* n'est pas seulement pour lui une voyelle palatale arrondie de fermeture extrême: c'est soit un "membre" d'un phonème susceptible des "variantes combinatoires" *o*, *u*, *ü* (resp. devant *a*, *u*, *i*) comme en v.h.a., soit un phonème opposé à *i* d'une part, à *u* de l'autre par l'articulation, à *ö* par l'ouverture (m.h.a.); il se trouve en toute position (français), ou seulement en syllabe "principale" (allemand); dans un système qui n'a que *i* et *ü* (alsacien), ou qui a *i*, *u*, *ü* (suisse) etc. Or ces caractéristiques supplémentaires de l'élément considéré sont aussi la résultante d'évolutions historiques; le phonologiste apporte une seconde

moisson de données sur les éléments qu'on compare; il peut espérer ainsi lever nombre d'indéterminations, résoudre des problèmes encore sans solution sérieuse.

Des contributions sur la sémantique, deux ont un caractère général: M. S. Ullmann passe en revue les types d'étude sémantique encore insuffisamment introduits dans l'étude comparative:

- (a) étude des évolutions métaphoriques et métonymiques les plus communes
- (b) étude des interactions entre mots (attraction paronymique, conflits de synonymes)
- (c) étude des champs sémantiques (*Bedeutungsfelder*)
- (d) étude des rapports entre sémantique et niveau culturel.

On remarquera que les trois derniers points reviennent à considérer non plus un rapport mot-sens isolé, mais un rapport solidaire d'un ensemble.

M. E. Reifler montre que la contribution de la sémantique devra se fonder sur une sémantique générale, qui compare les faits de sens dans des langues non apparentées; par opposition aux études génétiques faites sur une famille. Il apportera des exemples tirés de son expérience de sinologue.

M. J. Ellis parlera d'expériences sur la méthode "glottochronométrique" de Swadesh: méthode fondée sur ce qui reste commun du vocabulaire de base dans deux langues qui ont divergé à partir d'un ancêtre commun. Cette partie du lexique se renouvelle de façon relativement lente.

Nous objecterons que la dialectologie a ruiné l'idée de parenté uniquement de type "généalogique" (*Stammbaumtheorie*); l'évolution des dialectes et leurs acquisitions de vocabulaire peut se faire par vagues d'innovations qui introduisent des éléments communs dans des dialectes déjà différenciés par ailleurs; les aires dialectales restent perméables (*Wellentheorie*).

M. J. Engels attire l'attention sur le problème du calque; la forme appartient à une langue, la création sémantique à une autre; il en résulte une indétermination qui peut-être ne pourra être levée que par le recours à des données non-linguistiques.

M. P. Meriggi fait une comparaison qui rappelle un peu les débats sur l'isomorphisme en linguistique structurale; les sens, comme les sons, ont une certaine "latitude de réalisation"; un écart un peu fort peut se fixer, devenir la norme (l'emploi

métaphorique de *testa* devient simple nom de la tête; une articulation appuyée d'une longue, qui tend à la segmenter, aboutit à une diphtongaison). De même qu'il y a des évolutions phonétiques dues à une articulation négligée (affaiblissements en finale), ou au contraire appuyée (diphtongaison), il y a des évolutions sémantiques dues à des emplois dans un sens plus vague (*domina: dame*), ou dans un sens expressif (*testa:tête*), qui se sont fixés et sont devenus la norme.

Il nous paraît vraisemblable qu'on verra se préciser plusieurs réactions contre la méthode comparative traditionnelle:

- (a) tendance à introduire plus systématiquement dans l'étude historique des langues par la méthode comparative les principes dégagés de l'observation *in vivo* sur les langues actuelles, et non seulement sur les langues d'une famille, mais sur toutes les langues du monde.
- (b) tendance à ne plus opérer sur des éléments isolés, mais à tenir compte de la solidarité entre éléments du langage à l'intérieur d'un système — qu'il s'agisse de sons ou de sens.
- (c) critique des raisonnements comparatifs trop étroitement fondés sur le schéma généalogique; sorte de *stemma*; les langues d'une famille ne ressemblent pas à une tradition pure, mais à une tradition contaminée (emprunts, vagues d'innovation).

TEXT OF CONTRIBUTIONS

1. Swadesh's glottochronometry (I am indebted to Professor Marcel Cohen for acquaintance with the subject and for some general comments on its relation to problems of linguistic history) claims to date the prehistoric separation of languages by the amount of basic vocabulary they retain in common. He starts from the observed fact that in all languages tested, over a thousand years there is between 77% and 85% retention in the words for a list of about 200 notions which he has devised. From this he concludes that with the same list one could discover approximately the date at which any two related languages separated.

Whether or not the method has any validity it raises important problems, and it is therefore worth discussing.

What constitutes the identity of a language (as distinct from a dialect) is its phonemic and grammatical systems and basic vocabulary, but this identity is constituted in different ways by the former, which differ at different periods but always derive as a whole from the development of the previous system of the same language (or parent language), and by the latter, which may be of any source but changes very slowly — according to Swadesh, also very steadily, part of it at least at a rate calculable within 5%.

It is possible that Swadesh's list is basic vocabulary (or part of it) for "primitive" languages and not (the same part) for "civilized" ones, or not all for all places (e.g. "ice"). It is also possible that the rate of change is different in languages of types (or of the general type "primitive") which he has not tested (and could not test), and in prehistoric stages of familiar ones. These two facts might cancel each other out, or they might not. They would not effect the existence of a basic vocabulary in the sense of a steady rate over a period.

If the method were valid (tests of Swadesh's list being carried out by my students on languages not given as tested by Swadesh but of the same type give confirmatory results — e.g. the amount of correspondence of English and Russian — except to a slight extent the apparent results to date of

comparison of Germanic languages), its significance, apart from practical utility, would be that basic vocabulary is then unique among linguistic components, and the process of replacement of its units unique among semantic phenomena, and that "separation" (of dialects into languages) is an event susceptible linguistically of "dating."

J. ELLIS

2. Parmi les contributions que la sémantique peut faire à la linguistique comparée, les calques occupent une place particulière.

(1) Lorsque, en étudiant un vocable (locution) d'une langue déterminée, on est amené, par suite de la comparaison de mots de sens analogue dans d'autres langues, à admettre la possibilité d'un calque, cela entraîne des conséquences pour la façon de poser les problèmes sémantiques de ce vocable, notamment celui de l'origine de la gamme de ses significations. En effet, si le mot est un calque, le problème de l'origine sémantique de ce vocable ne se posera plus dans la langue à laquelle ce vocable appartient (et dans laquelle la solution était impossible). Le problème se déplace alors vers la langue calquée (vers le pays, vers l'époque du calque) où ce sémantème est né et où donc le problème peut être posé et, souvent, résolu.

(2) Lorsque, dans une série de langues, se présente pour un groupe de vocables la possibilité de les expliquer par le calque, il s'agit de déterminer quel est le vocable calqué et quels sont les calques. En d'autres termes: il s'agit—le calque étant une forme de l'emprunt—de déterminer la voie et la direction de cet emprunt. A cette fin la seule étude conjointe de la sémantique et de la phonétique ne suffira d'ordinaire pas; elles s'accommoderont généralement de conclusions qui s'excluent mutuellement. Pour trancher ce problème il faudra le plus souvent recourir à des critères d'ordre non-linguistique.

(3) Le problème méthodologique de la "polygenèse" sémantique. Position du problème et éléments d'une solution.

J. ENGELS

3. Traditional comparative linguistics, with its historical bias, took shape in the last 30 years of the 19th century and was based, so far as its phonetic aspect was concerned, either on the orthographic-phonological approach or on the now outmoded phonetics of the Sweet-Sievers school. Semantics became articulate with Bréal in 1883, but has not influenced developments in linguistics till quite recently.

Phonetics has made considerable advances since last century, especially in the fields of experiment and phonology, and semantics is rapidly becoming a new discipline. It is the less recondite findings of modern phonetics and semantics that have still to be absorbed and systematised. In the comparative study of unwritten or recently illiterate languages and dialects there should be much less difficulty in applying our new techniques than where the comparative method is dominated by the inertia and lustre of a potent tradition.

The contribution of both phonetics and semantics to the study of language might be of considerable importance if they are used to revise and supplement rather than to overturn. Revision would imply the introduction of more precise values and formulations, a clearer inventory and assessment of facts, a general widening of horizons.

Continuing improvements in phonetic and semantic techniques are bound to have a beneficial effect on lexicology and syntax, the cardinal divisions of grammar. These improvements must be envisaged not in terms of increasing subtlety, but of increasing clarity, such as a disinterested pursuit of truth demands.

W. K. MATTHEWS

4. È generalmente ammesso che l'evoluzione semantica riposa sostanzialmente sulla limitata libertà, per cui il parlante può sconfinare dall'uso, applicando p.es. un nome a cosa che di solito così non si chiama, o attribuendo un aggettivo a un sostantivo che solitamente non lo comporta e simili. Spesso si attenua dapprima l'ardimento con un direi o sim., che si tralascia poi se l'innovazione incontra, se cioè esiste già la "tendenza collettiva" in quel senso. Domina su ciò, notoriamente, la ricerca dell'espressività.

Dell'evoluzione fonetica non si è ancora trovata una ragione che regga alla critica più superficiale. Premessa è anche qui quel margine di libertà articolatoria classicamente descritta dal Paul. Ricerche sperimentali mostrano che le evoluzioni che riposano su un rilassamento dell'articolazione (tipo classico: esplos. sorda intervoc. > fricativa > *h* > O) si manifestano dapprima nei tratti della frase meno decisamente articolati, soprattutto alla fine. Col propagarsi in altra sede l'evoluzione si compie: la pronuncia trascurata diventa quella normale, come in semantica la parola o forma volgare sale agli onori dell'uso normale e anche letterario.

Ma vi sono evoluzioni, come le dittongazioni e l'innaturale passaggio di sonore in sorde e di fricative in esplosive che sono il contrario di un rilassamento. Solo una ricerca d'espressività fonetica può spiegarle, o meglio quell'espressività conduce alla distribuzione di chiaro-scuri: le parti rilevanti tendono a una pronuncia esagerata, quelle in ombra a un rilassamento. Ma tanto l'una che l'altra pronuncia può generalizzarsi, donde le due categorie d'evoluzione fonetica che sembrano contraddirsi, in perfetto parallelo coi due sensi dell'evoluzione semantica: lo scadere della parola nobile (lat. *domina* > ital. *donna*; ted. *Frau*) e il salire di quella volgare (lat. *testa* > ital.)

PIERO MERIGGI

5. THE IMPORTANCE OF COMPARATIVE SEMANTICS FOR HISTORICAL AND COMPARATIVE LINGUISTICS

Professor Joseph Vendryes, in an article published in 1945 and entitled *La Comparaison en Linguistique* (Bulletin de la Société de Linguistique de Paris, Tome 42ème, Fasc. 1), made a strong appeal for a general linguistic approach to the linguistic problems of grammar and vocabulary, an approach in which the linguistic is concerned not with questions of the genetic relationship between languages, but with a collection, study, classification and comparison of similar and dissimilar, parallel and divergent developments in unrelated languages. He speaks here in this connexion of comparative semantics.

In the course of many years of research in the Chinese language a fairly large number of "unusual" cases of semantic

change have been found in which the Chinese language happened to parallel a number of unrelated languages. Among these those concerning the evolution of grammatical meaning are of special interest.

The paper will attempt to show how comparative semantics may become a valuable source of information relative to the cognacy of phonologically compatible forms.

ERWIN REIFLER

6. Recent progress in semantics may have important repercussions on the establishment of comparative etymologies, on the reconstruction of hypothetical word-meanings, and on our ideas concerning the lexical structure of the protoglossa.

(a) Some general tendencies governing semantic change have been ascertained by empirical investigations, showing e.g. that certain types of metaphor and metonymy are more common than their opposites. Such tendencies, though by no means universally valid, may provide criteria for assessing the probability of reconstructions and for choosing between alternative explanations.

(b) The interrelations and mutual influence of words in the protoglossa should be interpreted in the light of what we know about these factors in modern languages and dialects. Particular attention should be paid (1) to homonymic clashes, contacts between paronyms, contamination etc.; (2) to the analogical influence of synonyms, antonyms, words belonging to the same sphere of thought; (3) to the feeling-tone attaching to certain spheres.

(c) The theory of "semantic fields" (closely organized sectors of the vocabulary) can be, and has been, applied to the protoglossa, showing the lexical resources corresponding to a certain sphere of experience (colour terms, family relations etc.).

(d) The semantic habits of races at approximately the same level of civilization as were the speakers of the protoglossa may give an idea of the lexical structure of the latter. Such features include the predominance of particular over-generic terms, the crucial importance of the context of situation, the part played by taboo etc.

S. ULLMANN

7. Relatively little attention has been given to pitch and intonation in comparing languages which are thought to be non-tonal or not really tonal. Consequently, the classification of languages by their tone qualities is unsatisfactory. In general, tone languages with significant pitch contrasts and non-tonal languages without them are distinguished, which appears to be insufficient. The following grouping is suggested:

(1) Real tone languages. Characteristics: the tones are (a) inherent parts of each syllable and not parts of the phrase melodies; (b) have no independent meanings of their own; (c) are contrastive; (d) syllable tones are unchangeable or change according to definite principles; (e) tones are not correlated with stress or quantity. Syllable tones may have semantic value: tone languages with semantic tones, or they may not: tone languages with ornamental tones.

(2) Quasi-tonal languages: (a) to (d) as above, (e) syllable tones are correlated with stress or quantity.

(3) Non-tonal languages without syllable tones, exclusively with intonation (meaningful phrase melodies).

(4) A category between (2) and (3): languages with a basic intonation pattern which is modified in numerous but regular ways by the sequence of the stresses. The syllable tones resulting are not correlated tones as in (2) (e), but modifications of the phrase melodies, and are only partly defined in their relation to the tones of adjoining syllables. Certain Turkic languages belong to this category, but the existence of further languages of this type is likely.

STEFAN WURM

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SECTION B

COMPARATIVE LINGUISTICS

- (3) In what sense and to what extent is comparative grammar of value in descriptive linguistics? Can a more comprehensive grammatical method be developed in traditional comparative linguistics?

REPORT BY L. HJELMSLEV

(Not yet received)

Contributions from

- | | |
|---------------|-----------------------------------|
| 1. T. BOLELLI | 3. J. ELLIS and M. A. K. HALLIDAY |
| 2. E. BUYSENS | 4. E. P. HAMP |

THE HISTORY OF THE

REIGN OF HENRY THE SEVENTH
OF ENGLAND
BY
JAMES HALLAM, ESQ.
OF LINCOLN'S INN

LONDON:
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THE HISTORY OF THE
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TEXT OF CONTRIBUTIONS

1. Dopo l'esperienza fonologica e strutturale non sembra più possibile avere un quadro completo della continuità dei fenomeni senza tener conto dei principi della fonologia e dello strutturalismo.

Nelle fasi successive di una lingua oggi si tende a vedere non una serie di sistemi chiusi, opponentisi l'un l'altro, bensì un risolversi dell'uno nell'altro attraverso una evoluzione di funzioni la cui realtà, sebbene si possa cogliere solo sincronicamente, richiede, per non cadere nell'astratto, una precisa individuazione storica. La comparazione offrirà, quindi, come è sempre avvenuto, un generale e prezioso contributo di ipotesi di lavoro, anche se oggi non si può prescindere dalle condizioni degli elementi contigui che spiegano la formazione del sistema.

La conciliazione di sincronia e diacronia, che sola permette la comprensione completa di un fenomeno, può attuarsi a patto di considerare che ogni sistema non è completamente isolato, chiuso in se stesso, ma ha una sua fisionomia in quanto è il prodotto di una evoluzione e contiene i germi di una evoluzione successiva.Cogliere un attimo di questo processo significa cogliere, nella loro organicità, la storia degli elementi formatori del sistema stesso.

TRISTANO BOLELLI

2. Nous n'avons pas d'autre méthode pour connaître un objet que de la rapprocher des objets auxquels il ressemble, et de l'opposer aux objets dont il diffère: ces deux opérations font ressortir les caractéristiques de l'objet étudié. Par exemple, on ne parlerait pas de gérondif en français si on ne comparait pas cette langue au latin; le grand usage que les Romains faisaient du gérondif nous permet de bien connaître les caractéristiques du gérondif, et de reconnaître le gérondif français comme tel.

La comparaison entre la phrase française " Il réussira probablement " et son équivalent anglais " He is likely to succeed " montre que l'expression du futur n'est pas le propre du verbe.

La comparaison entre " The bed had not been slept upon " et " Personne n'avait dormi dans le lit " montre qu'il est faux de définir le complément d'objet direct comme désignant ce qui subit l'action exprimée par le verbe: " le lit " désigne ce qui subit l'action, mais ce n'est pas l'objet direct.

E. BUYSENS

3. We must distinguish between comparative grammar of languages in general, i.e. any principles that may be induced from the grammar of each language, and the comparative grammar of language-families, which aims at a description of the original grammar of the parent-language; and between description of a living language at a given time, which will have a unitary system of " langue " inducible from its spoken texts, and " description " of a language known only from written texts of varying provenance (e.g. OHG), which will consist in comparison of the texts, and/or in description of each text.

It is usual to take together the first of each of these pairs, constituting " linguistics " in the modern sense, and the second of each, constituting traditional " philology." And it is quite true that modern linguistic methods were made necessary by the inability of traditional philological method to deal adequately with living languages.

But, while the techniques of the latter method cannot be dispensed with in the treatment of written languages, this too may be integrated into the wider " linguistics," and the resultant procedure will involve the application of both kinds of comparative grammar in descriptive linguistics, and in particular of general comparative grammar in the comparative (hypothetical-descriptive) grammar of language-families.

The procedure would consist in the induction from each text (established by philological methods) of a grammar, by methods of general grammar; the comparison of the grammar

of each text with each other and with related languages; and the construction of a grammar of their parent-language based not only on all the related languages (as was the Neo-Grammarians account of the Indo-European verb) but on general comparison with other languages (as is say Prokosch's account of the Indo-European verb).

J. ELLIS

M. A. K. HALLIDAY

4. Je voudrais traiter la troisième proposition, Section B, en changeant l'ordre des termes à lire: En quel sens et à quel point la grammaire descriptive a-t-elle une valeur pour la linguistique comparée?

Bien que le 19^e siècle eût pour la linguistique descriptive plus de respect que l'on n'a allégué, il est vrai que les savants de cette époque-là n'ont pas poussé très loin la précision des procédés de l'analyse descriptive et comparée. Plus récemment pas mal de linguistes ont prétendu que nulle analyse comparée ne peut marcher jusqu'au bout sans être fondée au premier abord sur des descriptions approfondies et complètes. Des autres ont douté de la valeur de la description structurale pour les buts comparés. On peut démontrer, à mon avis, que ni l'un ni l'autre jugement n'a tout à fait raison.

On peut démontrer que la phonologie comparée ne peut marcher le plus efficacement que sur la base de descriptions phonologiques structurales: Dans des formes que l'on sait associer en conséquence d'épreuves sémantiques, au moyen de la comparaison de suites de phonèmes dans les langues filiales nous arrivons à des suites de phones dans la langue maternelle. Ensuite, il faut analyser ces phones de façon structurale le long de la couche synchronique de la langue maternelle pour aboutir aux phonèmes de cette langue. De partir d'éléments phoniques (phonétiques) en dehors des phonèmes c'est de prolonger le procédé d'analyse ad infinitum.

Au point de vue morphologique le procédé est différent: Je crois démontrer qu'en partant de listes de morphèmes (dressées avec leurs sémèmes) et de formulations structurales monolithes de leurs distributions (plus les sémèmes de ces

distributions) nous rencontrons des difficultés graves dans le procédé théorique de l'analyse comparée. Plutôt on fait mieux en partant, dans les langues filiales, de séries polymorphiques écrites en transcription phonématique, pour restituer les séries maternelles heuristiques, lesquelles à leur tour peuvent se résoudre sur cette couche en leurs éléments morphologiques.

Cette bipartition de procédé répond immédiatement aux résultats de recherches récentes qui paraissent affirmer la stabilité des correspondances phonologico-lexicales pendant des époques immenses et la constance relative de la raison d'attrition lexicale vis-à-vis la vaste décomposition et recomposition structurales qui ont lieu dans toute famille linguistique.

ERIC P. HAMP

SECTION B

COMPARATIVE LINGUISTICS

- (4) Are there areas of *affinité grammaticale* as well as of *affinité phonologique* cutting across genetic language families?

REPORT BY A. MARTINET

Contributions from

- | | |
|----------------------|----------------------|
| 1. J. ELLIS | 3. A. G. HAUDRICOURT |
| 2. M. A. K. HALLIDAY | 4. E. LEWY |
| 5. C. REGAMEY | |

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CHAPTER I

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REPORT

by

A. MARTINET

The existence of a type of linguistic relationship which does not result from a common prototype is today accepted as a fact by a large and probably steadily increasing number of scholars. But it cannot yet be said to enjoy the status of a generally accepted truth. Linguists are probably still to be found who would dismiss any alleged structural similarity between genetically non-related languages either as fanciful, or as due to chance and to be ascribed to a psycho-biological substratum common to all men, a case of what Hugo Schuchardt has called 'Elementarverwandtschaft.' The whole problem still awaits an exhaustive and authoritative treatment, and no such treatment can be expected before more data have been gathered and sifted.

The existence of areas of 'affinité phonologique' was pointed out, about two decades ago, by linguists of the Prague School, and a theory of this type of 'affinité' was presented by Roman Jakobson to the Fourth Congress of Linguists in 1936 with impressive illustrations on a continental scale. Unfortunately the projected phonological atlas of Europe which was to afford conclusive evidence for the existence of synchronic isoglosses cutting across genetic linguistic boundaries was doomed from the start. Too few convinced structuralists were available, and the sympathy of professional dialectologists could not be aroused.

Yet detailed and painstaking research is necessary before the theory of 'affinité phonologique' may be considered more than an arresting hypothesis. Jakobson's sketch of a vast Eurasian area of generalized palatalization stretching from Poland to the shores of the Pacific is intriguing, but its full validity has been doubted, and doubts could only be dispelled by a recording, made along structural lines, of the host of languages and dialects spoken in and around that huge territory. The extent and difficulty of this task may well persuade us that it is more practicable to test the general

hypothesis than to solve this particular problem. Such a test could be made by ascertaining the existence of a number of relatively small structural phonetic areas with the limited aim of showing that certain isoglosses do not coincide with genetic boundaries.

But linguists cannot be satisfied with the pure and simple registration of this non-coincidence; the synchronic data thus obtained acquire significance only if they are interpreted as the reflexes of evolutionary processes. It is not a case of geography vs. history, of statics vs. dynamics, but of convergence vs. divergence. The importance of convergent evolution can be assessed and its varying patterns recognized only if the survey is exhaustive. All facts should be recorded (1) whether they confirm or invalidate traditional views, (2) whether or not they are found to be structurally integrated and functionally comparable (a borrowed phonetic feature may exist in a language for some time as a phoneme variant before it attains independent status in the structure; e.g. [ū] may be introduced as an allophone of *u* in palatal environment). At another stage of the research it will become imperative to distinguish, as far as practicable, inside one and the same area between (1) sub-areas where a given synchronic feature can be ascribed to the same evolutionary process (e.g. *ū* as the result of 'Umlaut,' or as a 'spontaneous' reflex of *u*, (2) sub-areas where one should reckon with imitative expansion, an expansion that may be incomplete, leaving a residue of unchanged forms. (*u* generally appears as *ū* except in a few forms which preserve *u*). In the last stage only, linguists may try to explain the existence of areas — whether or not they cut across genetic language families — as resulting from such factors as substratum, bilingualism, or parallel evolution determined by similar non-linguistic conditions.

The problem of grammatical affinity is also a sub-division of the vast chapter of linguistic convergence. Here again, in order to prove the existence of this type of affinity, one has to show that the geographical extension of some phenomena does not coincide with genetically homogeneous domains. It is likely that the factors involved in the shaping of areas of grammatical affinity are at least partially the same as the ones that may be assumed in the case of 'affinité phonologique.' But the different nature of the phenomena may impose in practice fairly divergent approaches.

Our first duty is to try to identify and discount cases of 'Elementarverwandtschaft.' As pointed out by C. REGAMEY, what has been called the 'ergative construction' characterizes a linguistic type that is found in all parts of the world, and one should be cautious in interpreting its existence in two contiguous but genetically non-related languages as an indication of grammatical affinity.

The next problem is that of determining what type or types of area one should try to delineate. M. A. K. HALLIDAY presents us with a vast South-Eastern Asiatic group of languages where he finds a large measure of agreement in matters of syntactic patterns and general grammatical categories. J. ELLIS, on the contrary, concentrates on a specific nucleus of features common to Turkish and Bulgarian, and stresses the fact that the area for one phenomenon may overlap with that for another. These two lines of approach obviously correspond to different forms of socio-linguistic reality: on the one hand loose contact and participation in one fairly homogeneous material culture for several millennia, on the other hand less protracted but more intimate contacts, with a large amount of bilingualism. The Western European area adumbrated by E. LEWY would seem to result from a combination of both types of socio-linguistic contact and participation. Both lines of approach are therefore fully justified. If however our immediate aim is to afford incontrovertible proof of the existence of grammatical affinity, it is, here again, preferable to concentrate on rather limited areas where similarities should result from intense and comparatively recent intercourse.

The importance of structural factors is duly emphasized by J. ELLIS when he points out that isolated similarities may be ascribed to chance, but similar patterning cannot, and also when he observes that the expansion of a given pattern is favoured or hampered by certain structural features of the recipient language. This will contribute to explain why the synchronic isogloss of a certain grammatical phenomenon which in some quarters may cut across genetic boundaries, may in others coincide with them. Generally speaking, extraneous linguistic forms or patterns will be more readily favoured if they can easily be integrated into the structure. Little resistance will be offered if they can fill existing gaps (cf. phonemic 'holes in the pattern,' 'cases vides').

Yet this does not mean that, in the detailed studies that

are needed if we want to give grammatical affinity the status of a universally recognized phenomenon, structural integration should be made the only criterion for determining what material should be utilized. In a section of Northern France contiguous to the Germanic speaking domain, both in the vernaculars and in the local forms of Standard French, *assez* is placed after the adjective (*grand assez* just like Engl. *big enough*, Dutch *groot genoeg*, etc.). This syntactic detail does not really affect the grammatical structure of the Romance dialects where it appears. Now, evidence of all sorts points to a Germanic origin of this construction; yet if this were the only feature of its type to be found in Northern France, chance could not be entirely ruled out. But if it can be shown that the Romance area of *grand assez* coincides in the main with others of a similar nature, it will be difficult to deny the existence of an area of grammatical affinity extending from Germanic territory into Romance.

In practice, if not in theory, it is not too difficult to distinguish between areas of lexical affinity, whose existence no one would question, and the ones with which we are concerned here, although borderline cases can be thought of. It is normal for purely lexical units to preserve their phonic (or graphic) appearance as they spread across genetic boundaries. Loan translations result in the creation of new isolated syntagms by combination of two (or more) specific lexical items. In our present field, 'grammatical words' are not too commonly borrowed and bound-forms more rarely still, hence investigators have to focus their attention on syntactic combinations and types of syntagms found in contiguous languages rather than on etymological identities and the semantic equivalence of individual items. Whether grammatical affinity, inasmuch as it is not a product of parallel development, always results from the spread of syntactic patterns which may ultimately coalesce into morphological types, or whether morphological types can be directly imitated, is a question which falls outside of the frame of the present survey.

As in the case of 'affinité phonologique' it will be advisable to proceed to an exhaustive examination of the grammatical structures of the various languages spoken in the presumed area in order to be able to assess the comparative importance of hereditary and acquired features. It is clear that any study of affinity in a given area should involve parallel research in the two fields of phonetics and grammar.

TEXT OF CONTRIBUTIONS

1. Similar grammatical phenomena are sometimes found in neighbouring languages which are not related genetically, or in related languages where the phenomenon cannot be derived from the original unity. This could be described as an area of 'affinité grammaticale,' it being understood that the area for one phenomenon may overlap with that for another, and that the reason may be common origin of the phenomenon itself in one language. Thus, the features of Balkan linguistic unity are not all distributed through all the Balkan languages; and some of them may be traced to an origin in Greek. Here it will be shown that one feature, which occurs also in remote languages, nevertheless in its specific form is a link between the Balkan languages in which it occurs.

The use of the Perfect tense to express that the action leading to the present state was not witnessed by the speaker is not unknown to languages like English, and the development of it into a systematic distinction of past actions witnessed and merely reported is found for example in Sanskrit or in Modern Persian. Such a development in Albanian, Bulgarian and Turkish could therefore be a mere coincidence.

But the possibility of coincidence is excluded when we find that Bulgarian has the distinction in all tenses and a further distinction (of belief) in the Past, with forms corresponding to the Turkish, e.g. *târsel, arayormuş* 'he is said to be' or 'to have been seeking,' *târsil aramış* 'he is said to have sought'; *târsil e, aramıştır* 'I believe he sought,' *târsil bil, aramış imis* 'it is said — but I do not believe — he sought.'

When we further consider the age of the phenomenon in each language, and the external history of the languages, it becomes clear (as Yanakiev has shown in detail) that Turkish is the source. The area of this phenomenon thus cuts across that of other phenomena in the Balkan verb, for example the Infinitive periphrasis originating from Greek, which is unknown to Turkish. At the same time, its extent is partly determined by the previous formal resources of the given language, e.g. Bulgarian participial expressions.

J. ELLIS

2. Languages genetically unrelated sometimes show structural similarities: in isolated features, a series of features or the whole grammatical system. Languages showing such similarities may be geographically contiguous or not. Isolated similarities between non-contiguous languages are coincidental; between contiguous languages they may be coincidental, as also a series of similarities between non-contiguous languages. Similarities in the whole grammatical system between unrelated but contiguous languages demand explanation.

Comparing Chinese and Malay, we find both consist chiefly of invariant words and morphemes, expressing grammatical relations by word-order. Both use classifiers; in both qualifiers occur in fixed position relative to the qualified (preceding in Chinese, following in Malay). Malay contains some determinatives of a type not found in Chinese but the categories expressed grammatically in both languages are semantically closely parallel.

Lexically both languages show a high level of abstraction in concrete nouns, expressing the general rather than the specific; whereas in many languages specification is normal in these languages it is often impossible not to express general lexical categories. Concrete verbs of physical action are however extremely specific.

Structural similarities of the whole grammatical system are found among the languages of South-east Asia (Sino-Tibetan, Thai, Mon-Khmer, Malayo-Polynesian groups). Thus there is here an area of grammatical affinity, possibly a continuum, Thai for instance being structurally 'in between' Chinese and Malay. The problem arises of whether this can be explained by the spreading outwards of certain modes of expression; and whether the general homogeneity of material culture would tend towards a uniform linguistic structure.

M. A. K. HALLIDAY

3. En Indochine, il existe une aire grammaticale englobant les langues Thai, le Vietnamien, le Cham et ses dialectes (Rhadé, Jarai), le Khmer et les langues apparentées (Bahnar, Srê, Samré); on peut parler de structure grammaticale

indochinoise: distinction nette entre nom et verbe, adjectifs sont des verbes d'état, présences de particules "numérales" individualisantes, sujet précédant le verbe, complément suivant le nom ou le verbe, emploi de mots pleins comme particules morphologiques. Ainsi le nom "richesse" (Thai *khong*, vietn. *cua*) est employé pour traduire le génitif, le verbe "donner" (Thai *höü*, vietn. *cho*, Khmer *oy*, Cham *brei*) s'emploie pour introduire un complément, au sens de "à," "pour."

Pour savoir si ce type grammatical s'étend à des familles de langues différentes, il faudrait avoir une idée claire de la parenté des familles entre elles. Or il est difficile de s'en rendre compte par suite de l'absence de flexions. Les langues Mon-khmer les plus nombreuses et les plus anciennes d'Indochine, ont bien ce type grammatical, mais sont apparentées par leur vocabulaire aux langues Munda d'un tout autre type.

Le Cham appartient à la famille Malayapolynésienne, et est arrivé par mer dans le Sud de l'Indochine; par son vocabulaire cette famille est apparentée aux langues Kadai du sud de la Chine auxquelles se rattache la langue Thai. Par son vocabulaire le Vietnamien appartient sans aucun doute à la famille Mon-khmer, ses ressemblances avec le Thai sont dues: au voisinage, à une même influence chinoise (époque Han) et à une lointaine origine commune; il est en effet possible que les familles Malayapolynésiennes et Thai aient la même origine que la famille Mon-Khmer. Les similitudes actuelles seraient donc dues à une évolution parallèle. Il n'est pas moins vrai que la famille Mon-khmer se rattache par la famille Munda (dans l'Inde) et la famille Miao-Yao (en Chine), à la famille Tibétobirmane dont fait partie le Chinois, et dans ces familles la perte des flexions a abouti à d'autres types grammaticaux que le type "indochinois."

A. G. HAUDRICOURT

4. Als ich vor 15 Jahren über die Nordsee nach England fuhr, erwartete ich, dass ich in ein Land mit verwandter Sprache kommen würde; der verwandte Wortschatz würde mir schon helfen mir die Sprache anzueignen. Unfassbar war

das Wörtchen *but*. Es enthielt zwei Bedeutungen, die für mich völlig getrennt waren; aber es war im Englischen ein Wort. Zwar schuf die verhältnismässig feste Wortstellung grammatische Kategorien mit grösster Schärfe, aber mir völlig neue Kategorien erschienen — trotz des Schwindens der Endungen: die bestimmte Konjugation, ein Partizipium-Gerundium; negative und interrogative Konjugation waren gleich gebildet; die Relativsätze waren gänzlich anders gebildet; was ich später als 'bracketed phrases' kennen lernte, spielte eine beträchtliche Rolle. Selbst sehr gewöhnliche Ausdrücke und Worte waren durchaus fremd und offenbar nicht auf germanischem Boden gewachsen. Vielleicht am meisten überraschte mich, dass das subjektive Verbum, das als eine besonders rühmliche Qualität einer Sprache anzusehen ich gelernt hatte, obwohl formal kaum bezeichnet, das Zentrum des Satzes blieb. Im ganzen, das wurde mir klar, war ich in eine neue sprachgeographische Landschaft übersiedelt: aus dem wortflektierenden Gebiet war ich in ein flexionsisolierendes gelangt, das mit den nordgermanischen, den westromanischen Sprachen und dem Baskischen eine Einheit bildete.

Ich blieb nicht in England. Seit 30 Jahren war es mein Wunsch gewesen nach Irland zu kommen. Ich habe nicht sehr viel Irisch lernen können. Aber soviel, um zu sehen, dass das rätselhafte *but* seine genaue Entsprechung im irischen *acht* heute noch hat. Negativ- und Interrogativ-Form gehen im Irischen zusammen, zu meinem Staunen fand ich in der berühmten irischen Erzählung *Séadhna* die bestimmte Konjugation in beiden Sprachen gleich verwandt; der Relativsatz bot ähnliche Schwierigkeiten wie der englische; ein merkwürdiges Verbalnomen, auch mit Possessivpronomina, trat auf; verbale Postfixe erschienen. Viele Züge, die mich im Englischen überrascht hatten, fand ich zum Teil genau, zum Teil annähernd im Irischen wieder. Aber, das war auch klar, aus der flexionsisolierenden Landschaft hatte ich mich wiederum in eine andere begeben: in die anlautflektierende. Denn die Anlautflexion war eine wesentliche, und eine neue, Eigenschaft der irischen Sprachlandschaft. Und noch ein wichtiger Sprachzug trat mir entgegen: so formenreich das Verbum noch war, das subjektive Verbum spielte nicht die Rolle, wie im Englischen: das unglaublich

mannigfache Gebiet der Idiome zeigte eine Vorliebe für impersonelle Ausdrücke.

In Irland bin ich geblieben. Neue Meere zu überfahren, wird mir nicht mehr beschieden sein. Aber mir scheint, nach mässigen Proben, dass dieses eigentümliche Überwiegen des impersonellen Verbs auf der nächsten Station auf der Fahrt nach Nordwesten, auf Island, zu finden ist. Nun brauchen wir zur Erfüllung unserer sprachgeographischen Sehnsüchte nur noch zu finden das Land der Anlautflexion.

ERNST LEWY

5. Affinités structurales ne résultant ni de la parenté génétique ni de l'appartenance à un même "Sprachbund" sont créées par la présence de certaines conditions amenant, dans les familles indépendantes, des structures analogues. Celles-ci se créent même dans les systèmes à structure différente, si ces conditions y apparaissent. Exemple: langues à "construction ergative" qui, elle-même, n'est qu'un cas particulier d'un système plus général.

Conditions du système: absence du "verbe fini," fonctions verbales exprimées par des formes nominales, c'est-à-dire déclinables et ne distinguant pas les personnes. Condition secondaire qui n'est pas toujours présente: le verbe nominal distingue des diathèses et des aspects qui sont étroitement associés (subjectif = imperfectif, objectif = perfectif).

Caractéristiques du système:

(1) le verbe nominal occupe la place centrale dans la proposition.

(2) a la place de l'opposition sujet-objet apparaît l'opposition "agens-patiens"; ces deux éléments sont des compléments du verbe central.

(3) nominatif et accusatif font défaut dans le système de déclinaison; le cas qui est ainsi nommé dans les manuels est un absolutif.

(4) le verbe forme avec l'absolutif le syntagme "ouvert," avec les autres cas le syntagme déterminatif.

(5) la fonction de l'absolutif ("agens" ou "patiens") dépend de la signification du verbe (transitif, intransitif) et

de sa diathèse: le complément absolutif du verbe subjectif est patients et dans ce cas l'agens est exprimé par un cas déterminatif (ergatif, génitif, subordonné, etc.); inversement le complément absolutif du verbe subjectif est agens et c'est le patients qui est exprimé par un cas déterminatif (datif, locatif, modalis).

Exemples des variantes de ce système:

A. Distinction des diathèses et des aspects, déclinaison nuancée: tibétain, certaines langues nordaméricaines et caucasiennes du Nord.

B. Absence de déclinaison morphologique, fonctions "casuelles" déterminées par la séquence et par les diathèses du verbe: malais, javanais.

C. Déclinaison réduite aux fonctions les plus générales de subordination et d'indépendance: eskimau, adigé.

D. Type hybride où cette structure empiète sur une structure différente: géorgien, certaines langues indo-aryennes modernes.

CONSTANTIN REGAMEY

SECTION B

COMPARATIVE LINGUISTICS

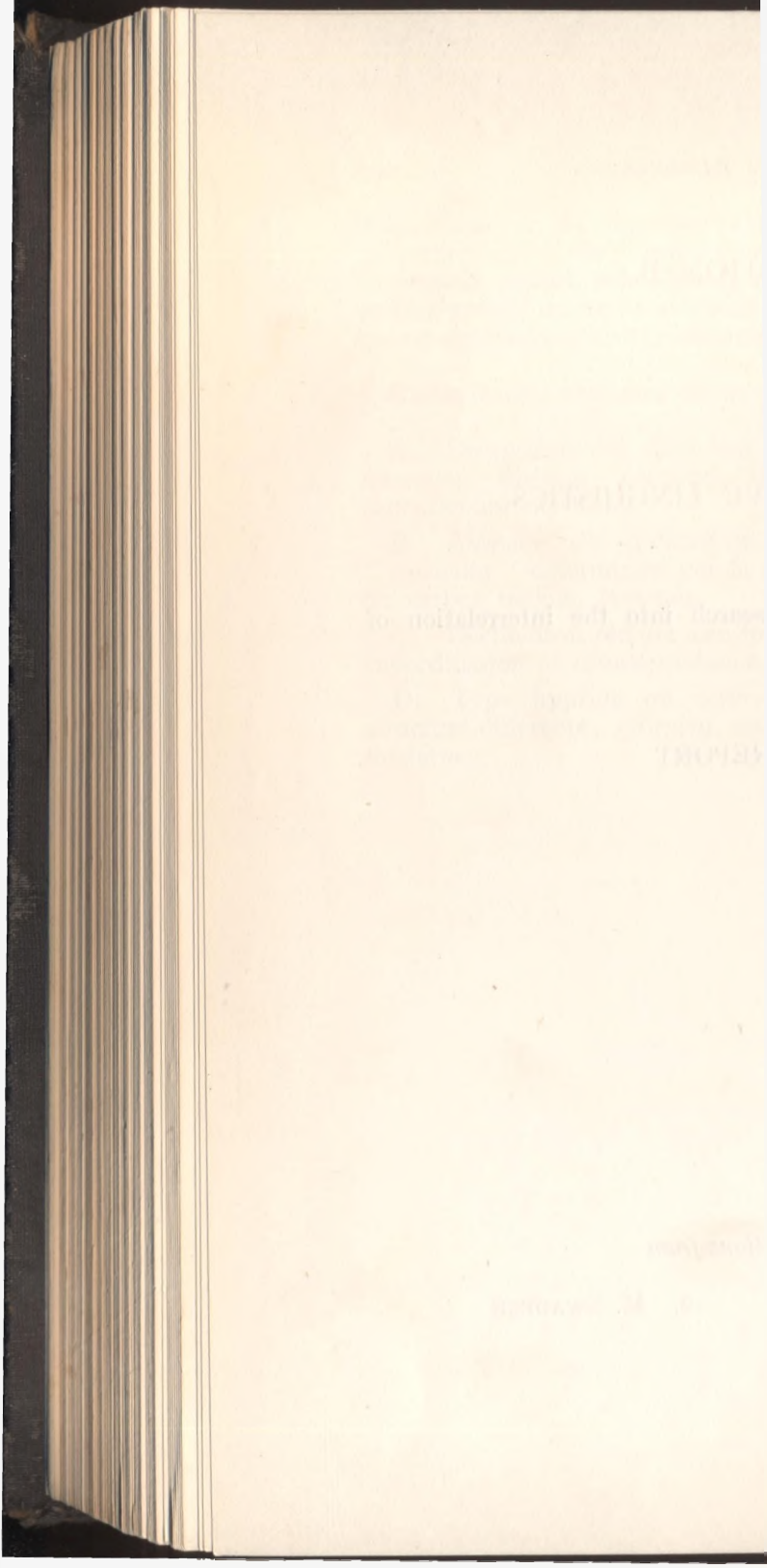
- (5) The present state of research into the interrelation of language families.

NO REPORT

Contributions from

1. B. COLLINDER

2. M. SWADESH



TEXT OF CONTRIBUTIONS

1. Apart from the bewildering linguistic diversity of the Western Hemisphere, there are several unsettled affinity questions. If we keep to those languages which Holger Pedersen calls "nostratic" — Indo-European, Semitic-Hamitic, Uralic, Altaic, Eskimo, Yukaghir, and the Chukchee group — the following hypotheses are *sub judice*: Indo-Hamitic (IE-Semitic-Hamitic), Indo-Uralic, Uralic-Yukaghir, Ural-Altaic, Uralic-Chukchee.

The Indo-Hamitic theory has no grammatical evidence to support it. The word comparisons are numerous, but the phonological correspondences are mostly handicapped by the fact that the Semitic languages have a wealth of fricatives, whereas for Prim IE we can hardly count with more than one (*s ~ z*). In matter of phonological structure, the recent laryngeal theory has brought IE rather close to Semitic and Hamitic, and the triliteralism of Semitic implies no serious difficulties.

As to Indo-Uralic, the common words are few, and the common grammatical elements are chiefly derivative suffixes. The pronominal stems show a striking resemblance.

There are about fifty good lexical correspondences between Uralic on the one hand, and one or two of the Altaic groups on the other. Most of the Prim Uralic case endings have been spotted in Altaic; the pronominal stems may be identical to a considerable extent.

Yukaghir is no doubt related to Uralic. The Chukchee group is likely to be distantly related to Uralic and Yukaghir.

None of these hypotheses has been strictly proved or disproved. If for instance the Ural-Altaic theory is true, we shall probably be able to find out the hidden regular sound-correspondences and discover hundreds of words common to Uralic and Altaic.

BJÖRN COLLINDER

2. How far back in time is it possible to penetrate by comparative linguistics? How does this affect the problem of relationship among recognized stocks?

To answer these questions it is helpful to have an objective measure of linguistic divergence, and this has now been provided by the discovery of an approximately reliable "lexicostatistic constant."

Many scholars, it would seem, take an extremely negative *a priori* attitude towards any theory of relationship involving greater time depth than that, say, of the Slavic family or the Balto-Slavic complex. They would never accept the relationship of modern English with modern Russian, Romanian or Hindustani, were it not for the existence of documentary material from older epochs. Other scholars might accept such relationships but balk at slightly more remote ones, for example, Hamito-Semitic, Eskimo-Euralic-Indoeuropean or Nadene-Sinotibetan. We need objective quantitative and qualitative standards of what constitutes proof of genetic relationship — as against chance similarities and diffusional agreements (borrowings).

By calculating the probability of chance and by empiric study of borrowings in non-cultural vocabulary, I conclude that time depths up to about 12,000 years can be demonstrated in favourable circumstances by present-day comparative methods.

MORRIS SWADESH

SECTION C

THE COMPARATIVE PHILOLOGY OF THE INDO-EUROPEAN LANGUAGES

- (1) The laryngeal hypothesis and the theory of phonemes in
Indo-European

and

- (2) Is there still a root-determinative problem in Indo-
European?

(Combined meeting)

REPORT BY J. WHATMOUGH

Contributions from

- | | |
|--------------------|--------------------|
| 1. T. BOLELLI | 3. L. L. HAMMERICH |
| 2. R. A. CROSSLAND | 4. J. WHATMOUGH |

CONTENTS

NARRATIVE PHILOLOGY OF THE
AFRICAN LANGUAGES

A FURTHER EXAMINATION OF THE
PROBLEM OF THE ORIGIN OF THE
AFRICAN LANGUAGES

By J. WATKINS

Continued from

THE JOURNAL OF THE

AFRICAN STUDIES SOCIETY

REPORT

by

J. WHATMOUGH

Only four answers have been received (Bolelli, Crossland, Hammerich, Whatmough). Only one of these (Whatmough) touches upon the second question, except for the allusion which Bolelli makes to the fact that Benveniste's theory of the root, and especially of the suffix which commonly accompanies it (CVC + VC), was made possible through the discovery of Hittite and the recognition of the role which its *H* must have played in the phonematic structure of Indo-European, both consonantal, and in part (at least in its later stages) vocalic. Yet the two questions are fundamentally related, as Bolelli's hint implies, and as Whatmough's treatment of the two questions as essentially one was also meant to indicate. It is never enough to be satisfied with mere description and phonetic justification. Phonematics inevitably involves function as much as distribution; and function is bound to lead into morphomatics. Therefore the question of root-determinatives must, sooner or later, be considered *pari passu* with the phonematic problem. A satisfactory solution of the latter, when it is reached, will bring troublesome factors of the former into place.

But it would seem that the root-determinative is still very much with us, in fact if not in name; and it is still a lively question whether or not patterns such as *yek-* : *yok-* : *ik-*, or *wek-* : *wok-* : *uk-* are or are not comparable with *trep-* : *trop-* : *trp-*, not to mention *bheidh-* : *bhoidh-* : *bhid-* and *bheudh-* : *bhoudh-* : *bhud-*. It may be that the silence, on this question, of those who received the Questionnaire indicates at least as much an unwillingness to come to grips with the problem in the present state of knowledge as it does a conviction that this problem will find its final answer together with greater certainty about the laryngeals themselves.

In passing, I cannot refrain from noting that those of them who have made use of Sturtevant's *Comparative Grammar*

are working with the first edition of 1933. By this time perhaps most will have seen Volume I in the revised edition, 1951; but attention should also be called to the recent work of Winfred P. Lehmann, *Proto-Indo-European Phonology*, 1952, of which further mention will be made below.

First, then, there is now manifest general agreement that Hittite is an Indo-European language in the same sense as the other Indo-European languages. In other words, the "Indo-Hittite" hypothesis is generally abandoned, and Sturtevant himself now speaks of "Indo-Hittite, or if you prefer proto-Indo-European" or the like, even in his public discussions. Most scholars, it may be conjectured, are more likely to say quite simply, and to mean, "Indo-European." Second, it is also generally admitted that this Indo-European had *H* (I use *H* as a comprehensive symbol for those two or more than two varieties of laryngeal which may eventually be proved to have existed in Indo-European). Then the question is: What has happened? This is not such a hard question in Hittite. But what about the other Indo-European languages? The problem should "be treated on the basis of phonetic knowledge of laryngeal sounds," and "it is no longer permissible to postulate, even tentatively, effects of a laryngeal if such effects cannot be checked by parallels from known languages" (Hammerich). Moreover, our researches must be more than this; they call for a clear notion of structural distribution and function in Indo-European itself. This demand seems to be largely disregarded, except by Lehmann in his recent book; and even he shies away like a frightened horse, when it becomes a matter of taking this particular fence, and soon takes refuge instead in Germanic pastures. Hammerich does venture on setting up something like a pattern with his *a-A*, *o-O*, and *e-E*; but it will take a much more detailed justification, in Indo-European, than has been offered so far, if this is to be accepted as anything more than a pattern of the kind that is so often damned with the faint praise, if praise it be, of the single word "schematic." Instead, therefore, of the necessary and painstaking investigation in the separate Indo-European languages (and this should mean in their very early stages), directed at discovering just what has become of the laryngeals, the tendency seems still to be toward an elaboration of highly hypothetical states of affairs in Indo-European as a whole — as

Bolelli so rightly points out, and which he so justly condemns. We cannot forever proceed on the assumption that these exciting laryngeals, whose discovery was to be so revolutionary, have merely performed a series of vanishing acts; or guess at their "effects."

All this leads to important questions. For example: may we justly appeal to the laryngeal theory for lengthened *i* and *u* in Germanic (Primitive Germanic, Old English, Old Saxon, Old High German) i.e. as being accounted for satisfactorily by assuming, in certain cases, the former presence of a laryngeal? This, of course, is a matter first raised by H. L. Smith. The theory has been severely and hostilely criticized. But it is now taken up anew by W. P. Lehmann and occupies Chapter 4 of his new book. In the same way the following five chapters of his book are concerned with possible reflexes of the Indo-European laryngeals in Germanic (I give the chapter headings): primitive Germanic *g* and *k(k)* corresponding to Indo-European consonantal *w*; laryngeals in the vicinity of Germanic syllabic *r*, *l*, *m*, *n*; OHG *r*-preterites; the development of Indo-European semivowels, sonant liquids, and sonant nasals in Germanic; the origin of Germanic $\bar{e}^2$. In all of these appeal is made at some point or other to laryngeal theory. There is a good deal of daring in the venture, but it seems to me that it is only by means of explorations of this kind that we can hope eventually to deal in a satisfactory way with our proposed topic, which is more than the laryngeal hypothesis. It is also the theory of phonemes in Indo-European, and there we must always keep an eye upon distribution and function. Similar questions are raised by Crossland in his comments, and his remark that his equations "indicate the existence of at least one laryngeal phoneme in proto-Greek, proto-Italic or proto-Italo-Keltic, and proto-Indo-Iranian"; but it must be admitted that far more evidence is required in order to prove the point. Again there is the matter of the treatment of Indo-European initial consonantal *y*- and especially its representation by Greek ζ . This is a question to which both Hammerich and (in Chapter 10) Lehmann devote attention, just as Lehmann does also to the Indo-Iranian breathed aspirates, while Hammerich has a very neat scheme in the patterns *p t k* : *P T K* : *b d g* : *bh dh gh*. To what extent is the Homeric lengthening, for example before *m* in $\mu\epsilon\gamma\alpha\varsigma$, a relic of the

laryngeal, the existence of which in this instance (Latin *magnus*, Hittite *me-ik-ki-iš*, Sanskrit n. *māhi* Gothic *mikils*, Keltic *maglo*-in *Maglo-cunus*) Pedersen has demonstrated (**mgHlo*-)? This question, to which I alluded in the *American Journal of Archaeology*, Volume 52, 1948, page 48 is far-reaching, just as is the question of Homeric lengthening before initial *ρ*- which I discussed in my presidential address before the Linguistic Society of America in December 1951. The explanation given by Monro (*Homeric Grammar* edition 2, page 346) rests too heavily upon analogy to furnish a satisfactory answer. What Monro did not know was that no Indo-European word ever began with *r*-, so that some other consonant always preceded it in an initial reduced grade; accordingly initial *ρ*- is regularly preceded both in the Homeric hexameter, and even in Attic verse, by a long syllable at the end of a preceding word, even when that syllable terminates in a short final vowel; and hence even in those few places with which initial *ρ*- does not "make position" are accounted for by the loss or vocalization of a laryngeal consonant, leaving forms which are only in a superficial way augmented or reduplicated. Is there any reason for believing that in the oldest Greek the smooth breathing was, after all, as Hirt always maintained, the "fester Einsatz," — a relic in fact of the laryngeal? Thus the acrophonic principle would apply in the vowels as well as in the consonants of the Greek alphabet. As for the rough breathing, it is significant that the Semitic *cheth*, not *hē*, was taken over by the Greek alphabet to fill the place where Indo-European initial (and in part inter-vocalic) *s* had stood. This proves that *s* had not yet become *h* and was still (*x*). That being so, there is less difficulty in assuming, at least in the oldest Greek, something like a glottal catch or "fester Einsatz," such as the disparity between contraction and crasis suggests.

It is hard to see how the question of the number of laryngeals, to which Bolelli, Crossland and Hammerich all allude, the number ranging from two to four, can be given more than a tentative answer. Hardly enough stress has been placed upon the fact that Hittite provides no *eHH* : *eH* or *aHH* : *aH* contrasts, but only a contrast *aHH* : *eH*; or upon the fact, to which Hendriksen called attention, that where laryngeals might have been expected internally in Hittite words they are represented by *H* : *HH* only before or after *r l m n y w š*.

Hammerich has some interesting suggestions with regard to Slavonic vowel quantity and syllabification; more convincing is his pronouncement that Hittite is neither a *centum*-language nor a *satəm*-language; a view which I have long held and expounded in my classroom, though I have never ventured to publish it.

It begins to appear, however, that at last we are getting clearer notions of phonematic pattern in Indo-European. Greek reflexes of *Hr* and *mH* contribute something; and may we not begin to think of consonantal *y*-clusters, consonantal *w*-clusters and *r*, *l*, *m*, and *n*-clusters in such things as *bheidh*-, *bhendh*- and the like (cf. Lehmann p. 14)? Is not this a new (and better) line of attack upon the problem of the suffix or root determinative? Is it possible to extrapolate backwards from the status that we find in each of the early Indo-European languages, and so discover the status of Indo-European?

These are all questions calling for answers. It is no part of my present duty to do more than to ask them; and to summarize the discussion that appears in the answers to the questionnaire, not to theorize. But there remains one matter to which attention may properly be called. Surely the time has come for a broad step forward. At present there is a very, very small minority of scholars who reject the laryngeal theory entirely. They will have to present much stronger arguments than they do now, if they are to win any attention at all. But there is a real difference of opinion between those who assume that the laryngeal status antedated Indo-European as it is presented in the Brugmann tradition, and those who seek to plaster, as it were, the laryngeals on to the Brugmann scheme. What is really needed is neither of these expedients, but a total reconstruction of the Indo-European phonematic pattern, a reconstruction which will be in accord not only with the early history of the separate Indo-European languages, Hittite included, but also with modern phonematic theory.

TEXT OF CONTRIBUTIONS

1. Mentre alcuni studiosi si sono fondati sull'ipotesi delle laringali per le più nuove ed audaci costruzioni, quale l'ipotesi indo-ittita, altri l'hanno decisamente respinta come priva di fondamento.

Non vi è dubbio che tale ipotesi si sta per alcuni linguisti trasformando in un mezzo euristico troppo meccanico al quale si ricorre per la spiegazione di fenomeni la cui soluzione non è ancora matura (caso tipico, la quarta laringale di Kuryłowicz, corrispondente alla seconda dello Sturtevant, pura ipotesi di lavoro che non presenta ancora nessuna garanzia di sicurezza); resta tuttavia molto interessante il fondamento stesso delle tre prime laringali del Kuryłowicz che hanno permesso la costruzione della più razionale teoria sin qui esposta della radice indoeuropea, quella di E. Benveniste.

Non credo che si possano ormai respingere i seguenti punti: il parallelismo delle classiche alternanze apofoniche *ei/oi/i* con la nuova *ea/oa/a*; la conseguenza che la vocale *o* la sonante lunga siano date da una breve + *a*; la precisazione del valore di *a*₁ *a*₂ *a*₃ che pare imporsi per spiegare casi come *ἑρός*, *στατός*, *δοτός*; l'identificazione di *a*₁- e *a*₃- con *H*- ittito: la distinzione fra *o* che alterna e *o* che non alterna (e quindi originato da *a*₃e).

La teoria dei fonemi indoeuropei e delle alternanze apofoniche risulta indubbiamente più chiara se si fa ricorso all'ipotesi delle laringali, i cui ulteriori sviluppi dovrebbero, però, fondarsi su un più preciso accertamento dei fatti, il che non è sempre avvenuto nel recente passato.

TRISTANO BOLELLI

2. Professor E. H. Sturtevant has concluded that the "laryngeals" lost phonemic status in all dialects of IE except proto-Anatolian, (that from which Hittite evolved), before the peoples who spoke them lost contact with each other.

(See *The Indo-Hittite Laryngeals*, [Linguistic Society of America, 1942], pp. 23-29, especially pp. 28-29). Four arguments may be advanced against his conclusion.

(1) The force of his argument depends partly on the number of "laryngeals" postulated for IE. The writer postulates two. (See *Transactions of the Philological Society* 1951, pp. 88-122, especially pp. 121-122).

(2) It is not improbable that two or more fricatives similar in quality should have lost status contemporaneously in one language in similar sound-sequences, nor is it unlikely that each such fricative should have lost status independently in similar sequences in a number of cognate languages not in contact.

(3) The sequence **Hw-* appears to have developed differently in Greek *ἄισα*, *ἄησι*, and Sanskrit *vásati*, *vāti*. (*ἄησι* < **Hwēti* < **Hwēti*; *vāti* < **Hwēti*).

(4) The sequences **-nH-* and **-rH-* appear to have developed differently in Gk. *δμᾶρός*, Skt. *dāntás*, and Latin *grātus*, Skt. *gūrtás*, respectively. (*δμᾶρός* < **dm^hHtós*, *dāntás* < **d^mHtós*; *grātus* > **gr^hHtós*, *gūrtás* < **g^rHtós*).

The equations in (3) and (4) indicate the existence of at least one "laryngeal" phoneme in proto-Greek, proto-Italic, (or proto-Italo-Celtic), and proto-Indo-Iranian after the peoples who spoke them lost contact.

R. A. CROSSLAND

3. The laryngeal hypothesis as framed by F. de Saussure, Herm. Møller and A. Cuny has been verified in 1927 by J. Kuryłowicz through his identification of "ə indoeuropéen et H hittite" — Hittite having preserved the simple consonantal phoneme in changing the articulation from laryngeal to velar. The problem has henceforth to be treated on the basis of phonetic knowledge of laryngeal sounds. It is no longer permissible to postulate — even tentatively — effects of a laryngeal, if such effects cannot be checked by parallels from known languages.

The vocalized form of a laryngeal must, originally, be a laryngealized vowel. With laryngealized vowels a strong

thrilling of the vocal cords accompanies the articulation of the vowel. In several European languages laryngealized vowels are used incidentally in passionate expressions of e.g. indignation (*A:*), astonishment (*I:*), childish disappointment (*O:*), shivering with cold or fright (*U:*). In some Extra-European languages laryngealized vowels may be phonemes, thus in Hindustani, where e.g. the Arabic loan-word *vage:ra* 'and so on' has approximately the same vowel as Eng. *the bear*, whereas *bE:ra* 'deaf' (Skr. *bahira-*) has the corresponding laryngealized vowel.

Indo-European has had a double series of vowels: *a* (as in Skr. *kakubh-*) — *A* (the vocalized form of the laryngeal, as in Skr. *sthita-*: the special Aryan *i*-quality being part of the Aryan palatalization); *o* (the common *o*, in apophonic relation to *e*) — *O* (the *o*² of Armenian, the *ō*² of Lithuanian); *e* (in apophonic relation to *o*) — *E* (e.g. in words as Lat. *egō, esse, sex* — without apophony, but with vocalic irregularities in several languages). It lies at hand to explain *a, o, e* as ordinary vowels, *A, O, E* as laryngealized vowels.

Hittite delaryngealizes completely, and thus has no trace of the laryngealized series. Centum has still traces of the difference between *e* and *E*, but only Satem, besides this, preserved both *a* and *A*, both *o* and *O*.

Nobody ever seems to have doubted seriously that IE possessed laryngealized consonants: as compared with the ordinary mediae *b, d, g* the aspirated mediae *bh, dh, gh* of Centum-Satem are just laryngealized mediae. Recently Holger Pedersen has shown that the enigmatic lack of initial *b-* in IE must be due to Proto-Indo-European loss of *p-* (which is a common phonetic development) and ensuing sound-shift of mediae to tenues and of tenues to mediae. In the same way — but later — the laryngealized mediae *bh, dh, gh* of Centum-Satem had developed out of laryngealized tenues *P, T, K* (i.e. the same sounds as the emphatic tenues of Semitic). Proto-Hittite again delaryngealizes completely, so that laryngealized *P, T, K* and non-laryngealized IE *p, t, k* coincide in these latter sounds. In this way it may perhaps be explained that Hittite *p, t, k* represent both the *p, t, k* and the *bh, dh, gh* of Centum-Satem. The voiced character of IE *b, d, g* seems primarily to have been preserved in Hittite.

Hittite is no Centum-language. Whereas Centum has only labiovelars and non-labiovelars, and Satem has only palatovelars and non-palatovelars, Hittite reflects all three classes of velars: the palatovelars, the pure velars, and the labiovelars. Thus Hittite must be of earlier origin not only than any Centum- or Satem-language, but even than the separation between Centum and Satem.

LOUIS L. HAMMERICH

4. The laryngeal theory (it has long since passed the stage of hypothesis) has the advantage (among others) that it reduces the unlikely total of seventy-two "IE phonemes" of the Brugmann enumeration to the far more reasonable number of thirty-three (by substituting *H* for *a*, *eH* and *He* for *ā/ā*, *ē/ē*, *ō/ō*, etc.). This brings the IE phonematic range into closer conformity with those of observed languages.

But the structure *CVC* + *VC* seems to leave the "suffix" (*VC*) not very far removed from the "determinative" of the older terminology. Formally *ter-es* (:-*em*-, -*ep*-) and *bheu-eH₂*- are more satisfying; but functionally, even though suffixes in -*m*-, -*s*-, -*H*-, -*dh*- etc. may be brought into the IE morphological pattern, others (in -*p*-, for example) resist classification. To postulate an alternation -*p*- : -*b*- : -*bh*- : -*m*- as an effective pattern is only a subterfuge.

JOSHUA WHATMOUGH

The first of these is the fact that the United States is a young nation, and that its history is a history of growth and expansion. The second is the fact that the United States is a nation of immigrants, and that its history is a history of the struggle for assimilation and the creation of a new American identity. The third is the fact that the United States is a nation of free men and women, and that its history is a history of the struggle for liberty and justice for all.

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SECTION C

THE COMPARATIVE PHILOLOGY OF THE INDO-EUROPEAN LANGUAGES

- (3) How far can the methods and principles of linguistic geography be successfully applied to the study of Indo-European?

REPORT BY M. LEJEUNE

Contributions from

- | | |
|--------------------|----------------|
| 1. T. BOLELLI | 3. W. DROHLA |
| 2. R. A. CROSSLAND | 4. J. FOURQUET |
| 5. M. LEROY | |

THE HISTORY OF THE
CITY OF BOSTON

FROM THE FIRST SETTLEMENT
TO THE PRESENT TIME

BY
JOSEPH NEALE

VOLUME II
CONTAINING THE HISTORY OF THE
CITY OF BOSTON FROM 1630 TO 1700

NEW YORK

REPORT

by

M. LEJEUNE

Cette question n'a suscité qu'un petit nombre de réponses. Encore l'une d'entre elles (M. Leroy, Bruxelles) n'aborde-t-elle pas vraiment le problème; mais elle rappelle, justement, qu'il est risqué d'étudier la répartition dans l'espace de données linguistiques qui sont, ou qui peuvent être, chronologiquement disparates. Les autres réponses, qui émanent de MM. Bolelli (Pise), Crossland (Newcastle-upon-Tyne), Drohla (Heidelberg), Fourquet (Strasbourg), marquent, dans l'ensemble, des réserves sur l'application des principes et des méthodes de la géographie linguistique à l'étude de l'indoeuropéen, au moins au stade actuel de nos connaissances.

Il convient, nous semble-t-il, de distinguer, à cet égard, deux ordres de tentatives.

Les unes sont étroitement limitées dans leur objet. Elles s'appliquent à un domaine géographique restreint, et à une période historique déterminée, l'un et l'autre choisis en fonction de l'abondance et de l'intérêt des données conservées par les textes; elles étudient, dans les limites indiquées, la répartition des données, et aboutissent à la constitution d'une carte ou d'une série de cartes; elles proposent ensuite une explication de la distribution constatée, en reliant celle-ci, dans la mesure du possible, à des faits historiques plus ou moins bien connus. C'est le cas des études de Kieckers sur la Crète (1908), de Schrijnen sur le Latium et les régions voisines (1922 et 1931), de Vandervelde sur la Thessalie (1924) et la Béotie (1929) etc. A ces tentatives (que M. Drohla est seul à évoquer), on peut adresser plusieurs ordres de critiques. Trop souvent, chaque fait est considéré pour lui-même, sans égard à ses connexions dans le système de la langue (voir la réponse de M. Fourquet); les faits relevés et confrontés dans chaque cas sont trop souvent seulement approximativement contemporains, l'approximation étant de l'ordre du siècle, sinon plus; l'interprétation phonétique des graphies anciennes est loin d'être

toujours évidente; enfin, les lacunes de notre information rendent fragiles les conclusions de ces études, qu'une découverte imprévue peut remettre en question; si, par exemple, on admet que *aisu-* dans les inscriptions de Gurina (Carinthie) est le nom vénète du "dieu," les conclusions de Schrijnen (sur la racine *ais-* en Italie) appellent une révision. Il va de soi que des objections analogues (— et, ici, les lacunes peuvent aller jusqu'à l'ignorance totale d'une langue ou d'un groupe de langues —) peuvent être opposées aux tentatives qui visent l'ensemble du domaine indo-européen.

De celles-ci, on ne refera pas ici l'histoire. Rappelons seulement, à la date de 1908, les *Dialectes indo-européens* de Meillet, projetant dans une langue commune fictive, sous forme de particularités dialectales, un certain nombre des différenciations par lesquelles s'opposent entre elles les langues historiquement attestées, et admettant, ce qui échappe à la preuve, que les "dialectes" indo-européens et les langues qui en sont issues, ont conservé, dans l'espace, les mêmes positions relatives. Une vingtaine d'années plus tard, Bartoli proposait sa théorie des aires, et Meillet lui-même corrigeait ses vues antérieures en distinguant, en gros, des langues "périphériques," conservatrices, et des langues "centrales," novatrices; si les vues de Bartoli étaient trop systématiques, et celles de Meillet trop floues, le facteur temps n'en entraînait pas moins en jeu. Mais il s'agissait d'un temps, en quelque sorte, abstrait. Une tendance s'est manifestée, alors, à la concrétiser, c'est-à-dire à essayer de lier, notamment sous l'influence de Kretschmer, les évolutions des langues aux vicissitudes des populations qui les parlaient; mais le risque est alors de reconstruire hasardeusement cette préhistoire, et cette préhistoire linguistique, à partir de l'archéologie (ainsi chez Specht); ou bien encore (et il y a alors cercle vicieux), le risque est de reconstruire, à partir des données linguistiques, la préhistoire qui doit les expliquer (risque auquel n'échappent pas toujours, par exemple, les recherches de Pisani). Il est, cependant, incontestable que grâce aux travaux de ce dernier demi-siècle, on tend, de plus en plus, à se représenter l'indo-européen sous les espèces du réel: au départ (qui n'est un départ que par notre ignorance de ce qui précède), un ensemble linguistique qui évolue dans le temps, et est, dialectalement, diversifié dans l'espace; une dissociation progressive, les émigrants emportant avec eux des parlers dialectalement

divers et à des stades d'évolution différents, chacune de ces dislocations pouvant avoir entraîné des regroupements nouveaux au sein de la masse indo-européenne, ou des contacts de telle ou telle fraction de cette masse avec des populations allogènes; puis, pour chaque groupe d'émigrants, une évolution linguistique propre, commandée d'abord principalement par le système initial, puis plus ou moins largement influencée par les contacts successifs avec d'autres populations, elles-mêmes indo-européennes ou non.

Mais, à cette représentation, assurément juste dans son principe, peut-on, par récurrence, appliquer valablement des méthodes d'investigation qui, jusqu'ici, n'ont fait leurs preuves que sur du réel? Si la géographie linguistique, travaillant sur le domaine roman ou sur le domaine germanique, *in vivo*, permet d'apercevoir comment s'amorcent et se propagent les évolutions, sommes-nous en droit de transposer ces méthodes à un domaine de recherches partiellement inconnu, à des faits dont la chronologie, même relative, est le plus souvent incertaine, à des époques pour lesquelles tout arrière-plan historique fait défaut? Telle innovation commune à deux langues indo-européennes historiquement voisines comporte, *a priori*, une série d'explications possibles: coïncidence fortuite, développements indépendants et parallèles, action d'une tierce langue (substrat, etc.), extension de l'innovation d'un des parlers à l'autre, cette extension elle-même restant, d'ailleurs, à dater (entre l'époque de communauté initiale et l'époque historique): il s'en faut que le choix de l'explication s'impose toujours. La méthode statistique qui consiste à relever, entre langues indo-européennes prises deux à deux, toutes les concordances, ou toutes les différences, a par elle-même une portée typologique plutôt qu'une portée historique, car chacune des concordances ou des différences peut avoir son explication propre, et les lignes isoglosses ainsi définies sont de très inégale signification; et le choix de celles qu'on considère comme significatives demeure largement subjectif.

Tel est le sens général des réponses reçues. Sans nier la possibilité d'appliquer, quelque jour, et jusqu'à un certain point, de façon valable, les principes et les méthodes de la géographie linguistique à l'étude de l'indo-européen, elles s'accordent à formuler des réserves de méthode, que nous soumettons à la discussion du Congrès.

TEXT OF CONTRIBUTIONS

1. I principi della geografia linguistica trovano nella loro applicazione allo studio delle lingue indoeuropee delle gravi limitazioni dalle condizioni stesse dell'indagine. Nel caso delle lingue romanze, in cui il metodo geografico ha segnato le conquiste più notevoli, si ha, nella maggior parte dei casi, la possibilità di controllare i dati della geografia linguistica nei documenti storici e questo è un grande vantaggio se si riflette che l'indagine geografica si risolve sempre in un'indagine storica, in quanto si propone di determinare, fra due fasi linguistiche, quale è la più antica e quale la più recente. Inoltre, la posizione reciproca delle lingue romanze è conosciuta perfettamente e la storia della romanizzazione si può seguire con grande precisione. Per le lingue indoeuropee, non solo non abbiamo documenti del periodo unitario, ma dobbiamo presumere — senza, peraltro, averne alcuna prova — che la loro posizione reciproca fosse nella preistoria identica a quella dei tempi storici. Infine (e questo è un limite del metodo anche nella sua applicazione alle lingue romanze) occorre tener presente il valore della sinonimia: la possibilità di coesistenza di voci sinonimiche è nelle lingue indoeuropee ostacolo assai maggiore che nelle lingue romanze, sempre perché, per queste, l'indagine si svolge con una documentata storia delle fasi precedenti. Quando un termine manca ad una lingua indoeuropea, possiamo sempre pensare che tale perdita sia fortuita e non, perciò, significativa per l'indoeuropeo unitario; meno grave è tale aporia nell'indagine delle lingue romanze, tanto più ricche di possibilità di controllo.

Pur con tali limitazioni, i principi della geografia linguistica, se applicati correttamente e cautamente, possono essere, anche nella linguistica indoeuropea, utili integrazioni della ricerca storica.

TRISTANO BOLELLI

2. The following remarks are concerned with some observations on Professor G. Bonfante's application of the principles of linguistic geography to the study of the relationship between

(cuneiform) Hittite and the languages generally recognized as Indo-European. The phrase 'generally recognized as Indo-European' has been used to indicate that Professor Bonfante's comparisons of Hittite and 'Hieroglyphic Hittite' forms have been ignored as premature. Cf. *Transactions of the Philological Society* 1951, p. 89, footnote 2, (i) and (ii).

In five articles published since 1933 Professor G. Bonfante has considered a number of isoglosses shared by (cuneiform) Hittite, and other Indo-European languages (Cf. (iii) in footnote quoted above), and has concluded that proto-Anatolian, the IE dialect from which Hittite evolved, belonged to a "central" group of dialects and had particular affinity to proto-Greek and proto-Armenian. (See *Indogermanische Forschungen* 52, 55; *Revue belge de philologie* 18; *American Journal of Philology* 67; *Journal of the American Oriental Society* 64). His study would seem to show particularly clearly the difficulty of applying the principles of areal linguistics to the study of epigraphic languages known from documents written in non-contiguous areas, different chronological periods, or both, and the danger of attempting to apply them mechanically. Professor Bonfante appears to accept isoglosses as significant without considering:

(i) Whether the features shared by historical languages may be archaisms which had been lost in others related to them.

(ii) Whether they may result from an innovation so simple that it might easily have been made in languages not in contact.

(iii) Whether they may result from the adoption of cognate languages by peoples speaking languages or dialects which shared features or tendencies which became established independently in the adopted languages.

It would seem that while a statistical study of isoglosses may establish similarity between some of a number of cognate languages, it cannot indicate whether it results from "close relationship" of the dialects from which they evolved, that is, from contiguity of the areas inhabited in the prehistoric period by the peoples who spoke those dialects, or from those peoples' having remained in contact for some time after losing contact with other dialect-communities.

Only probabilities can be established in the study of the interrelation of the dialects of IE.

R. A. CROSSLAND

3. Die zunächst in der deutschen und romanischen Dialektologie erarbeiteten grundsätzlichen Einsichten der Sprachgeographie haben unbestreitbar viel dazu beigetragen, unsere allgemeinen Vorstellungen vom Begriff des Indogermanischen, von seiner vermutlichen lokalen Differenzierung sowie der Entstehung und Entwicklung der Einzelsprachen lebendiger und natürlicher zu gestalten. In dem Antriebe zur Überprüfung unserer theoretischen Grundanschauungen scheint mir bisher auch der Hauptwert der "neolinguistischen" Methode in ihrer Anwendung auf die Indogermanistik (Bartoli, Bonfante) zu liegen. Diskussion R. A. Hall Jr./G. Bonfante/E. H. Sturtevant in Lang. 22, 23 (1946, '47); dazu die gerechte Abwägung von J. R. Ware, Lang. 25 (1949) S.81 f. Vgl. auch H. Arntz, V^{me} Congr. intern. des Linguistes, 1939, Rapp. S.128 f.

Die sprachgeographische Methode ist jedoch an beliebig verfügbarem Sprachmaterial entwickelt worden, auf jeweils beschränktem geographischem Gebiet, das geschichtlich leicht überschaubar ist. Bei ihrer Übertragung auf einzelne Probleme der vergleichenden und historischen idg. Grammatik werden zu leicht die völlig anderen Voraussetzungen und die sich daraus ergebenden Schwierigkeiten ausser Acht gelassen: die Sprachzeugnisse sind lückenhaft und chronologisch ungleichmässig, die hinter der Sprachentwicklung stehenden geschichtlichen Vorgänge in dem weiten eurasischen Raum sind zumeist unbekannt, waren aber gewiss oft recht kompliziert. (P. Kretschmer, Einleitung S.152, Glotta 22, S.195 f.; F. Specht, KZ 62, S.29; H. Arntz (siehe oben) S.114 f.). Daraus erklärt sich unter anderem die Uneinheitlichkeit und Unsicherheit in der Beurteilung von "Rand- und Zentralsprachen" (Meillet, Schrijnen, Bartoli). Die verschiedenen hypothetischen Versuche zur graphischen Darstellung von Dialektverhältnissen des voreinzelsprachlichen Zustandes (Meillet, Johansson, Pisani, Arntz u.a.) sind aus dem gleichen Grunde ohne praktischen Wert.

Das Aufsuchen lautgeschichtlicher und morphologischer Isoglossen in den Einzelsprachen (Meillet, Bonfante, Pisani) gehört zu den selbstverständlichen Erkenntnismitteln unserer historisch-vergleichenden Methode und ist insofern nicht erst durch die Sprachgeographie angeregt worden. Die Beurteilung der idg. Isoglossen bleibt notwendigerweise subjektiv (Vgl. M. Lejeune, *Mémorial Ét. lat.* 1943, S.16.); für eine

Frage wie die, ob das Augment aus gesamtidg. Zeit stammt oder von den südöstlichen Sprachen sekundär (weiter-?) entwickelt wurde, kann die Sprachgeographie keine entscheidenden Argumente liefern.

Wie für die Erforschung indogermanischer Einzelsprachen in besonders günstigen Fällen sprachgeographische Methoden und Grundsätze förderlich sein können, hat unlängst E. Risch, *Mus. Helvet.* 6 (1949) S. 19 ff. für das Altgriechische gezeigt. Entsprechende Versuche für Altitalien haben nur fragwürdige Ergebnisse erzielt. Es wäre zu wünschen, dass die Neolinguistik ihre Bewährungsprobe an altgriechischer Dialektologie ablegte.

WOLFGANG DROHLA

4. La première grammaire comparée a été édifiée sur le postulat que ce qui est commun à plusieurs langues était présent dans l'ancêtre commun ('Stambaumtheorie'). Devant de nombreuses difficultés d'application, il a fallu envisager la possibilité d'une autre origine de la communauté: une innovation née dans une langue s'est étendue à toute une aire ('Wellentheorie' de Johann Schmidt).

La géographie linguistique (ALF, DSA, etc.) a confirmé de façon éclatante l'importance énorme de cette seconde origine de communauté, et en a analysé *in vivo* le mécanisme, qui prend des formes très variées.

Il en résulte qu'à chacun des raisonnements de la grammaire comparée traditionnelle, le linguiste initié à la géographie linguistique peut opposer une autre explication possible; de sorte que chaque fois la solution reste indéterminée.

Un fait commun à toute une famille peut venir de l'ancêtre commun, ou d'un développement commun postérieur à la séparation des dialectes: le parfait formé avec l'auxiliaire *avoir* ou *être* et le participe passé est commun aux langues germaniques actuelles: il apparaît dans des groupes différenciés depuis plus d'un millénaire. (cf. Meillet: Note sur une difficulté générale de la grammaire comparée; Développements convergents).

Le fait qui a la plus grande extension n'est pas nécessairement

le plus ancien: le parfait en question est plus récent que le futur formé avec *man*, et l'infinitif, propre au seul islandais.

Les particularités communes propres à un ensemble de parlers peuvent être ce qui subsiste d'un état commun non différencié, ou résulter d'un mélange de dialectes: il y a un type silésien né d'un nivellement portant sur un mélange de dialectes thuringiens, franconiens, hessois etc., pas de 'urschlesisch.'

Ce qui distingue un dialecte d'un autre peut être la somme des innovations propres à ce rameau ou simplement une certaine combinaison unique en son genre de caractères qui tous se trouvent aussi dans d'autres dialectes (la base dialectale de la 'koinè' définie par Th. Frings comme l'aire située du côté *haus* de la ligne *hūs*:*haus*, du côté *ich* de la ligne *ik*:*ich*, du côté *gên* de la ligne *gân*: *gên* etc.).

La présence d'un type d'articulation dans une langue peut s'expliquer par une vraie loi phonétique ou par une fausse: évolution insensible portant sur une certaine articulation dans tous les mots, (kl > kly > ky dans "clou, clouer, clé" etc.), ou adoption du résultat final par 'Formübertragung,' invasion de mots présentant la forme nouvelle, suivie d'une généralisation de la nouvelle articulation.

Ce ne sont que les principaux exemples des indéterminations que comporte la méthode comparative, c'est à dire le raisonnement fondé sur l'extension des faits linguistiques à un nombre plus ou moins grand de témoins.

Les linguistes connaissent aujourd'hui ces enseignements de la géographie linguistique; ils savent théoriquement que 'gemeingermanisch' n'est pas identique à 'urgermanisch.' Mais l'enseignement fondé sur l'ancienne 'Stammbaumtheorie' continue, (accompagné de quelques réserves) parce que nous ne savons pas encore par quoi le remplacer.

Tandis qu'il subsiste, s'élabore une linguistique historique fondée directement sur la géographie dialectale: *Grundlegung einer Geschichte der deutschen Sprache* de Th. Frings, *Entstehung der romanischen Völker* de W. von Wartburg. Grâce aux atlas linguistiques et aux témoins écrits des états anciens on reconstitue l'histoire d'une grande aire dialectale, Romania, Germania, dans la période où elle est directement saisissable.

La méthode comparative est, par essence, un essai pour reconstituer ce qui est au delà des plus anciens témoins, pour

expliquer "historiquement" leurs rapports. On voit aujourd'hui que c'était une erreur de tenter cette reconstitution sur une "vue de l'esprit," non sur l'observation à date historique, des processus de la différenciation dialectale.

Il n'y a pas lieu d'admettre que les indéterminations que nous avons évoquées plus haut sont irréductibles, et que toute tentative pour remonter au-delà des plus anciens témoins est vaine.

Il est probable que la recherche fera des progrès considérables:

(1) si on définit mieux ce qu'on cherche à reconstituer par la méthode comparative: non une 'Ursprache' ('urwest-germanisch,' 'urgermanisch,' 'urindogermanisch'), mais un échelonnement de "situations linguistiques" dont chacune correspond à un état de langue avec des variantes dialectales, et à une aire dialectale comportant une certaine structure.

(2) si, dans l'interprétation des faits, on cherche à lever l'indétermination chronologique par une meilleure analyse interne des faits, qui révèle ce qui est survivance et innovation, qui établit une chronologie relative, non par l'extension du fait ('gemeingermanisch,' 'gemeinwestgermanisch'), mais par ses relations avec d'autres faits. Cela implique que la linguistique historique surmonte l'atomisme actuel de la méthode diachronique et devienne "structurale." Cette méthode doit du reste être essayée sur les périodes connues, en liaison avec la géographie linguistique, avant qu'on puisse la transporter définitivement dans le domaine conjectural de la linguistique comparative.

J. FOURQUET

5. L'indo-européen ne devrait plus être considéré comme un ensemble mécanique et régulier de correspondances symboliques mais bien comme une langue vivante avec sa norme et ses aberrances, et ayant comporté des différenciations spatiales et diachroniques. Deux remarques sur les occlusives dans l'état phonologique de l'indo-européen; on a attribué à ce dernier:

(1) Trois séries de "gutturales"; or l'étude des rapports

entre les dialectes indo-européens, et la phonétique expérimentale enseignent clairement que l'avancement relatif (pouvant être retardé par l'entourage phonétique) du point d'articulation des vélaires et labio-vélaires est une innovation des langues 'satəm'; la distinction entre diverses qualités de gutturales ne peut être reportée à l'indo-européen commun.

(2) Une série de sourdes aspirées; or ces phonèmes (a) n'apparaissent guère que dans le vocabulaire expressif — (b) ne sont représentés qu'en indo-iranien, arménien, grec et aussi, mais partiellement, slave.

C'est donc une erreur de méthode — déjà soulignée par Meillet et d'autres — que de projeter dans l'indo-européen commun des faits qui n'apparaissent que dans des états de langue différenciés et postérieurs; on gonfle ainsi démesurément le tableau phonologique des sons de l'indo-européen alors que le système normal de toute langue vivante n'en présente qu'un nombre beaucoup plus restreint.

Il n'est pas inutile de revenir sur ces distinctions illusoires prônées par les néo-grammairiens car il est décevant de constater qu'elles continuent d'apparaître dans des ouvrages de base dus à des linguistes appartenant à des écoles diverses, comme Buck, Pederson, Schwyzler, Prokosch, Kent, Pisani, etc.

MAURICE LEROY

SECTION C

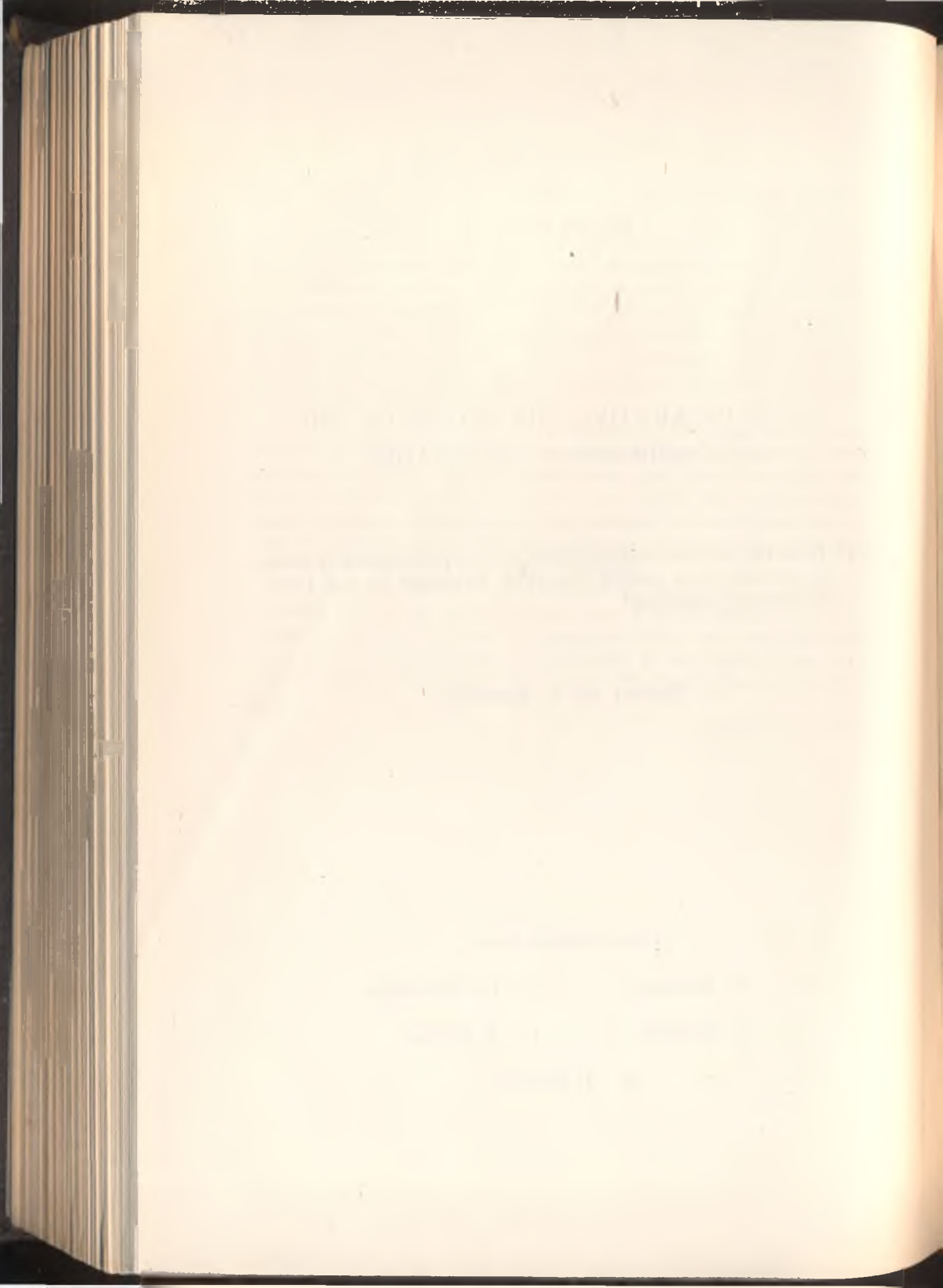
THE COMPARATIVE PHILOLOGY OF THE INDO-EUROPEAN LANGUAGES

- (4) How far can conclusions about cultural and social systems be drawn from purely linguistic evidence in the Indo-European languages?

REPORT BY A. SCHERER

Contributions from

- | | |
|---------------|---------------|
| 1. E. BLESSE | 3. E. BUYSENS |
| 2. T. BOLELLI | 4. J. ELLIS |
| 5. J. GONDA | |



REPORT

by

A. SCHERER

Die Frage, ob und wieweit kulturelle und soziale Systeme der Vorzeit aus sprachlichen Tatsachen erschlossen werden können, setzt zunächst eine Einigung darüber voraus, ob überhaupt sprachliche Beweisgründe ausreichen, um vorgeschichtliche Kulturzustände zu erkennen. Die Ansichten darüber gehen weit auseinander. Wie T. Bolelli mit Recht betont, hat das Problem keinen Sinn für den, der nicht an die Existenz einer indogermanischen Grundsprache glaubt; wenn man aber am Indogermanischen als einer Gesamtheit von alten Isoglossen, die nicht auf Entlehnung beruhen können, festhält, sind Schlüsse aus sprachlichen Erscheinungen auf kulturelle und soziale Tatsachen gerechtfertigt, soweit eine einwandfreie Methode befolgt wird. Da schon der Begriff "indogermanisch" ("indoeuropäisch") rein sprachlicher Natur ist, hat die Sprachwissenschaft in der Erschliessung der indogermanischen Kultur das erste Wort gegenüber anderen Wissenschaften.

E. Blesse glaubt, "dass die sprachliche Forschungsmethode, wissenschaftlich und objektiv-kritisch angewandt, bei der Erforschung der ehemaligen Kulturzustände eines Volkes immer Erfolg verspricht" und veranschaulicht das an einer psychologischen Ausdeutung der reflexiven Verba des Slavischen und Baltischen, die ihm auf eine Zeit starker Hervorhebung der tätigen Persönlichkeit zu deuten scheinen.

Den optimistischen Betrachtern darf man ohne weiteres die Stimmen derer zurechnen, die, ohne sich mit den grundsätzlichen Fragen auseinanderzusetzen, kulturgeschichtliche Schlüsse aus der Sprache ziehen: so J. Ellis, J. Gonda, und die meisten Verfechter der im späteren Teil dieses Berichtes zu erwähnenden Thesen.

Auf der anderen Seite bestreitet E. Buyssens jede Möglichkeit, den Sprachstoff zur Rekonstruktion kultureller und sozialer Tatsachen oder Systeme auszuwerten, die nicht durch Texte verbürgt sind. Er zeigt an Beispielen aus

dem Französischen die Problematik solcher Schlüsse: das grammatische Geschlecht könnte an animistische Vorstellungsweise der gegenwärtigen Franzosen denken lassen; das Fehlen eines Oppositums zu *ivre* "betrunken" legt einen falschen Schluss nahe, und ebenso die Grundbedeutung "(Wasser =) Behälter" des Wortes *tank* "Kampfwagen." Man kann aber nach meiner Meinung solche Fälle auch etwas optimistischer beurteilen. Die beiden ersten Fehlschlüsse würde eine vorsichtige Methode vermeiden: auf animistisches Denken dürfte man (bestenfalls) nur für die Zeit der Entstehung des grammatischen Geschlechtes schliessen, und die Gefährlichkeit eines *argumentum ex silentio* ist wohlbekannt. Vor ausgesprochenen "Fallen" wie *tank* kann freilich auch die vollkommenste Methode nicht schützen. Wo unerkennbare besondere Umstände im Spiele sind, muss die etymologische Betrachtungsweise irreführen. Aber in der Regel würde sich wohl kein so scheinbar glattes Resultat ergeben wie bei jenem Wort. Das Schlussverfahren und die Methodik werden durch solche Einzelfälle kaum erschüttert. Die Möglichkeit unerkennbarer Fehlerquellen ist ja in jeder Wissenschaft gegeben und würde auch gegenüber etwa überlieferten urindogermanischen Texten bestehen bleiben. Wir werden aber genötigt sein, die methodische Forderung zu stellen, dass als gesichertes Ergebnis nur gelten darf, wofür mehrere selbständige Beweisgründe sprechen.

Was lässt sich nun etwa tatsächlich über kulturelle und soziale Systeme der Vorzeit feststellen oder vermuten? Leider liegen hier nur wenige Äusserungen vor. J. Ellis verweist auf den Gedanken von Prokosch, dass der morphologische Zusammenhang zwischen Kollektivum und Femininum auf eine Ackerbaubevölkerung mit Viehzucht deute, für die die Haustierarten durch das weibliche Tier repräsentiert werden; er selbst glaubt aber, dass in jenem Verhältnis sich vielmehr, oder wenigstens in erster Linie, die Rolle der Geschlechter in der menschlichen Gesellschaft widerspiegle. — Nicht so unmittelbar auf kulturelle Systeme beziehen sich die Vermutung (J. Ellis), dass die Scheidung von Optativ und Konjunktiv aus dem Gegensatz zwischen Gebet und primitiver Magie hervorgegangen sei, sowie die Anhaltspunkte für eine besondere Bedeutung der paarweisen Zählung bei den Indogermanen (J. Gonda).

Wie in diesen Fällen, so handelt es sich auch bei den in der sprachwissenschaftlichen Literatur aufgestellten Thesen oft mehr um anregende Gedanken und um Möglichkeiten als um gesicherte Erkenntnisse. Aus der Fülle des Vorgebrachten suche ich im Folgenden das Kennzeichnendste herauszugreifen.

(a) Kulturstufe der Indogermanen

An der Ausübung des Ackerbaues ist bei der grossen Menge alter agrarischer Termini kein Zweifel möglich. Umstritten ist aber, ob sich die Kenntnis des Ackerbaues auch auf die Vorfahren der Indoiranier erstreckte.

Wie der Ackerbau so war auch die Viehzucht den Indogermanen unzweifelhaft bekannt. Sie kann bei der formalen Gestaltung des grammatischen Geschlechtes eine Rolle gespielt haben, s.ob. (zu Prokosch und J. Ellis). Dass es sich ursprünglich um Kleinviehzucht handelte, geht aus der Bedeutungsentwicklung von **peku* "Schaf" zu "Vieh" hervor. Lat. *pecunia*, *peculium*, got. *faihu* "Geld, Vermögen" weisen auf das Vieh als Maszstab des Besitzes und als Tauschmittel hin.

Dass die Indogermanen feste Häuser kannten, zeigt die für den Hausbau entwickelte Terminologie. Damit ist aber wieder nicht gesagt, ob nicht erhebliche Unterschiede innerhalb des indogermanischen Raumes bestanden und etwa gar teilweise Nomadentum herrschte wie später noch bei den Skythen.

Die Kenntnis eines Metalles ist bezeugt durch das alte Randsprachenwort lat. *aes* "Erz, Bronze, Kupfer," got. *aiz* "Erz," ai. *áyas* "Metall, Eisen," aber es bleibt unsicher, ob damit zuerst Kupfer oder Bronze gemeint war (vgl. A. Nehring in Wiener Beiträge IV, 29f., J. Pokorny, *Indog. Wörterb.* 15, die beide die vermutete Entlehnung aus dem Namen *Alašia* = Kypros zurückweisen). — Die übrigen in grösseren Teilen des Indogermanengebietes verbreiteten Metallbezeichnungen erweisen sich als Wanderwörter durch lautliche Abweichungen, die nicht gestatten, sie auf einheitliche, "indogermanische," Grundformen zurückzuführen. Bezeichnenderweise sind sie fast alle *o*-Stämme.

Aus dem Mangel an Töpfereiausdrücken schliesst W. Brandenstein, Wiener Beiträge IV, 265 f., 273, auf eine nomadische Steppenkultur. Aber das Verschwinden von

solchen Wörtern könnte leicht durch den Wandel der Arbeitstechnik und der Gefäßformen verursacht sein.

(b) Spuren älterer Kulturstufen

Die Gruppe der heteroklitischen Substantiva, die nach ihrer Bildung und Flexionsweise den altertümlichsten Eindruck macht (womit nicht gesagt ist, dass sie je allein herrschend war: Specht, *Ursprung der Deklin.*, 6 f.), liefert eine Art Kulturbild einer sehr alten Zeit, in der der Pflanzenbau fehlte (H. Pedersen, KZ. 32, 240 ff., vgl. Nehring a.a.O. 149). Specht, *Ursprung d. Dekl.* 9-103, hat das Kulturbild auf alle "unregelmässigen" Nomina, das heisst solche, die nicht o- oder ā-Stämme sind, ausgedehnt. Die indogermanischen Ackerbauausdrücke, Nomina wie Verba, gehören grösstenteils den jüngsten Schichten des Wortschatzes an (Nehring, 149 ff., Specht, 66 ff.). Damit erscheint es als sicher, dass die Ausbildung des Ackerbaues in einer verhältnismässig späten Zeit erfolgte. Vorher haben wir eine Viehzüchterperiode anzunehmen: die Bezeichnungen der ältesten Haustiere werden in altertümlicher Weise flektiert (Specht, 32 ff.).

Nach Anhaltspunkten für eine noch ältere Pflanzensammelstufe sucht Brandenstein, a.a.O. 240 f.

Recht unsichere Überreste aus der Steinzeit sind ahd. *sahs* "Schwert, Messer" (: lat. *saxum* "Felsstück") und *hamar* "Hammer" (: ai. *āśman-* "Stein"?).

(c) Soziale Systeme

Die Familienform war die der Grossfamilie, worauf sprachlicherseits die ererbte genaue Scheidung auch bei entfernteren Verwandtschaftsgraden hindeutet. Vaterrechtliche Organisation wird bezeugt durch das Hervortreten des Familienoberhauptes (**potis* einerseits "Gatte" andererseits "Herr"; lat. *pater familias*). Die Frau wird in die Familie des Mannes gebracht: lit. *vedū* "führe" und "heirate (vom Manne)", ai. *vahate* "führt sein Weib heim," gr. *γυναικα ἀγεσθαι*, lat. *uxorem ducere*. Mit der vaterrechtlichen Ordnung hängt das Zurücktreten des Begriffes "Eltern" zusammen; Ausdrücke wie *τοκεῖς*, *γονεῖς* verraten durch den Plural statt des erwarteten Duals, dass sie sich zunächst nicht speziell auf das eine Elternpaar eines Individuums bezogen,

sondern auf die Gesamtheit der Elterngeneration in der Sippe (und dazu wohl auch noch auf die Vorfahren, vgl. Nehring, 167).

Umgekehrt will man in dem Aufkommen der grammatischen Kennzeichnung des Femininums eine Folge der grösseren wirtschaftlichen und sozialen Bedeutung der Frau als erster Pflanzenzüchterin und erster individueller Bodeneigentümerin sehen (P. W. Schmidt, *Sprachfamilien*, 534; vgl. W. Havers, *Handbuch der erkl. Syntax*, 103).

Auf Sippensiedlung deuten Etymologien von Siedlungsbezeichnungen sowie pluralische Patronymika als Siedlungsnamen (vgl. Nehring, 175 ff.).

Der Zusammenschluss von Sippenverbänden, allerdings in einzelsprachlicher Zeit, lässt sich erkennen aus Stammesbezeichnungen wie gall. *Tri-corii*, *Petru-corii* "aus 3 bzw. 4 Mannschaften bestehend," gr. *Δωρίτες τριχά-*(F)ικες, τ 177 "die aus je drei Phylen bestehen," vgl. auch lat. *tri-bu-s*. Bemerkenswert ist dabei das Hervortreten der Dreizahl, das vielleicht auf eine ererbte Organisationsform weist. Das Vorhandensein eines gemeinsamen Wortes (**teutā*) für "Gemeinde, Bürgerschaft, Volk" bei der westlichen Gruppe der Indogermanen könnte man auf einen von den übrigen Indogermanen abweichenden Aufbau der volklichen oder staatlichen Organisation beziehen.

Der Etymologie nach zu urteilen ist die wesentliche Funktion des "Königs" (**rēg-s*) nicht die Herrschaft sondern die Gewährleistung der Rechtsordnung: Er ist der, "der geraderichtet," also "der die Ordnung, das Recht herstellt," vgl. ai. *rājiṣṭha-* "der geradeste, gerechteste," air. *recht* "Gesetz," lat. *rēctus*, got. *raihts* "gerade, recht." Das zugrundeliegende Verbum "geraderichten, aufrichten" hat nur im lateinischen *regere* sekundär die Bedeutung "lenken, herrschen" angenommen.

Aus der bekannten Tatsache, dass in Ausdrücken der rechtlichen und sakralen Sphäre vielfach nur Italisch, Keltisch und Arisch zusammengehen, schliesst G. Devoto auf eine "demokratische Umwälzung" bei den zentralen Indogermanen (u.a. in *Cultura* 10, 1931, 8 ff. und Hirtfestschrift II, 535).

A. Meillet (*Introduction*⁷, 415 ff.) glaubt den Wortschatz einer sozialen Unterschicht vor allem an dem nicht ablautenden

Vokal *a* erkennen zu können. Da dieser Wortschatz aber fast nirgends mit den *e/o*-haltigen Wurzeln in etymologischem Zusammenhang steht, wird man ihn einer besonderen Komponente des Urindogermanischen zuschreiben müssen, die in ihrer lautlichen Eigenart an alteuropäische Sprachen mit überwiegendem *a*-Vokalismus gemahnt. Das gleiche Hervortreten des Vokals *a* findet sich in dem System alteuropäischer Flussnamen, das H. Krahe in einer Aufsatzreihe der *Beiträge zur Namenforschung* und in *Sprachverwandschaft im alten Europa* herausgearbeitet hat.

Umgekehrt dürfte der sozialen Oberschicht das System der indogermanischen Personennamen zugehören, das auch nach dem Bedeutungsgehalt der Namenstämme sozialgeschichtliche Schlüsse gestattet.

(d) Weltanschauliche Systeme

Nur eben erwähnt seien noch Versuche, mit sprachlichen Mitteln charakteristische Züge der indogermanischen Weltanschauung nachzuweisen, z.B. Animismus (grammatisches Geschlecht), Dämonismus (Abstrakta auf *-ya* ursprünglich Namen von dämonischen Wesen, A. Meillet, BSL. 25, 137, W. Porzig, *Namen für Satzinhalte*, 349 f.), Machtglauben (W. Havers), Gegensatz zwischen Gebet und Magie (J. Ellis, s.ob.). — Das Beiwort "Vater" des Himmelsgottes scheint auf eine nach dem Vorbild der menschlichen organisierte Götterfamilie unter dem höchsten Gott als *pater familias* zu deuten.

In weiterem Sinn gehört zu unserer Frage noch das Zahlensystem, dessen dekadischer Aufbau teilweise durch ein fremdes, vigesimales, gestört ist; von einer Einwirkung des sumerischen Sexagesimalsystems kann dagegen keine Rede sein (F. Sommer, *Zum Zahlwort*, Münch. Sb. 1950). — Zur Bedeutung der paarweisen Zählung äussert sich J. Gonda, s.ob.

TEXT OF CONTRIBUTIONS

1. Die bisher noch bestehende Ansicht, vergangene Kulturzustände und die ehemalige Geisteswelt eines Volkes können mit vollem Recht auf Grund sprachlicher Tatsachen erforscht und festgestellt werden, behält fortwährend noch mit Recht ihre volle Gültigkeit. Nach Möglichkeit muss man nur bei der Forschungsarbeit auch die Tatsachen der Geographie des Landes, wo das von uns zu erforschende Volk lebt oder gelebt hat, die Tatsachen seiner Geschichte, seiner volkstümlichen Dichtung und Literatur heranziehen. Je mehr man das tut, desto reichhaltiger wird das von uns entworfene Bild sein. Aber trotzdem müssen die Sprachmaterialien immer im Zentrum unserer Forschung bleiben; darauf müssen wir uns hauptsächlich stützen. Wir müssen bei dieser Forschung so weit vorgehen und so tief vordringen, wie unsere wissenschaftliche Methode, die allgemeine Lage des Tatsachenbestandes, das System unserer Ergebnisse, unsere Forschungserfahrung, unsere Selbstkritik und allgemeine menschliche Logik erlaubt. Gelingt es uns die ehemaligen Kulturzustände mit Hilfe der Sprache aufzuklären, so erhalten gleichzeitig auch die sprachlichen Tatsachen selber eine tiefere Beleuchtung, sie werden in das allgemeine System der Kultur- und Geisteswelt eingefügt. Dadurch werden uns gewissermassen auch gleich schon die psychologischen Zusammenhänge klar, die zwischen der Kultur und der Sprache bestehen und auf Grund deren auch viele grammatischen Formkategorien in der uns zur Erforschung vorliegenden Sprache zu erklären sind. Bei diesen Versuchen, eine psychologische Erklärung der grammatischen Tatsachen zu geben, tritt die Wichtigkeit einer rein autonomen Erforschung der Sprache besonders hell zu Tage.

E. BLESSE

2. Il problema non può esser posto se non si chiarisca prima che cosa si intende per indoeuropeo. Per chi sostiene che l'indoeuropeo non è mai esistito, la domanda non ha senso;

per chi ritiene che l'indoeuropeo si possa definire un insieme di isoglosse antiche non considerabili come prestiti, resta piena la validità di trarre dai fenomeni linguistici illazioni su fatti culturali e sociali.

Naturalmente, anche in questo secondo caso, tali illazioni non possono essere considerate allo stesso modo di quelle degli antichi cultori di paleontologia linguistica perché il metodo, specialmente negli ultimi cinquant'anni, ha subito importanti modificazioni filologizzandosi sempre più, e le esitazioni sulla legittimità e l'opportunità di porre sullo stesso piano termini di varie lingue etimologicamente identici ma attestati in epoche molto diverse si sono venute facendo sempre più gravi.

Non si può, tuttavia, negare che le conclusioni della linguistica, per quanto non assolute, siano le più attendibili anche nei confronti dell'archeologia preistorica e della paleontologia; né potrebbe essere diversamente in quanto il concetto stesso di indoeuropeo è puramente linguistico.

Se la linguistica storica ha dato degli eccellenti risultati nella valutazione di fondamentali fatti sociali e culturali degli Indoeuropei (penso soprattutto a noti lavori di Meillet, Vendryes e Devoto), non bisogna dimenticare quanto è stato fatto nella ricostruzione di interi capitoli di storia della cultura nello studio dei rapporti fra popoli indoeuropei e popoli mediterranei sul fondamento dell'indagine linguistica (e basti per tutti ricordare i lavori di V. Bertoldi).

TRISTANO BOLELLI

3. Si l'on ne dispose pas d'un texte relatif aux systèmes culturels ou sociaux, il est pour ainsi dire utopique de tirer des conclusions. Celui qui étudierait le français moderne uniquement dans une grammaire pourrait s'imaginer que l'existence des deux seuls genres masculin et féminin signifie que les Français du 20^e siècle sont animistes, c'est-à-dire qu'ils attribuent par exemple à la table, à la rue et à la plante un caractère propre aux être de sexe féminin.

Le français du 20^e siècle n'a pas d'adjectif à opposer à "ivre," alors que l'anglais oppose "sober" à "drunk";

si l'on ne disposait d'aucun texte, on pourrait se demander si les Français du 20^e siècle savent qu'un homme n'est pas toujours ivre.

Celui qui constate que le mot anglais " tank " (réservoir) a été choisi pour désigner les chars de combat, pourrait s'imaginer que les premiers chars de combat étaient faits avec d'anciens réservoirs, ou que les premiers chars servaient à " déverser " leur contenu sur le champ de bataille, alors que les textes nous apprennent que le mot " tank " a été choisi pour dérouter les espions pendant qu'on fabriquait les premiers chars de combat.

ERIC BUYSENS

4. Besides lexical evidence (which on " Wörter und Sachen " principles might not be regarded as " purely " linguistic), it is worth considering whether linguistic evidence of Indo-European institutions might be found outside lexis, in grammar, which is subject to slower change than vocabulary. Two examples will be mentioned, from gender and mood. Such possible evidence might be combined with lexical evidence, e.g. gender with family-relationship terms.

The masculine-feminine distinction has been traced back to Indo-Hittite distinction of individual and collective (neuter to what cannot be subject, and neuter plural also to collective). Prokosch suggests that this may reflect " certain general facts of cattle raising among the early Indo-Europeans," since " under agricultural conditions, the general type of domestic animal is represented by the female animal "; we may compare (lexical evidence) the etymology of *ox* (with singulative *n*-suffix) as " the sprinkler." With it may also be associated the determinative (lexico-morphological) distinction of *-bh-*, the collective or typifying, and *-r-*, the individual, in one etymology of Germanic **wība-*/IE **wīros*, namely " the typical/individual person of our tribe"; and then it may reflect not (or not only, if it also reflects a community of conception of human and animal relations) conditions of cattle-raising but the relation of the sexes in the human community.

The distinction of optative from subjunctive (additional

*-i- beside thematic vowel (cf. C (5) 2) has been attributed to the differentiation of prayer from primitive magic, i.e. of wish whose fulfilment is beyond the speaker's control from will. We may compare the Arabic use of "perfect" (i.e. non-present) aspect in prayers (cf. C (5) 2). It would remain to explain both why comparative conditions of development of religion led to such a distinction only in some language-families, and how the formal distinction was lost in some IE languages.

J. ELLIS

5. From the use of words denoting a group, a whole, a totality, from particulars in connexion with the numerical system, and especially from the meaning and function of part of the pronominals, and of the lower numerals including the words for 'half,' etc., the conclusion may be drawn that the prehistoric Indo-Europeans, like other so-called (semi-) primitive civilizations and circles, often counted and operated with pairs and other groups in cases where modern man would prefer other means of expressing himself. In this connexion the meaning of the element **sem-* is reconsidered. Neither 'eine aus mehreren Teilen zusammengefasste Einheit' (Johannes Schmidt), nor 'eins, in eins zusammen' (Walde-Pokorny) seems to be adequate. In contradistinction to **oino-*, **oiwo-*, **oiko-*, which denoted absolute unity, **sem-*, in all probability, expressed the idea of the single person or object, a complement of which exists, is supposed to exist, is present; the single person or object which forms part of a whole. From these observations and considerations some conclusions about Indo-European thought, cultural and social systems may perhaps be drawn.

J. GONDA

SECTION C

THE COMPARATIVE PHILOLOGY OF THE INDO-EUROPEAN LANGUAGES

- (5) What inferences can be drawn from the evidence of morphology and word-formation about the development of proto-Indo-European?

(Rapporteur to be announced later)

Contributions from

- | | |
|--------------------|--------------------|
| 1. R. A. CROSSLAND | 3. L. L. HAMMERICH |
| 2. J. ELLIS | 4. A. SCHERER |

THE EXPLANATION OF THE
HISTORICAL FACTS

It is the object of this work to explain the historical facts of the life of the great statesman and statesman of the world.

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TEXT OF CONTRIBUTIONS

1. The normal 3rd sg. and 3rd pl. pres. ind. med.-pass. forms of Hittite end in *-ri*, and the corresponding pret. forms end in *-t*. 3rd sg. and 3rd pl. med.-pass. forms in *-a* also occur. Some are usually listed in the grammars as pres., others as pret. (cf. J. Friedrich, *Hethitisches Elementarbuch* I, pp. 35-36). The following observations may be made on their distribution and employment.

(1) They may be divided formally into the following groups:

(i) 3rd sg. in *-C-a* corresponding to 3rd pl. in *-C-anta* : (*esa*, *esanta*).

(ii) sg. in *-C/-V-ta*, pl. in *-C/-V-anta* : *kitta*, *kianta*; *arta*, *aranta*. (Note on *kitta* : On the doubling of the *t* in normalized transcription cf. Crossland, *Transactions of the Philological Society* (1951), pp. 125-128, (v) and (vi)).

(iii) sg. in *-V-ta*, pl. in *-(V)V-nta*:

(a) from *sk*-verbs: (cf. Friedrich, op. cit., p. 33, § 154): *uiskitta*, *uiskanta*.

(b) from other verbs: *sarkutta*, *arpassatta*; *ammassanta*. (*arpassatta* and *ammassanta* are marked with the 'glossenkeil.' *ammassanta* should perhaps be read *ampassanta*).

(2) The frequency of *a*-forms from a small number of verbs shows that not all *a*-forms are the result of graphic errors.

(3) Groups (i), (ii) and (iii) (a), and some forms in (iii) (b), are confined to texts in which archaic language is to be expected.

(4) The only *a*-forms which occur in texts in which normal language is to be expected are some in group (iii) (b). Some of these are preceded by a cuneiform diacritic known as the 'glossenkeil.' These are generally held to be Luwian pret. forms. (cf. Friedrich, *Revue hittite et asianique* VIII (47); Rosenkranz, *Jahrbuch für kleinasiatische Forschung* I, p. 2). Others without the diacritic are formally identical with them.

(5) Only the forms mentioned in (4) appear to have narrative pret. value.

(6) The other *a*-forms which occur in pret. sequence express a state or a continued or repeated action in the past.

The usage of the *a*-forms, other than those which may be Luwian, in Hittite would appear to be more primitive than the employment of cognate forms as specialized secondary tense-forms in Greek and Sanskrit.

R. A. CROSSLAND

2. In order to advance our understanding of proto-Indo-European morphology, it is necessary to combine the latest discoveries about proto-Indo-European phonemic structure (signally the consonantal structure of the root, comparable with Semitic) with the most comprehensive grammatical method. It will then be possible to give an account of the Indo-European verb which will integrate the various researches of the last half-century (on tense, aspect and 'Aktionsart,' the perfect, voice and transitivity, moods, thematism, etc.).

On this basis it may be possible to show that the various categories expressed by gradation and other morphological means go back to less differentiated categories (semantically and to some extent morphologically comparable with other families, especially Hamito-Semitic). The gradation and accent of "Present" and Aorist, the thematic vowel in Subjunctive (Homeric *ῥομεν*, Vedic *āsati*) and Future (Latin *erimus*), and even the distinction of Primary and Historic endings, may go back to one distinction of immediate (temporally (aspect and/or tense) and modally) action (say **dēik-ti* 'he is pointing') and remote action (say **dik-é-t* 'he might point'), later differentiated (into all combinations between Present Indicative athematic and Injunctive of Aorist) on the principle formulated by Guillaume: "Deux termes A et B s'opposant, l'opposition qu'ils expriment tend à se répéter en chacun d'eux et le système simple A, B à se résoudre, conséquemment, en un système double, Aa et Bb, d'une part, et Ab et Ba, d'autre part, ou les termes Aa, Bb développent en eux-mêmes la nature qui leur a été initialement accordée, et les termes Ab, Ba la nature qui leur a été initialement refusée."

It would remain to relate this original distinction to that of the Perfect with *o*-gradation explained by Prokosch as gradation independent of accent with semantic function (originally physio-psychological) as in Semitic "aspects" (actually 'Aktionsarten') and other categories, and to the transitivity-distinctions which e.g. Vaillant has postulated for proto-Indo-Hittite.

J. ELLIS

3. An important distinction of IE morphology is that between thematic and athematic verbs. The main characteristic is the thematic vowel, *o* in apophonic relation to *e*, which is lacking in the athematic verbs. In the most ancient layers of IE (Homer, the Vedas, and especially Hittite), the same verb has often both thematic and athematic inflexion. There is no semantic difference between athematic and thematic verbs or forms: the thematic vowel has no function.

Parallel to this distinction is that between the *o/e* (fem. *ā*)-stems and the consonant stems of the nouns. In the oldest layers of IE, especially in Hittite, it is sometimes impossible to distinguish between vocalic and consonantal inflexion; there is no semantic difference; the thematic vowel of the nouns is likewise functionless.

IE adjectives have an early propensity for being *o*-stems, thus of presenting the thematic vowel. But in the more ancient layers of IE, especially in Hittite, instead of an attributive adjective we find not rarely an abstract noun which is governed by a genitive: 'depth of the earth' instead of 'deep earth,' 'the strength of Hector' instead of 'the strong Hector,' 'the enmity of the king' instead of 'the hostile king.' Why?

Finally, we just note that the ordinals are *o*-stems.

Years ago, inconsistencies of the IE cases, especially the seemingly meaningless relations between the suffixes of the genitive and the nominative, made eminent scholars assume that behind the IE case-system an older system could be dimly perceived, where one and the same case would correspond to both a genitive and a case of the subject (of a transitive

verb), thus yielding a natural basis of both the genitive and the (or: one) nominative of Indo-European.

The case thus described is, in a series of languages from Tibetan to Eskimo, the case of subordination. The correlative of this case is a case of superordination (the Semitic 'status constructus'). And, in fact, this is what we find in several extra-Indo-European languages, and — this is noteworthy — in both nouns and verbs, which here are not distinguished in the same way as in Indo-European.

If the old case-system breaks down, through the IE propensity for special characteristics of the subject and through a new distinction between noun and verb, then an old case of superordination will become functionless in both nouns and verbs. Is this the explanation of the thematic vowel?

We may note that in a language of elaborate subordination — superordination like Eskimo, there are no adjectives, and the ordinal is based upon the case of superordination of the cardinal: 'the third (of them)' is expressed by 'their 3.' The mark of the Eskimo case of superordination is an *a* in apophonic relation to an *i*; we should be extremely cautious in drawing any sort of inference from this curious fact.

LOUIS L. HAMMERICH

4. Die endungslosen Stämme, welche als Wortformen und in der Zusammensetzung vorgefunden werden, führen nicht auf eine Sprachstufe zurück, in der man sich durchaus mit blossen Stämmen behelft: Ebenso alt wie die Stammkomposita sind Zusammensetzungstypen mit flektiertem Vorderglied. Stammkomposition herrscht bei prädikativem (cf. Σελλοι ἀνιπτό-ποδες "die Sella — ungewaschen (sind) ihre Füße," ῥοδο-δάκτυλος "Rosen (sind) die Finger," νεο-γνός "als neu geboren."), appositionellem (ιατρό-μαντις "ein Wahrsager, der Arzt ist."), und attributivem (Der spät entstandene Typus ἄκρο-πολις scheint Fehlen der Kasusendungen und der Motion beim Adjektivum für eine ziemlich junge Zeit zu bezeugen.) Vorderglied. Das deutet auf eine Zeit, in der die Kongruenzverhältnisse noch nicht bezeichnet wurden und das Adjektivum

der Kasusendungen und der Motion entbehrte (Endungslosigkeit des generell gebrauchten Prädikatsnomens noch in: *mutabile semper femina, mentiri turpe est* (als "Neutrum" umgedeutet). Aber auch für eine frühere Zeit ist keine Periode der reinen Stämme erreichbar, denn wir haben Reste eines überdeckten älteren Formensystems (*yakr-t* : *yak-n-*, *asr-k* : *as-n-*; **me* : **moi*; *n*-Infixe beim Verbum). Überdies können die nur durch ablautende Stämme charakterisierten Formen, besonders dehnstufige, einen Endungsvokal oder -konsonanten verloren haben.

Im Gegensatz zur athematischen Flexion zeigt die thematische festen Akzent und meist zwei oder mehr Vollstufenvokale. Mir scheint deshalb die Möglichkeit erwägenswert, dass die thematische Flexion von ursprünglich unflektierten, weil adjektivischen (Die *o*-Stämme waren wohl ursprünglich Adjektiva, vgl. H. Ammann, V^{mc} Congrès intern. des Linguistes, 1939, Rép., p. 5.), Nomina agentis auf *-e/o* ausgegangen ist, z.B. **bhère* (daraus in Zusammensetzungen **-bhore/o* : *φωσ-φόρο-ς*) und **bheré* (> **bhre/o* : *δί-φορο-ς*) "tragend." Erst nach der Periode des quantitativen Ablauts bekamen sie Kasusendungen, von der athematischen und der pronominalen Deklination (vgl. den Gegensatz *δί-φίλος* : *θεο-εἰκελός*). In prädikativer Verwendung müssten sie den ursprünglichen (athematischen) Verben durch Uebernahme ihrer Personalendungen angeglichen worden sein (aber noch endungslos in Zusammensetzungen wie *φερῆ-καρπύς* und als 1. pers. sg. **bherō*, Imp. **bhère*). Dazu würde die Identität der meisten Präsensstammsuffixe mit adjektivbildenden passen, vgl. *vī-vi-t*, *jī-va-ti* "ist lebendig" (: *vī-vo-s*, *jī-vá-*), *ve-na-ti* "ist sehnsüchtig" (: *ve-ná-*).

ANTON SCHERER

The following contribution from the Director of English in the University of Michigan arrived too late to be included among the texts of CONTRIBUTIONS FOR PLENARY SESSION I: LINGUISTICS AND THE PROBLEM OF MEANING. Its proper place is on p. 12 between 4 and 5.

THE USE OF MEANING IN LINGUISTIC ANALYSIS

The belief that American linguists seek "to analyse linguistic structure without any reference to meaning" rests upon a misunderstanding. The challenging of certain uses of meaning as the basic tool of analysis does not lead to the conclusion that meaning is ignored nor that it forms no part of the framework of descriptive or structural analysis. On all levels of analysis certain types and certain features of meaning furnish a portion of the apparatus used. For clarity and understanding, as well as for rigorous procedure, it is essential that, for each level of analysis, we state, as completely as possible, the precise uses of each type of meaning that our procedures require and assume.

Basically we assume that the linguistic analyses that concern us here seek to discover and to describe the signals of linguistic meanings — that is the signals of lexical meanings and of structural meanings. We assume also that all the signals of linguistic meanings are arrangements of formal features that can be described in physical terms. The analytical procedure that seeks to discover the contrastive features that differentiate lexical forms must in some way control enough of the lexical meaning to determine whether two phonetically (physically) different forms are for the particular language "same" or "different" — whether one is "substitutable" for the other. Some control of lexical meaning is thus part of the frame in which to test the formal signals of structural meanings. The frame to test contrastive structural arrangements does require, however, a similar control of structural meanings.

CHARLES C. FRIES

PLENARY SESSION I

LINGUISTICS AND THE PROBLEM OF MEANING

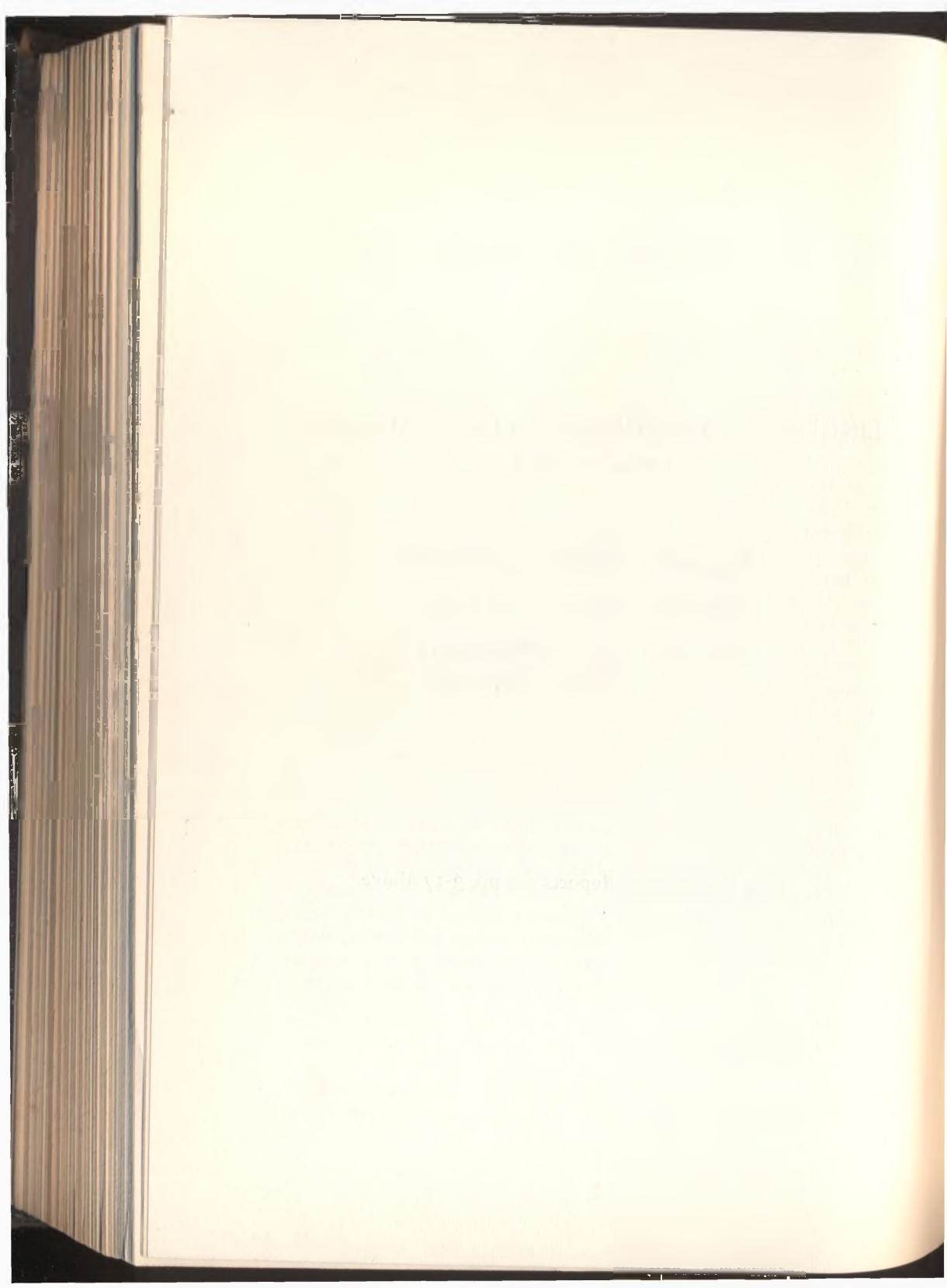
(Two Meetings)

Chairman: PROF. A. SOMMERFELT

Rapporteur: PROF. J. R. FIRTH

Recorders: MR. K. NORTHCOTT
MR. A. E. SHARP

(For Preliminary Reports see pp. 3-17 above)



PRELIMINARY REMARKS ON THE REPORT

The preliminary report (see above pp. 5-9) is intended to prepare for a general discussion on linguistics and the problem of meaning. It is not confined to a review of the contributions submitted but notices questions and problems arising from the contributions, which naturally reflect the growing interest of linguists everywhere in the main subject of the plenary sessions of this Congress.

The report also presents the main features of the contextual approach to the study of meaning, in which some of us in Great Britain have been interested for over twenty years.

In speaking to my report, may I, Mr. Chairman, submit five points; I suggest

(i) the application of the word meaning to linguistic functions at all levels.

(ii) The strictly linguistic interpretation of the term "meaning" within one world unobstructed by non-linguistic theories of minds, thoughts, concepts, ideas and their expression.

May I make the comment in passing that mathematical theories of communication are not free from such conceptualist and expressionist views, which are not regarded with favour, to say the least, by many linguists to-day.

(iii) Attention must be given to the contribution of Professor Fries which appears on page 178 above, and which reached me after my report had gone to Press, and on similar observations on the exclusion or inclusion of meaning in linguistic analysis.

(iv) We must notice certain aspects of the mathematical theory of communication mentioned in Professor Whatmough's contribution.

(v) The dangers of confusion, and the desirability of getting one-sided approaches into perspective at this Congress with a view to the rapid advance of linguistic science which our present circumstances seem to demand from us, and perhaps even promise us.

First of all, then, as is suggested in the first paragraph of my report, there would appear to be no point in trying to

discover the essence of meaning either in the intentions of humans or by analysing thought processes or setting up methods of analysing the stream of consciousness in parallel with the chain of speech. Even the psychologists themselves do that sort of thing less and less. There are many alternatives to this, and the contextual approach, while not concerning itself either with the inclusion or exclusion of "mentalist" factors, regards linguistic action as generally meaningful in the sense that the maintenance of the patterns and processes of our common lives is regarded as meaningful activity, and the systemic and structural analyses of such activity, as studies of meaning at a series of levels¹.

Secondly, the conclusion of my report emphasizes the strictly linguistic application and interpretation of the word meaning. It would follow, therefore, that linguistics would not accept the linguistic world we live in as split up or previously prepared or dissected for us by other disciplines or approaches. The linguist should go it and accept the whole context of language. He may find it very disorderly. The order he introduces should be of his own making strictly within the range of the co-ordinated disciplines and techniques of linguistics. I repeat co-ordinated. So that really, there need be no quarrel about the meaning of meaning, or disagreement about fundamentals. What we look for is agreement to co-ordinate our studies of *how* this language activity works and helps in life.

Thirdly, the question of whether certain American linguists seek to analyse linguistic structure without any reference to meaning or not depends on their technical use of the word "meaning" within the framework of their own schematic constructs of linguistics. According to the contextual or functional view of meaning, a great deal of American phonemics and certainly the glossematic approach of Professor Hjelmslev and the Copenhagen group, are studies of function or meaning at one or more levels, sometimes excluding other levels regarded as external to the main business of the linguist. However, Professor Zellig Harris in his recent book *Methods in Structural Linguistics*, employs the word "meaning" in an extraordinary way, if you care to refer to pages 166-171, and to many

¹ See J. R. Firth, "The Technique of Semantics," *Transactions of the Philological Society*, 1935, pp. 36-72.

others in all the later chapters of his otherwise stimulating book.

I quote — "a morphemic segment meaning *female*", "a morphemic segment in Arabic meaning *the*", in English, "the change *ey* — *u* in *take, took*, meaning *past*", "a morphemic segment meaning *noun*", "a segment meaning *verb*", "contrastive or *emphatic meaning*", "morphemic segment meaning *question*". On p. 195, the attempt to correlate very vague and undefinable "meaning distinctions with morphemic segmentation" is admitted.

Here, meaning of a sort is certainly not excluded. Personally I cannot help feeling that unsystematic "meanings" of this kind should be excluded.

Fourthly, I come to the Mathematical Theory of Communication and, specifically, to the work of Shannon and Weaver mentioned by Professor Whatmough in his contribution. We here in London, have a traditional interest in this and related subjects, though I cannot myself claim any professional knowledge. We have never forgotten the Bells: Melville Bell and his Visible Speech and Graham Bell and the telephone. The teaching of phonetics at University College, London, can be traced back to the Bells who taught in that College between 1865 and 1870. During and since the War, the Department of Phonetics and Linguistics at the School of Oriental and African Studies has been in touch with our counterpart of the Bell Telephone Laboratories, i.e. the Post Office Research Station at Dollis Hill, and Dr. Fry, Head of the Department of Phonetics at University College, is actively engaged in research connected with communication problems.

As Professor Whatmough suggests, there is undoubtedly a possibility of correlating "communication" theory with the contextual approach. This is especially so in connection with paragraph 5 of my report on meaning by collocation.

I have endeavoured to expound my views on this subject in an article in *Essays and Studies*, 1951, and remain convinced that the "communication" theory would benefit from a closer study of the contextualist approach, and especially of meaning at the level of collocation.

First of all, collocation and what are called stochastic processes and Markoff processes. I cannot claim to give any definition of these terms — but may be permitted to read a composite sentence based on Shannon and Weaver:

"A physical system, or a mathematical model of a system which produces such a sequence of symbols governed by a set of probabilities is known as a stochastic process, and the special case of a stochastic process in which the probabilities depend on the previous events is called a Markoff process or a Markoff chain."

Commenting on this, I would add that in the delivery of this sentence I am embarked upon, it is not only the preceding words which are determinants of what I am at this moment saying but all those which follow in time and relationship. The ancient Indians used to speculate on such problems as these — have you the seeds of everything in your opening syllables, or must we wait for the last? In my *Tongues of Men*, published in 1937, there occurs the following passage (on pp. 126-7) — "Such a (context of) situation is 'a patterned process conceived as a complex activity with internal relations between its various factors' . . . Even within the language system itself, what is said by one man in a conversation prehends what the other man has said before and will say afterwards. It even prehends negatively everything that was not said but might have been said".

This comes very near some of the theories of information and *ensembles* which the (communication) statistical mechanicians have put forward².

I am firmly convinced of the inevitability of the study of words in collocation in the light of statistics, as a linguistic method. The distribution and mutual expectancy of words in collocation is a fascinating study and I maintain a basic contribution to the study of meaning.

If this introduction could be lengthened, I would offer criticism of what I consider extremely doubtful general linguistic theory adopted by Shannon and Weaver when they leave what they call level A, which is their own technical

² "This idea that a communication system ought to try to deal with all possible messages, and that the intelligent way to try is to base design on the statistical character of the source, is surely not without significance for communication in general. Language must be designed (or developed) with a view to the totality of things that man may wish to say; but not being able to accomplish everything, it too should do as well as possible as often as possible. That is to say, it too should deal with its task statistically . . . The idea of utilizing the powerful body of theory concerning Markoff processes seems particularly promising for semantic studies, since this theory is specifically adapted to handle one of the most significant but difficult aspects of meaning, namely the influence of context." Shannon and Weaver, *The Mathematical Theory of Communication*, University of Illinois Press, 1949, pp. 116-117.

pidgin, and speculate at "level B. How precisely do the transmitted symbols convey the desired meaning? (The semantic problem)," " (C) How effectively does the received meaning affect conduct in the desired way? (The effectiveness problem)."

The general linguistics of the theory of communication seems to me to need serious re-examination. I do hope Professor Whatmough will follow up his points. I feel a great deal could be learnt especially from Dr. Weaver's article.

Finally, to refer back to my fifth point — It may be desirable to aim at a description of a language from all aspects, but this is very different from confusing the aspects, which I think is our present situation. Can we not get the various one-sided approaches into some sort of perspective, looking at them all, as we must, from the standpoint of general linguistics?

J. R. FIRTH

Comme toutes les sciences, la linguistique se trouve devant la difficulté d'avoir à analyser avec des méthodes appropriées, des parties de son objet sans perdre de vue l'ensemble; en l'espèce l'instrumentation que permet la communication entre les hommes en société.

Par ailleurs la linguistique doit prendre conscience de sa propre jeunesse comme de la jeunesse des sciences voisines, dans l'ensemble d'un développement qu'on doit considérer d'une manière optimiste comme étant à son début. En fait, si tout le fonctionnement de l'émission et de la réception du langage (qui peuvent rester intérieurs à l'individu, dans la réflexion silencieuse) est forcément conditionné par des possibilités physiologiques (physio-chimiques) extrêmement complexes, nous ignorons à peu près tout des phénomènes qui se produisent. C'est en ayant conscience de nos ignorances que nous pouvons apprécier sainement ce que nous connaissons d'une part en productions et perceptions de sons, d'autre part en actes mentaux. Dans cet esprit nous devons étudier de notre mieux les éléments du langage, en relation les uns avec les autres.

En ce qui concerne les phonèmes, il faut penser que s'ils se trouvent être dans telle ou telle langue en nombre plus ou moins grand, s'ils sont plus ou moins variés dans leurs régions

et dans leurs modes d'articulation et si leurs combinaisons sont, elles aussi, plus ou moins diverses, c'est en liaison avec des conditions très complexes. Il y intervient deux ordres de faits différents: d'une part l'exercice minutieux de divers petits muscles (dont l'acquisition demande des efforts particuliers dans la petite enfance) d'autre part des actes mentaux que peuvent se ranger sous les rubriques principales de l'attention, de la discrimination, de la mémoire.

Or ce sont les mêmes actions mentales qui, en propositions également différentes suivant les langues, interviennent dans le fonctionnement du langage en général.

On peut dire en conséquence que tout phonème est en fait inséparable du système total de la langue, avec tous ses équilibres compliqués, et qu'en somme l'ensemble de la phonologie a sa place dans l'étude des significations.

Comme chaque état de langue comporte une structure, les éléments audibles en doivent être étudiés avec leurs relations réciproques, de manière à faire apparaître les équilibres imparfaits, instables et cependant suffisants du système.

Pour les phonèmes, en petit nombre, on doit considérer non seulement le tableau plus ou moins symétrique, mais aussi les emplois dans la constitution des mots: chevauchements de phonèmes, limitations des suites et des combinaisons (longueurs de radicaux, incompatibilités), non-emploi de certain phonèmes dans les affixes: au total aspect sémantique de la phonologie.

Pour les morphèmes, de nombre sensiblement plus grand, on doit considérer comme normaux au cours de l'évolution les emplois d'une même marque à plusieurs usages et les emplois de différents morphèmes avec la même valeur, ainsi que les réemplois des disponibles pour de nouvelles fonctions.

Pour les mots, dont le nombre très grand est maintenu dans certaines limites, on considérera toutes les synonymies, y compris les métaphores complexes.

Les phrases, théoriquement en nombre indéfini, seront examinées dans leurs cadres plus ou moins fixes et dans leurs variations, en pratique restreintes.

Pour des essais d'explication, la non-connaissance des fonctionnements nerveux, surtout centraux, est un inconvénient majeur. Mais on doit prendre conscience des possibilités et des commodités, des manques et des suppléances de l'attention, du discernement, de la mémoire, de l'invention, etc.

Ainsi, et non en évoquant de soi-disant concepts innés ou des schèmes idéaux, on pourra viser à se rendre compte du fonctionnement des instrumentations complexes qui satisfont aux besoins de communication dans les cadres sociaux.

MARCEL COHEN

M. Firth a posé une question: "Are there any clear indications that the distinction *semanteme/morpheme* is helpful in the study of meaning?" (p. 6). Je voudrais répondre à cette question.

On définit souvent le sémantème comme un élément désignant une idée, et le morphème comme un élément désignant la relation existant entre les idées exprimées par les sémantèmes. Autrement dit, la classification des éléments linguistiques est basée sur une classification des éléments psychologiques. Pareille base est suspecte: elle fait songer aux anciens appels à la logique, ou plutôt à une logique inventée pour la circonstance.

L'opposition entre sémantème et morphème est née en réalité d'une opposition formelle, à savoir celle qui existe dans les langues flexionnelles entre les éléments flexionnels et le restant du mot. Si on s'en était tenu là, les choses n'auraient pas tourné mal.

Mais on a vu, par exemple, qu'en français l'article a une fonction partiellement similaire à celle de certaines terminaisons latines; et du coup l'article a été appelé morphème. On a fait de même pour les prépositions dont le rôle a été comparé à celui des désinences casuelles: la préposition a été appelé morphème.

Mais ceux qui ont procédé à cette assimilation ont confondu la forme et le sens. Tant que l'on définit le morphème comme une partie de mot, on reste sur le plan de la forme; mais dès qu'on appelle morphèmes des mots comme l'article ou la préposition, on passe à des considérations de sens: Le morphème cesse d'être une partie de mot, et devient un signe exprimant un certain sens.

Pareille transformation des conceptions serait acceptable s'il était possible de définir le sens qui est commun à tous les morphèmes et qui les oppose à tous les sémantèmes. Or cela est impossible.

D'abord, si l'on dit que le morphème désigne une relation, on s'aperçoit qu'une relation n'est pas toujours exprimée par un morphème; le substantif *relation* en est lui-même la preuve; ce n'est pas un morphème, bien qu'il exprime une relation.

D'autre part, les morphèmes n'expriment pas tous des relations; en particulier l'article.

On a aussi dit que le morphème est un outil grammatical; par exemple, le substantif est un mot qui peut remplir diverses fonctions syntaxiques.

En conclusion, l'opposition entre sémantème et morphème n'a par de valeur scientifique. La seule chose qui soit conforme aux faits, c'est l'opposition entre mot entier et parti de mot; mais il ne faut pas chercher à y associer une distinction de sens.

E. BUYSENS

Let me express first my slight disappointment at the fact that no outstanding logicians are participating in this Congress which dedicates so much time to the discussion of the problem of meaning and the relations between language and logic. In their absence, I shall try to present the viewpoint of a modern logician to the best of my abilities.

Admitting the disastrous ambiguity of the terms "semantic" and "meaning," I would like to point out that other basic terms of linguistics, amongst them "linguistical" itself, suffer from the same excessive amount of ambiguity. Few linguists, however, would like to renounce this latter term. I propose to reduce the ambiguity of "meaning," for instance, by using some qualifying adjectives like "lexical," "structural," "junctional," without implying thereby that ambiguity would be completely eliminated.

It is not any more fruitful to put the differences between American structural linguists and European linguists in terms of neglecting or recognising "meaning" as an important feeler in linguistic investigations. Structural linguists claim to have shown that much — at a certain time the claim was even "all" — of lexical meaning is exhibited by, hence replaceable by, structural meaning, whether this latter meaning is now treated in terms of substitutions or oppositions. This

is an important claim whose validity certainly poses an interesting problem. In view of the so-called "redundancy" of ordinary languages, by which meanings are expressible in more than one way, this claim sounds not unreasonable. But however this may be, heated discussions between structuralists and others are now pointless. In addition to that, some structural linguists claim that some of the methods of linguistic investigation, especially of eliciting information from native speakers, are faulty. But again this is no reason why such a thesis should cause any split between linguists.

The generalised fear of treating the dimensions of the signification relation except that between the signs (in the sense of de Saussure's "signifiant"), understandable in its time as a healthy reaction against old-fashioned mentalistic linguistics, is not overcome, and other dimensions of "semiotics" (the general theory of the signification relation) are now extensively studied in the United States.

Linguistics, as an empirical science, cannot be but based upon the behaviour of individuals and groups. If some linguists claim that psychology is outside linguistics, this may indicate two utterly different attitudes. In the one case, this thesis may be founded upon the universal scientific practice of abstracting from certain situations, refraining from excluding, at this moment, the relevance of the situations abstracted from, hence remaining completely open-minded with regard to other studies in which those situations are explicitly taken into account. This attitude is certainly a fruitful one and I have no quarrel with it. If, however, the thesis is based upon the conviction that psychology is irrelevant for linguistics, then it is a very dangerous one. What will and must happen is, then, that the linguist who overtly adheres to this thesis will practically adopt some psychological theory as the basis of his studies, and since this is done uncritically and unconsciously, usually some antiquated theory will be evolved. Whoever, as a linguist, prefers to abstract from the semantical (and psychological) dimension, may do so at his own peril. He takes upon himself nothing more than the usual scientific "calculated" risk connected with this practice. Whoever, in addition, denies the relevance of these dimensions will certainly mislead himself and probably also others.

The situation is similar with regard to the attitude toward logic and epistemology, perhaps even more obvious. No satisfactory solution of the problems connected, for instance, with the morpheme-semantic relationship can be expected without a close collaboration with modern logicians and semioticians. Let me therefore end my remarks by extending the plea of Prof. Firth for more co-operation not only to all so-called linguistic schools but even to logicians and semioticians.

Y. BAR-HILLEL

I wish to say a few words on Meaning in Linguistic Analysis. — In the contribution I ventured to submit (see above p. 12, No. 5) I took it for granted that Descriptive Linguistics cannot do without some notion of meaning or meaningfulness. Linguists trying to reduce references to Meaning to a minimum can never go so far as to eliminate such references; Zellig S. Harris, for instance, in his *Methods in Structural Linguistics* operates with "hearers' responses to utterances", which is nothing else than a particular notion of Meaning. — The question then, as Professor Fries has pointed out (p. 178 above), is not whether in linguistic studies we need refer to meaning, but only how we should do so.

I suggested that the meaning of a symbol should be regarded as a function of it: a function in linguistic environment. The main point of such a conception of meaning is that it implies a certain method of ascertaining and defining the meaning of a symbol. What that method is, that becomes clear through comparison with other approaches which are excluded. More especially, the approaches I wish to exclude as unprofitable for the purposes of linguistic analysis are all those which try to get at the meaning of a symbol by analysing it into non-linguistic facts.

Analysis of a linguistic symbol into extralingual facts: whether a physical and a psychological (Pure Form and Pure Meaning), or, if we are behaviourists, two sets of physical facts (Stimuli and Responses) — such analysis, I would suggest, is no part of linguistic analysis, no part of linguistic description. I do not wish to say that such non-linguistic interpretation of Meaning is false or useless; all I wish to maintain is that it

is of no use to Descriptive Linguistics. Instead of saying that its meaning is part of a symbol, one side, one aspect, one face, one plane of it, I suggest that we say that the meaning of a portion of speech is a function of it.

Of course, we must be more explicit: What do we mean here by "function"? — Generally, the function of a portion of speech is its distinctive occurrence in certain environments. I suggested that we should distinguish between linguistic and extralingual environment, and that, as linguists, we should confine ourselves to studying occurrence in linguistic environment, i.e. studying the occurrence of symbols amongst symbols. To the question: What is the meaning of (say) *house*? we shall answer: It is its distinctive occurrence in certain "linguistic frames," or (as I would prefer to say) in certain sentential functions such as (using x, y, z , etc. to mark positions): *This x is big., I wish to buy a y., John's z., etc.* *House* may be substituted in x, y, z , etc.; that is its meaning.

The merit of such an interpretation is clearly that our semantic statements come to be all strictly verifiable in terms of concrete linguistic facts.

Much more needs to be said; especially on the relation of the relevant sentences to one another. But there is only one further point I should like to mention, because it is essential to a functional interpretation of Meaning and is yet often overlooked. The point is this: It is not quite sufficient to say that the meaning or function of a portion of speech is its distinctive occurrence in certain environments (i.e. in what Prof. Marcel Cohen called "relations réciproques"). What there is more to be taken account of is this: That our symbol plus its linguistic environment represents another (more complex) symbol — a higher-order unit, in Prof. Whatmough's terminology.

In other words: We cannot define the meaning or function of a symbol in terms of its relations to environmental symbols, unless we remember that these syntactical relations obtain within the framework of a more complex whole, i.e. within the higher-level unit of a more complex symbol. E.g., we do not say that *house* occurs in the environment *very x*, for *very house* does not represent any higher-level unit, whereas *very good*, *very near*, etc. do. But we shall say that *house* occurs in *this very z*; for *this very house* does represent a sentence.

Thus, a twofold relation of the symbol is relevant: (1) its syntactical relation to the environment (a part-part relation, e.g. of *house* to *this very*) and (2) its relation of satisfying a higher-level function, ultimately a sentential function (i.e. a part-whole relation, e.g. of *house* to *this very house*). I have expressed this by saying that the syntactical relation obtains between "complementary positions"; *house* is complementary to *this very*, but not to *very*.

It is clear then that our functional definitions of meanings always presuppose "at some point in the process of analysis, utterances of underived meaning; and it is to sentences (the unit-utterances) of a language that we assign the part of semantic data." To determine the meaning of *house* we presuppose knowledge of meaningful utterances in which it occurs.

I argued that this need not in any way affect the autonomy and rigour of linguistic procedures. On the other hand, it clearly means that the task of the linguist is not to present an ultimate theory of Meaning (to say what it is to have Meaning). His task is to analyse complex symbols into simpler symbols, and so reduce the indefinitely numerous utterances of a language to a limited number of elements. This is linguistic analysis, as distinct from the various psychological and philosophical interpretations of speech, which (even if not compatible with one another) are all required to be compatible with the results of linguistic analysis.

W. HAAS

1. Mir scheint es unmöglich, das Studium der "Inhalte" oder "Bedeutungen" aus der Sprachwissenschaft auszuschliessen. Andererseits glaube ich freilich, dass man die Verhältnisse unnötig kompliziert auffasst und sich mit Scheinproblemen abmüht. Aus meinem Arbeitsgebiet, der Strukturanalyse einer gegebenen Sprache — des heutigen Deutsch — möchte ich einen Weg skizzieren, der auf eine möglichst objektive Weise gestattet, die rein sprachlichen Bedeutungsverhältnisse zu fassen.

2. Die Schwierigkeit ist ja die, dass man die Inhalte der Einzelzeichen und Zeichenkomplexe ("le signifié") nur selbsterleben kann; man kann sie keinem andern direkt

geben und sie bei keinem andern unmittelbar beobachten, wie man das mit den Klangbildern ("le signifiant") jederzeit tun kann. Dazu werden diese Inhalte meist gar nicht in bewusster Weise erlebt und analysiert, sondern bleiben weitgehend unbewusst und leiten nur das Endresultat in klareres Bewusstsein. Das gilt schon für das Verständnis ganzer Sätze, und es gilt umso mehr, je mehr wir uns den Wörtern als kleinsten Bestandteilen nähern. Auch wer "Sie haben ein Fest gefeiert" mühelos versteht, kann sofort und genau angeben, welche Komponenten dieses Gesamtinhaltes in "sie," in "haben," in "ein," in "Fest," in "gefeiert" stecken. Aufgabe des Linguisten ist es also, diese grossenteils unbewussten Einheiten und Beziehungen möglichst objektiv und genau ins Bewusstsein zu heben.

3. Damit man dabei nicht ein Opfer vorgefasster Begriffe und Einteilungen wird, ist es sehr nützlich, zunächst auch dem eigenen Erleben gegenüber misstrauisch zu sein und mit einer Art Experiment zu beginnen, um die Inhaltseinheiten abzugrenzen: Umstellen von Stücken, Formänderungen an Stücken, Ersatz von Stücken durch ähnliche oder entgegengesetzte — wobei man ständig die Inhalts- oder Wertänderungen beobachten muss, die sich dabei ergeben.

<i>Sie</i>	<i>haben</i>	<i>ein Fest</i>	<i>gefeiert/Ein Fest haben sie gefeiert</i>
Die Leute	hatten	dieses	organisiert
Viele	hätten	jenes	veranstaltet
Er	hat	diesen Anlass	begangen
	hätte		
Dieser	hatte	den Geburtstag	feiern wollen
usw.	usw.	usw.	usw.

Solches Experimentieren liefert den Rahmen der Einheiten, deren Inhalt zu deuten ist. Dabei sind die Experimente von jedem kontrollierbar, der der betreffenden Sprache mächtig ist. Sie müssen nämlich immer wieder zu sprachrichtigen, sinnvollen, wenn auch gegenüber dem Originalsatz mehr oder weniger stark veränderten Ausdrücken führen.

4. Erst wenn diese objektiv kontrollierbare Arbeit vollzogen ist, kann man mit gutem Gewissen daran gehen, den so gewonnenen Einheiten oder Umgrenzungen Inhalte zuzuschreiben, d.h. "bewusst zu formulieren, was jeder Sprachteilhaber beim Hören oder Sagen der betreffenden Einheit gleichartig erleben muss, damit er versteht und verstanden wird und nicht aus der Sprachgemeinschaft

herausfällt." So wird "gefeiert" nicht für jeden, der deutsch kann, völlig gleiche innere Bilder, genau sich deckende seelische Inhalte hervorrufen. Aber ein gewisser "kleinster gemeinsamer Nenner" muss da sein, auch wenn er sich nicht als exakter logischer Begriff, sondern als diffuserer "Erlebnisbereich," ja vielleicht nur als "eine vieldeutige Grundgebärde menschlichen Handelns" entpuppen sollte. Dieses Bewusstmachen der Inhalte, das Aufsuchen der "kleinsten gemeinschaftlichen Nenner," das nach erfolgter experimenteller Abgrenzung der Einheiten vorzunehmen ist, möchte ich Interpretation nennen. Die Interpretation muss aus dem eigenen Erleben des Forschers, der ja zugleich vollberechtigter Sprachteilhaber ist, gewonnen werden. Insofern ist sie subjektiv. Aber sie ruht auf objektiven Umgrenzungen, und sie kann von allen Mitforschern aus ihrem eigenen Erleben nachgeprüft und dementsprechend angenommen oder modifiziert werden. Insofern wird sie auch wieder objektiv, nämlich gerade so objektiv wie die Sprache selbst, deren Objektivität und Verbindlichkeit ja auch nicht in irgendwelcher ontologischen, metaphysischen Weise besteht, sondern nur auf der übereinstimmenden Geltung für eine bestimmte Sprachgemeinschaft.

So glaube ich einen gangbaren Weg zu sehen, der die Inhaltsseite der Sprache, dies an sich nur von jedem für sich erlebbare "signifié," oder eben die Bedeutung, auch in genügendem Masse objektiv fassbar, kontrollierbar und damit erst wissenschaftlich diskutierbar macht.

(Für genauere Ausführung dieser Gedanken verweise ich auf mein Buch "*Die innere Form des Deutschen, Eine neue deutsche Grammatik*," Bern 1952; vor allem Kap. 4 des "vorbereitenden Teils," S. 46-58).

H. GLINZ

Die von meinen Vorrednern berührten Probleme betreffen wenigstens teilweise, Fragen, die ich selbst in meinem Bericht über die "Wortklassen" (see pp. 254 ff. below) erwähnen wollte. Ich möchte mir erlauben, auf zwei prinzipielle Punkte gleich hier zu Beginn hinzuweisen.

Ich will diese Bemerkungen in meiner Muttersprache beginnen und werde dann in englischer Sprache fortfahren.

Der erste Punkt betrifft die Terminologie in der heutigen Linguistik. Ich glaube, dass wohl viele mit mir übereinstimmen werden, wenn ich sage, dass die Vielfalt und Vieldeutigkeit der Terminologien verschiedener Schulen eine äusserst gefährdrohende Situation bedeutet, dass dieser Zustand es dem einzelnen Linguisten nahezu unmöglich macht, jeweils im einzelnen Fall die Bedeutung eines Terminus zu verifizieren. So ist z.B. das Wort "meaning," von dem bereits Ogden und Richards etwa dreizehn verschiedene Bedeutungsschattierungen feststellen konnten, seit jener Zeit an Vieldeutigkeit noch reicher geworden. Gewöhnlich war "meaning" doch im Sinne von lexikaler Bedeutung eines Wortes oder im Sinne von syntaktischen Funktionen gebraucht worden; andere oder abgelegenere Bedeutungsvarianten sollten unbedingt definiert werden, ehe die betreffenden Probleme besprochen werden. Eine möglichst klare oder klar definierte Terminologie gehört m.E. zu den Voraussetzungen wissenschaftlicher Linguistik.

Apart from Terminology, another important point is the position of "psychology" in the linguistic sphere. If language "is a purely human and non-instinctive method of communicating ideas, emotions, and desires by means of a system of voluntarily produced symbols" (i.e. Sapir's definition which I fully endorse), then psychological aspects cannot be excluded from linguistic interests. This aversion to psychology is due — so it seems to me — to several reasons which I must sum up very succinctly — (a) to behaviourism, on the one hand, which tries to describe external stimuli and responses; (b) to phenomenology, on the other hand, which tries to hypostasise "meanings" by placing them in the cloudless sky of Platonic ideas; (c) an aversion to psychology may partly be due to the influence of the "logistic" school which uses "syntax" (i.e. group-form) for its operations and excludes word-sense. Yet Carnap himself clearly saw the difference between such logical calculi and normal every-day language. I quote in German (from his introduction to *Logische Syntax*): "die Wortsprache ist nicht bloss ein Kalkül (i.e. a pure group-form), sondern ihre Wörter haben Bedeutung; das wird von der Semasiologie betrachtet . . ."; (d) we often find, moreover, an almost hopeless confusion of the terms "psychology," "logic," and "ontology." Now, logic is or ought to be a normative science, ontology is interested in

problems of ultimate realities. Psychology, on the other hand, falls into two branches: the descriptive one has to describe and to analyse our mental phenomena, and as far as these mental phenomena are part of our linguistic symbols they are not extralinguistic. Genetic psychology has to inquire into the coming and going of these phenomena.

(e) Aversion to psychology may perhaps even be attributed to a misconception of de Saussure himself who proclaimed the necessary autonomy of linguistics and of his dangerously ambiguous dictum: Language is "une forme, non une substance."

If taken literally, i.e. "form" understood as external or sound-form, this dictum would make nonsense; pure form, thus understood, would also be the sound-form of a language unintelligible to the listener. Yet de Saussure certainly understood by form something else, namely "sound-form + thought-form"; otherwise he could never have asserted so emphatically the two-sidedness of the linguistic unit or word: the "signifiant" + the "signifié." And de Saussure's conception of "langue" is purely psychological: according to him it is the store-house of sound-forms + meanings, and their various associative structures, a complex network with its similarities and oppositions, resting in the memories of the speech-community. Such a collective of engrams consists not only of sound-structures, not only of relations, but also of sense-values without which relations are impossible. It is therefore not pure chance that the Geneva School (Sechehaye, Bally) strongly stresses psychological aspects, that e.g. Bally, in the first part of his *Linguistique générale et linguistique française* investigates the fundamental psychic structures. It is rather superfluous to mention names of other scholars (e.g. H. Paul, Wegener, Schuchardt, Bréal, Brunot, Sweet, Noreen, Wackernagel, Jespersen, Gardiner, Spitzer, Ullmann etc.) who cultivated and cultivate the psychological side of linguistic science.

The Danish School, as far as I understand their intentions, distinguishes, in analogy to de Saussure's "signifiant" + "signifié," between "expression" and "contenu" (content); yet they understand by "contenu" "syntactic valences" and would leave word-sense (lexical or class-meaning) out of consideration. How or whether this is possible I should like to discuss in my report on "word-classes."

The American Structuralists, on the other hand, intend programmatically to abandon semantics altogether; they demand to keep only to formal aspects, those of group-form and word-form. Here again the question arises whether the whole semantic aspect can be excluded altogether when attempting to describe a language-system. We shall see that they introduce semantic elements in a hidden way, an attitude which I might name (according to a suggestion of my friend Prof. Wrenn) "crypto-semantics."

O. FUNKE

The purpose of this contribution is to report on a method of semantic investigation whose chief merit may be that it circumvents certain divisions and problems customary in our discipline, and in particular the division between synchronics and diachronics.

The method in question is related to that of the "semantic field" but represents something of a departure in that it lays particular stress on the "embryological" aspect, that is on the formation of the group. The problem is to select a field where the need can be clearly defined and also where the available material is copious enough to approximate to everyday speech conditions.

Difficulties of interpretation and definition of the field to be investigated are clearly minimised, though by no means abolished, by choosing a group which refers to a material innovation.

These and similar considerations led to a specimen investigation being carried out into the formation of a technical terminology during the Industrial Revolution, specifically that resulting from the introduction of railways into France, with the object of showing in detail how urgent, large-scale terminological needs were met in a rapidly-growing (but not ephemeral) technique exempt from the systematic reform as scientific terminologies were not.

The conclusions of this investigation seem relevant to more than one of the points raised here to-day. A striking feature is the polysynonymy of the initial period — a dozen or more terms, covering the full range of metaphor, calque and borrowing, were suggested to designate new objects such as rails or

locomotives and moreover for reasons more or less directly traceable to industrial conditions under the Restoration, most of these terms continue in use side by side for 10 or 20 years.

The evolution of a standard terminology from this profusion, the succession of approximate solutions of their inter-action, the exploration of this state of polysynonymy to express distinctions which are not all retained — all these vicissitudes add a historical dimension to conclusions based on the characteristics of the surviving forms.

These aspects may be considered as partly psychological and partly sociological, that is, as partly the semantics of creation and partly the semantics of normalisation, the two of course overlapping. In particular the semantics of creation must include the adaption to existing patterns, — the process, by no means instantaneous, by which the meaning of new terms is established in limiting cases — a process which we may call the establishment of semantic frontiers, and which applies to concrete objects as much as say to a Christian concept of the Old English period.

One example — puzzle for the conceptual cartographers — the group around bridge/viaduct/tunnel.

P. J. WEXLER

ON THE DESCRIPTIVE METHOD IN SEMANTICS

The synchronistic approach which has revolutionized most branches of twentieth-century linguistics has so far found scant acceptance in semantics. The reason is not far to seek: the vocabulary is not systematized in the same way as phonemes or grammatical elements, and seems at first sight ill suited to synthetic treatment. Modern semantics has evolved two techniques to meet this difficulty. First, it has begun to investigate the structure of closely organized sectors of the vocabulary: this is the programme of the theory of "semantic fields." Secondly, it has worked out several criteria for ascertaining the semantic features and tendencies characteristic of a given language; some of these have been enumerated in my contribution to the *Preliminary Reports* (see above, p. 15 f.).

The precision of our criteria varies according to the nature of the problems involved. Most definite among them are those based on tangible devices: the formal means employed by language to convey emotive nuances or to distinguish between homonyms and between different meanings of the same word. Next come certain structural tendencies amenable to statistical formulation, such as the relative frequency of homonyms in a given system. Thirdly, there are some broad trends which cannot be exactly determined but which stand out none the less with great clarity: such is for example the dosage of arbitrary and motivated words and of concrete and abstract, particular and generic meanings. Some of these tendencies are highly complex: they result from the interplay of several different forces which may not all work in the same direction. This is particularly noticeable in the sphere of motivation where the most diverse factors are involved: onomatopoeia, metaphor, derivation, composition, "popular" etymology and so on. Finally, it should be borne in mind that all our criteria are essentially relative: the characteristic features of a language can only be established by comparing it to other languages and other stages in its own history. It has often been stated, for example, that German, with its precision of verbal and adverbial shades, is highly concrete as opposed to the abstract character of French; yet even German will appear abstract if set against the over-concrete vocabulary of American-Indian or Polynesian tribes.

The prime purpose of our criteria is to determine the semantic peculiarities of a given language; other systems will only be adduced as a foil, to set them off by contrast. This is the method applied with much success by Bally, in his structural comparison between French and German. But the same criteria may also serve a wider purpose: they may bring to light certain structural affinities between languages, which may or may not be connected with their historical affinities. The position of English between French and German is particularly instructive in this respect. On nearly every point, French and German represent the two opposite poles of semantic structure, and English oscillates characteristically between the two. It shares a number of fundamental features with French. Like French, it often conveys by a simple, unmotivated word what German expresses by means

of a transparent compound: *thimble* - *dě* - *Fingerhut*; *glove* - *gant* - *Handschuh*. Like French, it is given to the use of loan-words in preference to native derivatives. Synonyms in both languages are organized on the same pattern, though the English arrangement is more complex: native versus learned in French, native versus French and Graeco-Latin in English. Both languages are rich in monosyllables and consequently in homonyms; both have retained an archaic mode of spelling as a precarious safeguard against homonymic clashes. On the remaining points, however, English is closer to the German language-type. There is even one minor feature which French and German have in common whereas English differs from both: in the former, gender serves occasionally to distinguish between homonyms or different meanings of one word, while English, like the Finno-Ugrian languages, is deprived of this safeguard.

Although the method outlined above is essentially synchronistic, it is perhaps in the diachronic field that its applications are most fruitful. Comparisons between different stages in the history of a language are instructive in two ways: they show the emergence of new features, the passage from one type of structure to another, and they also reveal the historical factors which brought about the changes. Thus, the structural affinities between French and English seem to be due mainly to the parallel effects of two great influences: far-reaching phonetic erosion on the one hand, and the influx of foreign elements on the other. Between them, these two heterogeneous forces account for all the points of structural affinity noted above.

A combination of our criteria will also help us towards the solution of what is perhaps the central problem of semantics: the autonomy of the word and its greater or lesser dependence on context, verbal and non-verbal. Here again, the contrast between German and French is complete: the French word, generic and unmotivated, exposed to semantic and homonymic ambiguities, is more dependent on contextual determination than the relatively self-sufficient German word. English occupies once more a middle position between the two. This state of affairs has close parallels in other sectors of the language-system. There are, in particular, obvious connections between motivation and synthetic structure on the one hand,

arbitrariness and analytic structure on the other. In this way, our rudimentary semantic typology merges into a wider linguistic typology in which some of the traditional criteria of classification are harmoniously blended with the new insights of contemporary research.

STEPHEN ULLMANN

I have no prepared paper, and I am taking part in the discussion only because of the references made by Professor Firth to my contribution, and to what has come to be known as "information" theory. As to that it is important to observe, in the first place, that "information" does not mean *information* in the ordinary sense. I well remember sitting in my College rooms in the spring of the first year (1926-27) that I lived in the United States and, on answering a knock at the door, being confronted by a telegraph boy with the question "did I want to send a telegram?" "Why should I want to send a telegram?" I asked; "Because to-morrow is Mother's Day." "But" I objected "if I did for that reason, my mother would think I had taken leave of my senses." Such a telegram, had it been sent (at a trifling cost) would have been coded in a very few digits since its "information" would be low, i.e. the "bits" would conform to a standard (but short) code. In fact, I venture to suggest, in the second place, that *conformation* is what the communication engineers mean when they say "information." They are concerned with the statistical regularity of the pattern in messages, i.e. the degree of conformation to the probable sequence of order in the stream of speech.

Professor Firth is quite right when he speaks of content of meaning as determined by collocation of items in their contextual environments. In fact the determinacy can be plotted on log-paper, rising for example to 100% in the termination *-ly* in English adverbs, which might well be, and often is, omitted (e.g. *Go slow*; cf. in German, adverb and adjective identical in form). If this method is to be developed, it would be well to adopt Trier's notion of the field, since certain semantic values, especially in specialized discourse (of technical language, for example; or of particular subject-matter), recur in contexts in which again the determinacy is high. In this way the first

steps in developing a suitable technique may perhaps be worked out. In any utterance of $1 \dots n$ units the choices and order must necessarily be governed by meaning as well as by structure. Mandelbrot's theorem sets up a formula for encoding and decoding (and this is certainly concerned with meaning), viz:

$$pn = P(n+m)^{-B}$$

the interpretation of which in historical terms is simply the maintenance of semantic equilibrium, i.e. selective variation in meaning. The formula has to do also with relative frequencies of occurrence, which also are to be taken into account in the attempts that are now being made to devise a machine for translation (at least of vocabulary-items).

J. WHATMOUGH

COMMENT AT THE END OF THE FIRST MEETING

I should like, Mr. Chairman, to read a few lines from Dr. Weaver's article in confirmation of what Professor Whatmough has been saying:

"Fourthly, it is hard to believe that levels B and C do not have much to learn from, and do not have the approach to their problems usefully oriented by, the development in this theory of the entropic ideas in relation to the concept of information.

"The concept of information developed in this theory at first seems disappointing and bizarre — disappointing because it has nothing to do with meaning, and bizarre because it deals not with a single message but rather with the statistical character of a whole ensemble of messages, bizarre also because in these statistical terms the two words 'information' and 'uncertainty' find themselves to be partners.

"I think, however, that these should be only temporary reactions; and that one should say, at the end, that this analysis has so penetratingly cleared the air that one is now, perhaps for the first time, ready for a real theory of meaning."

While commending the above quotation to the attention of the meeting, you will appreciate, in the light of my previous remarks, that a reconsideration of the two uses of the word 'meaning' in the quotation is desirable.

J. R. FIRTH

SECOND MEETING

M. Vendryès présente quelques observations sur la distinction du sémantème et du morphème, qu'il ramène à la distinction du vocabulaire et de la grammaire. Dans tout énoncé, il faut distinguer les éléments mêmes de l'énoncé (les notions sur lesquelles il porte) et la façon dont ces éléments sont répartis à l'intérieur de la phrase suivant les rapports qu'ils ont entre eux. Les rapports sont variables d'une langue à l'autre, mais ce qui varie surtout, c'est la manière de les exprimer. Cela se fait tantôt ou au moyen de désinences (comme en latin), ou d'outils grammaticaux (comme les prépositions du français), tantôt par des alternances vocaliques ou consonantiques dans le sémantème, par des variations de l'intonation du sémantème ou enfin par l'ordre des mots. Les procédés sont équivalents et dans bien des cas ils se recouvrent exactement. Si l'on décide de réserver le nom de morphème à l'un d'entre eux, c'est par une décision arbitraire, et qui ne doit pas faire perdre de vue les autres. Le domaine du sémantème et celui du morphème n'en sont pas moins dans tous les cas nettement délimités. Il y a toutefois des liens entre les deux: on observe une tendance générale qui consiste à créer des morphèmes en vidant certains sémantèmes de leur sens concret pour leur conférer une valeur abstraite d'outil grammatical. Toutes les langues offrent des exemples de cette tendance à un degré d'évolution plus ou moins avancée. En conclusion, la distinction du sémantème et du morphème doit être étudiée à part dans chaque langue, d'après les principes d'une morphologie générale, qui embrasse tous les procédés en usage pour exprimer les rapports entre les notions.

(J. VENDRYÈS)

Je pense que nous arriverons plus facilement à voir clair dans le problème de la signification si nous considérons séparément les divers aspects de ce problème. Je n'en considérerai que deux.

Prenons d'abord la signification de la phrase; j'entends sa signification littérale, abstraction faite des sous-entendus et autres associations qui peuvent l'accompagner.

Cette signification comprend deux parties que j'appellerai la modalité et la substance. Pour faire ressortir cette distinction il suffit de comparer les quatre phrases que voici: 1. Tu réussiras; 2. Réussiras-tu? 3. Réussis! 4. Puisses-tu réussir! Leur substance est la même: il s'agit quatre fois de réussite future de mon interlocuteur; mais la modalité est tour à tour assertive, interrogative, impérative et optative.

Les logiciens ont beaucoup étudiée la substance, mais ils ont négligé la modalité. Et partout c'est la modalité qui constitue l'élément le plus caractéristiquement linguistique. En effet elle révèle le genre d'action que nous désirons exercer sur notre interlocuteur: nous l'informons, nous l'interrogeons, nous le commandons ou nous le prenons à témoin de notre désir.

En faisant passer la modalité à l'avant-plan, nous faisons ressortir le caractère social de la langue en général et de la signification en particulière. La langue est un moyen d'agir sur l'interlocuteur, et la signification (modalité et substance réunies) d'une phrase et le genre d'action que nous voulons exercer sur l'interlocuteur. La réaction de l'interlocuteur est le critérium qui nous permet de vérifier s'il a compris la signification.

Je rejoins donc les behavioristes en faisant du comportement social la base de la définition de la signification.

Je passe maintenant au sens du signe. Et cela nous transporte dans un tout autre climat. Chacun sait qu'en morcelant une phrase en signes, nous détruisons la signification de la phrase, tout comme nous détruisons la vie d'un animal en le disséquant: Pour commencer l'intonation disparaît, et vous connaissez son importance primordiale. Ensuite tous les rapports syntactiques sont détruits. La conséquence de tout cela, c'est que la somme des sens des signes d'une phrase ne suffit pas à reconstituer la signification de cette phrase. Il est donc évident que le sens d'un signe ne peut pas être défini sociologiquement comme la signification de la phrase. Mais je puis baser la définition du sens du signe sur la définition de la signification de la phrase.

Je définis le sens du signe comme une partie de la signification de la phrase, à savoir la partie qui correspond au signe. Mais cette définition est un peu tautologique. Pour éviter ce défaut je définirai le signe et vous verrez que la définition du sens y est donnée implicitement.

Le signe est le plus petit élément de phrase qui à la fois pour la forme et la signification permet deux opérations complémentaires: (1) opposer deux phrases par ailleurs semblables; (2) rapprocher deux phrases par ailleurs dissemblables.

En résumé, je définis la signification de la phrase par son rôle social, et le sens du signe par sa fonction dans la phrase.

Ma théorie n'exclut pas la psychologie; au contraire la psychologie est l'hypothèse indispensable pour expliquer le comportement. Mais j'évite de recourir à des classifications psychologiques inventées par la circonstance.

E. BUYSENS

SUR DEUX PRINCIPES SAUSSURIENS

La théorie saussurienne du signe linguistique est évoquée dans le rapport préliminaire sur le problème de la signification, au paragraphe I (b), haut de la page 6. L'objet de cette communication est de montrer qu'elle offre des ressources sérieuses pour son étude, en la complétant par un second principe, emprunté à la même source.

I (1). Tout le monde connaît cette théorie, mais il n'est peut-être pas inutile d'insister sur sa valeur, explicative et heuristique, dans toutes les parties de la science du langage. C'est sous une autre forme ce que Leonard Bloomfield appelle, dans son ouvrage aujourd'hui tout à fait classique, "the fundamental assumption of linguistics . . . in certain communities, some speech utterances are alike in form and meaning" (*Language* 2e édition 1935, réimpr. 1950, p. 144). C'est donc aussi le postulat qui sert de base à la phonologie, sous le nom de "Relevanzprinzip" (en français "principe de pertinence") formulé d'abord par Karl Bühler (*Phonetik und Phonologie, Travaux du cercle linguistique de Prague* IV, 1931, pp. 22-53).

I (2). Le fait que certains traits des phonèmes ne sont pas pertinents ne constitue pas une entorse au principe. Car à moins de se réduire à des accidents contingents, ils conservent une valeur diacritique, sur des plans autres que le représentatif, suivant divers usages normalisés, servant à caractériser le sujet ou à solliciter l'interlocuteur, ainsi que l'expose Trubetzkoy dans ses *Grundzüge*. L'axiome fondamental reste donc valable par application de l'*Organonmodell* de Karl Bühler

qui conditionne cette vérification et en tire une espèce de nécessité. On entrevoit ici une déduction axiomatique des propriétés du langage, sur le terrain de la signification.

I (3). Dans le même ordre d'idées le caractère discursif ou articulé du langage humain, également posé par Karl Bühler comme son trait le plus fondamental, a été rattaché de façon irréfutable à la dualité saussurienne du signe, par le professeur A. Martinet dans son lumineux article *La double articulation linguistique* (*Travaux du Cercle linguistique de Copenhague* V, 1949 pp. 30-37). En ce cas aussi K. Bühler ne pouvait prévoir la démonstrative de Martinet.

I (4). Nombre de faits s'éclairent dans la théorie saussurienne, comme l'édification de la grammaire comparée sur les lois uniquement phonétiques, l'expression étant la moitié du signe, et la plus accessible à l'observation; comme aussi la revendication surprenante de la phonologie à représenter toute la linguistique, qui se comprend du moment qu'en réintégrant la considération des signifiés dans l'examen des signes, elle peut prétendre saisir le fait linguistique dans sa totalité.

I (5). Une théorie aussi féconde ne peut manquer de permettre d'autres découvertes. Il y a sans doute des conséquences à tirer du "lien indissoluble" entre signifiant et signifié, pour le parallélisme des deux plans, et la théorie de l'isomorphisme, si intéressante et contestée (exemples tirés, en dehors de L'Ecole de Copenhague, de Bally, Cantineau, Haudricourt). Il suffit ici de noter que le principe de dualité exige d'accorder à l'étude des signifiés une importance égale à celle des signifiants et à la revendiquer au même titre comme un des objets naturels de la linguistique, en accord avec la conclusion du rapporteur, p. 9.

II (1). Des difficultés surgissent tenant à la nature du problème, qui appelle l'intervention de techniques et d'interprétations psychologiques. Par ex. le rapport préliminaire à la page 5 confronte les interprétations "mentalistes" et "mécanistes," dont les points de vue sont exposés plus en détail par Bloomfield dans l'ouvrage précité, en particulier page VII et 32-33.

II (2). Un exemple caractéristique est la polémique esquissée par W. Porzig (*Das Wunder der Sprache*, Bern 1950, chap. III pp. 92 et 94) contre la linguistique américaine, accusée de

“behaviorisme négateur de la conscience psychologique” auquel il oppose le langage comme preuve de l'existence de la conscience et de l'esprit. Les deux attitudes supposées par cette discussion semblent justiciables des objections de Karl Bühler (*ibid.*) : “Psychologismus” défini comme “Psychologie am unrechten Ort,” ou *μυτάβασις ἐς ἄλλο γένος* (d'après Husserl).

II (3). Mais ces critiques contre la linguistique américaine sont-elles justifiées? Il faut insister d'autant plus sur leur examen que l'exposé oral a dû être écourté à partir de ce qui suit, et que les critiques sont restées sans contrepartie. Quand on se reporte aux sources, comme par exemple à l'ouvrage de Bloomfield déjà cité, on trouve expressément spécifié que les interprétations psychologiques n'ont pas d'importance pour le linguiste, et que la choix de l'une ou de l'autre procède d'exigences de méthode.

II (4). Il est incontestable que les linguistes américains qui partagent avec ceux de Prague la gloire de la découverte du phonème, et peuvent même se prévaloir d'une priorité reconnue par Trubetzkoy en 1933 (*Journal de Psychologie* XXX p. 230), ont les meilleures raisons pour soumettre à une critique sévère la notion de signification, devenue indispensable à la méthode. Et si certains d'entre eux, dans un esprit de rigueur scientifique, s'efforcent d'éliminer de leurs formules tous les termes sans définition précise, ils méritent la même gratitude que les mathématiciens inventeurs de métagéométries, ou de logiques non-aristoteliennes, pour le profit durable de la science.

II (5). Le problème n'est donc nullement d'apprécier défavorablement certains ordres de recherche, mais de faciliter la collaboration entre des savants de disciplines différentes. Il est évident que l'étude d'un aspect mental du langage conduit à consulter les spécialistes des faits mentaux, les psychologues. Mais comment concevoir la collaboration avec eux? C'est pour resoudre des problèmes de ce genre que F. de Saussure a trouvé sa fameuse distinction entre “langue” et “parole.”

Ce n'est pas la première fois que semblables difficultés embarrassent les linguistes. La théorie de la langue a eu pour fonction d'y porter remède. Il est permis de penser qu'elle y est encore propre.

III (1). Une étude minutieuse de Kristen Møller (*Travaux du cercle linguistique de Copenhague* V 1949 pp. 87-94) confirmée par une autre plus ancienne de Doroszewski (*Journal de Psychologie* XXX p. 82) établit que l'on trouve dans le *Cours* deux interprétations, l'une sociologique, l'autre structurale sans compter toutes celles qu'ont introduites les linguistes qui l'ont adopté. Mais le *Cours* indique tout à fait clairement, comme le souligne Doroszewski, les intentions méthodologiques de Saussure, préoccupé d'assigner un objet à la linguistique.

III (2). C'est à des motifs semblables qu'obéit Trubetzkoy, lorsqu'il rattache à la même théorie la distinction entre la phonologie et la phonétique, au lieu de recourir à la définition du projet de terminologie: "phonétique fonctionnelle." Il se propose d'assigner un objet à la nouvelle science, et adopte la même solution que son prédécesseur.

III (3). Si cette analyse est exacte il suffit de généraliser et d'étendre à d'autres sciences ce que Trubetzkoy dit de la phonétique, le précédent de Trubetzkoy devrait faire autant pour les phonologues. Si la doctrine est bien saussurienne, elle devrait convaincre les linguistes de Copenhague qui sont des disciples orthodoxe du maître.

III (4). Peut-on espérer, pour cette seconde théorie saussurienne, un accueil sans objection? Il est bien entendu qu'il s'agit uniquement de classification des sciences et de terminologie. N'est-ce pas Saussure qui donne l'exemple des inconséquences nécessaires, en préconisant au chap. IV du *Cours*, une "linguistique de la parole," après avoir déclaré que la langue est l'unique objet de la linguistique. La phonétique est-elle étrangère à la linguistique, depuis que certains la considèrent comme une linguistique de la parole? Il n'y a pas de raison que le laboratoire de psychologie ne devienne un jour aussi indispensable au linguiste que celui de phonétique.

Toutes les difficultés ne disparaissent pas pour autant: la première serait de choisir entre les interprétations de la "langue."

IV (1). Sans vouloir diminuer l'intérêt de l'interprétation structurale, on insiste ici sur celle qui oppose chez K. Bühler (*Sprachtheorie*, Jena 1933, p. 48) les notions de *Sprechhandlung* (chez Humboldt: *energeia*) "activité de langage," et *Sprachwerk* (*ergon* de Humboldt), "opus operatum," comme plus facile à saisir et à utiliser comme criterium de méthode.

IV (2). Une autre difficulté semble surgir à propos de la logique, dont le rapport à la psychologie est aussi celui d'un *ergon*, en face d'une *energeia*, et aussi d'une structure, en face d'un déroulement phénoménologique. Qu'en doit-on penser? identité de la logique et de la langue (remontant au même *logos*!), ou parallélisme, la psychologie des significations étant à la langue ce que la psychologie tout court est à la logique, en sorte qu'il y aurait un domaine spécial de psychologie linguistique?

IV (3). On voit que l'application de l'axiomatique saussurienne au problème des significations soulève immédiatement quantité de problèmes, et de plusieurs manières. Il serait regrettable de la voir tomber dans l'oubli.

J. LECERF

SHANNON'S SO-CALLED INFORMATION THEORY

The view that it is possible to predict the future state of a language (e.g. to predict the total extinction of the class of strong verbs in English by about the year 3000) was advanced recently by Professor Whatmough in the *Scientific American*. In support of this view, Professor Whatmough invoked the so-called information theory of C. S. Shannon.

With regard to this theory, one of us (F. Whitehead) is of the opinion that Shannon's approach to linguistics is very naïve; the other (W. Mays) takes the view that it has naïve physicalist implications, a not uncommon thing nowadays with theories propounded by those interested in automatic electronic devices. Since Professor Whatmough has raised the subject of "information theory," we may perhaps be permitted some comments upon it.

The significant thing for Professor Whatmough is that "information theory" permits of "functional definitions" in terms of transition probabilities. Thus, in the phrase, "Oh, my offence is rank; it smells to heaven," the occurrence of the word *rank* is determined by the context, that is to say, using Shannon's terminology, by the prior occurrence of "Oh, my offence" (S_1) and by the fact that S_1 is linked to S_2 (the word *rank*) by a transition probability $P(S_1S_2)$. But what determines this transition probability? According to us

(and indeed to Shannon himself if his treatment is properly analysed), the fact is that S_1 really means "the whole play of *Hamlet* up to and including the word *is* in the opening line of Claudius' soliloquy." The probability that S_1 will be followed by S_2 is simply the probability that a quotation from *Hamlet*, once begun, will be finished and finished in an accurate way. "Determination by context" thus means determination not by the rest of a linguistic utterance whose total length is nine words, but by the whole situation out of which the sentence arises. "The whole situation" will of course include our belief in the existence of a man called Shakespeare, who enjoyed a consciousness not dissimilar from our own and whose consciousness, while writing this play, was haunted by images of corruption and decay, especially of the decay of an unheathily exuberant vegetation.

Context here must be taken in the widest sense; consequently the statement that "rank" is highly probable in this context is simply a statement about the intentions of the creator or the user of this phrase — consequently a statement about the probable occurrence of a behaviour pattern (or let us say frankly a psychological state).

Psychological states are now out of fashion, yet it is impossible for a linguist to avoid either overt or concealed reference to them.

The "transition probabilities" that figure in Shannon's theory are only of significance if language is an ergodic process (i.e. one in which every sequence has the same statistical properties). Shannon's conclusion that language is in fact an ergodic process seems based entirely on a somewhat simple-minded study of the orthographical structure of English. Semantic considerations are simply left out of account. When these are brought in, it can be shown that the statement that language is an ergodic process is clearly false.

Transition probabilities have in any case no significance in an open system — i.e. one that comprises an indefinite number of states. A language is such an open system; it has to symbolize not merely a conceptual world, but a conceptual world in process of growth and change. Shannon models language on a logical calculus, arising from a number of fixed elements and postulates, settled once and for all in advance, and operating according to combinatorial laws. Only by assuming

this model is it possible to assert that we can make predictions about the evolution of a language; such predictions become wholly impossible when it is realized that the evolution of a language depends in large measure on behavioural situations which are hidden in a future which is yet to be born.

Those who believe that the Shannon theory offers us the possibility of an autonomous linguistics, in no way tributary to psychology, ought to be warned that the model for this theory is provided by the transition probabilities of sub-atomic physics and physical chemistry. Its contribution to linguistics seems to be to infect the latter with naïve physicalism.

F. WHITEHEAD

W. MAYS

In the discussion of Dr. Whitehead there are several statements that are not true. I do not think that Shannon himself has made any of the questionable applications of Information Theory to Linguistics of which he was accused by Dr. Whitehead though others may have done this.

The explanation given by Dr. Whitehead for ergodicity is incorrect, as any mathematician versed in this field will immediately recognize. It is, however, impossible to go into details before this forum.

Though Prof. Whatmough will certainly be able to defend himself against the criticisms of Mr. Whitehead, I would like to point out that his predictions for the development of the English language were beyond doubt meant to be valid only conditionally, on the condition, namely, that no factor unknown at the moment will come into play. And though I would not subscribe blindly to his predictions, it is in full accordance with best scientific procedure to make such predictions.

Y. BAR-HILLEL

So far as Dr. Whitehead's persiflage may be taken for argument, it would seem that he has misunderstood my point — that there is a degree of determinacy in the meaning (or meanings) which any two (or more than two) utterances

containing a given term exhibit — not necessarily quotations from Shakespeare and Burns. This it ought not to be necessary to say.

As for a formula (Shannon's) concerned with a function of time and a function of frequency, the fact that the utterance is decoded as readily as it is encoded indicates clearly its applicability to language; it is not simply a matter of electrical transmission. Moreover, graphematic expression is as valid, and in its own right, as phonematic, account being taken of transitional probabilities as well as of permitted sequences. It will not do to say "anything may happen," for neologisms regularly follow the existing pattern. And if historically a pattern is disrupted, in time a new pattern is established. The Shannon formula simply guarantees a linguistic status; nothing that I have printed or said about it implies more than that in my interpretation: to suppose so is a naïve misunderstanding.

In effect the whole procedure of structural analysis rests upon the same assumption of a closed system, or status.

Professor Firth rightly insists on the distinction between linguistic structure and symbolism. This is shown, for example, in Navya Nyāya logic, in which e.g. a tautology of contraposition (symbolic) is expressed by two compounds of normal Sanskrit structure.

J. WHATMOUGH

The question of the application of the so-called information theory of Shannon and Wiener to the analysis of language, which has here excited such spirited discussion, is a very technical one. Since, for a complete understanding of information theory, a broader mathematical background is necessary than most of us possess, and since, moreover, information theory is probably of very limited utility for the average linguist, it would seem unprofitable here to go very deeply into the matter, even if time permitted, and the subject were not so far removed from the central question under discussion in these sessions, which is, after all, "Linguistics and the Problem of Meaning." Nevertheless, since the issue has been raised, it is important that we be not left without some understanding of what information theory is all about. I am, I regret, unable

to recall any clear and direct statement from any of the partisans in this discussion as to what information theory is, and I suspect that it is for this very reason that I have been unable to particularize my vague feeling that the introduction of this question here and the disagreement upon it which we have seen are both due to some general misapprehension, shared by both camps, as to the nature of this all but esoteric doctrine.

Shannon and Wiener's information theory is a general statement about the quantity of information represented by the indication of one choice out of a finite number of possibilities, where the probability of any given choice is determinant. The exact mathematical expression which forms this statement and the unit of informational quantity which results from it need not concern us. Suffice it to say that, in accordance with our common sense expectation, the selection of a choice of low probability out of a large number of possibilities represents a greater quantity of information than the indication of a likely choice from a small number of possibilities. If someone tells you essentially what you have been expecting to hear, then you have received very little information, if any.

Given this device for quantifying information, we are able to regard any communication reducible to a sequence of such choices as representing a flux of information. As it happens, moreover, from this concept many interesting ideas can be developed; for instance, an information flux seems to have properties analogous to those of a flux of energy, and we can recognize in any transformation of information a sort of entropy. These results are certainly of interest to us, agreeing as they do with what we should expect from the broad concept of semiosis as a functional process subordinate to what we may call (in the terms of A. N. Whitehead's organic philosophy) the general process of an extensive functional universe.

Now, to what extent does language conform to the conditions of information theory, i.e. to what extent do linguistic events involve a choice from among a finite number of possibilities? On the phonological level, i.e. on the level of the second articulation (cf. A. Martinet: "La Double Articulation linguistique" *TCLC* V) it is clear that we are now dealing with precisely the situation that information theory describes: in each of a sequence of positions we must choose one of a

limited number of phonemes. Certainly, therefore, we can apply information theory to the qualification of certain data on this level. Whether it is useful for us to do so, whether we are in ordinary linguistic description concerned at all with the quantity of information and with information flux, is another question. Above all, though, the mistake must not be made of assuming that information theory gives us some means of phonological analysis; information theory cannot be applied at all until phonological analysis is completed.

The situation for the quantification of information is different, however, on the level of the primary articulation of language: it is only in a certain sense that we can say that the vocabulary of any language is finite, and it is obvious that the number of possible global references implicit in the potential sign complexes of a given language are unlimited. We have here to do, not with the selection of one of a number of possibilities, but with the presentation of a form which derives its actuality from its formal immediacy in the percipient experience, as well as by its opposition to an indeterminate number of other possibilities. Consequently, we are not dealing with information in the information theory sense, and we cannot quantify our data on the same basis, except in the terms of a calculus which fictionalizes the situation to meet its own purposes — the result, in any event, cannot be determinate in the same sense as on the phonological level.

In no way can information theory, useful as it may be in the design of communication circuits, reveal to us the formal structure of a language on either of its articulated levels. No statistical method alone can yield structural data. The inadequacy of any analytic method which considers linguistic manifestations without respect to their governing norms has been demonstrated e.g. by the famous experiment of Zwirner with vocalic length in German.

Now, there is a second question which has been raised by two different speakers: is a language an axiomatic ("closed") system, similar in its characteristics to a mathematical system? This question is, in fact, in no way related to the issue of information theory, since information theory is indifferent to whether we have an axiomatic or non-axiomatic system or no system at all (it can apply to isolated events). However, along with the assumptions that the entire structure of a

language must be ordered in the same fashion, the identification of the properties of languages with those of mathematical systems is responsible for no small part of the fruitless discussions upon which linguists have lavished their time. The one notion has resulted in efforts to demonstrate e.g. that *boy/girl* and *night/day* must represent valid ordered relations comparable to *look/looked* or *man/men*. The other has led to fantastic misunderstandings about the problems of translation and the construction of electro-mechanical models of linguistic structures.

Our difficulties on these points may be cleared up, I believe, by two considerations:—

(1) Languages cannot be completely dominated by order without losing their value as tools of communication. Linguistic events do not represent the repetition of perfectly regular patterns, but manifest transient novel characteristics which present the varying meaning content of speech. This novelty is, indeed, correlate in some way with the external order of "reality" itself, but only through the direct prehension of extra-linguistic facts and in no way clearly deducible from the linguistic data alone. There is order in language, to be sure, and it is this order which we as linguists wish to describe, but we must never forget that the purpose of this order is to serve as a background for the constantly varying and often disorganized patterns of reference which we project upon it.

(2) Languages and axiomatic systems such as mathematics make contrary assumptions about reality: languages presuppose an infinite empirical universe of unlimited variety; mathematics, though accepting anomalous propositions involving infinity, basically assumes the finiteness of its universe. It is a matter of taste whether infinity or limitation is regarded as paradoxical, and mathematicians make the latter choice when they refer to the observation of J. Richard as the "Richard Paradox." Richard pointed out in 1905 the mathematical principle that only a countable number of finite expressions can result from a finite set of strictly defined symbols subject to a finite number of operations. In other words, the number of references of any perfect axiomatic system is limited. Languages will tolerate no limitation; there is no theoretical end to what we can say in any language. Languages achieve their utility by departing in every way from the standards of axiomatic

perfection: they accept an unlimited number of signs in their vocabularies, they permit a certain convenient indefiniteness in the semantic spectra of some of these signs (or in certain parts of their semantic spectra), and they permit broad departures from the syntagmatic norms which define the possible operations.

The task of linguistics is to take language for what it is: a reflexion of something external, appearing as disorder or variation, upon the surface of an internal order. It is this internal order which is the structure of a language.

As a conclusion to these remarks, I might suggest that it would be amusing to consider one of the possibilities which would exist if I were mistaken in the things which I have just pointed out. Suppose that we could, in fact, see a determinate structure represented by the relative frequencies of occurrence of the various units and sequences of units in linguistic texts. We could then construct a tremendous tabulating machine which would record every word and sequence of words — no matter how long — typed into it. I am sure that the International Business Machines Corporation could assist us with this project and that the total cost would not exceed that of other large scale scientific investigations which we have seen in our time. Into our great machine we could feed the texts of all the novels, for instance, which are available in the modern English language, and the machine would digest these data, giving us tabulated relative frequencies of all words and sequences of words. All of these data could then be conveniently transferred to another machine which would synthesize from it an "original" novel constructed on the principle of the highest degree of probability. This machine would automatically have at its disposal from its inputs the most probable length of a novel in English, the most probable opening and closing words, and the most probable words and sequences of words to come in between.

I suspect that a novel so written would have great popular success and would permit an early liquidation of the initial capital investment in machinery. Indeed, there would certainly be at least one enthusiastic reviewer to cry "Burn all the novels, for their worth is collected in this book!" — But would it be? I shall leave you to ponder the question after one brief reminder of the significance of my earlier remarks. We all

know well that it is as impossible to quantify the worth of a book as it is to assign a numerical value to virtue, but in the terms of information theory, our machine-written novel, being the most probable of all English novels, the arch-type, as it were, of the English novel, would be by definition the one conveying the minimal quantity of information.

You will, I am sure, share with me in my gratitude that this monstrous project would be a complete impossibility for the theoretical reasons which I have already cited.

D. G. STUART

I. RECHTFERTIGUNG

1. Das Thema dieser Vollsitzung lautet "Linguistics and the Problem of Meaning." Wenn sich jetzt zu diesem Thema ein Terminologe äussern will — und gar einer, der naturwissenschaftlich ausgebildet ist —, so bedarf das einer Begründung, ja Rechtfertigung. Denn sehr viele von Ihnen werden der Meinung sein, dass die Fachsprachen, also z.B. die Nomenklaturen der Mathematiker oder der Maschinenbauer, die Sprachwissenschaft, auch die eigentliche Linguistik, überhaupt nichts angehen.

In der Tat scheint es auf den ersten Blick einleuchtend, dass Fachsprachen nichts sprachwissenschaftlich Interessantes enthalten, was nicht der Art nach schon in der Gemeinsprache mitenthalten und schon dort erforscht ist. Für sehr viele sprachliche Probleme ist das zweifellos auch richtig. Z. B. wüsste ich beim besten Willen nicht, was etwa eine "Phonetik der Mathematiker" für einen Sinn haben sollte. Und doch werde ich zu zeigen suchen, dass die Fachsprachen und die sich in ihrem Bereich immer stärker entwickelnde Sprachtechnik grundsätzliche Besonderheiten gerade bezüglich des Wesens und der Entwicklung der Bedeutung aufweisen, die so erheblich sind, dass sie im Gesamtbild der Sprache nicht vernachlässigt werden dürfen.

Andere Personen wieder erkennen wohl die besondere Artung der Fachsprachen ohne weiteres an. Aber sie tun das gleich wieder so gründlich, dass sie die Fachsprachen als künstliche Gebilde erklären, die für das echte Sprachleben überhaupt nicht kennzeichnend sind. Auch dieser antipodische Einwand mag einmal berechtigt gewesen sein. Nämlich in einer Zeit,

als die Fachausdrücke, wenigstens die wissenschaftlichen, ausschliesslich aus griechisch-lateinischen Elementen gebildet wurden und das Reservat einer sehr dünnen Schicht der Bevölkerung waren.

Heute aber ist jeder von uns Zeuge, wie im Laufe von wenigen Jahrzehnten Hunderttausende von naturwissenschaftlichen und besonders technischen Fachausdrücken von Wissenschaftlern gebildet werden, die bald darauf täglich vom werktätigen Volk verwendet werden und die daher für den sprachlichen Alltag mindestens ebenso kennzeichnend sind wie diejenigen Teile der Sprache, die man früher allein als natürlich gelten lassen wollte.

2. DIE BESONDERHEITEN DER FACHSPRACHEN

Welches sind nun die Besonderheiten der Fachsprachen?

2 (1). Der Begriff. — Alle Besonderheiten der Fachsprachen folgen aus einer einzigen Grundtatsache: Das Um und Auf der Fachsprache, zugleich ihr Grundelement, ist der Begriff.

Diese Vorherrschaft, ja Alleinherrschaft des Begriffes hat dreierlei Aspekte:

(i) Die Bedeutung ist es, auf die es in der Fachsprache fast allein ankommt. Das gedankliche Jenseits der Sprache ist für sie unvergleichlich wichtiger als der mit den Sinnen wahrnehmbare akustische oder optische Körper des sprachlichen Ausdruckes. Das "Problem of Meaning" ist daher das Zentralproblem der Fachsprachen.

(ii) Aber nicht jede Bedeutung — d.h. nicht jede Vorstellung, die sich an eine Äusserung knüpfen kann —, ist für die Fachsprache wichtig: nur der Begriff ist es. Also nur die abstrahierte und feststehende, die verstandesmässig erfassbare Allgemeinvorstellung. Bei der Übermittlung der gedachten Begriffe an den Gesprächspartner ist die Schönheit des Ausdruckes belanglos gegenüber den beiden Gütepolen "Genauigkeit" und "Bequemlichkeit." Gefühlswerte entfallen ganz. Fachsprache ist nur Begriffssprache. Fachsprache ist nur Zwecksprache. Selbst Mitteilungen, die infolge grober Missachtung der Grammatik, auch der Phonetik, gemeinsprachlich nur als Radebrechen zu werten wären, können terminologisch noch vollwertig sein.

(iii) Begriffe sind feststehende Gedankenelemente. So wie in der Fachsprache die ganze Bedeutung einer Äusserung in Begriffe aufgelöst wird, so sind für sie auch im sprachlichen Ausdruck die Elemente das wichtigste, d.h. die einzelnen Wörter, ja oft sogar ausserdem die blossen Affixe. Die terminologische Betrachtungsweise löst eben die Sprache auf in "Termini" oder "Benennungen."

Zusammenfassend: Die "Bedeutung der Bedeutung" schrumpft für den Terminologen auf einen kleinen Bruchteil desjenigen Bedeutungsumfanges zusammen, den ihr die allgemeine Sprachwissenschaft zuschreiben muss.

Damit nicht genug: Der verbleibende Bedeutungskern, also der "Begriff des Begriffes," ist weit mehr verdichtet, d.h. dem Widerstreit der persönlichen Auffassungen weit mehr entrückt als die gesamtsprachliche "Bedeutung." Darum wird ein unbefangener Terminologe die Erkenntnis von der Bedeutung der Bedeutung durch eine blosser Verbaldefinition nicht für sonderlich gefördert erachten.

Vielmehr wird er seine tägliche Anschauung vom Problem der Bedeutung vor allem dadurch wiederzugeben suchen, dass er beschreibt, auf welche Art und Weise, und zwar durch welche bewusste Denk- und Willensvorgänge in den Fachsprachen unaufhörlich neue Begriffe gebildet, zu Systemen geordnet, wieder umgestaltet und auf allen Entwicklungsstufen mit Namen belegt werden.

Denn die Vorherrschaft des Begriffes in den Fachsprachen ist gleichbedeutend damit, dass alle Triebkräfte, die die Entwicklung der Gemeinsprache bedingen, in der Fachsprache gebändigt und zurückgedrängt werden durch den Verstand, durch bewusste Konstruktion.

2 (2). Die Sprachnormung. — Auch bei der Entwicklung der Gemeinsprache sprechen Einflüsse des Verstandes mit, wenn auch nur in bescheidenem Masse neben übermächtigen anderen Einflüssen. Aber eine Betätigungsweise des Verstandes in der Sprachentwicklung ist auf die Fachsprache allein beschränkt, und auch dort ist diese Betätigung erst seit einigen Jahrzehnten in grösserem Massstab zu beobachten. Das ist die sogenannte Sprachnormung. Sie besteht darin, dass sich die massgeblichsten Vertreter eines Fachgebietes zusammentun, um nach sehr gründlichen, mehrjährigen Verhandlungen terminologische Vereinbarungen zu treffen.

Das Erstaunliche, für den fernstehenden Sprachwissenschaftler fast Unglaubliche an diesem Vorgang ist, dass diese Vereinbarungen nicht etwa auf dem Papier stehen bleiben, sondern dass auf diesem Wege heute die ganze lebende Berufssprache mancher Fachgebiete, z.B. der Elektrotechnik zentral gestaltet und weiterentwickelt wird. Hunderttausende von Benennungen und Begriffen dringen so auf dem Weg über die Hoch- und Fachschulen, über Firmenkataloge und über das Schrifttum bis in die Kreise der Arbeiter, und viele davon werden ein Bestandteil der Gemeinsprache.

Bei diesen Vereinbarungen handelt es sich nur zu einem kleinen Teil um die blosse Ausmerzung von Synonymen. In sehr vielen Fällen werden die Begriffe überhaupt erst geprägt (oder gemodelt) und in ein System gebracht. Auch die entsprechenden Benennungen werden neugebildet oder nach einer bestimmten Systematik umgeformt. Die Bausteine für diese Benennungen werden heute meist der Gemeinsprache entnommen.

Organisatoren dieser umfangreichen terminologischen Arbeiten sind die sogenannten "Normenausschüsse." In jedem grösseren Land gibt es einen solchen Ausschuss, und dieser richtet seinerseits eine grosse Anzahl von Fachausschüssen ein. So umfasst die Geschäftsstelle der britischen Normungsorganisation rund 300 Angestellte, während die Zahl der ehrenamtlichen Sachverständigen, überwiegend Industriefirmen, vielleicht 20 mal so gross ist. Der grösste Teil dieser Personen arbeitet allerdings nicht ausschliesslich an der terminologischen Normung, sondern diese ihre Tätigkeit überschneidet sich täglich mit der Normung materieller Erzeugnisse.

In Österreich wird zur Zeit im Auftrage der Unesco eine Bibliographie einsprachiger Terminologien und Fachwörterbücher ausgearbeitet, die in etwa einem halben Jahr erscheinen soll. In dieser Bibliographie werden vor allem die von den nationalen Normenausschüssen aller Länder veröffentlichten Terminologien verzeichnet sein. Die Zahl dieser vereinbarten Terminologien wird voraussichtlich nicht weit unter 1000 liegen.

2 (3). Die internationale Sprachnormung. — Die Vorherrschaft des Begriffes in der Fachsprache einerseits, die nationale Sprachnormung andererseits, haben einen sprachlichen

Vorgang möglich gemacht, der in der Gemeinsprache als Utopie verlacht werden würde: die internationale Angleichung der nationalen Begriffe, und Begriffssysteme, auf dem Wege demokratischer Vereinbarungen. Dieser Angleichungsvorgang ist heute auf manchen Fachgebieten in vollem Gange.

Die internationale Sprachnormung beginnt in jedem Einzelfall mit einem erschöpfenden partiellen Sprachvergleich. Es wird keinen Linguisten überraschen zu hören, dass die Hauptarbeit dabei nicht etwa im Vergleichen der Benennungen besteht, sondern im Vergleichen und Abgrenzen der Bedeutungen und Bedeutungsnetze. Diese Arbeit ist hochinteressant. Wenn es nach jahrelanger Arbeit zur internationalen Koordinierung einer bestimmten Begriffsgruppe kommt, dann werden oft an Stelle der bisherigen divergierenden nationalen Begriffe neue geprägt, von deren Überlegenheit die Vertreter aller Länder sich im Laufe der Verhandlungen überzeugt haben. Solche neue internationale Begriffs- und Benennungssysteme sind z.B. für die Arten der "Keile," der "Wälzlager" (Kugel- und Rollenlager) und der "Passungen" (auf französisch "ajustements," auf englisch "fits") geschaffen worden. Die internationale Angleichung der Begriffe wäre an sich möglich ohne gleichzeitige Änderung der nationalen Benennungen. Um aber Verwechslungen zwischen den modifizierten neuen Begriffen und den früheren zu verhüten, werden nach Tunlichkeit gleichzeitig auch die Benennungen modifiziert. Diese Gelegenheit wird dann sehr oft dazu benutzt, auch gleich die nationalen Benennungen aneinander anzugleichen.

Diese Angleichung der Benennungen betrifft aber nur zum kleinsten Teil die äussere Form, also etwa lateinisch-griechische Kunstwörter, die man gewöhnlich "Fremdwörter" oder "gelehrte Wörter" nennt. Vielmehr wird in den meisten Fällen die innere oder semantische Form angeglichen. Das Ergebnis sind dann also sanktionierte "Lehnübersetzungen" (wörtliche Übersetzungen). Mit anderen Worten: Auch bei diesem Angleichungsvorgang werden Bedeutungen aneinander angeglichen, jedoch nur Primärbedeutungen, die ihrerseits erst Träger, Symbol der effektiven Bedeutung sind. Das bedeutendste abgeschlossene Werk der internationalen Sprachnormung ist das 6-sprachige Definitionswörterbuch der Internationalen Elektrotechnischen Kommission (IEC). Es ist nach 30-jähriger Zusammenarbeit der führenden Fachleute

der Welt im Jahre 1938 erschienen. In ihm sind die 2000 häufigsten von den etwa 60000 Begriffen der Elektrotechnik verarbeitet. Durch Supplementbände ist die Zahl der durch dieses Werk erfassten Sprachen seither auf 13 erhöht worden.

Internationale Sprachnormung wird heute auch schon auf vielen anderen Fachgebieten betrieben. Um alle diese Arbeiten zu koordinieren, hat die Internationale Normungsorganisation (ISO, Genf) im Anfang d. J. ein technisches Komitee ISO/TC 37 "Terminologie" eingesetzt. Diesem Komitee gehören 19 Länder an. Im Entwurf des Arbeitsprogrammes stehen z.B. folgende Aufgaben:

Grundsätze für die Bildung von Benennungen, unabhängig von der einzelnen Lautsprache (innere Sprachform).

Grundsätze für die Bildung internationaler Fachausdrücke (äussere Sprachform).

Organisatorische Verfahrensregeln für die internationale Sprachnormung.

Einen kurzen Überblick über die organisatorische Entwicklung der internationalen Sprachnormung gibt mein Aufsatz "The Coming Concentration of International Terminology Work," der soeben in der *Revue de la Documentation* (Brüssel) erschienen ist.

3. DIE BESONDERHEITEN DER TERMINOLOGISCHEN LEXIKOGRAPHIE

Die grundsätzlichen Besonderheiten der Fachsprachen, zusammengefasst in der Vorherrschaft des Begriffes, bedingen auch Besonderheiten in der Gestaltung der Fachwörterbücher. Auch diese Fragen stehen im Arbeitsprogramm des Technischen Komitees ISO/TC 37 "Terminologie."

Über diese Dinge wird aber erst in derjenigen Sektionssitzung zu berichten sein, die den Begriffswörterbüchern gewidmet ist (Sektion A5).

EUGEN WÜSTER

Es ist sicherlich kein Zufall, dass die beiden Begriffe "Bedeutung" und "Innere Sprachform" in der modernen Sprachwissenschaft erneut zur Diskussion stehen: Beide Begriffe beziehen sich auf etwas, das der "Innenseite" des sprachlichen

Geschehens angehört, und dermassen den natürlichen Ausgangspunkt vieler psychologischer und philosophischer Erörterungen sprachlicher Probleme bildet, während andererseits die Sprachwissenschaft selbst, auch wenn sie sich bewusst auf die Behandlung der äusseren Form beschränken will, doch immer wieder zu diesem Gegenpol ihrer Forschungsobjekte Stellung beziehen muss.

Leider sind beide Begriffe — ich verwende hier den landläufigen Ausdruck der Logik, ohne auf seine Berechtigung einzugehen — überaus schwer zu fassen, ja man kann fast behaupten, dass es so viele Begriffsbestimmungen dieser Ausdrücke gibt, als Forscher dazu Stellung genommen haben. Immerhin sind die beiden Begriffe in dieser Hinsicht nicht völlig gleichartig: Während nämlich der Begriff der Bedeutung ohne Zweifel von jedem, der sich mit sprachlichen Dingen auseinanderzusetzen hat, verstanden wird, ohne dass er freilich eine völlig befriedigende Definition geben könnte, müsste man bei der sogenannten "Inneren Sprachform" gleich hinzusetzen, welche Begriffsbestimmung man dabei ins Auge fasst, da eigentlich seit W. von Humboldt über Steinthal, Marty, Porzig, Wundt, Weisgerber und Ipsen, um nur einige Forscher zu nennen, jeweils ganz verschiedene Erscheinungen mit der Bezeichnung "Innere Sprachform" belegt wurden.

Da ich nun nicht beabsichtige, das Problem der Begriffsbestimmung von der formal-logischen Seite anzugehen — wir müssen dies wohl mit einiger Besorgnis den Philosophen überlassen — will ich auch in der Folge keineswegs Definitionen der Bedeutung oder gar der Inneren Form versuchen: Ich möchte lediglich hier feststellen, dass bezüglich des Begriffes "Bedeutung" selbst die Formallogiker beträchtliche Schwierigkeiten zu überwinden haben werden, da eine Definition *per genus proximum* sich ebenso wie bei vielen anderen Dingen des psychischen Geschehens als unmöglich erweisen wird, da eben ein solcher Begriff sich ebensowenig wie etwa für die Sprache selbst finden lassen wird. Die innere Sprachform vollends kann nicht definiert werden, solange wir keine sichere Vorstellung haben, was wir darunter verstehen wollen: Humboldt's eigene Beschreibung ist so wenig klar fassbar, dass eine Begriffsbestimmung in seinem Sinne zwar philologisch möglich, praktisch aber völlig wertlos wäre.

Ich will es im folgenden unternehmen, auf einige Tatsachen hinzuweisen, die sich bezüglich der Bedeutung aus der

praktischen Sprachforschung ergeben, und will versuchen, mit Hilfe dieser Beobachtungen zu einer brauchbaren Auffassung bezüglich der "Inneren Sprachform" oder wie ich es eher nennen möchte, der "Inneren Sprachstruktur" zu kommen.

Im Sinne Humboldt's verstehe ich im folgenden die Sprache in ihrer primären Leistung als das Umschaffen der Welt in das Eigentum des Geistes, wobei die kleinste Leistungseinheit des Sprachaktes (d.i. des potentiellen Sprechaktes!) die Ausgliederung eines erlebten Sinnverhaltes ist. Sinn oder Sinnverhalt soll aber jeder einzelne subjektiv und bewusst erlebte Sachverhalt sein. Es vollzieht sich wohl bereits im Bereiche des Erlebens selbst eine Ausgliederung oder Segmentierung, doch ist dieser rein psychologische Vorgang ein Selektivverfahren in Bezug auf den Sachverhalt, der also vor-gedanklich und demgemäss auch vorsprachlich ist.

Die Ausgliederung des Sinnverhaltes aber, die zugleich ein Eingliedern in den geistigen Besitz des Individuums ist, ist dessen Zerlegung in Sinneselemente, welche wir Bedeutung nennen. Das Erlebnis, der Sinnverhalt einer blühenden Blume wird in diesem Ausgliederungsakt in die Bedeutung "Blume" und "blühen" zerlegt, es wird in dem Sinnverhalt ein Sinnelement gefunden, das ich auf frühere erlebte Sinninhalte beziehe, aus denen ich auf diesem Wege ein gemeinsames Element gewinne, eben die Bedeutung "Blume." Ebenso verfare ich natürlich mit dem anderen Sinnelement "blühen," wobei ich allerdings bei dieser Bedeutung bereits zu wesentlich vageren Ergebnissen kommen werde. Diese ständig sich vollziehende Aufgliederung von Sinnverhalten mit Reduzierung der Sinnelemente schafft uns eine "Fülle" von Bedeutungen, die sich, wie die äussere Sprachform zeigt, sogar als ein System bezeichnen lassen. Allerdings muss schon an dieser Stelle darauf hingewiesen werden, dass infolge dieser Art der normalen Schaffung von Bedeutungen der Grad der Genauigkeit sehr unterschiedlich bleiben muss: Sinneselemente, die oft in Sinnverhalten auftreten, sind durch die Vielzahl der Ausgliederungskombinationen bestimmter, während solche Bedeutungen, die nur selten in Sinnverhalten erscheinen, sehr unbestimmt bleiben und oft geradezu falsch ausgegliedert werden: Beispiele solcher falscher Ausgliederungen und demgemäss falscher Bedeutungen liefern einerseits die Kindersprachen, bei denen die Zahl der Sinnverhaltenswiederholungen

vielfach noch zu gering ist, andererseits begegnen wir ihnen auf Schritt und Tritt beim Gebrauch fremder Sprachen (map - card, ale - beer etc.). Aber auch gelehrte Fremdwörter, die ja doch durch unsere Presse immer mehr in den Alltag eindringen, liefern schöne Beispiele dieser Art (russ. Problem — "etwas, das nicht in Ordnung ist" etc.).

Es hängt aber auch die Klarheit der Bedeutung nicht nur von der Häufigkeit allein ab: eine massgebliche Rolle spielt auch die zentrale oder periphere Stellung des Elementes im Sinnverhalt. So kann es leicht vorkommen, dass Elemente, die immer nur in Verbindung mit anderen wichtigeren Elementen erlebt werden, nie in das Zentrum der Ausgliederung gerückt erscheinen und so immer blass bleiben. Andererseits kann man bewusst einzelne Elemente in das Zentrum der Ausgliederung stellen, indem man in einem Sinnverhalt alle anderen Elemente nach Tunlichkeit neutralisiert: Die häufigste Form dieser Neutralisierung ist die Sprachform: Das ist ein A. Eine — wenn auch meist verborgene Kette solcher neutraler Ausgliederungen ist das, was wir in der Logik als Definition oder Beschreibung bezeichnen.

Diese Elemente nun, die mit mehr oder weniger grossen Variationsbreiten bestimmt sind, erscheinen nun den konstitutiven Elementen der äusseren Sprachform, also den sprachlichen Zeichen akustischer oder visueller Natur koordiniert: Man kann bis zu einem gewissen Grade aus praktischen Gründen die äussere Sprachform in eine Reihe von einzelnen Strukturebenen aufgliedern und etwa die Ebenen der Phoneme, der Morpheme, der Semanteme, Tagmeme und Syntagmeme unterscheiden, und es ist vielleicht sogar möglich, gewissen Aussagen über die Beziehungen zwischen diesen einzelnen Strukturebenen zu machen, ähnlich, wie man, um de Saussure's Schachfigurenbeispiel heranzuziehen, über die einzelnen Schachfiguren eine ganze Reihe von Aussagen machen kann: Der Läufer ist grösser als der Bauer, Die Dame kleiner als der König, und der Turm dicker als die Dame usw. Aber erst in dem Augenblick kann ich wirklich von sprachlichen Elementen bzw. von Elementen des Schachspiels sprechen, wo ich die innere Seite in meine Betrachtung einbeziehe, d.i. wo ich bei der Sprache und im Schachspiel den Wert oder die "Bedeutung" der betreffenden Gebilde mitberücksichtige. Ich erinnere in dieser Beziehung besonders an die Polemik von Ingrid Dal gegen die Phonemdefinition Trubetzkoy's, in der

die Bedenken einer Strukturalistik ohne Bedeutung klar zum Ausdruck kommen. Ich möchte daher das bekannte Gleichnis von dem Bündel von Strukturebenen dahingehend modifizieren, dass ich die Sprache als ein Bündel von solchen Ebenen auffasse, die sich alle in der Achse der Bedeutung schneiden. Dieses Bild hat insofern auch Bedeutung als man zeigen kann, dass fast alle Beziehungen zwischen den Strukturebenen nicht untereinander direkt gegeben sind, sondern nur über die Achse der Bedeutung feststellbar erscheinen.

Eine der einfachsten Phänomene dieser Art ist die Transponierung einer Erscheinung einer bestimmten Ebene in eine andere Ebene, wie sie sowohl in einer Sprache als auch bei der Übertragung von einer Sprache in die andere Sprache immer wieder vorkommen: So kann etwa eine morphematisch ausgedrückte Beziehung — Deminutiv, Zeitform des Verbums, Aktionsart — in derselben Sprache oder aber in einer anderen Sprache durch eine tagmatische Erscheinung ausgedrückt werden, ein Semantem kann durch ein Syntagmem wiedergegeben werden; immer aber ist dabei die entscheidende Achse, um die sich diese Transformierung bewegt, Bedeutung.

Ich muss hier die Bemerkung einschieben, dass ich mir natürlich des Unterschiedes voll bewusst bin, der zwischen den Begriffen Bedeutung und Bezeichnung besteht, dass ich es aber in dieser kurzen und schematischen Darstellung aus Gründen der Einfachheit vorziehe, beide Phänomene, die eine Art Zweiweg-System bilden, unter dem Ausdruck Bedeutung zusammen zu fassen.

Wie eine Untersuchung der strukturalen Aufgliederung der äusseren Sprachform zeigt, beschränkt sich die Bedeutung nicht bloss auf die Korrelate der Semanteme, sondern es kommen vielmehr auch den anderen Strukturebenen mit Ausnahme des Phonems — über das wir schon gesprochen haben — semantische Funktionen zu, so dass das Gesamtgebiet der Bedeutung in eine Vielfalt von auto-semantischen, synsemantischen Funktionselementen zergliedert erscheint, die für jede Sprache — zumindest aber für jedes Individuum zu einer festen Struktur gefügt erscheinen. Es hat jede Sprache — und jeder Sprecher im besonderen — ein bestimmtes System von Morphemen, das in Verwendung steht, die Aufteilung der Semanteme ist eine gegebene, wenn auch leicht Änderungen unterworfenen Grösse, und auch die

Gebrauchsweise der tagmatischen und syntagmatischen Mittel ist zunächst für jedes Individuum, sicherlich aber für jede "Sprache" fixiert.

Ich schlage nun vor, ganz konkret "das Bildungsgesetz des tragenden Bedeutungsgefüges der Sprache, das eine kategorial geformte Welt als Wirklichkeit meint" (Ipsen) als jene durch den Sprachgebrauch erhärteten Strukturgesetze im Bereiche der Bedeutung in meinem weitesten Sinne zu verstehen, und dieselben als "Innere Sprachstruktur" zu bezeichnen. Ich wähle dafür lieber den Ausdruck Sprachstruktur als Sprachform, um einerseits dem berechtigten Einwand gegen Humboldt's Formulierung, wie er von E. Stolte, *Zeitschrift für Phonetik* II, 205 vorgebracht wurde, zu entsprechen, andererseits aber auch, um der Verwendung des Terminus durch Marty auszuweichen, der ja mit der inneren Sprachform jeweils ein singuläres Bildungsgesetz für eine einzelne Bezeichnung meint und sich selbst dagegen verwahrt, von einer inneren Sprachform schlechtweg zu sprechen.

Diese Innere Sprachstruktur (ISS) variiert nun, da sie auf dem gewohnheitsmässigen Gebrauch der sprachlichen Mittel beruht, von Individuum zu Individuum, ist aber in grossen Zügen für eine Sprache als feste Einheit gegeben, wodurch sie sich von der ISS einer anderen Sprache abgrenzt. Sie umfasst all das, was wir im Bereiche der morphematischen, tagmatischen und syntagmatischen Struktursysteme als Grammatik bezeichnen können, greift aber einerseits noch in das Gebiet des Wortschatzes über, während andererseits auch die Probleme der Syntax, der Idiomatik und Stilistik eingeschlossen sind. Es haftet ihr aber trotz dieses Umfanges keineswegs die Unbestimmtheit und Abstraktheit der früheren Definitionen der ISF an, da ja die einzelnen Strukturgesetze jederzeit statistisch am gegebenen Sprachmaterial verifiziert werden können.

Ich wähle hier zum Abschluss ein einfaches Beispiel, wie ich mir etwa die praktische Anwendbarkeit dieser neuen Fassung vorstelle: Aus der äusseren Sprachform ergibt eine strukturelle Betrachtung, dass z.B. das Ai., Slowenische etc. in ihrer morphematischen Struktur neben der Einzahl und dem Plural auch die Zweizahl kennen (verifizierbar!). Diese Scheidung ist in der ISS fixiert und zwingt jeden Sprecher dieser Sprachen, bei jeder Ausgliederung eines Sinnverhaltes die drei Möglichkeiten zu berücksichtigen. Die anderen Sprachen, wie z.B.

das Deutsche, Englische etc. können natürlich auch ausdrücken, dass gerade zwei Leute etwas ausführen, aber in ihrer ISS erscheint diese Möglichkeit nicht als eine obligatorische Form der morphematischen Struktur, sondern als optionelle Variante der tagmatischen Struktur. Die Folge davon ist, dass wir in diesen Sprachen auf Grund eines durchgehenden Bequemlichkeitsgesetzes nur dann den Sinnverhalt so genau ausgliedern, wenn das Element der Zweiheit besonders in den Vordergrund gerückt erscheint. Sonst aber bevorzugen wir die bequemere ungenauere Ausgliederung und sagen auch ruhig von zwei Leuten: "Sie sitzen in der Ecke und trinken ein Glas Wein," etc. Ähnliche Beispiele würden die slawischen Aktionsarten, die Verwendung von Abstrakta-Bildungen wie Schönheit etc. liefern. Aber auch in der Struktur der Semanteme würde sich das gleiche Gesetz der strukturell bedingten Genauigkeit auswirken. Wenn Sie etwa mit einem Bauern über negative Auslese sprechen wollen, so ist das ohne Zweifel möglich: Da aber etwa weder das Element "negativ," noch das Wort "Auslese," wie man auf Grund von Statistiken seines Sprachtextes nachweisen kann, im Bereiche seiner Semantem-Struktur gegeben ist, muss zunächst eine Transponierung auf eine andere Strukturebene erfolgen, die Ausdrücke müssen mit Hilfe von Sätzen erklärt werden, was aber die Unterhaltung "schwerfällig" und kompliziert macht. Die gleichen Gesetze, die wir hier an simplen Beispielen erläutert haben, führen also dazu, dass in verschiedenen Sprachen Sinnverhalte mit einem verschiedenen Grad von Bequemlichkeit ausgegliedert werden können, was sich letzten Endes auf die Denkgewohnheiten entscheidend auswirkt: Es ist von vornherein jede Sprache geeignet, jeden Sinnverhalt auszudrücken, aber die jeweilige Sprachstruktur erleichtert oder erschwert bestimmte Aufgliederungsweisen, so dass sich daraus eine bestimmte Art des Umschaffens der Welt in das Eigentum des Geistes ergibt, so wie dies W.v.Humboldt vorschwebt.

Es wären hier im einzelnen noch viele Eigenheiten der ISS aufzuzeigen, wie etwa die von Sprache zu Sprache schwankenden Grade der Strukturverfestigung, die Auswirkungen der individuell verschiedenen Grade von Mechanisierungen im Bereiche der ISS: Solche Mechanisierungen, die letzten Endes vor allem in der semantischen Struktur in Form der genormten Begriffsbildung des Philosophen und Technikers auftreten, machen den wesentlichen Unterschied verschiedener

Bildungsgrade aus und liefern dem einen komplizierte Ausgliederungselemente, also Bedeutungen, in weitgehend präfabrizierter Form, während ein anderer erst mühselig solche Bausteine behauen muss: Immer wieder kommt hier der jeweiligen individuellen oder nationalen ISS eine entscheidende, aber verifizierbare Bedeutung zu, denn die ISS ist das Prisma, welches uns den farbigen Abglanz der Sprache liefert, an dem wir schliesslich das Leben haben.

K. AMMER

Owing to my regrettable old age, I have become, not only somewhat hard of hearing, but also, I fear, somewhat hard of understanding. It is probably due to these infirmities that I have not gathered from the previous speakers any reference to some very elementary things about the nature of linguistic facts. No one, I take it, will dispute the truth that these linguistic facts are, of their nature, two-sided. There is clearly the side of sound, but there is also what I, being a relatively peaceable man, will merely describe as the side of meaning. Probably this has been implicit in all the views expressed, but I doubt whether any allusion has been made to another kind of two-sidedness inherent in linguistic phenomena, though surely it ought to be equally obvious and clear-headedly recognized. The two-sidedness I refer to is due to the fact that when anybody speaks, not only the past is involved, but also the present. It is upon this fact that rests the distinction between Language and Speech, and that which I have rather clumsily characterized as the distinction between "meaning" and "thing-meant." Or, if you prefer to put it that way, say that the term "meaning" involves a dichotomy. Now the "meaning" of words, "meaning" as a fact of Language, comprises all those possibilities of right application that have resulted from innumerable applications in the past. These possibilities constitute the knowledge which entitles anyone to consider himself the member of a particular linguistic community. The other side of "meaning" is that which becomes added to a word through its use on the present occasion, the reference to what I call the "thing-meant," because it is best seen and exemplified in the reference to some material object one has never beheld before. It is this second kind of two-sidedness in linguistic phenomena to which I have desired to draw attention.

ALAN H. GARDINER

CONCLUDING REMARKS AT THE END OF THE
SECOND MEETING

Mr. Chairman: First of all, I must say for myself and I feel sure for all of us here to-day, how much we enjoyed the privilege of hearing Professor Vendryès who has given us a clear and classic exposition of the semanteme/morpheme theory, and indeed, no one has a better right to do so than our distinguished French colleague.

This Congress here in London might have been lacking in characteristic English qualities had it not been for Sir Alan Gardiner's remarks. We all appreciated their purpose and enjoyed their simplicity, directness and brevity. Those of you who may have experienced what someone once described as "stratospheric linguistics" will appreciate the renewal of connection with *terra firma*.

Dr. Wüster's work on the standardization of scientific and technical terms is well-known to us all and has basic theoretical implications. Is it really necessary to employ such words as "Begriff" and "Begriffsinhalt" and similar terms in connection with processes of normalization and standardization? I would emphasize that these processes do not take place only on paper as a result of conferences. Just as electrical equipment and tools need to be standardized in order that we may maintain electrical services, the words employed by the workers on the job must follow the same process. These verbal instruments are taken into action and work, in order to maintain the routines of scientific and technical life. The linguist then meets them in context. It is to be hoped that there will be similar international convergence and co-ordination in our own subject.

In this connection, I would like to link up this subject with Paragraph 7 of my report in which reference is made to Bridgman's "operationalism." In this approach also, the distinguished scientist emphasizes that the meaning of his terms depends more on what he does with them than on anything else.

There have been a number of misunderstandings and misrepresentations of the work of well-known linguists, with the discussion of which I cannot take up your time. In my own case, I feel that certain criticisms of my employment of the word "function" do not appear to be well-founded. The

word "function" is not a mere stylistic substitution of what looks like a scientific word for the more ordinary word "meaning." My employment of the word "function" has been in connection with systems and structures. Closed systems are certainly provisionally set up by linguists, the terms of which stand in functional relation to one another and the interrelations of elements in structures are also justifiably regarded as functional. Even the alternants of a substitution series or of a paradigm are in a kind of functional relation. This employment of "function" is not just a figure of speech.

I suggest that the words "Substitution" and "Commutation" be employed as distinct technical terms in descriptive linguistics. "Substitution" should be used to refer to the replacement of one element by another at the same level of abstraction (word, morpheme, phonological unit, etc.) in a structure abstracted from a text, in a frame of Categories. "Commutation," on the other hand, should be used of the alternation of terms within a system. When, by abstraction at a particular level of analysis, closed systems of interrelated terms have been set up whereby all the relevant facts can be accounted for, there is Commutation of the terms within such closed and exhaustive systems and their subsystems, as we apply the systems to name elements or constituents, or to give values to notations in linguistic statements.

Thus Substitution finds order and place within structures, in parallel with texts, whereas Commutation properly applies to systems, which may be multidimensional. In research procedure, Substitution in a framework of structure generally precedes the setting up of a Commutation system, though not all structures lead to Commutation systems. The statement of both sets of relations is desirable in linguistic description.

To turn to the morpheme. In discussion of the morpheme, there would appear to be some confusion between the grammatical categories set up and the phonological shapes or segments which, in the older terminology, are said to express them. I think it is a help, for instance, to make up our minds whether, in dealing with the "s" flexion of an English finite verb in such a sentence as "he comes on Mondays," we regard the "-s" as a morpheme with several "meanings" vaguely defined, as I have noted in my comment on Professor Zellig

Harris's recent book, or whether it should be regarded as the exponent of several grammatical categories, both the exponents and the categories having function in grammatical systems and structures. To which of these the term *morpheme* should be applied is a matter of convenience and consistency.

Finally, three points in brief: there will be considerable agreement with Professor Whatmough that writing is an attested primary form of language. Following the line of paragraph 1 (c) of my report, I would suggest that the logicians at any rate must so regard it. There will be many who would give their warm support to Professor Whatmough's remarks on English spelling which is not the chaos which some people suppose it to be.

I have urged the need for closer collaboration, and may I in conclusion repeat the emphasis. Collaboration, I should like to suggest, could be organized ideally if we all spoke the same language — but we don't. But there is a double problem. First of all, we work in different national languages and, secondly, as will be seen increasingly as the Congress proceeds, even those employing any single language cannot be said to speak the same technical language. A renewed appeal for the standardization of terminology would have no more success than previous efforts. The Congress will bring out the increasing Babel in linguistics. It is to be hoped that it will also show the need to get the various approaches, each expressed perhaps in its own language, into some sort of perspective in view of the great tasks so obviously looming ahead of us. If we work on these great tasks in the manner of electrical engineers for example, we shall ourselves bring about the necessary convergence and co-ordination in the languages we use in the scientific handling of language.

In conclusion, I would like to adapt the brief but cogent remarks of Professor Devoto which were linked with Professor Vendryès' linguistic translations, at the level of *semantemes* and *morphemes*, to the hoped-for convergence of our technical languages and indeed of our personal technical dialects as well. There is first the translation of the general national language in which the linguist is writing, and in which his linguistics is carried; secondly, the translation of the elements and materials of the language being studied; and, thirdly, of the technical languages and dialects of linguistics. While

recognising that complete translations are impossible, we must do the best we can in all three cases, in our own work and when using the work of others. As Professor Devoto emphasized, translation not only tests our understanding but also our sensibility, even our sensitiveness. Without deliberate attention to what I may call "the three translations" we cannot promote that convergence and co-ordination in our joint labours which is the purpose of all our Congresses. The three translations are the keys to mutual understanding and scientific progress in linguistics.

J. R. FIRTH

PLENARY SESSION II

THE POSITION OF LANGUAGE IN PHILOSOPHY, LOGIC, AND SOCIAL ANTHROPOLOGY

- (a) Can a logical calculus be devised that shall be structurally independent of the grammatical patterns of any language.

Chairman: M. J. VENDRYÈS

Rapporteur: PROFESSOR E. J. L. BUYSSSENS

Recorders: MISS R. WALLBANK
MR. M. A. TANNER

(For Preliminary Reports see pp. 19-26 above)

PLANNING SECTION II

THE POSITION OF LANGUAGE IN PHILOSOPHY,
LOGIC, AND SOCIAL ANTHROPOLOGY

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independent of the grammatical patterns of any language.

Chairman: M. J. Vanderschuer

Reporters: Professor E. J. J. Boersma

Members: Mrs. R. W. W. W. W.
Mr. M. A. T. T.

For Planning: Report on the progress of the work

INTRODUCTORY STATEMENT BY RAPPORTEUR

Je ne vais pas vous infliger la lecture du rapport dont vous avez tous pris connaissance. Je me bornerai à rappeler que dans l'ensemble les réponses envoyées sont concordantes: personne n'a rejeté la possibilité d'un langage logique, c'est-à-dire exprimant fidèlement la pensée.

En y réfléchissant, je crois deviner que ceux qui ont répondu à la question qui nous occupe et encore plus ceux qui n'y ont pas répondu considèrent que c'est aux logiciens qu'il échoit de créer le langage logique. C'est en tout cas mon opinion personnelle. Et ce que nous avons entendu hier matin au sujet des langues spéciales des savants et des techniciens semble le confirmer: ces langues spéciales ne sont pas l'œuvre des linguistes, mais des usagers de ces langues.

Seulement nous ne pouvons évidemment pas nous croiser les bras et parodier la parole de Fontenoy: "Tirez les premiers, Messieurs les logiciens." C'est pourquoi je me suis permis de formuler six questions, dans l'espoir que certaines d'entre elles provoqueront des réponses. Ces questions m'étaient d'ailleurs suggérées par les réponses au questionnaire.

Depuis que mon rapport a été imprimé, un logicien de mes amis m'a dit que les logiciens d'avant-garde avaient renoncé aux notions de sujet et de prédicat; cela rend donc la question no. 3 superflue.

E. BUYSENS

M. VENDRYÈS n'admet pas qu'on puisse séparer le langage et la logique. Tous deux sont nés en même temps et se sont développés conjointement au fur et à mesure des progrès des sociétés humaines. Dès le début la logique a consisté à mettre de l'ordre dans le chaos du monde, à organiser l'univers; mais cette organisation n'a pu se faire qu'au moyen de mots. L'homme ne pouvait raisonner qu'en donnant une forme linguistique à son raisonnement, comme un habile artisan perfectionne ses outils et en invente de nouveaux au fur et à mesure de ses besoins. Il n'y a donc pas à s'étonner de rencontrer de la logique dans les langues, puisque c'est le résultat

du travail de l'homme. Les bases logiques sur lesquelles repose chaque langue, c'est l'homme lui-même qui les a posées.

Mais deux considérations importantes sont à retenir.

La première, c'est que malgré un effort poursuivi depuis des siècles et qui se continue sans cesse, aucune langue n'est parvenu à réaliser pleinement l'idéal d'une structure logique parfaite. Cela tient à ce que d'une part dès le début l'esprit humain n'a jamais travaillé sur un plan général logiquement établi, mais seulement par des tâtonnements successifs au hasard des circonstances; et d'autre part à ce que au cours des âges la logique ne s'introduit dans le langage qu'au moyen de l'analogie. L'histoire des langues n'est qu'une lutte entre l'analogie et l'anomalie; mais cette lutte ne porte jamais que sur des détails de la structure, si bien que le résultat du travail de l'analogie n'est le plus souvent que d'augmenter le désordre, ou du moins de le déplacer, sans le corriger. Une langue comme le français, pourtant cultivée depuis tant de siècles, est remplie d'incohérences et de contradictions qui font échec à la logique; des catégories s'y sont maintenues et même développées, qui ne peuvent se justifier logiquement; d'autres qui étaient utiles ont été éliminées; certaines enfin, non moins utiles, n'y ont jamais trouvé d'expression.

Le second fait à considérer est que la logique n'est pas nécessairement la même dans toutes les langues. Nous sommes dupes de notre éducation gréco-latine en croyant à une logique universelle, issue de notre propre expérience linguistique (malgré toutes les imperfections que cette expérience révèle). Mais l'étude de certaines civilisations exotiques fait connaître des types linguistiques qui reposent sur des bases logiques différentes des nôtres. Cela tient à ce que chaque peuple s'est créé sa logique en même temps que sa langue. On n'a pas vu l'univers partout de la même façon. En considérant le monde si complexe des apparences, certains se sont appliqués à faire ressortir davantage telle ou telle relation et à négliger les autres. Les choses ont pu changer aussi avec le temps. De là des différences qui nous choquent et qui nous étonnent.

(J. VENDRYÈS)

Die treffenden Bemerkungen, die wir soeben von Ihnen, Herr Vorsitzender, gehört haben, veranlassen mich hier — etwas unvorbereitet — das Wort zu ergreifen. Sie sagten,

die Anomalie sei "une force rétrograde." Ob es sich hier um eine "Kraft" handelt, muss ich dahingestellt bleiben lassen. Aber das Wort "rétrograde" möchte ich in der vollen Bedeutung seiner Bestandteile nehmen. Denn die uns anomal scheinenden sprachlichen Fakten gestatten tatsächlich ein "Rückwärtsschreiten" in der Sprachgeschichte. Sie erwähnten, dass die Verteilung der beiden Bewegungsverba *arriver* und *partir* auf zwei verschiedene Konjugationsklassen und viele ähnliche Beispiele einen Ansatzpunkt für die "activité logique" bieten, die im Sinne eines analogischen Ausgleichs wirkt.

Die Verhältnisse des Lateinischen gestatten hier schon eher, die grammatische Kategorie einer Flexionsklasse mit der logischen Kategorie, die zu ihrer Bildung beigetragen hat, in Übereinstimmung zu bringen. Viele Verba auf *-eo* bezeichnen, wie bekannt, einen Zustand: *albeo* "ich bin weiss," *ardeo* "ich bin glühend." Im Französischen ist diese Kategorie der Zustandsverba natürlich verschüttet. Wenn wir weiter zurückgehen, erkennen wir in dem Unterschied der *o*- und *mi*-Konjugation der indogermanischen Ursprache eine tiefergehende Einteilung in Tat- und Zustandsverben, die wiederum in den Einzelsprachen verschüttet ist und anderen Versuchen einer logischen Kategorisierung Platz machen musste.

Nach meiner Meinung können wir beim weiteren Rückwärtsschreiten zu logischen (oder "prälogischen") Kategorien vorstossen, die uns heute durchaus fremd sind und nur in den sogenannten "Primitiv"sprachen ihre Analogie haben. Diese Kategorien ergeben sich aus dem unmittelbaren Betrachten der Natur. In der Sprache der Feuerlandindianer und im ausgestorbenen Ubychischen findet man Singularitäts- bzw. Pluralitätsverben. Es wird dort der Plural von einer anderen Verbalwurzel gebildet oder der Singular mit einem eigenen Affix versehen. Über die suppletive Bildung der romanischen Verba für "gehen" ist schon viel geschrieben worden, aber meines Wissens ist dieser entscheidende Punkt bisher unbeachtet geblieben: bei dem Naturmenschen lagen hier eben zwei ganz verschiedene Eindrücke vor, die auch verschieden wiedergegeben werden mussten. Ihm machte es einen grossen Unterschied aus, ob da nur einer ging oder eine Masse sich in Bewegung setzte. Ich möchte damit allerdings nicht die französische Sprache als "primitiv" hinstellen.

Wir finden andererseits im Indogermanischen auch die Singularitätsverben, nur sind sie bisher nicht erkannt und anders erklärt worden. Das griechische *-ka*-Präteritum (Aorist und Perfektum) lässt deutlich erkennen, dass die Erweiterung ursprünglich dem Plural fremd war. Ich erkenne hierin eine Singularitätsbildung, die ich auch im Nominalsystem beim Singulativum *senex* (Plur. *senes*) wiederfinde. Bei der Bedeutung des Greises für das politische Gefüge und im Familienleben mag sich gerade hier der Rest einer archaischen Denkform erstarrt in einer unverstandenen grammatischen Unregelmässigkeit erhalten haben.

Wenn wir diesem ständigen Umbau alter, verfallener grammatischer Kategorien rückwärtsschreitend nachgehen, eröffnet sich uns der Zugang zu den frühen "primitiven" Denkformen der Menschheit.

JOHANN KNOBLOCH

Le Professeur CARNOY fait remarquer, dans une courte improvisation, que les hommes primitifs n'ont vraisemblablement pas mis de logique au début dans leur langage qui, comme le disait déjà Vico, était sentimental, exprimant des états d'âme généraux.

La logique s'y est, évidemment, introduite graduellement, mais tout d'abord sous la forme de ce que les ethnologues appellent: le prélogisme dans lequel le sentiment commande presque les idées.

Il ajoute que M. Vendryès a surtout souligné l'illogisme des systèmes linguistiques et des moyens d'expression mais ce n'est pas exactement là le problème qu'on discute. Il s'agit plutôt de savoir si, comme la logistique actuelle le prétend, il y a moyen de substituer au langage vivant un système différent d'expression des idées qui serait conforme à la logique. M. Carnoy croit, du reste, que cette façon rigide et nécessairement incomplète de rendre la pensée humaine, dans laquelle intervient, à tout instant, le sentiment, n'aurait que peu de rapports avec le langage et ne rendrait guère de services aux linguistes.

(A. J. CARNOY)

Les faits linguistiques n'ont pas à être examinés en raison d'un schème logique donné à l'avance. Faire état d'un système préexistant de concepts qui aurait modelé les différentes langues au cours de leur développement est une démarche idéaliste, héritée d'un long passé de philosophie spiritualiste. Il est plus profitable pour la recherche d'adopter un point de vue matérialiste suivant lequel les états de langues sont les aboutissants d'actions et réactions très complexes, d'où résultent les structures qui constituent l'appareil de communication, en état d'équilibre imparfait. Dans chacun de ces états de langue, les idées à communiquer suivant les besoins sociaux s'expriment soit par des grammaticalisations soit par des lexicalisations, avec toutes sortes de compensations, dont on doit essayer de saisir le mécanisme sans supposer que les sujets parlants possèdent a priori telle ou telle logique fixe.

D'autre part, suivant un enseignement dû particulièrement à A. Meillet, des ensembles de langues paraissent refléter dans leur évolution certains changements dans les manières collectives de réagir aux circonstances naturelles. C'est aussi que les précisions de temps plutôt que d'aspect ont pris une grande place où même la prépondérance dans le système du verbe de la plupart des langues indo-européennes et qu'un mouvement analogue est indiqué dans l'évolution des langues chamito-sémitiques. Or le phénomène a commencé à se produire bien avant que l'usage généralisé des horloges et des montres ait marqué clairement le besoin de régler minutieusement la plupart des actes sociaux.

Si des faits de ce genre invitent à mettre en relation les évolutions de langue et les évolutions des sociétés et spécialement de leurs conceptions collectives, on peut apercevoir en même temps combien il serait dangereux de rattacher précisément tel fait linguistique, en particulier grammatical, à tel ou tel détail des institutions et des idéologies.

Au total, il faut se tenir prêt à examiner tous les rapports entre les choses, et une condition essentielle du succès est de ne pas être guidé par la considération de quelques rapports de concepts abstraits.

MARCEL COHEN

What I have to say will be only on the general question of this Session, namely, the relation between Logic and Grammar — which is strictly speaking not a question of Linguistics,

but of Metalinguistics, or (to use a good old-fashioned term) of the Philosophy of Language.

What I tried to do in my contribution (see above p. 25) was to indicate: what is it that constitutes the essential difference between ordinary language and a logical calculus. "Language differs from a calculus as an organism differs from a machine": I have tried to elaborate this somewhat metaphorical statement; that is, to express the contrast in grammatical and semantic terms. In fact, I was trying to answer Professor Buyssens' questions 2 and 5 (see above p. 22).

A calculus, I meant to suggest, represents certain restrictions imposed on ordinary linguistic usage. The main purpose of my contribution was to show (if, indeed, that is possible in 250 words) that these restrictions of "Logical Grammar" so-called were not, and could not be, part of any Universal Grammar of Language; and my argument was that, if all linguistic usage were required to conform to them, i.e. if (as many have thought or implied) they represented something like a general structure of language, then no language could be a living one.

Some statement or other in my contribution must have been misleading, or Professor Buyssens could not have suggested that "seul M. Haas est radicalement opposé à la logique." May I say then that I am not at all opposed to Logic. I am not here warning against imposing rules of Logical Grammar on our ordinary language. For, of course, we must impose such rules, when we wish to have logical argument. All I mean to say is that those rules of Logical Grammar are not general rules of grammar.

To clear all misunderstandings, I would like to try to state some general implications of this position, in so far as it concerns the linguist's practical work:—

As linguists analysing a language we do, of course, speak the logical language of analysis. However, what we speak about is not Logical Language, not a calculus, but Living Language. It is only the logician who speaks both in and about logical language. Thus, Carnap's *Logical Syntax of Language* is really a Syntax of Logical Language; as also, his later *Introduction to Semantics* is a Semantics of Logical Language.

Now, it seems to me that, since Aristotle, linguists have been hindered in their exploration of linguistic structure, by borrowing from the logician grammatical and semantic categories

which, however adequate they may be to the task of describing the language of a calculus, are useless for describing the organic structure of Living Language. If those categories of Logical Grammar, instead of being derived from Aristotle's somewhat adulterated Logic (which we take in almost with our mother's milk), are derived from the purged armoury of the modern logician, then they are even less applicable to ordinary language. If we are tempted to accept the logician's grammar of a calculus as a general framework of our language-grammars, we can only be left with a huge residue — really the vast majority of linguistic facts — unaccounted for, lumped together as (to quote Carnap) "logical imperfections" of Language. It is as if someone should make the restrictive categories of barrack-square language fundamental to our grammars, saying, for instance, that fundamentally all sentences were commands, and those which were not, were military imperfections of language.

I am afraid, therefore, that the very special and more intense co-operation between Symbolic Logic and Linguistics, for which Mr. de Witte is asking would only intensify the mischief arising from our present habit of confounding our proper task, namely the logical analysis of language, with the analysis of logical language, which is the logician's task.

This is not to deny that knowledge of Logic, especially of the more potent modern brand, is useful to Linguistics. Logic will tell us something very important about our tools of research and their use: that is, about the language of Grammar, and generally about the logical language of Linguistics in which we speak about languages. But (and this is the crucial point) what Logic tells us about the language of Linguistics will be essentially the same as what it tells the physicist about the language of physics, or the economist about the language of economics, or the psychologist about the language of psychology. To put it briefly: Logic has no specific relevance to Linguistics. — To say this, I would claim, is to express opposition, not to Logic, but only to assigning to Logic a job which it cannot do.

W. HAAS

Formal logic is concerned with the validity of inference (in a procedural sense); it is not concerned with truth. To put it in other words: it is concerned only with the form, not with

content, and one may go further and say: The logician investigates not the form of a statement as such, but the form of the procedure of arriving from one assertion (or more) to another. Truth, no matter how it is defined, is entirely irrelevant.

To repeat: Form and content are separate and distinct.

Linguistics is concerned with form and with meaning, that is to say, it is concerned both with the form and the content of communications of thought. Meaning, of course, is entirely separate from truth or correspondence with facts. E.g. "This table is made of chocolate" is not a true statement, but it is meaningful, that is to say, if I were to utter such a statement, I would presumably (1) know what I mean, even though it be an untruth, a joke or simply a mistaken assertion, and (2) I should expect my audience to be in some way affected by it.

In logic, then, there is no connection at all between form and content, in fact, one might say, there is only form without content. In linguistics, on the other hand, form and content (meaning) are inseparable.

Sir Alan Gardiner has shown in a recent article (*A grammarian's thoughts on a recent philosophical work*) the danger into which some philosophers have been led by an excessive preoccupation with logical form as applied to linguistic complexes. The danger consists in incurring a loss of commonsense meaning instead of a gain in logical precision. An equally great danger is the confusion between, on the one hand, the truth or correspondence aspects of the linguistic material (e.g. inference or assertion) investigated by the philosopher, with, on the other hand, its meaning-content.

The question before us, however, touches another point: if you regard any articulate human utterance from the point of view of the linguist, then every single significant element of that utterance is a complex of innumerable segments of meaning; every word has a cultural and social history, a history in the development of the individual speaker, it is influenced by context and situation, it is coloured by delivery and even the manner of printing; one could go on *ad infinitum* in enumerating the elements constituting the meaningful novelty of every instance of the use of a word. In devising a logical calculus that should do away with the pattern and material of existing language, one would by necessity have to abstract all, or all but one, of these elements making up

meaning. There would thus be no gain of precision (which, I presume, is the object aimed at) but rather a loss of it. A further consequence would be a complete atomization, disintegration, of communicable thought.

There is a further point. A logical calculus, in the sense desired, would be completely non-lingual, but it would be so only on the surface. Symbols do not replace the word any more than they replace the thing. Even mathematical symbols when understood by the hearer or reader are either understood as standing for names, words, or phrases, or else they become words or names themselves. When we see in a mathematical text-book the symbol x , we need not translate that into the phrase: "an unknown quantity"; x has become a word in its own right.

A. WASSERSTEIN

Se è vero, come risulta dalle risposte date al questionario e come il prof. Buyssens ha messo in rilievo nel suo rapporto, che la logica e la lingua non vanno d'accordo su molti punti, mi pare anche evidente che si debba negare la possibilità di costruire un linguaggio più logico delle lingue perchè, se si ammette questa possibilità, si potrà avere soltanto un sistema di simboli assolutamente immobili e senza vita o un sistema di segnalazioni stradali che non avrebbero nulla a che fare con le lingue. La ragione è molto semplice. Non bisogna dimenticare che il linguaggio non è soltanto il frutto della ragione e della logica ma è l'espressione più diretta e immediata di tutta la personalità umana.

La tradizione filosofica che va da Vico a Croce ha sempre sottolineato l'importanza della forza della fantasia creatrice nel linguaggio individuale ed è perfino arrivata all'identificazione dei due termini "intuizione" ed "espressione." Studiare a fondo i rapporti di questo linguaggio individuale con le lingue, mezzo di comunicazione di una comunità storica di parlanti è compito dei linguisti.

La logica e la facoltà creatrice dell'individuo, che è alla base del suo linguaggio espressivo, contribuiscono necessariamente alla formazione e alla vita di tutte le lingue, ma è impossibile sopprimere uno di questi termini senza sopprimere, nello stesso tempo, lo stesso linguaggio.

TRISTANO BOLELLI

In trying to give an answer to our question — which is not the general relation between language and logic —, it might not be unwise to have a look at what the logical content of the means hitherto used to describe the grammatical pattern of most languages is. Only, I should recommend to follow the example of our medieval predecessors, and start with the negative.

I take as an example the combinations of semantic elements. Here are four possibilities, in accordance with the practice hitherto used:

(1) An element can be placed outside the context, so that it need have no relevant connection with any other element. We might call this "exotaxis," and give the element thus used, the mark O. Well-known examples are vocatives, imperatives, interjections.

(2) Two or more elements can be placed together, quite simply, without any distinctive feature of the element as such. This is what is generally called "parataxis"; for an element in this combination we may use the mark of the Greek letter delta: Δ . In adding negation in an adequate way, parataxis, as is equally well-known, is divided in addition, disjunction, and contraposition, exemplified in the English *and, or, but* (Eskimo *-lu, -lu:, -li*). Only with the combination called addition we are free to combine more than two elements.

(3) Two elements can be combined in such a way that they are not equal in rank; one is superior to the other, i.e., one is super-ordinated, the other subordinated, or, in other words, one is determined, the other determining. This is what is generally called "hypo-taxis"; we may give the two elements in this sort of combination the marks I and II. Simple examples which can be supplemented from everywhere, are *beautiful women, well done, sun-dial*, which are all II-I (subordinate-superordinate). A fundamental characteristic is, furthermore, that the combination of the two elements may be substituted for any of them, e.g., *the beautiful woman's marvellous hat* II (II-I) - I (II-I).

(4) The logical counterpart of this is a combination of two elements in such a way that they are equal in rank, but that a combination of them may be substituted for one element only, — we may call it *a* — not, for the other element, which we will call *b*. This is "anataxis," called by Jespersen "nexus." The relation between the two elements, which is here defined

in a purely formal way, corresponds to the relation of subject and predicate in logic. Common examples are: *it is*; *the sun shines*. An example of the substitution of *a* by *a-b* is *he is found guilty a (a-b) - b*: what is found is that he is guilty. But you may not substitute *a-b* for *b*, if you do not add something which is outside this relation. It is just this peculiarity of the basis as against something which is not the basis, that gives the parallel to the logical relation subject-predicative.

It is very important that the combinations may be combined: *sun-dial* is II-I, *the sun shines* is *a-b*, but *sunshine* is both II-I and *a-b*. What we call "apposition," is generally a double combination, mostly "exotaxis" and "hypotaxis," sometimes "exotaxis" and "parataxis."

Furthermore: one expression may contain two elements e.g., Lat. *est* "it is," Eskimo *tikiṭpoq* "there is arrival, somebody arrives, he arrives." This leads to the final observation that sometimes only one element is expressed, because it is supposed that the hearer may supplement it from the context. Some grammatical forms are regularly used without expression of more than one element of the combination in question.

I have found this simple system applicable to Eskimo and to any of the IE languages which I know.

LOUIS L. HAMMERICH

The answer to the question posed as the theme of our discussion is "Why not?" An even more satisfactory answer lies in the fact that such calculi, as e.g. the calculi of Combinatorial Logic, have actually been constructed, and *de esse ad posse valet consequentia*.

The doubts behind this question are probably based on the thought that one might not be able to construct with the help of an ordinary language as metalanguage a logical calculus that shows no connection with the structure of this metalanguage. This is, however, a fallacy. Just as one can show, using measuring equipment built in accordance with Euclidean Geometry that our universe is not Euclidean, so one can show in ordinary English that ordinary English is not the most adequate language for certain purposes and that a completely differently structured calculus might be more adequate for these purposes.

Y. BAR-HILLEL

CONCLUDING REMARKS AT THE END OF THE MEETING
BY THE RAPPEUR

Vous avez pu vous rendre compte que dans mon rapport préliminaire j'avais pris nettement position; je l'avais fait délibérément dans l'espoir de susciter la contradiction. Et c'est ce qui s'est produit; nous avons entendu des communications très variées: les unes ont refusé catégoriquement d'avoir rien à voir avec la logique formelle des logiciens, les autres ont assimilé la logique à la langue, ce qui est une autre façon de la neutraliser.

Maintenant que le débat est terminé, nous voyons les choses de plus haut. Nous nous demandons qui a suggéré cette question un peu inattendue et source de malaise. Pourquoi a-t-elle été posée? Avait-elle un rapport avec le congrès des logiciens qui a eu lieu récemment? Je l'ignore. Mais je regrette qu'aucun logicien ne soit venu apporter sa contribution.

Sir Alan H. Gardiner a fait remarquer que les mots ne sont que des moyens de suggérer; l'auditeur doit accomplir un travail considérable pour compléter les données qu'il perçoit. Les logiciens s'en sont rendus compte, et ils cherchent à créer un langage explicite. C'est cela qui m'a guidé dans mon rapport: j'ai cherché à susciter des réponses qui puissent satisfaire les logiciens.

Mais la réaction a été négative; les linguistes n'aiment pas la logique et ils n'ont pas déguisé leurs sentiments. Le langage dont les logiciens ont besoin est quelque chose de sec, d'abstrait, d'impersonnel, alors que la langue touche de près à notre cœur. Et surtout nous nous méfions de la logique parce que nous nous souvenons du tort immense qu'elle nous a causé par ses intrusions.

Cette réaction des congressistes est des plus réjouissantes, car elle a pour effet de nous rapprocher des Américains: en repoussant la logique comme une intruse, nous faisons exactement comme Bloomfield qui banissait tout recours à des catégories psychologiques qui ne sont d'aucun secours. Il semblait que deux écoles s'affrontaient; nous découvrons qu'il n'en est rien. Le Congrès de Londres s'inscrira dans l'histoire de la linguistique comme celui qui a réalisé l'accord des écoles.

La question du calcul logique valait donc bien la peine d'être posée.

E. BUYSENS

PLENARY SESSION II

THE POSITION OF LANGUAGE IN PHILOSOPHY, LOGIC, AND SOCIAL ANTHROPOLOGY

- (b) On what principles are word-classes to be defined and named?

Chairman: PROF. A. BELIĆ

Rapporteur: PROF. O. FUNKE

Recorders: MR. P. F. GANZ
MR. K. NORTHCOTT

(For Preliminary Reports see pp. 27-45 above)

THE POSITION OF LANGUAGE IN PHILOSOPHY,
LOGIC, AND SOCIAL ANTHROPOLOGY.

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LOGIC, AND SOCIAL ANTHROPOLOGY.

(1) On what principles are words chosen to be defined?
and named?

THE POSITION OF LANGUAGE IN PHILOSOPHY,
LOGIC, AND SOCIAL ANTHROPOLOGY.

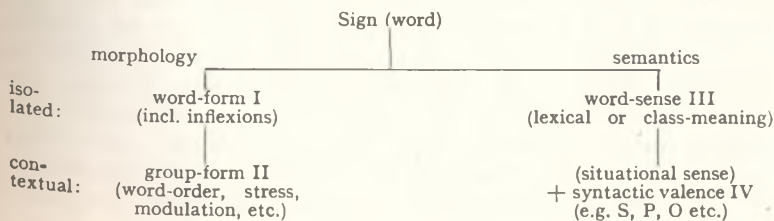
(2) The Technology Report, see pp. 15-18, 20-21.

PRELIMINARY REMARKS : O. FUNKE

My preliminary report (see above pp. 29-34) does not satisfy me any longer. It was written rather in haste under the pressure of academic duties, it was not exact enough and did not include recent books which, meanwhile, have given me valuable information, although I do not endorse all their doctrines. These studies are: Ullmann's *Principles of Semantics* (1951); Togeby's *La structure immanente de la langue française* (1951), representative of the Danish circle; Zellig Harris's *Methods in Structural Linguistics* (1952), representative of the "formal" current in American structuralism; Glinz's big volume — *Die innere Form des Deutschen* (1952); and last of all a book which came into my hands only two days ago, Fries's *The Structure of English* (1952). All these works are, in one way or another, concerned with our problem of "word-classes."

I have, in my preliminary report, hinted at some questions of principle: i.e. the almost bewildering terminology used by different linguistic schools and the situation of psychological considerations in linguistics. Having had the opportunity of speaking on these topics in the discussion of our first plenary session, I need not dwell on these problems here again. I perfectly agree with Eringa, who, in his contribution, stands up for clear definitions concerning the different levels and parts of a linguistic utterance.

I shall try to use as unambiguous a terminology as possible, and I have therefore made the following diagram in order to mark the four predominant aspects, which have, since the times of the ancient grammarians, been used—in different distribution—to define "word-classes."



To give a short explanation, I take the following sentence: "Yesterday the little black boy came to our house." We start from this context and take out of it the nominal block, the S-group — "the little black boy." In this utterance it has the syntactic valence (IV) of being the subject; it consists of a word-group which in itself shows different syntactic valences: the head "boy" and its qualifiers "little, black." These three words have also lexical meaning: the concept of a young male human being and his qualities. The article "the" as such has no lexical meaning; it gives the whole group a certain definiteness or individualisation (i.e. the situational sense). This S-group has its group-form (II): word-order, stress, modulation, etc. Let us now isolate words of this group: "boy" has a word-form (i.e. sound-form; I) which is associated in our memory with the forms "boys" (whether we think of the "boy's home" or "the boys in the street"). This word-form has its sense; in the context it denotes a definite individual; taken out of the context it becomes a pure class-name. Thus we have, in short, the four aspects of our diagram: in the context syntactic valence — situational sense, group-form; isolated word-form and word-sense. So much for the present moment.

WORD

To speak of word-classes involves the presupposition that there are "words." Fortunately enough I am spared the trouble of unrolling this problem in any detail. Reichling (1935), Rossetti (1947 *Le Mot*), and quite recently Ullmann (1951 *Principles of Semantics*) have given detailed surveys of the whole question. "Words" are often called either "minimum free forms" (Bloomfield) or "the smallest semantic units."

Are there "pseudo-words"? This problem has a history reaching back to Aristotle and his followers when they distinguished, in a statement, the linguistic terms for the Subject and the Predicate (i.e. noun and verb) as the essential or autarchic parts of the utterance (*categoremata*) whereas the others would only serve as a kind of minor attendants (*syn-categoremata*). Ullmann refers to this problem and seems inclined to consider what he calls "purely syntactic elements or tools" (articles, conjunctions, prepositions, even pronouns)

as pseudo-words; such tools would have only syntactic valence (IV) and no lexical meaning.

I think we must here make a distinction between different facts:

(I) In "parole" (i.e. speech-context) all words have a certain syntactic valence; most of them have also some lexical meaning, yet in different degrees. In our S-group "the little black boy" the head-word "boy" has self-sufficient autarchic sense; "little" and "black" are not so self-sufficient, they denote a qualification of something i.e. "boy." The article "the" only contributes in so far as the whole group gets a definite sense, yet it has no lexical meaning of its own and its syntactic valence I should like to call "group-function."

(II) The personal pronouns, on the other hand, e.g. "I", "you" are of a peculiar type; in speech they denote the individual personalities of the speaker and the listener; they are no pseudo-words because they are semantically absolutely self-sufficient. Taken out of the context they become indefinite, signifying only an indefinite speaker and listener; thus they become "determinable." In this respect they resemble, to a certain extent, proper names.

(III) Quite different are the so-called "particles" (conj., prep.) which never have self-sufficient meaning, neither in "parole" nor in "langue." They contribute in different ways to the sense of the group they belong to. Yet if we appeal to the commonsense of the everyday speaker, he would probably be astonished at the question whether English *if*, *on*, *at*, even *the* are words or not. His answer would doubtless be in the affirmative. It was de Saussure himself who pronounced a warning that the grammarian should not be too sophisticated in his reflexions, that he ought to take into consideration the ordinary speaker's feeling for his native tongue.

It was this consideration, together with others, which caused Marty to define the "word" semantically as the smallest organic speech-unit which is treated by the ordinary speaker as if it were a kind of semantic unit, and he added that even particles ought to be considered as words as long as they would evoke in the listener preparatory expectations concerning some necessary supplement. In this way such words as *in*, *at*, *but* adsignify e.g. local relations, contrasting statements — to mention only a few examples. Such a prospective

function cannot be attributed to isolated inflexional endings and to most of the affixes because — when isolated — they convey no meaning whatsoever.

The grammarian may, of course, by way of comparison, split up meaningful units (i.e. words) into smaller parts if he likes. He may e.g. say that in a word like Latin *amo* the ending *-o* signifies the first person sing. act. present, but he must be aware of the fact that from the standpoint of pure description he creates in this way a fictitious entity which as such has no memory-pattern in the speaker's mind. The speaker knows only the full living word-form *amo* in opposition let us say to *amas*, *amamus* etc. The grammarian may give such separated splinters whatever names he likes (formans, ending, morpheme) yet such parts are nothing but abstract creations of the grammarian's mind. On the other hand, the so-called stem (also called root, theme, even morpheme) *am-* or *ama-* is a similar fictitious entity from the speaker's point of view; it is of the same kind as the single ending *-o*, a purely grammatical abstraction.

As to word-classes: there seem to be scholars who would be inclined to group words not with regard to their living full inflexional word-forms, but according to their stems (or themes), believing that these stems alone would form semantic classes. Unless such stems do coincide with full living word-forms they can never furnish the basis of such a semantic classification in a descriptive survey. Thus Holt would like to question the existence of word-classes by referring to polysemantic words and would prefer a grouping according to themes, a term which roughly corresponds to the usual stem. I shall return to this question immediately.

WORD-CLASSES

To which part in grammar does our problem of word-classes belong? I am alluding to the most crucial problem concerning the division of descriptive grammar; for I hardly need mention that our word-class problem is, first of all, a descriptive (synchronic) one. It is probably this aspect which Glaesser seems to have in mind when, in his contribution, he asks for a linguistic theory of relativity. He means that word-classes have to be considered as component elements in a synchronic language system.

The sixth International Linguistic Congress discussed the relation between morphology and syntax — with few tangible results, as Ullmann sums up. He takes up the question once more (in his *Principles of Semantics*) and, following Ries (*Was ist Syntax?*) would propose a division into phonemics, lexicology and syntax. In this scheme lexicology and syntax would, each of them, have morphological and semantic counterparts. According to Ullmann's opinion lexicology would comprise whatever pertains to the word *qua* symbol standing for the "thing-meant": i.e. word-stems, word-formation together with their semantic aspects. Inflection would lie outside the domain of lexicology because it is a formal device to convey relations; it would therefore belong to syntax and the parts of speech which are akin to other syntactic categories would belong to syntax.

Here seems to appear the first discrepancy: the formal side of inflections, their paradigms certainly make up mnemonic clusters or memory-patterns in the mind of the speaker (e.g. *boy, boys; foot, feet; hortus, -i, -orum* etc.); therefore they belong as to their paradigmatic character to morphological lexicology; their syntactic valence, their syntagmatic character is dependent on the context, therefore this problem would be part of syntax.

But this is not the only point where difficulties arise. If lexicology ought to deal with words, their external forms and their sense, do all these words represent equal values, do they all stand on the same level? Nobody will doubt, I think, that an "adverb" normally functions only in a speech-context, that a preposition has no autarchic semantic value (because a relation necessarily requires some *relata*), and the same might be said of a good many other words (even finite verbs, except the imperative) down to the particles. There is a certain gradation in semantic values; whether you will call thing-words ("boy," "house," "garden") autarchic (auto-semantic) or not, you cannot deny that such nouns have more self-sufficient sense-value than adjectives and prepositions. Such semantic attendants, such synsemantics adsignify, contribute only — in different degrees — to the sense of the group they belong to. Using Bühler's terminology we might speak of "different field-values" ("Unterschiede in den Feldwerten der Sprachmittel"). It is for this reason that

lexicology and syntax cannot be strictly separated. We have, in my opinion, only two sides in descriptive grammar: morphology and semantics. Whether one starts from the smallest sense-units and goes up to the phrase or takes the reverse way, there will nowhere be watertight regions because morphological and semantic aspects will always intertwine, overlap and certainly cross each other. Our problem of word-classes therefore belongs as well to lexicology as to syntax (in Ullmann's sense), as well to morphology as to semantics.

It is clear, that word-classes can only be distinguished in a language if its words as such have some distinctive morphological marks or characteristics that oppose them to one another. Isolated words (i.e. words in "langue") without any morphological difference and of polysemantic character cannot be assigned to a special word-class. Such a word as English *like* is such a polysemantic element with uniform phonemic and graphic structure. It may in speech be used with different semantic and syntactic values. In the language system such words are indefinite or ambiguous; they cannot be assigned to a definite word-class. They gain their actual sense and syntactic valence only in "parole," in the context of speech. If a language had only such polysemantic words with no morphological differentiations whatsoever, then we could speak of its having only contextual or, as far as the system is concerned, potential word-classes.

HISTORICAL RETROSPECT

When we survey the problem of word-classes in its entirety, we observe that from the time of the Greek grammarians up to the present day all four aspects on our diagram have been taken into consideration, with various distribution and with different stressing.

(I) The ancient grammarians (Greek and Roman), basing their word-class system on the highly inflexional structures of old Greek and Latin, distinguished eight parts of speech, and it goes without saying that this word-class system cannot satisfy the structures of all languages. They introduced all four aspects into their classification, though certainly not in a strictly consistent manner.

The chief parts (i.e. noun and verb) they characterised by their class-sense (III) and their word-forms (I ; *cum casu*,

cum tempore). The indeclinables, on the other hand, they defined either by their group-form (II; e.g. preposition) or by their syntactic valence (IV; e.g. conjunction).

Criticism of this classical scheme went on throughout the ages; in our days we find critical remarks in nearly all works and papers dealing with our problem (Paul *Prinzipien, Kalepky Neuaufbau der Grammatik* 1928, Hjelmslev *Principes de Grammaire Générale* 1928, Brøndal *Les parties du discours* 1928, E. Otto *Sprache und Sprachbetrachtung* 1943, Robins *Ancient and Mediaeval Grammatical Theory in Europe* 1951). On the other hand, Thomas (*Transactions of the Philological Society* 1949) undertook a kind of revaluation by considering what a difficult task confronted the ancients in systematising the bewildering structure of ancient Greek.

We, nevertheless, still use this classical terminology, and it is generally and internationally understood.

(II) Thescholastics, with their *modi essendi* and *significandi*, turned to word-sense (III); they tried to embed the classical scheme of the eight parts of speech in a kind of ontological and conceptual order.

Thus Scotus (J. von Erfurt): *nomen* est pars orationis significans per modum *entis*, vel determinatae apprehensionis; *verbum*: . . . per modum *esse* distantis a substantia. *conjunctio*: pars orationis per modum conjugentis duo extrema significans.

(III) Side by side with these attempts went another which I have already mentioned, i.e. the classification according to syntactic valence (IV): noun and verb on one side (categoremata), the other members of a sentence on the other (syncategoremata).

This classification entered grammar, as far as I can see, only in the sixteenth century with the Spaniard who grouped the words into three classes: *nomen*, *verbum*, *particula*. This tradition too is still alive. We find it with philosophers like Mill and Marty; we find it when scholars lay special stress on the importance of noun and verb; we recognise it in a more hidden way with linguists when they speak of full and form-words (Sweet), "Vollwörter und Formwörter" (Sütterlin) "Voll- und Knappwörter" (Kalepky) etc.

(IV) A turn to purely formal aspects (I, II) had already come in with the Roman grammarian Varro who used to

classify: *nomina (cum casu)*; *verba (cum tempore)*; *participia (cum casu et tempore)*; *particula (sine casu et tempore)*.

In the sixteenth century it was Petrus Ramus who consciously replaced the former semantic grouping (categoremata and syncategoremata) by a purely formal one which he arranged in dichotomous oppositional classes and sub-classes: *voces numeri* (noun, verb); *voces sine numero* (the indeclinables). These indeclinables he characterised partly by their group-form (II), partly by syntactic valence (IV). Ramus tried to avoid semantic aspects, yet the knowledge of word-sense is silently presupposed; how could he otherwise have known of number at all? He is, in principle, a pure formalist who tried to apply his method to a French grammar written in honour of Catharine de Medici. I rather doubt whether she enjoyed this dry little book.

So much about the tradition. We see: all the four aspects were applied for the classification of words, with different emphasis and distribution. There is nothing absolutely new under the sun; for we find the same aspects, with variations, in modern attempts at defining our word-classes.

MODERN ATTEMPTS AT CLASSIFICATION

Recent surveys of modern attempts have been given by Brøndal (*Les parties du discours*) who does not go beyond the twenties, and by de Groot (*Lingua* 1948) who gives a small selection of authors up to the forties.

Again I find, all in all, four major groups; they present, to a certain extent, analogies to their historical predecessors which I have just been discussing.

I. CLASSIFICATIONS ACCORDING TO SENSE-CLASSES (III)

It is the psychological and ontological line which represents a kind of counterpart to the scholastic *modi significandi*. The classical tradition with its eight parts of speech stands in the background. Psychologists, philosophers and linguists have approached the problem from the semantic angle of word-sense (III). You will understand that it is impossible for me to go too much into details.

Wundt may head this group: according to him substantives, adjectives, verbs and particles would stand for the categories

of thing, quality, state ("Zustand") and relation. He spoke of "Ding, Eigenschafts-, Zustands- und Beziehungsbegriffen"; yet he himself doubts a clear delimitation (*Sprache* II; Introduction). Others follow, at least partly, similar lines with slight modifications; I mention E. Otto (*Grundlegung der Sprachwissenschaft*, 1919, *Sprache und Sprachbetrachtung*, 1943, and other papers), Hermann (*Die Wortarten*, 1928) Kalepky (*Neuaufbau der Grammatik*, 1928), Sloty (*Das Wesen der Wortart*, D. N. Schrijnen 1928; *Wortart und Wortsinn*, *Travaux* I 1929) Sandmann (*Folg. F.*, 1940).

E. Otto emphasises the distinction between lexical meaning (III) and syntactic valence (IV). He states in a contribution which has appeared quite recently: "Die Wortklassen sind zu definieren als syntaktische Beziehungsmittel. Die Bezeichnung der fundamentalen Wortarten lautet: Subst. Adj. Verb und Relationswort." Yet he adds in an introductory remark, "dass die Wortklassen den Gegebenheiten der vorgestellten Umwelt nachgebildet sind" which, in my opinion, would refer to word-sense as well.

E. Hermann somewhat modified Otto's earlier doctrines, especially by his creation of a terminology of wider range (Subst.= "Wesenswort", Adj.= "Eigenheitswort", Verb= "Tätigkeitswort"); yet he seems to me to mistake metaphorical elements e.g. the halo of substance which surrounds abstract nouns or that of activity which accompanies the finite verb in Indo-European for the essential meaning or word-sense. With this important problem various scholars are concerned, e.g. Sweet who said long ago (*Logic and Grammar*, 1876): "Language is based on things not as we know them to be, but as they *seem* to us"; Gardiner (*Speech and Language*): "Meaning of the parts-of-speech is not primarily based upon the nature of the 'objects' to which they refer, but upon the mode of their representation."

Sloty and Kalepky try to deduce a classification from ontological realities; the former from substance and accident, the latter by reference to four categories ("Das Seiende" = being, ens; "Verläufe" = processes; "Verhältnisse" = conditions; "Beziehungen" = relations). Sandmann arrives at another semantic grouping under Neo-Kantian influence: the background for the verbal and substantival types would be Time and Space in the Kantian sense of pure forms; the adjective-

adverb type might be deduced from the non-numerical sphere of values and qualities (the accidents). The question may be allowed: where are the substances which are to be valued or which are to be qualified? Are the Kantian forms realities or fictions?

Against such semantic aspects let me place Bloomfield's remarks (*Language* § 16.5): "Our knowledge of the practical world may show that some linguistic categories agree with classes of real things. It may be e.g. that our non-linguistic world consists of objects, actions, qualities, manners and relations, comparable with the substantives, verbs, adjectives, adverbs and prepositions of our language. Moreover, we should still have to determine the English parts-of-speech not by their correspondence with different aspects of the practical world, but merely by their function in English syntax." I cannot share Bloomfield's belief in the decisive factor of syntactic valence alone; compare the following expressions: "The man works hard" = "is a hard worker", "the flower is blooming" = "is in bloom" where we have different word-classes in the predicate with exactly the same semantic value.

On the other hand, I think it is not too difficult to show that the eight parts of speech are not strictly delimitable sense-classes and cannot be grouped consistently in such a way. Apart from the abstract nouns which, like the verbs, may also signify states, processes, etc., the adjectives certainly do not always denote qualities, the verbs may express states, processes, actions; yet how are we to delimit their differences? "To see = to look at", "to hear = to listen to": are the former compared with the latter actions or passivities? And what about the modal verbs and the so-called copula which contains the purely assertive element? I may refer to Marty's *Satz und Wort* where he treats of these problems in some detail.

A unique and original attempt at describing word-classes by their semantic values was undertaken by Brøndal in his *Les parties du discours* (1928; 1948 French version). As to the parts of speech, it was his opinion that neither the morphological structure nor syntactic valence (i.e. I, II, IV) was an infallible guide — a view which I am inclined to endorse. Therefore, he thought, the solution must be found in word-semantics (III). He tried to approach the problem by going

down, as he believes, to the four most elementary concepts which he takes over from the Aristotelian categories (substance, quality, quantity, relation, symbolized by R d D r respectively). By almost countless combinations and different accentuations of these four concepts he attempts to establish a system of meanings for the possible parts of speech in nearly all languages. It seems to me to be a highly ingenious play with these four symbols, yet mental phenomena can not be symbolised by purely mathematical additions or multiplications. One example will illustrate the difficulties: according to Brøndal R d (substance, quality) would denote the semantic value of a concrete noun; yet it is a great mistake to think that concrete nouns have always such relatively simple concepts. Take e.g. the word "room." In this concept you will find all the four elemental ones united; "room" stands for something substantial, having qualities, quantity and relational features. When looking up Brøndal's symbol-system we are rather astonished to find that the group r R D d is the symbol for an interjection. Thus it seems to me that the principal weakness lies in the imperfection of psychological analysis, for the whole problem belongs to the sphere of descriptive psychology.

2. CLASSIFICATIONS WITHIN THE LINGUISTIC SPHERE BASED ON SYNTACTIC VALENCE (IV) AND WORD-SENSE (III)

This is the line continuing the ancient aspect of the categorematic and syncategorematic. Among this group Marty, Noreen and Bühler may be mentioned. Marty modified the tradition by calling these two categories "autosemantic" and "synsemantic" expressions, and by reckoning among the auto-semantics sentences (statements and emotives) and thing-words; the other parts of speech would be synsemantic or adsignificant, with certain differentiations. I should only like to remark that this classification seems to me valuable for three reasons: it abandoned the still wide-spread belief in the equally self-sufficient semantic character of all words; it turned against the opinion of a strict parallelism between linguistic form and meaning; and it tried to draw attention to linguistic abbreviations and measures of economy in linguistic expression. Noreen (*Vårt språk*) held, apparently independently, a similar view when he distinguished between substantial and accidental glosses.

The third author I must mention among this group is Bühler and his *Sprachtheorie* (1934). His theory of representation ("Darstellung") is well-known, yet seems to me wanting in clearness. Well-known, too, is his distinction between two linguistic fields, that of indicating ("Zeigfeld") which concerns the actual, immediate speech-situation, and that of symbolic character ("Symbolfeld") — a not very suitable term — when things not being immediately present are being talked about. According to Bühler two groups of words would correspond semantically with these two fields: the personal and demonstrative pronouns would be indicators ("Zeigwörter"), all the others — except interjections — would have the function of representation ("Darstellung").

I cannot go into details, but I believe that Bühler's "Zeigfeld" is a metaphor and that the real art of indicating belongs only to gesture. Thus the personal and demonstrative pronouns are, above all, individualising situation-words which certainly have representational function. On the other hand, I consider it absolutely erroneous to call all the other words (apart from interjections) representing or naming ones without any differentiation. I need not dwell on this point any more after having already said enough about semantic gradation. For the rest, Bühler's intention to build up a language system in a kind of parallelism between form-groups (word-compound-sentence) and strictly corresponding semantic counterparts seems to me to be fallacious. And, last of all, there is one serious gap: Bühler has forgotten — besides his two fields of indicating and representation — the only really symbolic field, i.e. the imaginative field of poetry!

3. THE STRUCTURALIST CLASSIFICATION

The structural linguistic formulation of our problem seems to derive from de Saussure when he raises the question whether word-classes are to be classified according to logical (i.e. ontological and psychological) principles — he calls them extralinguistic which they are as long as they do not find linguistic expression — or according to their place in the morphological and syntactic sphere of language (word-form, group-form, syntactic valence). The respective passages in de Saussure's *Cours* are rather fragmentary and not unambiguous. One might even deduce from them that he took also sense-classes

(III) into consideration when, after having spoken of the associative clusters of case-forms, he continues "Des associations de même ordre, mais plus larges encore, relient tous les substantifs, tous les adjectifs, etc. et *fixent la notion* des parties du discours." (p. 190).

However that may be, modern structuralists try to arrive at word-classes in two different ways:

(1) By keeping, on principle, to word-form (I) and group-form (II): the Bloomfield-school. They proclaim that they would exclude semantics; thus Bloch and Trager (*Outline of Linguistic Analysis* §§ 4.6, 4.11): "all our classifications must be based exclusively on form — on differences and similarities in the phonemic structure of bases and affixes, or on the occurrence of words in the particular types of phrases and sentences. In making our classification there must be no appeal to meaning, to abstract logic, or to philosophy." We are reminded of Petrus Ramus.

(2) By keeping, on principle, to morphology (I, II) and to syntactic valence (IV); they intend to exclude word-sense (III). This opinion seems to prevail among the Danish circle; I hope that I understand them right and that I am not tilting at windmills.

Now my contention is the following: as soon as you pass the limit of senseless sound-combinations and enter the sphere of words i.e. of meaningful units, you cannot escape semantic aspects (neither IV nor III) because they are by the very nature of the linguistic sign bound up with it.

(A) THE AMERICAN STRUCTURALISTS

As far as they proclaim open opposition to mentalism such an attitude seems to me an illusion. Bloomfield himself, for whose work, on the whole, I have great respect, intends to keep to the formal side of language when describing a linguistic system. But in order to find out the word-classes he starts from syntactic group-forms (II), to which he assigns, as it were, hidden and vague semantic names (IV), and he deduces from them even the class-sense (III) of their leading constituents. He speaks of the actor-action construction ("John runs"), of the relation-axis ("beside me"), of the substance-character construction ("fresh milk"), of descriptive and limiting adjectives ("fresh milk," "this milk") of subordination

(" if he came " is called a modifier), and he gets from them the word-classes of nouns, verbs, adjectives, conjunctions etc. — classes which he sometimes defines by their seeming class-sense (III) e.g. a substantive denoting an object of such and such a species, a verb denoting action, an adjective the character of a species, and so on. Thus for English he formulates six word-classes with their old classic names. What else have we here but hidden psychological determinations and structures? I should like to call this hidden semantics, or, using a term suggested to me by Prof. Wrenn, crypto-semantics. We observe that syntactic valence (IV) and word-sense (III) stealthily creep in.

The most advanced American structuralist, Zellig Harris, goes far beyond Bloomfield and intends to keep to pure form (word-form I and group-form II): he speaks of phonemes, morphemes, their smaller and larger groups up to the utterance. Yet, I think, it is impossible to speak of a morpheme whatever you understand by this glittering term, (stem? affix? both together or separated?) without taking into account semantic aspects (lexical as well as syntactic ones). Harris tries to avoid the term " word," but he nevertheless uses it occasionally without any definition. The most obvious way, however, to hide semantics is the apparently formal method of substitution. If you substitute words and phrases by others to make out their equivalence, you cannot do so by simply comparing their group-form (II), you can only do so by comparing, first of all, their semantic value. How can you substitute e.g. an English adverb in *-ly* (" truly ") by another differently structured adverb (" very ") without having taken into account a vague similarity in their semantic value? How do you know at all that these word-types are adverbs? What about "yours very truly "?

All such substitutions, commutations or reductions, so favoured by modern system builders, are primarily nothing else but semantic operations. If one reduces a sentence like " The King of England opened Parliament " to a simple one like " John runs " in order to find out the syntactic group belonging together, you must have first of all found out that " The King of England " is the Subject-group! Then you may, if you really find it necessary, substitute " John " for it. In such a way, by means of crypto-semantics, Harris establishes

"morpheme"-classes for English to which he gives symbolic marks (N, A, V etc.) without any explanation. It is easy to find out that these symbols stand for nothing else but noun, adjective, verb etc.; that is to say: we have again our old classical terminology in hidden symbols, or word-classes defined by their word-form (I), group-form (II), lexical and syntactic values (III, IV). Last of all, the sentence types are again symbolised by the same letter symbols which are nothing else but our old classical friends.

When Prof. Fries in our *Preliminary Report* (above p. 178) protests against the view that the American structuralists are said to exclude semantics he is perfectly right. He himself (in his *Structure of English* where he arrives at four word-classes in English) does not do so and the others cannot; yet they try to do it in a hidden way. If one deprives language of its semantic side, one deprives it of its very essence.

(B) THE DANISH CIRCLE

They would like, so I understand, to exclude word-sense or lexical meaning. For the sake of completeness I mention Hjelmslev's earlier view as to word-classes (*Principes de grammaire générale*, 1928); I do not know whether he still holds this view. He tried to combine Jespersen's rank-system (primary, secondary, tertiary words) — which concerns first of all syntactic valence, yet is a mixture with lexical meaning — with a purely morphological aspect i.e. the nominal case-forms. Thus we have: subordination on the one hand, the possibility of case-endings on the other (IV, I). He arrives at five word-classes with their classical names which almost coincide with those established by Jespersen (*Philosophy of Grammar*). Yet, whatever one thinks of Jespersen's ranks, the point is that, whenever you speak of subordination, you must take into account lexical meaning (III). How should you know otherwise what is subordinated? E.g. "a blue ball", "a common ball". You cannot exclude word-sense.

Togeby in his *Structure immanente de la langue française* starts, when analysing the content of a text, from the proposition (which is very vaguely defined with reference to the modulation of a phrase), and he undertakes, by way of continuous subgrouping, to arrive at smaller units, and the word. The kernel of a proposition is for him S (subject) and P

(predicate). What does such a formula mean? How does one know at all of S and P? Probably from empirical examples of the text or living speech: there must be something which is made S, something which is made P. The relation between S - P (which is in its very nature an identification) is impossible without its members which as linguistic symbols must have also lexical meaning or word-sense (III). Such a formula hides: (1) a general lexical meaning of S and P, and (2) a generalised relation between the two; i.e. a semantic operation is always presupposed. And the same applies to all subgroups of the S-P *contenu* down to the word itself. It is an illusion to believe that grammatical abstractions and relations are real linguistic entities unless they have their foundation in concrete empirical terms, a fact stated by de Saussure himself. Relations alone can never exist without something which is related to something else. Togeby arrives at three chief word-classes: the noun (with a large number of subgroups), and the verb, both distinguished by their inflexions; then the indeclinable parts of speech, the particles, defined by their group-form and syntactic valence.

Looking back we observe that this classification resembles that of Sanctius in the sixteenth century.

The same aspects come to the foreground in recently published articles (in *Grammaire et Psychologie*, Paris 1950); the contributors are Buyssens, Fourquet, Martinet, Larochette. These articles especially discuss the nominal and verbal categories. These scholars are in agreement that word-classes differ in different languages, that they are not strictly delimitable sense-classes, that word-classes ought to be defined according to word-forms (I) and group-forms (II), partly according to syntactic valence (III). The authors differ slightly in the stress they put on these various items.

And nearly the same may be said of those contributions which have been sent in lately in answer to our question; I try to sum up very succinctly: Robins takes the standpoint of extreme American formalism (I, II); Haas advocates the consideration of morphological features together with syntactic valence, this latter aspect especially for the indeclinables (I, II, IV); a whole group (Buyssens, E. Otto, Matthews, Togeby) would put chief stress on syntactic valence (IV).

Haudricourt distinguishes between those parts of speech which are represented by large groups in the vocabulary (*nom*, *adjectif*, *verbe*, which he calls "sémantèmes"); the others (*nombres*, *pronoms*, *prépositions*), he would name "morphèmes." The word-class ought to be defined by the possible combinations of words with others in the discourse which probably means according to group-form (II) and syntactic valence (IV). He adds some interesting remarks on Vietnamese and Tahitian which I am unable to judge because I do not know these languages.

All modern attempts, as far as we have seen, flow in one way or another back again into the classical line and into the classical terminology.

4. CLASSIFICATION WITH MORPHOLOGICAL AND SEMANTIC ASPECTS

A last group of scholars is composed of those who, on principle, do not exclude any of our four aspects on the diagram, yet use them in different gradations, or more or less separately. Among them I should like to mention: Sütterlin (*Die deutsche Sprache der Gegenwart* 1910), Sweet (*New English Grammar*), Jespersen (*Philosophy of Grammar*), Bally (*Linguistique générale et linguistique française* 1944); last of all, de Groot (*Structural Linguistics and word-classes*, *Lingua* 1948) and Glinz (*Die Innere Form des Deutschen*, 1952).

Sütterlin saw clearly that no aspect of classification would in itself yield a satisfactory classing of words. Therefore he chose the way out of establishing three separate schemes: (1) According to word-form (I): declinables and indeclinables. (2) According to class-sense (III): "Erscheinungsbezeichnungen" (noun, adj., verb); "Beziehungsbezeichnungen" (quantity, relations: numerals, adverbs, preps., conjs.). (3) According to syntactic valence and group-form: full words and form-words. His general result is: four predominant types (subst., adj., verb, particle); the interjections express feelings.

Sweet, whose introductory part to his *New English Grammar* is still one of the best expositions of grammatical lore, and Jespersen, whose works are so well-known, hardly require a detailed mention. They have both some very interesting remarks on all our four aspects, also on lexical semantics. When it comes to a final classification of words, they keep more or less to the morphological aspect as the major term

of their scheme. Sweet distinguishes seven, Jespersen five word-classes; they use the classical terminology throughout. Jespersen concludes his survey with the following remark: "I therefore venture to maintain that the demarcation of these five classes (noun, verb, adj., pronoun, particles) is consonant with reason, though we are unable to define them so rigidly as to be left with no doubtful or borderline cases. Only we must beware of imagining that these classes are absolutely notional: they are grammatical classes and as such will vary to some extent — but only to some extent — from language to language. They may not fit such languages as Eskimo and Chinese."

Bally (§§ 175 ff.) starts from ontology, goes on to word-sense (III) and ends with syntactic valence (IV). According to him the lexical categories potentially denote substances (*choses et êtres*), qualities, processes, and modalities of quality (subst.: *Pierre, homme*; adj.: *rouge, bon*; verb: *marcher*; adv.: *bien, très*). These four classes are to be looked upon as ontologically justified. "They are predestined to be actualised in speech, yet they exist in language only as far as they are characterised by distinctive signs . . . If a language, like the Chinese, permits the use of one and the same word as a subst., adj., adv., verb, it is impossible to range it with any of those classes." "These lexical categories are further on characterised by their "valeur" (syntactic valence IV and group-form II?) and this "valeur" is inseparable from their function (IV). Thus an adjective is predestined to function as an epithet of a substantive, the finite verb does not exist without a subject . . ." These four word-classes are, according to Bally, the chief types based on their word-sense and syntactic valence. The rest are the ligaments (prep., conj., quantifiers) which have the function of grouping those four categories within the syntagma.

This result is almost identical with that of Sütterlin and not much different from Jespersen's scheme.

Separately stand de Groot and Glinz. De Groot's intention is to unite the morphological and semantic aspects and to establish for three languages (Chinese, Latin, Dutch) first the word-classes on morphological (I) and partly syntactic principles (II) and then to arrange them in, as he believes, consistent word-class systems by introducing Bühler's aspects of "Ausdruck" (expression) and "Darstellung" (representation) as well as his distinction of "Zeigwörter" (indicators) and "Nennwörter" (naming or representational words).

My first contention is: even expressive utterances are representational; e.g. *How lovely this is! What a beautiful view!* Secondly, Bühler's distinction between "Zeigwörter" and "Nennwörter" seems to me untenable as I have tried to hint at previously. Thus de Groot's semantic superstructure seems to me questionable. Yet it is some of his other axioms which I must dispute; he maintains: (1) the distinction of word-classes is a distinction based on the meaning of word-stems, and (2) A word-class system has a logical structure.

(Ad 1) In order to prove the first axiom de Groot starts from inflexional types such as Latin *hortus, horti; mensa, mensæ; amo, amamus; lego, legimus* and declares that these nominal and verbal paradigms respectively have the same meaning. What is meant here by meaning? Not lexical meaning (III), but syntactic valence (IV). Then suddenly the decisive turn appears when the author draws the conclusion that such inflexional systems, because of their similar syntactic valences (IV), may be equated with semantic stem-classes or stem-categories. Now we are no longer in the sphere of syntactic valence (IV), but in that of lexical or class-meaning (III). Meaning has changed its meaning. The conclusion is as follows: because the verbal paradigms show certain similarities in syntactic function (tenses, moods) therefore the stems of all verbs must form one semantic (i.e. lexical) category. What is this stem-meaning? It is vaguely defined by "process" (something taking place in a passage of time) which, however, must not be interpreted in a logical way, but linguistically, and then process simply means — according to de Groot — what is denoted by the verb in a given language! This seems to me a vicious circle: we are obliged to interpret a problem which we have approached in regard to semantics in regard to form, to morphology.

This confusion of meaning and form permeates the whole article; it occurs in the arrangement of his oppositional groups and subgroups where we again meet our classical terminology. And, to conclude, what about the different formal stem-categories among substantives, verbs, adjectives in Latin? They are, as it were, brushed aside by being declared unessential or morphological. Where is the evidence for this assertion? All in all, in my opinion this method of classification hardly leads to a satisfactory solution.

(Ad 2) The second axiom is that a word-class system has a logical structure.

Before dealing with this problem I will turn to Glinz's book *Die innere Form des Deutschen*. I cannot deal with this volume of five-hundred pages in detail; it must suffice to say that it is the most painstaking investigation into the system of one language. It is not my intention to consider Glinz's method critically, his way of going up and down the larger and smaller structures of German sentences, his finding out of word-types and word-classes by way of substitution, commutation and reduction in the most detailed manner and with a steadily growing new terminology of German compounds which are simply untranslatable. Our interest here will be concentrated on his result concerning word-classes. In his contribution Glinz maintains that word-classes cannot be derived from general semantic categories, that they must be discovered for each linguistic system, that they cannot be delimited for themselves alone, but only in connexion with word-forms, members of the sentence and the whole sentence structure. Two points are essential for my present purpose: (1) Glinz's scepticism in questions of psychology is not justified because he contradicts himself: his substitutions, reductions, commutations are, first of all, semantic (i.e. psychological) operations. His interpretations and attempts at defining word-classes are nothing else but attempts at descriptive psychology. (2) Glinz arrives at five word-classes to which he assigns the following terms: (a) *Vorgangswörter* (process-words - the verbs); (b) *Grössenwörter* (whose most important sub-group are the nouns); (c) *Artwörter* (qualifier: adj., adjectival adverbs); (d) *Stellwörter* (situation-words of local, temporal or subjective colour; not strictly delimitable); (e) *Fügwörter* (ligaments: preps., conj., many other particles; not to be defined as a uniform class).

None of these five classes is strictly delimitable; not even verbs because, as he himself remarks, they do not always denote processes. The nouns (*Grössennamen*) are questionable units; mass-words such as "milk", "gold", "iron" are uncountables and especially excel in not denoting limited objects. The qualifiers melt into the *Grössenwörter*. The *Stellwörter* and *Fügwörter* do not form morphological groups at all; they can only be defined as very vague functional

classes. In going down from the verbs to the ligaments the word-classes become more and more indefinite and almost dissolve in the linguistic air. Glinz himself is fully aware of this fact, and he comes to a conclusion which stands in extreme opposition to de Groot's assertion that a word-class system is a logical one (Glinz, p. 465 f.).

Where are we now? What is the result of all our considerations? A general failure? Where does the fault lie? Are the grammarians completely mistaken? I would answer: yes and no. Let me remind you of the saying of famous linguists (Paul, Jespersen, Bally, even Bloomfield) that an attempt to establish a fully consistent word-class system is a Utopian undertaking. And this is my last point: no word-class system of any language whatsoever is a perfectly harmonious, gapless unity or a strictly logical system; from whatever angle we approach the problem we shall always arrive at the same result: we can only define certain predominant types, be they morphological or semantic ones, whose spheres are not strictly delimitable, whose margins will fade away, grow hazy, merge or melt into one another. The word-forms (I) have their irregularities; the group-form (II) is not always decisive (cf. *we five: we strive*); syntactic valence (IV) leaves us in the lurch (cf. *he works hard: he is a hard worker*); the word-senses (III) do not allow of a strict categorisation. You can express the same content in manifold variations (cf. Jespersen, *Philosophy of Grammar*, p. 91).

Why is this so? Who is at fault? The grammarian is at fault if he forces his material by a neglect of the deviations from the typical; he is not at fault if he realises that he cannot construct a strictly logical system. No language whatsoever was created and transmitted on a preconceived logical plan; the speaking community has been and is always at work, intentionally for the moment yet planless for the future. No one of them overlooks the whole; the grammarian comes later and tries to get order into the primarily planless work which by custom, analogy and linguistic intercourse has gradually taken on features which make it appear as if it were a more or less planned structure. From this point of view the fault does not lie with the linguist, but with the systems of languages themselves, having been created not by philosophers, logicians, grammarians, but by practical and very often illogical speakers in everyday life.

Thus we must, whether we like it or not, deny the possibility of constructing a fully consistent word-class system; the idea of linguistic system must always be taken in a broad sense, it must not be exaggerated!

Nevertheless, one thing is striking: did we not meet certain categories again and again? e.g. noun, verb, pronouns, etc. They appear again and again in whatever hidden or marked form. Has not already Aristotle stressed the predominant valence of noun and verb in a statement? Have not scholars (Vendryès, Meillet) attempted to reduce the word-classes to noun and verb as the most essential ones? Has not Haudricourt in his contribution stated that even in languages of strange structures (the Vietnamese, the Tahitian), types corresponding to the Indo-European nominal and verbal ones might be discovered? Why is this so?

The reason, so it seems to me, is not so far to seek: there are psychic structures common to all human beings whatsoever: representations and conceptual capability, ("Vorstellungen" and "Begriffsbildung") judgments and emotions (feelings, willing); or: the faculty of abstract thought, of saying yes or no, of loving or hating. The simplest drawings in prehistoric caves, the simplest tool of stone or iron pre-suppose these psychic phenomena. A man must have some idea before he starts working; he must be able to judge of the material he uses, he must prefer one way of working to another. Therefore any language whatsoever must and will find means to express and communicate these various fundamental mental phenomena. The ways and methods may differ widely — we who know best the Indo-European type are accustomed to attribute the sphere of conceptual thought to the nominal sphere, the sphere of our judgments to the verbal one, whereas the emotions may find expression with both spheres by form and meaning. Yet noun and verb are not necessary formal means, and we find in a language like the Chinese, as Karlgren expressly remarks, the same psychological categories as in the languages best known to us.

5. CONCLUSION

I do not think I need dwell any longer on the question of nomenclature.

Most modern linguists prefer the retention of the classical terminology because of its ancient standing, its international

currency and practical usefulness. In spite of all its shortcomings it contains some basic truths. When used with certain provisos and the necessary warnings the old terms will still serve their turn until something better has been devised and generally acknowledged.

A few scholars try to avoid it and would prefer to substitute it either by phonematic terms or by a newly created terminology taken from the vocabulary of the respective language. Yet they must be aware of the fact that new terms, especially of the compound type, bring not only new difficulties, but also impede mutual understanding and isolate at the same time their own research work.

The greatest evil in linguistics is and will be ambiguous, glittering and sophisticated terminology.

O. FUNKE

The subject to be discussed at to-day's meeting is: On what principle are word-classes to be defined and named? It will be seen that the question is phrased ethically, and since my own ethical philosophy, if I have any, is distinctly utilitarian in tendency, I will attempt to answer the question along those lines. I think it will be admitted that any intelligent description of a foreign language is in essence an implicit comparison between that language and one's own. Now such a comparison, in order to be effective, pre-supposes the use of those technical terms to which one is accustomed, those which one learned at school. If one goes abroad and buys food or wine or clothes at the prices charged in the country in question, but without reference to the rate of exchange, one will soon find oneself in a scrape. My answer to our question then is: Use the traditional classical terms so far as they prove usable, and introduce innovations only insofar as they are absolutely necessary. It is because I hold this view that I greatly welcomed it when my friend M. Gustave Lefebvre wrote an Egyptian grammar which in the views it expresses is almost identical with my own, but was given a terminology and a form more in accordance with French pedagogic usage. If a Japanese scholar were to write an Egyptian grammar, I think it ought to employ Japanese grammatical terms. A year or two ago an eminent linguist was reported to me as having said that the Arunta

tribe of Central Australia had only one part of speech and that a gerund. I admit that it would be rather hard for an Arunta tribesman to write a grammar of Egyptian, but we need hardly consider such a predicament.

In compiling my Egyptian Grammar I found that on the whole our own grammatical categories served my purpose well enough, and in the few cases where they did not, it was quite easy to invent suitable and unambiguous terms. Thus Egyptian has a negative verb which is followed by a special verb-form which I called the negatival complement: *ir tm.f sdmw md̄t tn* might be imitated in English by "Is nots he hearing this word"? Again, Egyptian prepositions have mostly developed an adjectival form to agree in gender and number with a preceding noun, like say *casA meA inA rure*: "my cottage in the country". It suffices to call this an adjective derived from a preposition or a prepositional adjective.

Unhappily the modern tendency is to invent entire new systems of terms without troubling overmuch to see whether they are applicable practically. I submit that this mode of procedure is unscientific. The proper procedure is to analyse your facts as and when you find them and to invent new terms according to the requirements of the case, and without preconceived notions.

But the question remains, what account are we to give of the terms we already possess and of those which we may feel compelled to add to them? I have written a good deal on this subject in my book *The Theory of Language and Speech* and will not weary you with a repetition of my views. I was glad to see that our rapporteur, my friend Professor Funke, quoted with some degree of approval my definition of a noun. I must, however, continue my contribution to this discussion by a few seemingly very dogmatic contentions. I maintain that to try to define word-classes without reference to meaning is to talk arrant nonsense. It seems to me almost as bad to attempt to define them in reference solely to outer form. Our English word *good*, though originating in inflected forms like *guter, gute, gutes*, itself possesses no differentiating outer form, but does not cease to be an adjective on that account. To talk of words like prepositions as having no independent meaning also seems to me a mistake; would anyone maintain that the difference between German *in* and German *an* is merely the

difference of the vowels? I do not much like the term *outils grammaticaux*; it may have some justification in the French *de* and in the obligatory use of the articles in certain languages, but even in these words there is a residuum of meaning. Again, I do not see how the word-classes can be defined merely according to syntactic function; in the sentence "Jack found Jill sitting in his aunt's chair" the four nouns have four different syntactic functions. So too most adjectives can be used congruently either as epithet or predicate. It is true, of course, that function in connected speech was the historical source of the distinctions between the parts of speech; for that truth the very name *partes orationis* "parts of speech" is sufficient guarantee. But it may be fairly argued that differences of syntactic function are not the *raison d'être* for the distinctions between the word-classes, but are themselves consequences of deeper-lying differences of semantic status. To that extent Brøndal seems to me to have been upon the right track, for the factors which he assumes (substance, description, quality and relation, if I remember rightly) do seem something like an exhaustive statement of the ways in which we can conceive of our concepts. But I do not think that a system of parts of speech built up upon that basis will be found adequate to the needs of any actual language. I repeat that the only serviceable basis is that which results from a careful examination and analysis of each separate language in turn. It will then be found that the word-classes are by no means mutually exclusive or a logically sound classification. As M. Vendryès reminded us, there are nominal parts of the verb, like the infinitives and participles in Arabic and Egyptian, while many substantives like "murderer" or "solution" have a clear verbal element in their make-up.

In conclusion, I should like to call attention to a type of word-classification which cuts across that underlying the commonly recognized parts of speech, which is in fact utilized to some extent in our grammars, but the principle governing which is perhaps not so clearly recognized as it should be. One may, I think, make a dichotomy of all words into those which have inherent sentence-quality and those that have it not. The latter class is of course far the larger, but the former class is very interesting. The commonest forms are interjections and interrogative words, but also the copula in

modern European languages has definite statement-value, whether in actual affirmation or in sentences where the statement is called in question. To that kind of classification belong also all those words which indicate the attitude of the speaker towards what is said, words like *not*, *doubtless*, *assuredly*, *moreover*, *but*, *however*, and so forth.

ALAN H. GARDINER

Ich möchte Ihnen in möglichst anschaulicher Weise ein Stücklein des Weges zeigen, der mich in meinem Neubau der deutschen Grammatik zu den "Kategorien zwischen Satz und Wort," d.h. zu den Satzgliedern, Wortformen und Wortarten geführt hat. Es ist dabei keine Willkür, wenn ich das Problem "Wortarten" durch "Wortformen" und "Satzglieder" erweitere: die Einheiten aller drei Aspekte sind so miteinander verflochten, dass man nur dann auch einen einzelnen von ihnen richtig denken kann, wenn man die beiden andern dazu nimmt.

Doch sollen uns die theoretischen Grundlagen nicht mehr länger aufhalten. Der Bericht von Prof. Funke, die andern Beiträge, und die verschiedenen Diskussionen haben mir zu meiner grossen Freude gezeigt, dass hier — Willkür des Zeichens, Schlüsselnatur, synchronische Arbeit, Ausgehen von der Form, jedoch mit steter Beobachtung der Bedeutung — trotz oft verschiedener Ausdrucksweise eine weitgehende Einigkeit besteht (see above, pp. 33-34).

Die drei Punkte Jespersens gleich als Leitlinie; nur in umgekehrter Reihenfolge:

Syntactic use	=	Satzfunktion (Gliederfunktion)
morphology	=	Wortform
meaning	=	Grundprägung → Wortart

Die auf den Tafeln dargestellten Experimente, d.h. Formänderungen führen uns zu einem ersten festen Punkt: Wir finden ein Stück, das nur einwortig ersetzbar ist und das, wenn es überhaupt in einem Satze vorkommt, einen ganz bestimmten Platz beansprucht: den zweiten, letzten oder ersten, wenn nicht nach Wörtern sondern nach umstellbaren Gliedern gemessen wird. (Syntactic use). Ferner haben alle Stücke dieser Art ganz bestimmte Möglichkeiten, ihre Form und damit auch

ihre Bedeutung teilweise zu verändern (morphology). Wir finden die Reihen "beschreibt, beschreibe, beschriebe, beschrieb" "ist, sei, wäre, war." Erst jetzt gehen wir von den beiden Ebenen Syntax und Morphology, wo die Resultate experimentell gewonnen und gesichert sind, zur dritten, zur Meaning. Wir interpretieren die Ergebnisse von 1 und 2: All diese Stricke prägen ihren Inhalt, so verschieden er an sich sein mag, als etwas Verlaufendes, etwas in die Zeit gebettetes, und in ihr bestimmtes, als einen Prozess, ein Geschehen.

Damit haben wir aus dem Satzglied (zweiter, letzter, erster Platz) und der Wortformenreihe eine Wortart gefunden: eine gemeinsame Grundprägung an sich ganz verschiedener Einzelinhalte. Wir haben das finite Verb gefunden (Wert der Ableitung, wenn jemand sie unnötig findet, Abtrennung der umschriebenen Formen, Sonderstellung des Imperatifs).

Einteilung der Sätze nach der Stellung dieses Zentralgliedes.

Von hier aus gehen wir nun weiter. Wir fragen: gibt es noch andere Stricke, die festen Platz haben, nur einwortig ersetzbar sind oder andere Gesetzmässigkeiten zeigen? Dabei dient uns das finite Verb gewissermassen als Modell.

Wir finden tatsächlich weitere platzfeste Stricke: Obschon, obwohl, weil, wenn, warum, wie, in Tafel 2, in Sätzen, die das finite Verb am Ende zeigen. Aber die Gegenprobe stimmt nicht für alle.

nur als Spitze mit	auch als Spitzen mit
finitem Verb am Schluss:	finitem Verb an zweiter Stelle:
wenn, weil, obwohl	warum, was, wie
	wer, usw.

Hier trennt sich also Satzglied und Wortart.

Die Satzfunktion "Spitze eines Satzes, die das finite Verb an den Schluss weist" kann durch Angehörige verschiedener Wortarten erfüllt werden: durch eine besonders zu diesem Zwecke geschaffene ("subordinierende Konjunktionen" weil, wenn, obgleich, und "Relativpronomina und -Adverbia": wo, wann, wie, wer, welcher, der, das, usw.). Innerhalb dieser letzteren finden wir ganz verschiedene Gruppen von Formveränderungen: wer/wessen/wem/wen, wofür, womit, wozu. dafür, damit, dazu usw. Die Gruppen haben keine scharfen

Ränder, überschneiden sich zum Teil; sie dienen in ganz verschiedenen Satzfunktionen, und wir können sie daher erst später interpretieren, wenn noch mehr Anhaltspunkte da sind. Methodisches Ergebnis für den Moment: Satzfunktion, Wortform und Wortart (Grundprägung), die wir beim finiten Verb so schön vereint gefunden hatten, treten auseinander. So sieht der Anfang des Weges aus, durch dessen konsequente Verfolgung ich am Ende den gesamten Band der Wortarten, Wortformen, Satzglieder und Sätze im Deutschen gefunden habe, wie er in meinem Buche dargestellt ist. Er ist nicht sehr bequem, dieser Weg. Er scheint oft in ganz anderer Richtung zu führen, als man möchte, und bietet manche unangenehme Überraschung. Aber ich glaube, dass er der sicherste wissenschaftliche, kontrollierbare Weg ist, den wir, wenigstens heute, zur Verfügung haben.

A.

Später beschrieb er diese wichtige Beobachtung genau.

Diese wichtige Beobachtung beschrieb er später genau.

Er beschrieb später diese wichtige Beobachtung genau.

(Man hofft),	Er beschreibt	"	"	"	"	"	
	er beschreibe	"	"	"	"	"	
	Er untersucht	"	"	"	"	"	
	Er verfolgt	"	"	"	"	"	
	Er prüft	später	diese	wichtige	Beobachtung	genau	nach.
	Er wird	{	"	"	"	"	{ beschreiben
	soll		"	"	"	"	{ untersuchen
	kann	}	"	"	"	"	{ nachprüfen
	Er hat		"	"	"	"	
	hatte	{	"	"	"	"	{ beschrieben
	habe		"	"	"	"	{ untersucht
	hätte	}	"	"	"	"	{ nachgeprüft

B.

Obwohl er später diese wichtige Beobachtung genau

beschrieb,

Obwohl er später diese wichtige Beobachtung genau

beschrieb, . . . usw.

Obschon " " " " " " " "

beschreiben wollte, " "

Weil " " " " " " " "

beschrieben hat, . . .

Wie " " " " " " " "

Warum " " " " " " " "

Wofür " " " " " " " "

Für welchen Zweck " " " " " " " "

C.

Beschrieb er später diese wichtige Beobachtung genau?

Prüfte " " " " " " " nach?

HANS GLINZ

Was ein Wort ist, ist schwer, vielleicht unmöglich, zu sagen; aber wir haben eine gewisse Empfindung dafür, die ich etwa so

formulieren möchte, dass ein Wort eine Laut- und Bedeutungseinheit innerhalb eines Redeganzes ist, die man ebenso oder ähnlich in einem andren Redeganzes wiederfinden kann.

Diese Laut- und Bedeutungseinheiten können eine Sachbedeutung haben; sie können Personen, Vorgänge, Zustände und schliesslich Beziehungen, Relationen verschiedener Art andeuten. Wie sehr im letzten Fall eine mehr oder weniger reine Beziehungsbedeutung ein Wort hat, erschliesst sich mir im Zusammenhang des Satzes, des Redeganzes, das ja ein Sinnganzes ist; das hat schon vorgestern in einer Diskussion mein Freund Professor Otto ausgesprochen. Dabei spielt die syntaktische Rolle, die das Wort im Satze hat, entscheidend mit, seine Beziehung zu den anderen Wörtern, seine Betonung etc. Nehmen wir ein ganz einfaches Beispiel: "Er ist zu Hause," und "Das Fenster ist zu". Im ersten Satz hat das unbetonte "zu" nur Beziehungsbedeutung, ist eine Präposition; im zweiten ist "zu" betont und bezeichnet einen Zustand, parallel zu dem englischen. Partizip in "the door is shut."

In manchen Sprachen hilft uns die Flexion zur Einordnung der Wörter in Klassen; in sehr vielen aber nicht; trotzdem kann man hier ebenso von Wortklassen sprechen, eben auf Grund der syntaktischen Funktion, die das Wort von Fall zu Fall haben kann.

W. PREUSLER

Wir sind Prof. Funke aufrichtig dankbar dafür, dass er das vorliegende Problem in einem umfassenden Rahmen behandelt hat, der allerdings noch etwas abzuändern wäre. Damit wird die Schwäche einseitiger Auffassungen offenbar.

Persönlich fühle ich mich zu besonderem Dank verpflichtet, da Prof. Funke die weniger zutreffende Darstellung (see above, pp. 29-34) nunmehr abgeändert hat.

Aber das Problem der Wortklassen muss wohl noch schärfer abgegrenzt werden:

(I) Dieses Problem fällt in das Gebiet der Syntax, nicht in das der Wortlehre (Bedeutungslehre). Daraus folgt:

(II) Wenn die Wortklasse ein syntaktisches Beziehungsmittel ist, dann ist es auch gleichwertig mit den anderen Beziehungsmitteln: Flexion, Wortstellung, dem musikalischen und dynamischen Akzent. Ich sage gleichwertig, nicht gleichartig.

(III) Welches ist nun die besondere Eigenart der Wortklasse?

1. Ihre Entstehungsquelle ist die Umwelt, wie sie vom Menschen vorgestellt wird: Wir bemerken in unserer Umwelt sowohl Gegenstände und deren Eigenschaften als auch Vorgänge und zwar in bestimmten Relationen (Ort und Zeit). Diese kategorialen Gegebenheiten werden sprachlich nachgebildet und bestimmen damit die Struktur der Satzgruppen: die Gegenstände (Personen) der Umwelt erscheinen als Gegenstandswörter, die Eigenschaften als Eigenschaftswörter, die Vorgänge als Vorgangswörter und die Verhältnisse als Verhältniswörter. Dafür werden die Vertreter der Logistik kaum Verständnis aufbringen.
2. Es werden also nur die Kategorien sprachlich nachgebildet, nicht die Begriffsgehalte.
3. Demnach scheint es nur vier Kategorien der angeschauten Wirklichkeit, also auch nur vier Wortklassen zu geben: Gegenstands -, Eigenschafts -, Vorgangs - und Relationswörter (Verhältniswörter).

Denn die Adverbien drücken Eigenschaften der Vorgänge aus. Die Pronomina sind teils Substantiva, teils Adjektiva. Der Artikel ist ein Gliedwort (Präfix) der Substantiva. Die Numeralia sind Eigenschaftswörter der Quantität. Die Interjektionen sind keine Wortklassen.

4. Die fundamentalen Wortklassen können sich in dieser oder jener Sprache noch sprachlich differenzieren, wie z.B. das Adverbium neben dem Adjektivum. In diesen Grundzügen stimme ich mit meinem Freunde Dr. Preusler überein.
5. Die Geschichte der Wortklassen kennt 2, 8, 10 und mehr Klassen von Wörtern; aber erst, wenn man die Wortklassen und die Kategorien der Wirklichkeit zurückführt, kommt man zu einem wirklichen Einteilungsprinzip der fundamentalen Wortklassen.
6. Die Bennennungen der Wortklassen sind Substantiv, Adjektiv, Verb und Relationswort (relational word).

Literatur:

Zur Grundlegung der Sprachwissenschaft, Bielefeld, 1919.

"Sprache und Sprachbetrachtung," *Abhandlung der Akademie der Wissenschaft*, Prag, 1942.

E. OTTO

Je voudrais d'abord préciser un point de la réponse que j'avais envoyée au questionnaire (see above p. 35); car la façon dont M. Funke a classé ma réponse montre que je n'ai pas été assez explicite.

Notre rapporteur classe toutes les réponses en trois catégories: (1) celles qui ne voient que le sens, (2) celles qui ne voient que les caractéristiques formelles, (3) celles qui combinent le sens et la forme. Je figure dans la deuxième alors que j'appartiens à la troisième.

En effet je fais appel à la syntaxe et pour moi la syntaxe n'est pas une affaire de pure forme. Lorsque par exemple je dis que tel mot remplit la fonction de prédicat, je suis dans le domaine de la syntaxe, j'étudie le rapport entre deux mots faisant partie d'une même proposition: la forme et le sens sont pris en considération. J'adopte donc un point de vue très proche de celui du rapporteur. Les oppositions de sens sont à la base de toute la linguistique; je ne dis pas le sens, mais les oppositions de sens.

Où je me sépare de M. Funke c'est quand il dit que le système des Parties du Discours n'est plus bon parce qu'il a été conçu pour les langues flexionnelles.

Il faut distinguer deux choses: d'une part, les faits grammaticaux appelés Parties du Discours; d'autre part, les définitions des Parties du Discours. Les définitions anciennes sont certainement désuètes, mais les faits restent les faits. Il suffit de définir les Parties du Discours syntaxiquement pour être en accord avec les faits. Et l'on constate alors que notre système des Parties du Discours est valable dans ses grandes lignes pour beaucoup de langues; c'est la conclusion à laquelle sont arrivés les sinologues.

Mais il y a une remarque très importante à faire. Le système des Parties du Discours est toujours présenté sous une forme simplifiée qui en fausse un peu la vérité.

Il est faux de dire que tout mot d'une langue comme le français appartient exactement à l'une ou l'autre Partie du Discours; à côté des mots qui réalisent pleinement la définition de l'une ou l'autre partie du discours, il y a des parties du discours imparfaites, défectives. Par exemple, le pronom français *on* ne peut exercer que la fonction de sujet, alors que les autres pronoms peuvent remplir bien d'autres fonctions: c'est un pronom défectif. De même — chacun le sait — il y

a des verbes défectifs. Mais l'existence d'un ou plusieurs verbes défectifs n'a jamais incité personne à rejeter l'idée d'une conjugaison; de même le fait qu'il existe des parties du discours défectives ne doit pas nous inciter à rejeter le système. Ce système est un ensemble de tendances qui se réalisent diversement.

Je conclurai en disant que si le système des Parties du Discours reste en faveur, ce n'est pas simplement pour sa valeur pratique; c'est parce que cette valeur pratique repose sur une valeur scientifique. S'il a un peu de flou c'est précisément parce qu'il est conforme à la réalité.

Je ne puis pas admettre qu'un mot ait un sens par lui-même. Un mot n'a de sens que dans une phrase; bien plus, il n'est un mot que dans une phrase. Je prends l'exemple du mot *a* en français; je n'ai le droit de dire que *a* est un mot que si je le prouve au moyen de phrases; p. ex. *il a un ami* — ici le premier *a* est un mot, le second n'en est pas un.

L'appel au sens en dehors de toute phrase est dangereux. Si on isole l'article défini on est fort embarrassé de lui donner un sens; est-ce un mot ou un pseudo-mot? On est embarrassé parce que l'article n'a pas de synonyme, et que nous sommes habitués à énoncer le sens d'un mot. Mais si nous opposons la phrase *Le voleur est entré* et *Un voleur est entré* nous constatons que la différence de forme correspond à une différence de signification; cela prouve que l'article a un sens.

Ce que je retiens de ces faits c'est que le sens d'un élément n'existe que dans la phrase, en vertu de la phrase. Le mot n'a pas de sens par lui-même. C'est une des raisons pour lesquelles je n'aime pas les notions de sémantème et morphème.

E. BUYSENS

(1) This is one of the most important questions raised at this Congress. It has the appearance of being a simple question of synchronic grammar, but in fact it encompasses the entire development of language with many transitional phases, few of which are completely finished. Therefore, the problem is to find principles common to all languages. The search for basic principles should be carried on even to-day, although much time has elapsed since the study of languages began.

(2) This is one of those questions which the traditional terminology constantly shelved. Greek or Latin terminology, together with the fundamental concept of word-classes, corresponded, to some extent, to the structure of Latin and Greek in which words differed considerably in their outward form. It required that words drawn from speech (i.e. sentences or syntagmas) should be divided into word-classes. Thus we obtained nine or ten groups of words which have been retained, more or less, in our grammars. We analyze all other languages on the basis of these word-classes, and sometimes describe their characteristics very inaccurately. For instance we say that Chinese has only "full words" and "particles," although we are aware that all word-classes can be expressed in Chinese as in our languages.

(3) It is high time to free ourselves from such an approach to the nature of words. For many decades the belief has prevailed that language represents speech and that all is performed in speech (that is in syntagmas and sentences). The word-classes themselves are in many European languages appropriately called the parts of speech, *les parties du discours*, *časti rječī* and so on, yet speech, *discours* or *rječ'* is not taken as their basis, but they are considered independently of the whole whose part they are, i.e. of the sentence.

(4) If we take speech, i.e. a sentence in which speech is displayed, we can easily see that in all languages the categories are approximately the same: categories of subjects, attribute-adjectives, pronominal categories, adverbial, predicative-verbal, prepositions, conjunctions, numerals, articles, interjections. But the question arises: in what manner is a category revealed? We can see that there are languages in which function is sufficient to determine word-classes in a sentence (or syntagma). The word *round* in English may be a noun, an adjective, a verb, an adverb and a preposition. Besides the function, some fixed or facultative elements are added to words to mark a word-class. Compare in Chinese the particle *šə* as a prefix to mark the predicate, with suffixed *tī* to mark the attribute, and so on. Suffixes are added to express different syntactic functions: e.g. adjectival, adverbial, verbal suffixes. Finally various complexes of sounds can be used to indicate different functions in a sentence. Compare in Basque various words for nouns, adjectives etc.; also different stems in the Indo-European languages.

(5) Thus we come to the structural or functional analysis of words. But this is not sufficient. For the analysis of various words in a sentence it is necessary to pay attention to their meaning. So we can distinguish between the words used subjectively, words which have the meaning of objects (nouns) or words which are made objective from adjectives and similar words, and those having only the function of independent words (e.g. personal pronouns); and the attributive words, those meaning a quality or characterizing objects through some external features (pronominal words); numerals — determinative words according to the number of units referred to, etc.

(6) The basis for every division of words in speech are their formal characteristics and their meaning. In the literal sense of the word, these would be the parts of speech. But how could we meet the requirements of our grammar by giving parts of speech as separate categories, divided into groups? In other words, the question is: which words of different syntactic categories have acquired such independent forms that they always have their specific pattern. This depends upon the language. There are languages in which this is the case (cf. the classical Indo-European languages, the Slavonic languages and other Indo-European languages to a larger or smaller extent); but there are languages to which this rule could not be applied. In Chinese there are, as already mentioned, only two word-classes. Neither of these corresponds to our word classes. They are drawn directly from speech, and therefore we cannot characterize them in any other way. The first class comprises, without any doubt, nominal, adjectival and verbal conceptual words, the second one many other categories: prepositions, conjunctions, particles and so on. Between Chinese and for instance some Indo-European language, such as Serbian, there is a whole series of languages with an unequal number of word-classes.

(7) What inference could we draw from this exposition which would supply an answer to our question?

(a) First, the real division of words: word-classes must be defined, in the first place, as real parts of speech, i.e. determined in a language as parts of speech.

(b) Secondly, the formal division of words: words must be defined as word-classes. That is, separate word-categories marked with invariable symbols for the given meaning and use.

The first division will be fundamentally equal in all languages, and will differ only in the symbols which are used for it. The second one will vary considerably in different languages. This would always be the characteristic of the state of a certain language in this respect for the time for which such definitions are given.

Naturally, it will not be easy to segregate from the large number of means for the definition of functions in a sentence those that frame separate words deserving to be called word-classes, for, undoubtedly, not all languages have word-classes. Bloomfield was quite right in saying of the English language "a system of parts of speech in a language like English cannot be set up in a fully satisfactory way: our list of parts of speech will depend upon which functions we take to be the most important" (*Language*, 1950, p. 209). This would be true in those cases in which a word can have several functions, and in English nearly every word can have at least two, and sometimes three, four, even five uses. Yet, even there we could find a method to separate nouns from verbs, adjectives from adverbs, and so on. All this would considerably facilitate the understanding of languages. It will be clear that expressions such as *in the house*, *on the lawn*, *in such a way*, *about two hours*, etc. and real adverbs such as *here*, *there*, *so*, *quickly*, and others with a similar meaning represent the same category, although formally the real adverbs are represented only by the words *here*, *there*, etc. There is only one step between these expressions and real adverbs. If the expressions mentioned represent adverbial syntagmas and the real adverbs separate words, both have the semantic function of real adverbial syntagmas. For instance, *here* and *at this place* mean the same. All this would disturb harmonious systems of word-classes which have been imagined in many languages, although they do not exist there, as against those languages in which they do exist; yet they would bring us considerably nearer to a true understanding of languages. Of course, on the other hand, this would help to determine thoroughly the functions of languages themselves. We shall see that the independence of the subject in a sentence, and every independent use of words leads to substantivity and consequently either to a real noun (object or word made such by its function) or words with nominal function (cf. *who*, *this*, etc.). One will understand then, what

the real independent function in speech is, and what invariably accompanies it.

Somebody will probably remark that here syntax is mixed with morphology, but in fact it is so in language. This would be neither "Misch-Syntax" for which John Ries reproached Miklosich, nor "Misch-Morphologie" for which we reproach Ries.

A. BELIĆ

My first point concerns the naming of word-classes. I think that when traditional names are used some care should be taken to see that the class so named has the functions — symbolizes in the mode — which the name would lead us to expect. Some of those who insist on formal procedures seem to me to be rather reckless in their naming of their classes. I would plead for due solemnity at christenings.

In the second place I should like to point out that even in the classification of words the analysis of the language functions in *The Meaning of Meaning* is sometimes relevant. Japanese, for instance, has different words with the same sense but showing different degrees of respect to the referent, and others with the same sense but showing different degrees of respect to the hearer. Here part of the Ogden-Richards scheme is directly applicable at least to the setting up of sub-classes. And in that connexion I might add that the hierarchy of main and subordinate classes does not necessarily reflect degrees of importance. You may find in some languages that only when the words in two primary classes work together do they produce a symbolization in a certain mode, and it may be better to elaborate the explanation of the function of such words in combination than to say much about the function of the class itself.

This leads on to my third point, which is that it is well to be clear what you are making your classification for. I do not believe in theoretical as distinct from practical purposes. Languages are described not for description's sake, but so that they may be taught to foreigners for instance, or used more efficiently, or compared with other languages. I see no necessary reason why one system of classification should be ideal for all purposes, and a good many difficulties may be avoided by keeping your purpose clearly in view.

F. J. DANIELS

I have recently investigated various classifications of fish. I find the following six:

- (1) edible — inedible
- (2) mediterranean — others
- (3) over 3 inches long — 3 inches long or smaller
- (4) those whose species-name starts with " A " . . . with " B " etc.
- (5) those which eat other fish but are eaten by no other fish—
those which are eaten by other fish but eat no other fish—
those which do both—
those which do neither
- (6) blimpsy — blumpsy — bloompsy

My first impulse was, of course, to ask myself which of these classifications was the correct one. But pretty soon I checked this impulse and looked instead into the background of the classifications. And then I found that the man who proffered classification No.—

- (1) was an economist.
- (2) was an Italian.
- (3) was the famous Eddingtonian fisherman.
- (4) was a lexicographer
- (5) was an ecologist
- (6) was a philosopher who claimed that his classification was the only correct one on intuitive self-evident grounds.

(I think there will be no great trouble to find some analogies between the principles underlying these classifications and those mentioned by Prof. Funke.)

So far, so good. But then I came across a few people who tried to prove that some of these classifications must, by necessity, coincide, that e.g. all edible fish are mediterranean and that all fishes over 3 inches long are blumpsy. Their arguments looked, however, somewhat peculiar and not convincing. Of course, it could have happened this way, but I tend to believe that such an occurrence would have been a curious coincidence indeed.

Is not the aim of those linguists who try to define word-classes by using simultaneously morphological, syntactical, and semantic criteria of a similar queerness? Is it not initially extremely implausible that the class of those words that belong to a certain paradigm, e.g. the class of the English words that

appear with or without an "s" at the end, the so-called nouns, should also have some interesting syntactic feature in common or designate an interesting class of entities? The sooner we give up these antiquated pre-conceptions, the quicker will linguistics become an empirical science.

By the way, what is the logical classification of fish? If by a logical classification one means one according to which two fish will belong to the same class if and only if they have all properties in common, then, according to a well-known principle of Leibniz, there exists exactly one logical classification, that which puts each fish into a separate class. This does not seem very helpful. Neither does it seem very helpful to look for a logical classification of words. The only criteria by which a proposed scheme by classification can be judged is that of its effectiveness in attaining a certain fixed aim. In this, I find myself in full agreement with Prof. Daniels.

Y. BAR-HILLEL

The definition of word-classes would presuppose the definition of "word" and their naming would depend to some extent on this. If we accept "word" in the sense of an autonomous semantic unit of language, however artless this concept may seem, we can choose our terminology for the most part from the vocabulary with which classical linguistics has endowed us. If, on the other hand, we prefer to see it exclusively in context, defined by its associations with other "words," we shall have to introduce certain modifications into our terminology, which however must aim in all cases at being comprehensible.

The correlation of form and meaning, no matter to what extent we may obscure it with pet terms, is fundamental to language, and its recognition must precede any attempt at linguistic analysis. There can be no form in language without meaning and *vice versa*. This granted — and even the pure formalist who pretends to make no appeal to meaning must admit that he cannot have knowledge of a language without such an appeal, — our task of classification is considerably lightened by our awareness that we have not to make the exclusive choice of form or meaning and therefore to set up mutually exclusive structural or semantic categories.

In studying the structure of an unwritten language the investigator inevitably begins with phonetic patterns, or form in its phonological aspect, but from the first he comes to associate his physiological impressions, passive (acoustic) and active (articulatory), with semantic values, and the correlation of the two assists him later as the result of extensive experiment, to piece together his picture, or total impression, of the design of the language. It will be his own picture, which subsequent research may modify very considerably. Where, on the contrary, a language has a long written history and has been repeatedly described, if only in traditional terms, the exclusively formalistic method is plainly artificial, especially if the language studied is the investigator's mother tongue (cf. Trager and Smith, *Outline of English Structure*). All that such a method implies in practice is the generalisation of a technique of investigating previously unwritten languages so as to include the investigation of written languages. This is a reversal of the common and inevitable habit of applying the classification of a familiar language, usually the investigator's own, to the data of an unfamiliar language. Our difficulties in setting up word-classes will now be obvious. To achieve uniformity of reference we appeal to logic, psychology, anthropology, sociology, and what not, to provide us with suitable criteria for sorting out and labelling linguistic phenomena. Or, if we realise the dangers of such a procedure, we may fly to the opposite extreme of asemantic formalism, which can never be achieved in practice, because language is not pure form. But a uniform method is surely desirable in our study of languages, and no doubt its desirability will appeal to those who prefer to be systematic. Such a method will in all likelihood emerge in due course as our knowledge of extant language-structures increases. At the present time however, in spite of the vast accumulation of linguistic material, our word-classes, determined by the conjoint application of formal and semantic criteria, must aim at a maximum of elasticity by avoiding the very real danger of too dogmatic definition. Another danger, to which I have alluded above (see p. 43) is that of jargon for jargon's sake. This is, as we know, a feature of a great deal of modern linguistic writing. Unfortunately for such writing accuracy does not lie in hair-splitting subtleties and truth never took refuge in obscurity.

W. K. MATTHEWS

Allow me, first of all, to express my grief, that at this congress one branch of languages is not represented too well — to say the least, though those languages have been studied all over Europe for hundreds of years. It is to Semitic languages that I refer. In our days, when the analysis of obscure American-Indian dialects seems to have become a must in linguistics, these languages through which immense cultural values have been formed, have become a kind of step-child to linguistics.

I therefore take the liberty of basing my remarks on our problems in these languages — drawing especially upon Hebrew and modern Arabic.

Well over a thousand years before the formal classification of words was so much talked about, the Arab grammarians grasped the problem quite clearly. It is still an open question, if it was the Greek classification into *onoma*, *rhēma*, and *syndesmos*, that produced the Arab classification into *'ism*, *fil* and *harf* — i.e. noun, verb and particle. In any case the Arabs had the advantage in that they could base their classification on a purely formal basis (though, of course, later on logic entered the field). In brief: any word that had a changeable case-ending (noun, adjective and numeral) was a noun; any word analysable into a root with changing prefixes and suffixes (denoting person and time) was a verb; and every unchangeable word — a particle. The ingeniousness, of course, with which those mediæval grammarians explained away the inconsistencies cannot be touched upon here.

This classification was borrowed by the Hebrew grammarians of that time, and although Hebrew — and by that time also Arabic — has not got case-endings, this classification has been quite useful; only later on the common Latin system was imposed by modern grammarians.

I do not venture to speculate upon the future development of grammatical terms. If the classical way of classification remains as it was — though perhaps with some change in the meaning of the terms — the grammar of the Semitic languages will follow suit — even if it be only for practical reasons of mutual understanding.

It seems, however, that it was not a mere chance, that some kind of formal classifications has become standard in Hebrew and Arabic mediæval grammars. We may say, that these

languages call for such a formal classification (although there will always remain some difficulties). Let me sketch this in two or three sentences.

A word which can add a plural suffix and — more important — a morpheme (if this term may still be used after our discussions), commonly called "possessive pronoun," is a noun. If it cannot have such a possessive pronoun, it is an adjective. (By the way: the moment an adjective can add a personal pronoun is the very moment when the adjective is used — as commonly said — as a noun).

A word analysable into a root with changing prefixes and suffixes (denoting in fact time and person) is a verb. A word able to take possessive pronouns, (but not plural pronouns) is a preposition (A bad term, in spite of its history). It is no mere chance, that we have got these fundamental classes — as has been demonstrated by Prof. Otto.

Words which can change from masculine to feminine and to plural in some extraordinary way that I dare not sketch here, are pronouns. Adverbs, conjunctions and the so-called interjections, cannot be separated in many of these languages on formal grounds.

This is, in short, the outline for a word-classification in groups of languages — and modern Hebrew especially, where formal analysis seems — so to speak — to be thought of as both natural and practicable.

M. M. GOTTSTEIN

Henry Sweet distinguished clearly between what we say and how we say it, and accordingly between the art of grammar and scientific grammar. Sweet's distinction between what we might call matter and form of language is still quite up-to-date. This distinction seems not to be identical with the distinction of Ferdinand de Saussure between *la substance* and *la forme*, a distinction which involves the distinction between the relative and the absolute. As the general relativity of *la langue* constitutes its *forme* and is the only object of synchronic linguistics as a science, that point of view could not even state any *substance*, still less any relation between *une forme* which involves relativity but excludes non-relativity on the one hand and *une substance* which involves non-relativity on the

other hand. Anyhow, Henry Sweet's distinction makes it possible to distinguish between material relations and formal relations. Formal relations such as "either-or" and "both-and" are the object of linguistics as a science. But material relations concern, what we might call, glottic interpretation and reflexion as an art. The inner form of the outer wordform *relation* designates, of course, relation in general. In other terms, relation is the matter of the minimal meaning which stands in a formal relation to this outer word form. But one meaning may be used in more than one sense. That applies also to the meaning of relation. Thus we may use that meaning as a scientific linguistic term but also in order to define our interpretation of such a feature as characterises for example in Latin both possessive case and possessive pronoun. But we should distinguish what we say as linguists who speak and what we say as interpreters, that is to say as all round philologists. That seems necessary in order to classify minimal meanings and features of minimal meanings, whether the classification be structural or functional or both structural and functional, and whether given meanings and features design things which might be designed by means of the term relation or not.

Finally I may suggest that only the term *sémantème* be used in the sense of a feature of a meaning, and that the analysis of the pronouns includes the classification of less generic classes of minimal meanings.

PIER ERINGA

In the *Native Irish Grammarian* (Sir John Rhys Memorial Lecture — British Academy, 1938), the late O. J. Bergin drew attention to the tracts extant in sixteenth-century manuscripts containing the grammatical doctrine taught in the schools of professional Irish poets from the fourteenth century on. Modern linguists might perhaps find some of the ideas of these mediæval Irish grammarians suggestive.

There was a tendency in their tracts to adopt a three-fold classification of words: (1) *oibriugud* (= working) [i.e. verb]; (2) *focal* (= word) [i.e. noun, adjective, adverb]; (3) *iarmberla* (unstressed particle; for its etymology see T. F. O'Rahilly, *Early Irish History and Mythology*, p. 85 ff.) [including simple

propositions, pre-verbal particles, unstressed forms of the verb "to be," etc.]

There was a tendency to use a very practical method of classifications. Thus to indicate that a certain word was a verb in the first person singular future indicative one could refer it to the typical verb *gonaim* "I kill" and say "this is its *gonfad*," literally "this is its 'I shall kill'"; or to indicate that *fióra* was the acc. pl. of *fer* "a man," one could say simply *buail fhíora*, literally "strike men."

The mediæval Irish grammarian was not interested in theory. His aim was purely practical. He wished to ensure that non-literary forms should not be used in professional poetry, which had to be understood all over the Gaelic world, where dialect forms were current in ordinary speech.

Inconsistency did not worry him, if it did not lead to misunderstanding.

When he had to classify the prepositional pronoun *leis* "with him," for instance, he called it a *focal*, meaning "a fully stressed word" as (distinguished from an *iarmberla* or "unstressed word," such as the simple preposition *le* "with,") it did not worry him that elsewhere he had used *focal* to indicate the fully stressed noun class, as opposed to the fully stressed verb class.

In spite, however, of frequent inconsistency of this nature, the tracts of the mediæval Irish poets could undoubtedly supply some ideas to the modern linguist in his efforts to define and name word-classes.

GERARD MURPHY

In discussing the problems of the parts of speech or major form-classes of a language, I believe it is necessary to make a distinction between:

(a) the procedures which the analyst uses in order to determine what the parts of speech of a particular language are, and
(b) the markers or the criteria through which the users (the speakers) of that language recognise or respond to these form-classes — i.e. the features which a grammatical description of the language must present.

As to (a), the analyst must discover for each separate language how many and what particular parts of speech must

be recognised. The structural system of each language will demand its own special kind and number of parts of speech and the precise procedures for determining the kind and the number of the major form-classes will vary in their usefulness from language to language. In English one can assume that all the "words" that occupy the same "set of position" (i.e. of structural positions) must belong to the same part of speech. In my study of English structure (which Prof. Funke has mentioned) I have found it necessary to recognise four major form-classes which I have not named but numbered 1, 2, 3 and 4. Many of the forms which I have included in Class 1 are conventionally called nouns, many of those of Class 2 are called verbs, many of those of Class 3 are called adjectives, and many of those of Class 4 are called adverbs. But the class numbers and their conventional names are not equivalent and the conventional names as they are usually defined cannot be substituted for the numbers. In addition to the four major form-classes, I have recognised a variety of separate groups of function-words.

As to (b) — the markers of the parts of speech — there seem to be no common features that will be applicable to all languages. Lexical meaning has not provided (in English, for example) any really useful criteria for identifying the parts of speech and has never been consistently applied as a feature for distinguishing each of the various parts of speech.

Parts of speech belong to the signals of a special layer of meaning — the meanings signalled by structural arrangements, that is, structural meaning as distinct from lexical meaning on the one hand and social-cultural meaning on the other. In English, the basic units of these structural arrangements are not words as lexical items, but words as belonging to certain form-classes. The particular lexical meanings of these items play no part in the signalling significance of these arrangements on the structural level. If, however, the words are not recognised or responded to as belonging to certain major form-classes, there will always be some kind of structural ambiguity.

In English, the particular structural meaning of "kind of utterance," whether Request, Call, Question, or Report, is signalled basically by contrastive arrangements of two major form-classes, Class 1 and Class 2. The utterance, *Ship sails today*, is ambiguous as it stands, because of the absence of clear

part-of-speech markers. If a clear marker, such as *the*, is put before the first word as in *The ship sails today*, there is no ambiguity; we have a statement. If, however, the same marker is put before the second word, as in *Ship the sails today*, there is also no ambiguity, but the utterance is different; we then have a request. Other clear part-of-speech markers would also resolve the ambiguity, as, for example, the use of the ending *-ed*: *Shipped sails today* or *Ship sailed today*.

Other types of structural ambiguity arise when the markers to distinguish Class 3 words from Class 4 words are not in the utterance, as in the sentence *The dog looked longer than the cat*.

The criteria for recognising these four form-classes of English cannot be given in simple definitions. One can only enumerate the various contrastive formal features by which each of these four form-classes is responded to or recognised.

By function-word I do not mean an empty word as contrasted with a full word — a distinction frequently made. Many of the items I have called function-words have lexical meanings distinct from their structural signals. The basis for separating these words from the four form-classes above, and for calling them function-words is the fact that one must know these words as items in order to respond to certain structural signals. In such a series as

- | | | |
|-------------------|---|--------------|
| (1) The man came | } | (Statements) |
| (2) The boys came | | |
| (3) They came | | |
| (4) Many came | | |
| (5) Who came | | (Question) |

the signal that 5 is a question rather than a statement lies in the word *who* as a distinct item, in this particular position. The separate function-words of this sort are very few in comparison with the thousands of words that belong to the four major form-classes. Unlike the four major form-classes also there are no general formal contrasts by which we can identify the function-words as belonging to certain lists. They must be remembered as items with specific structural meanings.

The vivid and stimulating discussion of the last few years since the first congress held at the Hague might seem to have split up linguists into several parties, for instance structuralists and their different groups, glossematists, adherents of the Immediate Constituent Method, etc. — and those, mostly older scholars, with their good old methods. These older scholars are the teachers of the younger generation, and many of them are scholars of high and eminent merit.

Reality, however, as it seems to me, does not always correspond to the loud — often too loud words. For the real difference between the two main tendencies in our science consists essentially in a sort of stylistic divergence, so that many ideas which are explicitly professed by the younger generation are implicitly present in the results and methods of those to whom we are indebted.

Then, I should like to say that it seems to me a step forward that contributors to our questionnaire agree on the idea that word-classes — or as I prefer to say — theme-classes should be defined by means of the syntagmatic relations they are engaged in. However, what is meant by such a statement? We must try to arrive at more precise conclusions. When we deal with units of expression nobody will deny that the most detailed analysis of such units is indispensable in linguistic research. For this purpose phonetic laboratories have been established.

But in the field of content-units — Professor Funke's report has made it very clear — such an intimate knowledge does not exist, nor does a non-contradictory description. But only if we can analyze content-units into their smallest elements can we succeed. Thus Lat. *ut* contains two elements — implicit mood + implicit case, each of them referring to different parts of the phrase. Such an analysis has long been well-known in the domain of *content-characters*, for instance *-ī* in Lat. *dominī*, which has been analyzed into two morphemes — according to the terminology of Professor Hjelmslev — into Genitive and Singular. But why should we stop our analysis at the frontier between content-characters and content-themes? The analysis given above of Lat. *ut* was an example of thematical content-analysis.

We come to this analysis by means of the well-known commutation test and the less known compatibility test, which I am going to deal with in a paper in the *Studia Linguistica*).

The elements we get by this analysis are named *pleremes*. We shall arrive, then, at a fully bilateral analysis of a unit such as

Expression	[w - a - i- f]
Content	human being-adult-married + Feminine

Engl. *wife* is implicitly feminine since it is referred to by *she* and not by *he*.

To achieve this work it will be necessary to work out the *plerematic* notation I have suggested in my contribution. By means of such a *plerematic* notation we shall — and must be able to write the content-elements just as we now can write the expression-elements by means of a *phonematic* notation.

In our example the *plereme* feminine is a marginal one in the unit concerned; the rest contains central *pleremes*.

Only after such an exhaustive analysis shall we be able to make statements of relations between content-units in different languages, so that the diachronic — let us use this old term — description of relations between content-elements — “content-shifts” can be put into formulas of scientific laws.

JENS HOLT

SECTION A

GENERAL LINGUISTICS

- (2) Can a purely formal grammatical analysis be carried out on languages such as Chinese, in which all or nearly all the words are invariable, and if so, on what principles?

Chairman: PROFESSOR W. SIMON

Rapporteur: PROFESSOR H. FREI

Recorders: MISS R. WALLBANK
MR. P. B. SALMON

For Preliminary Reports see pp. 53-59 above.

GENERAL PRINCIPLES

Can a purely formal grammatical analysis be carried out on languages such as Chinese in which all or nearly all the words are invariable, and if so, on what principles?

Chairman: Professor W. SIMON
Reporters: Professor H. FARR
Secretary: Miss R. WATKINS
Mr. R. H. SIMON

For preliminary reports see pp. 25-26 above

GENERAL DISCUSSION

The question consists of two parts. Unless the first part is answered in the affirmative, the second part is naturally not taken into account. My answer was affirmative.

However, in the first part of the question, in the clause "in which all or nearly all the words are invariable," I regarded the whole phrase "languages such as Chinese" (and not the word "Chinese" alone) as being the antecedent of *which*. It was for this reason that I excluded consideration of the problem of whether it can be admitted that in Chinese itself all or nearly all the words are in fact invariable. This problem involves a fundamental question of what the 'word' is in general and in Chinese in particular. This must be dealt with as a fresh problem.

On the assumption that in the questionnaire the term Chinese probably was intended to represent a type of language, the question may perhaps be interpreted as follows:

Can a purely formal grammatical analysis be carried out on languages in which all or nearly all the words are invariable, Chinese, in view of its special characteristics which have given rise to the classical term of monosyllabic isolating language, being considered as a case *par excellence*?

If we take this interpretation, it may not be pointless to assume the imaginary case as an extreme, provided that this imaginary language stands for those characteristics of Chinese only. On the other hand, it may not be relevant to this discussion to postulate an imaginary language of whose characteristics not all are indispensable to those of Chinese, to say nothing of that type whose characteristics diverge clearly from those of Chinese. Otherwise it would be possible to assume an imaginary linguistic structure in which not only all the words were invariable but also every semanteme-word would be marked off phonologically from every morpheme-word and *vice-versa*. In other words, we might even assume such an extreme case that not a single phoneme would be found existing in common both in a list of all phonemes constituting all semanteme-words and a list of all phonemes constituting all morpheme-words. But in fact I ignored cases such as the

above. Consequently, in my opinion, no grammatical distinction can be made between semanteme-words and morpheme-words from the morphological point of view within an imaginary type of language which I abstracted from the phrase "languages such as Chinese."

As for the employment of the traditional terms of morphology and syntax, Prof. Frei's opinion (see p. 56 § 2 above) is justifiable. But, on my part, these terms were used deliberately. From the wording of the question it seems to me that it implies the condition (1) that in languages such as Indo-European a purely formal grammatical analysis can be carried out, and (2) that a purely formal grammatical analysis mainly concerns morphology (in its traditional meaning). I tried at least to make it clear that although it is true that the more the proportion of invariable words in a language (from the viewpoint of grammatical category) increases, and the more the significance of morphology (in its traditional meaning) diminishes there, this does not produce any crucial effect on the carrying out of purely formal grammatical analysis. In this way, I meant to throw some discredit upon the traditional favour for morphology in grammar.

As I wrote in my contribution, it is my opinion that syntax alone (or possibly almost alone in proportion as nearly all the words are invariable) must become the principle of grammatical analysis to be carried out on languages of the type under discussion. Incidentally, in the languages under consideration, a very few examples, if any, of those words which appear to be variable may be properly dealt with as a problem of vocabulary in so far as they are too few to be regarded as forming any independent grammatical category or categories. It is, therefore, the concept of syntax only that is to be considered here. But this again must be dealt with as a fresh problem.

T. KAMEI

Of the preliminary contributions that of Prof. Knud Togeby is perhaps the most positive, and the words "possibilités de combinaison avec les autres éléments de la langue" recall Prof. Firth's rapport to the first plenary session and the importance he attached to meaning by collocation as a formal technique of stating meaning.

If it is permissible to apply the test of collocability which Prof. Firth describes in "Modes of Meaning" (English Association "Essays and Studies" 1951) at the grammatical level to words which are "invariable" then it is no hardship to classify these words according to the company they keep.

Thus in Chinese one might take as an example the negative particle *bu* and describe any word or word group which may follow this particle as a verb, and define verbs as words or word groups which have "grammatical collocability" with the particle *bu*. Thus *hao* "good" in *jeh-been shu hao* "this book is good" would be described as a verb because of the possibility *jeh-been shu bu-hao*. In a piece like *jeh-been haoshu* "this good book" *hao* would have to be described otherwise since there is no possibility of saying "*jeh-been buhaoshu*."

If the technique is applied to the classifiers discussed at a previous meeting, then one might proceed to describe a classifier as a word that has "grammatical collocability" in mid-position with determinatives and nouns, i.e. *Jeh* (determinative) *been* (classifier) *shu* (noun).

We find that determinatives like *jeh* "this" and *leang* "two" occur either singly or in groups, and when such groupings as *jeh-leang* "these two" occur, then word position is fixed enabling us to set up two further sub-classes of initial and end determinatives. Since the word material in each of these sub-classes is conceptually homogeneous, we are at liberty to term the former demonstrative and the latter numeral determinatives.

Lastly we may group together words like *shu* "book" which have "grammatical collocability" with classifiers in end position, distinguish them from words like *nian* "year," *tsyh* "occasion," *sheeng* "province," etc., which have not, and call them nouns. It is then possible to proceed to a reasonably exhaustive description of the "possibilités de combinaison" of words in this class, and we would arrive at the following table:—

<i>Jeh-been shu</i>	this book	Dem. Determinative-Class. Noun
<i>Leang-been shu</i>	two books	Num. Determinative-Class. Noun
<i>Jeh-leang-been shu</i>	these two books	Dem.-Num. Determinative-Class. Noun
<i>Jeh-been</i>	this one	Dem. Determinative-Class
<i>leang-been</i>	two (i.e. books)	Num. Determinative-Class
<i>Jeh-leang-been</i>	these two	Dem.-Num. Determinative-Class
<i>been-been shu</i>	every book	Class.-reduplication-Noun
<i>been shu</i>	a book or two	Class.-Noun
<i>shu</i>	books, the book	zero-Noun

Whilst this provides no more than a vague sketch of the possibilities of the method, it is hoped in this way to proceed to a detailed examination of the parts of the sentence, and finally to the sentence as a whole.

H. F. SIMON

La question qui nous est posée est formulée dans les termes en honneur au siècle dernier: les termes "purely formal grammatical analysis" employés en combinaison avec "words . . . invariable" ne peuvent faire allusion qu'aux flexions.

C'est pour réagir contre cet isolement de la morphologie que j'ai insisté sur le fait que cette discipline n'est qu'un chapitre de la syntaxe. Un mot comme *dominus* en latin est composé de deux signes de telle façon que chaque fois que *-us* se rapporte à *domin-*, les deux signes sont inséparables; ils constituent une unité syntaxique; mais comme le rapport qui les unit est un rapport syntaxique spécial, on en remet l'étude à la morphologie.

En chinois les unités morphologiques n'ont pas l'importance qu'elles ont dans les langues flexionnelles; c'est pourquoi je suis tenté de les négliger, laissant à la syntaxe le soin d'en rendre compte.

M. Frei dit que par exemple le chinois *t'a-men* est un syntagme; pour ma part, je ne vois pas l'utilité de recourir au terme *syntagme*; je préfère continuer à appeler cela un mot.

Mais entendons-nous. Un mot invariable est un élément unique, tandis qu'un mot variable est un groupe systématique: en latin *dominus*, *domini*, *domino*, etc. sont autant de vocables et ce n'est que l'ensemble de ces vocables qui constituent le mot variable.

E. BUYSENS

I

Die Formulierung der Frage ist wenig glücklich.

1. Wie auch A. Marty dargelegt hat, ist die Bezeichnung "Form" ("formal") recht vieldeutig. Man täte daher gut, auf diesen Fachausdruck zu verzichten.

2. Auch die Bezeichnung "invariabel" ist wenig treffend, denn dieser Ausdruck legt den Gedanken nahe, dass "invariabel" gleichbedeutend sei mit "flexionslos" und auch nichts mit Wortstellung zu tun habe. Aus diesen Unklarheiten erklärt sich der Gegensatz der eingesandten Beiträge.

II

Das vorliegende Problem ist ein grundsätzliches und will grundsätzlich vom Standpunkt der Allgemeinen Sprachwissenschaft behandelt sein.

Von diesem Standpunkt ist zunächst zu unterscheiden zwischen lexikalischen Begriffswörtern und syntaktischen Beziehungsmitteln. Das vorliegende Problem ist rein syntaktisch. Folglich muss die Frage auch für das Chinesische lauten:

- I. Welches sind die syntaktischen Beziehungsmittel? Die Antwort lautet:

(a) Wortstellung; (b) Flexion; (c) die rein klanglichen Mittel (Stimmführung, Akzent, Tempo etc.); (d) die Wortart, die als solche meist übersehen wird.

(a) Wortstellung: Hier herrscht Übereinstimmung, dass sich das Chinesische der Wortart bedient.

(b) Flexion: Von Flexion kann man nur dann sprechen, wenn man darunter lautliche Kennzeichnungen versteht (Suffixe etc.), die nur Beziehungsbedeutungen, — keine Begriffsbedeutungen — besitzen. Flexion liegt also nicht vor, wenn die Aktionsart zum Beispiel durch ein volles Begriffswort umschrieben wird. Diese Fälle wären also noch genau zu untersuchen.

(c) Stimmodulation (Akzent etc.): Die Lösung dieses Teilproblems wird dadurch erschwert, dass neben der Hochsprache auch noch dialektische Unterschiede heranzuziehen sind.

(d) Die schwierigste Frage ist die, ob das Chinesische auch Wortarten unterscheidet. Das Grundsätzliche dieses Problems wird in der Plenarsitzung zu erörtern sein. An

dieser Stelle ist folgendes zu beachten: die Wortart wird (ihrem Wesen gemäss) nicht lautlich ausgedrückt, wie die Flexion, sondern die Wortart erkennt man:

Zu 1. an der Struktur des Satzes, z.B:

Substantive haben Attribute — wie in der vorgestellten Wirklichkeit die Gegenstände auch Eigenschaften haben. Verba haben Ergänzungen (Bestimmungen), wie in Wirklichkeit sich die Tätigkeiten an Gegenständen (Personen) auswirken.

Die kategoriale Struktur der Umwelt spiegelt sich demnach in der Sprache wieder und trägt zum Verständnis der Rede bei. In diesem Sinne konnte A. Meillet sagen (*Linguist. hist et linguist. gén.*): das Subjekt ist ein Nomen "par sa nature."

2. Die Struktur des Satzes und damit die Art der Wortklasse erkennt man sodann daran, dass jedem Worte eine natürliche Wortklasse eignet. Wohl kann chinesisches *ta* auch die Grösse, vergrössern etc. bedeuten, aber seine ursprüngliche Wortklasse ist die Eigenschaft "gross," nicht die Substanz (die Grösse), was eine (sekundäre) Übertragung ist, worauf ja das Wesen der sogenannten Abstracta beruht.

Das Zusammenwirken dieser beiden Möglichkeiten mit der Einsicht in die Wortstellung bahnt das Verständnis der syntaktischen Beziehungen innerhalb der Gruppen und Sätze an, auch wenn die Flexion versagt. Insofern wir es mit der vernünftigen Rede zu tun haben, trägt die Begriffsbedeutung zur angemessenen Ausdeutung der Rede bei.

Zu 2. Damit weist sich auch das Chinesische als "variabel" aus, d.h. es bedient sich gewisser Beziehungsmittel. Wer die gestellte Frage verneinen wollte, verneint auch Sinn und Aufgabe der Allgemeinen Linguistik — und wir brauchten uns in der Sektion A nicht weiter zu bemühen.

E. OTTO

Though I agree with much that has been said by Prof. Otto, there are some points to which I have to take exception. Since a thorough discussion is impossible for the lack of time, let me state quite dogmatically that, in spite of Aristotle and Marty, I see no possibility of arriving at a universally

valid dichotomy of words that possess autonomous conceptual meaning ("selbstständigen begreiflichen Sinn") and those that don't. I do not believe, in spite of Wittgenstein, in the mirroring of reality in language ("Wiederspiegelung der Wirklichkeit in der Sprache") and I do not think that belief or disbelief in this is of any linguistic relevance.

Though I agree with Prof. Otto that word-classes need not necessarily be defined on the basis of flexions, I deny that reference to semantics is the only other way of defining them. I shall show with the help of a simple, and simplified example that word-classes can be determined in the absence of both flexions and any semantical informations.

Assume that we discover a very long series of inscriptions, of which I shall present only the first members:

$$\begin{aligned}
 2 + 3 &= 5, \\
 2 \times 3 &= 6, \\
 2 \times 4 &= 8, \\
 7 \times (3 + 5) &> 15, \\
 5 + (8 + 9) &< 90, \\
 (12 \times 3) \times 8 &= 12 \times (3 \times 8),
 \end{aligned}$$

.....

.....

.....

No linguist would have much trouble, with the help of the customary commutation test (and perhaps also with some less customary ones), to arrive at the result that the elementary symbols of this language distribute themselves among the following classes:

- class 1: 2, 3, 5, 6, 4, 8, 7, 15, 9, 90, 12,
- class 2: +, ×,
- class 3: =, >, <,
- class 4: (
- class 5:)

In addition, he would find that the complex symbol-sequences like $(3 + 5)$, $(8 + 9)$, (12×3) , (3×8) , also belong to Class 1. All this on the condition that he knows that the inscriptions are sentences and that the order of the symbols in these inscriptions is from left to right.

We see that order and scope alone suffice to determine the symbol-classes in this simple case, in spite of the fact that the

symbols are comparatively "invariable." Since ordinary languages do not have, in general, such a developed scope-system as the customary notation of arithmetic, some of them rely on other means such as flexion.

In summary, I think that the answer to the first part of the question that served as our starting-point is "Of course, how else?" The answer to the second is that order is such a powerful principle that, at least on principle, no other means (in addition to some scope indications) is necessary, though such means may be of help, in cases where order is not fully utilized or where scope-indications are not exhaustively given.

Y. BAR-HILLEL

A few remarks on principles involved in the description of colloquial Central Thai may perhaps be of interest.

In the first place it is necessary to state that words in Thai are of course to some extent variable as functioning at different linguistic levels but that this variability, though significant in syntax and morphology, can be demonstrated to occur in well-defined sub-systems. It is, though vital, nevertheless a minor feature and does not greatly ease the major difficulties before us.

A first principle is that the differentiation and delimiting of major groups (sentences) with reference to systematised contexts of situation may be approached formally in terms of (a) a description of certain features of group intonation; (b) a description of the organisation of initial, linking and final particles (see *Prosodies in Siamese* by Miss E. G. K. Henderson, *Asia Major* Vol. I pt. II 1949); and (c) by the delimiting of certain minor groups positionally significant initially or finally. (One such group may be described in other terms as Time/Place expressions).

An example:

<i>maa müərai</i>	come when	when did you come.
<i>müərai maa</i>	when come	when will you come.

When speaking of group initial and group final I do not imply any idea of an absolutely fixed place. Order and place, vital in the formal description of languages of the general type we are discussing, must be considered as indications of the significant relationship between units.

The delimiting of minor groups (phrases) within the major group may be carried out by describing the relational behaviour of certain exclusive agents. Some of these are particles, though distinct from the group already mentioned. Another is the classifier. The classifier in Chinese has already been mentioned by Prof. Simon (cf. above p. 303). The classifier in Thai occurs in two basic patterns:

A + B + Classifier and A + Classifier + B.

When the classifier is final in the group, B is always a "numerator." B will be found to include in its range many other lexical units besides the numerals. The final description is thus simplified.

In the pattern A + classifier + B, B represents "qualifiers, ordinals and demonstratives" and one interesting feature where the numeral "one" occurs, is best stated in an example:

<i>khwaai tua nūy</i>	Buffalo body one	=	a certain buffalo
<i>khwaai nūy tua</i>	Buffalo one body	=	one buffalo

The organisation of such features as classifiers into classes themselves, probably implies that word classes differentiated according to their function at appropriate levels can be formed throughout.

Finally, word structure in Thai must be stated in phonological terms. This involves a statement of the nature of the syllable. In Central Thai this may be stated as a complex of the following systematised features:

initial	major tone
final	minor tone
medial	

according to the linguistic level at which the considered piece is functioning.

It seems to me that the structure of the word and the minor group can be usefully stated in formal terms and that indeed formal criteria will be found significantly to delimit and differentiate major groups in relation to generalised contexts of situation.

E. H. S. SIMMONDS

In our contribution we explicitly permitted the inclusion of order analysis in "formal grammatical analysis"; in his report Prof. Frei, commenting that a direct answer to the question must depend on the definition of "formal," specifically included this ("les procédés tactiques . . . font partie de ces procédés"), and we may well give an affirmative answer and proceed to discuss the second part of the question, "on what principles?"

Our contribution was treating of New Chinese in the form of contemporary spoken Pekingese; the same remarks would apply to any New Chinese dialect, the same type-sentence e.g. in Cantonese being "Keydei ieddenq xai taidouzo Odei (la)"; for Old or Middle Chinese different examples would be needed. The question however refers to "languages such as Chinese," the criterion being that "all or nearly all the words are invariable," and an answer must be applicable to any form of Chinese, as well as to other languages of the Sino-Tibetan family, and languages of the Thai and Mon-Khmer families together with some others.

In fact the principles of formal grammatical analysis will be the same in dealing with all languages. It is the categories to be established that will be different. The language to be analyzed will be the language of a text, which may be spoken or written: a literary text, or material elicited or recorded in investigation. The analysis will begin with a study of the sentence-forms and other order syntactic forms. This, as we have said, will not tell us all we need to know; but it will carry us a great deal further in a language of this type than it would with a text in Russian or Mongolian. The characteristic of grouping languages (to use Meriggi's term in preference to the traditional "isolating" — see P. Meriggi: *La structure des langues groupantes*; Journal de Psychologie XXX 1933, pp. 185-216) is that grammatical relations are expressed syntagmatically (by "tactic processes") rather than paradigmatically, and formal analysis will elicit syntagmatic types such as subject-predicate, qualifier-qualified, verb-object and object-verb. Grouping language texts will show a greater number of semantic processes of Prof. Frei's type c) than will texts in languages of a non-grouping character.

The analysis of word-form will include word-combination and the presence of non-radical elements or morphemes. The

latter are few in New Chinese but they do occur, though it is possible (with Mr. Kamei) to imagine a language in which they did not. Regarding word-combination, Prof. Togeby's note that words can be characterized by their combinatory possibilities can be applied to the analysis of inner word-form, by the process of ascertaining the categories (formal) to which they are susceptible: e.g. those which can be followed by *le* indicating tense (Cantonese *zo*), by *-mn*, by *idian*; preceded by a numeral &c. Here in a spoken text prosodic analysis will contribute: e.g. type *x* words can be reduplicated in form stress-unstress, type *y* words in form stress-stress (the distinction "shsh" "everything," "shsh" "have a try"). (Cf. Meriggi: *op. cit.* p. 208, quoting Courant "the accent organises monosyllables into polysyllabic groups"; p. 209, "the rhythmic grouping of words corresponds exactly to the articulation of thought.") In grouping languages of course word-combination is a more frequent and word-modification a less frequent process than in other languages; but even among Chinese dialects there is some variation, e.g. Pekingese *kanzh bao*, where *zh* does not occur except as auxiliary element, Cantonese *taigen bouzi*, where *gen* is a word, both indicating the imperfective in sense "reading the paper."

In dealing with Chinese it is important to distinguish auxiliary elements like *-zh*, *-mn*, (morphemes, with no other function) from what Chao calls "bound-words," which are semantemes (cf. English "multi-") with radical meaning but no inner word-form. Here the existence and extent of the "word" can be confirmed by the combinations into which it enters and the categories to which it is susceptible: e.g. *shexui* "society" consists of two syllables neither of them "bound" in Chao's sense; but it can be shown to be a word by the fact that it can enter into combinations into which neither of its component syllables can enter separately, e.g. as qualifier *shexuizhuj* "socialism."

In a written text prosodic features such as Mr. de Witte mentions are not available for analysis, but the formal method will still be used, beginning with sequences of characters demarcated by punctuation, by final particles, &c. In the particular case of Chinese, the character may conveniently be taken as a unit if the text is so remote from any spoken language that no parallel text is available for guidance in

stress-groups &c., as would be the case with Old or Middle Chinese.

The fact that in grouping languages some categories (e.g. in Chinese, "sex") and some relations (e.g. in Cantonese, "possession") are expressed lexically or lexico-grammatically and not (as usually in Indo-European languages) grammatically, has led some people into thinking that formal grammatical analysis is impossible in these languages. However, as Prof. Frei has shown, it is equally true that others (e.g. in Chinese, "Aktionsart") are expressed grammatically in Chinese and lexically in Indo-European (see: *Un système chinois des aspects*; Acta Linguistica, II 3, 1940-1941, pp. 137-150). What one can perhaps say is that in grouping languages sentence-structure is determined by sentence-form rather than by word-form: speech is to a certain extent liberated by the absence of paradigms from the tyranny of language. But speech cannot exist without language, and language cannot exist without grammatical form. Where there is grammatical form there can be formal grammatical analysis.

M. A. K. HALLIDAY and J. ELLIS

SECTION A

GENERAL LINGUISTICS

- (3) What are the central problems of form and meaning in negation?

Chairman: PROFESSOR G. DEETERS

Rapporteur: PROFESSOR W. E. COLLINSON

Recorders: MR. P. B. SALMON
MR. I. RICHARDSON

For Preliminary Reports see pp. 61-74 above.

INTRODUCTION BY W. E. COLLINSON

The task of the *rapporteur* is to open the meeting by presenting a report on the present state of thought and research in the subject for discussion, taking into account the written contributions already submitted.

“What are the central problems of form and meaning in negation?”

First of all the contributions. You will have seen those of Messrs. Ellis, Eringa and Pottier and from my very brief summary you will see the main problems they raise. Mr. Guthrie's contribution is included in the Preliminary reports, but did not reach me in time for comment with the others. His observations on the Bantu phenomena show once again how varied the methods of indicating negation are, e.g. by prefix, suffix, infix or the provision of special negative tenses.

From the formal and descriptive point of view we might ask of a particular language (1) what negative words, affixes or procedures are actually employed?

(2) to what extent are these words or procedures attached to and how far do they negate particular elements of the sentence and how is the sentence as a whole negated?

(3) how far is the position of the negative expression regulated? (In spite of Jespersen's very clear and sensible treatise on “negation” published many years back, I think there is room for a full investigation into the various positions — often under different condition of sentence-stress occupied by negatives, e.g. *nicht* in the German sentence, or in Scandinavian languages where I have noted the ante-position of *ikke* before the imperative in a prohibition: *ikke gjør det!*).

(4) what is the implication of a cumulation of negatives? Emphasis e.g. in I don't know nothing about it (vulgar)?

(5) where there are specific negative auxiliary verbs, as in Finnish, what alternative procedures, if any, are there in the given language? This problem belongs to the synonymics of negation!

On the meaning side I do not think we are concerned with the philosophical or metaphysical status of negation any more than

we are with the status of glass mountains or unicorns. I think we are concerned, however, as some of the contributions show, with the manner in which a language discriminates between the "logical contradictory" and the "logical contrary" and to what extent it merges them, also to what extent a language distinguishes a prohibition from a denial. A further problem — and there may be many more — is as I stated briefly in my report — that of implicit negation, of which some of the best examples I have seen are quoted in A. Zobel's treatise *Die Verneinung im Schlesischen* (Breslau 1928 — Wort und Brauch).

With these few remarks I throw the discussion open.

W. E. COLLINSON

1. Einige Ergebnisse aus der Analyse des Deutschen. Ausgangspunkt: die sprachliche Formenwelt (Wortformen, Stellungseigentümlichkeiten, Verbindungsweisen) und *nicht* die allgemeineren denkmässigen Inhalte, die Begriffe. So können wir den Begriffskomplex "Negation" nicht zu Grunde legen, sondern wir werden ihn, ausgehend von den sprachlichen Formen, erst antreffen.

2. Betrachten wir:

Er kam häufig	Er kam immer
Er kam selten	Er kam nie
Er kam oft	Er kam nicht

Häufig, selten lassen die Umwandlung zu; "Sein häufiges Kommen, sein seltenes Kommen."

"Oft, immer, nie, nicht" gestatten keine entsprechende Umwandlung. Die Negation "nicht" zeigt sich also als ein Angehöriger der Wortart "unflektierbare Wörter" oder "Adverbien im engsten Sinn," "Partikeln." Eine besondere Wortart hat sich nicht gebildet. Immerhin besteht ein Ansatz, das Wort "nicht" von den übrigen durch eine besondere Stellungsgesetzlichkeit zu unterscheiden.

er kam oft/oft kam er

er kam immer/immer kam er

er kam nie/nie kam er

er kam nicht/keine Spitzenstellung des "nicht" möglich, trotz Schillers

“nicht grauet dem Schützen auf schwindlichem Weg.”
Zur Frage: “Negation einzelner Teile, Negation über den ganzen Satz ausgebreitet.”

Umstellungen eines Satzes von Goethe unter Beobachtung der Sinn-änderung bei anderem Platz des “nie”:

“Der Geist des Menschen kann sich mit einem andern *nie* vereinigen . . .

Der Geist des Menschen kann sich *nie* mit einem andern vereinigen . . .

Der Geist des Menschen kann *nie* sich mit einem andern vereinigen . . .

Nie kann der Geist des Menschen sich mit einem andern vereinigen.”

und mit noch stärkerer Umstellung:

Kein menschlicher Geist kann sich vereinigen.

4. Wir finden also keinen idealen, logischen Ausbau der Möglichkeiten der Negation, wie ein Logistiker ihn für seine Zwecke haben möchte. Die Sprache ist nur ein Übergang von einer rein empirisch-praktischen Gestaltung (wo die Negation einfach als adverbiale Bestimmung unter andern auftritt, oft auch mit andern Sachkernen eng verbunden, wie “nie” — nicht je, “nirgends” — nicht irgendwo) zu einer “reineren” Gestaltung, indem die besondere Denkwichtigkeit der Negation zu einer besondern Wortkategorie drängt. Diese logisch reinere Gestaltung ist aber nur ein Ziel; die Sprache strebt zwar danach, aber sie erreicht es nie, weil andere Kräfte und Ausdrucksbedürfnisse sie immer wieder von diesem Ziel wegziehen.

HANS GLINZ

In my contribution on negation I refer to a movement between negation being formally a property of the sentence (including of the verb) and being one among many possible adverbial attributes, i.e. being itself a lexical element. This variation may be thought of otherwise in terms of the distinction between word-negation and sentence-negation. In English, negation of the word is at the same stage of relative complexity as is negation of the sentence in, say, Japanese.

In Japanese, where the negation is inherent in the verb, like English prefixed word-negation, the negative sentence may be

the contrary, not merely the contradictory as in English, of the affirmative sentence, in cases like "he never comes" which in Japanese is the negative of "he always comes." Compare English "he must not come," contrary, beside "he need not come," contradictory, or "I do not think he will come," meaning "I think he will not come," which is termed by Dr. Gunnar Bech 'negatio obliqua.' In these terms the Japanese for "nothing," "never," and other negative pronouns and adverbs, is expressed by oblique negation.

The diametrically opposite state of affairs, with the negative entirely merged in the pronoun or adverb, is found in Middle High German, in the phenomenon to which Professor Collinson's report refers, which I should like to call a case of tacit negation, and in that mentioned in my contribution, which I shall call semi-tacit negation. In tacit negation as found in Middle High German the negative idea is contained in an indefinite pronoun or adverb. This happens in subordinate clauses with "daz" after verba curandi, and historically is by false analogy with verbs of prevention. In semi-tacit negation, by a process of litotes, words like "little" or "seldom" suffice for complete negation, e.g. *lützel ieman* nobody. This is so much felt as negative that it may enter into Middle High German double negative, e.g. *ez ist selten nimmer guot* "it is seldom ever good."

In conclusion, I should like to qualify my generalisation about the historical development being in one direction. This may well be true in the most general sense, taking the whole history, and prehistory, of languages and language-families of the world; but it could be established only after much more comprehensive study of them than has yet been organised. What is certain is the long periods within one family over which development is in the other direction, for example the Latin state of affairs with "ne" and "non" is younger than the less complex old Germanic state of affairs with "ni," while the French renews a modal distinction (i.e. akin to Latin) by the non- or incomplete extension of "pas" after "ne" to modal auxiliaries.

J. ELLIS

When I was an undergraduate, I read in Sir Walter Raleigh's essay on *Style* an observation that interested me very much. Literature, he pointed out, is the only one of the arts that can

deny. I did not understand the reason at the time, but later it became clear. Literature derives its forms from oral speech, and it is only in oral speech that two participants regularly take part and assume alternately the active and passive rôles. Painting and sculpture are frankly isolationist, and if in music two persons combine often, it is to be hoped that they do so in harmony. In speech, however, the speakers are very often opposed to one another, and differ in their views as to a given action or state of affairs. Negation has its roots in contradiction. Now contradiction may be either rude or polite, emphatic or tentative. It is very interesting to study the results of these attitudes in syntactical form. Emphatic contradiction is often marked by the use of adverbs or adverbially used words: *ich bin gar nicht hungrig*, I don't care a jot. Sometimes the adverbially used word becomes stabilised as in French *ne . . . pas*, in which case emphasis has to be achieved by a further adverbial addition: *pas du tout*. Late Egyptian and Coptic have a post-negative which is used just like French *pas* though only in restricted cases. Then there is the polite form of contradiction, giving rise to such forms of utterance as "do you not agree that . . ." etc.

But what I really wish to speak about is the difference of verb-forms in affirmative and negative sentences. This is most marked in Egyptian and the Semitic languages. Time allows me to quote only two cases from the foreign language I know best. When the Egyptian said "He who knows this spell goes forth from the judgment hall justified" he uses for "goes forth" the imperfective verb-form *peroref* which suggests repetition or continuity. But when he said "He who is ignorant of this spell does not go forth justified" he uses for "does not go forth" the *sedjemnaf* form, a form which signifies essentially a single action either past, present or future. The reason clearly is that there is no more effective way of breaking down a rule or a generalisation than by quoting one single negative instance.

Then the case of the imperative is very interesting. Here Egyptian uses a negative verb, "to not" as it were. This negative verb is put into the imperative and followed by a somewhat colourless verb-form or else by the infinitive. I have the feeling that the Egyptians would have felt an inherent contradiction in combining an ordinary negation with the

affirmative imperative. The imperative orders you to do a thing, and by adding an ordinary negation you can't wipe out the order. It is for classical scholars to say how far this applies to negative commands like *μὴ λέξης*, *ne dixeris*; there seems a parallel to Egyptian usage in forms like *noli me tangere*.

ALAN H. GARDINER

Negation ist eine Notierung zweiten Grades, indem sie eine positive Notierung voraussetzt. Das ist schon verschiedentlich beobachtet worden: *er kam nicht* ist gesagt im Hinblick auf jene andere Möglichkeit, die ebensogut realisiert sein könnte: *er kam*. Die Negation führt eine Affirmation nicht bloss zum Nullpunkt, sondern darüber hinaus in die entgegengesetzte oder gegenüber liegende Position. Bei einem mit Negation versehenen formalen Gebilde wie *er kam nicht* sind drei funktionelle Faktoren stets präsent: (a) die Aussage, zu welcher die Negation tritt, (b) die Funktion des Negationswortes, (c) die neue Aussage, der neue Sachverhalt, welcher durch Hinzutritt der Negation entstanden ist: *er kam nicht*. Infolge dessen liegt die Negation funktionell zwischen zwei Aussagen, die an sich positiv sind, die eine so gut wie die andere. Sie konstatiert nicht bloss, wie die traditionelle Definition behauptet, das Nichtvorhandensein des Sachverhaltes *a*, sondern sie *delimitiert* zwei Sachverhalte, *a* und *c*, wobei $c = \text{Negation} + a$. Wo eine Negation steht, sind immer zwei Sachverhalte evoziert; und das Besondere an einer Delimitierung mit Negation, im Unterschied zu anderen Disjunktionen, etwa mit *oder*, *aber*, besteht darin, dass die beiden Sachverhalte in einem Konträrverhältnis zueinander stehen, worunter auch ein Pendantverhältnis verstanden werden kann.

Wie sieht nun die Anwendung dieser Theoreme auf die Praxis aus? Wichtig ist es hier zunächst, den Unterschied zwischen Satznegation und Wortnegation zu beachten. Er geht parallel mit dem Unterschied zwischen Satzfrage und Wortfrage. *Je ne viendrai pas*, *He doesn't smoke* sind Satznegationen; die ganze Satzaussage, d.h. in den indoeuropäischen Sprachen im Prinzip das Verbum, ist verneint. An Wortnegation ist das Germanische besonders reich: *Untugend*,

Unglück, unschön, unhappy, indo-europäisch ist z.B. der Typus *ἄβροτος*, *amṛta*. Nun ruft allerdings die Aufstellung dieses Unterschiedes viele Probleme auf den Plan. Ich brauche nur das erste Beispiel einer Satzfrage: *he doesn't smoke* zu erweitern in *he doesn't smoke cigars but cigarettes* und zu fragen: was ist hier eigentlich verneint, die gesamte Satzaussage oder nur ein Nominalbegriff *cigars*? Wenn ich jedenfalls von logischen Überlegungen herkomme, muss ich sagen: hier kann nicht das Prädikat verneint sein sondern nur ein Einzelelement: *cigars*. Trotzdem zieht der sprachliche Ausdruck die Negation eindeutig zum Verbum.

Was man nun einzelsprachlich beobachtet ist nichts anderes als ein Kampf, ein Tauziehen zwischen Prädikat (Verb) und den Einzelelementen (Nomina) um die Negation. Es wird meistens zu Gunsten des Verbums entschieden. Frz. *tout ce qui reluit n'est pas or*, Engl. *we aren't here to talk nonsense but to act* zeigen deutlich die attraktive Kraft, die das Verbum, entgegen unserer logischen Vorstellung, auf die Negation ausübt. Im Altgriechischen gehören hierhin die Fälle *οὐ φημι* "ich sage dass nicht, ich leugne" *οὐ κελεύω* "befehle dass nicht, verbiete," wo der Verbalbegriff verneint ist und nicht dessen Objekt.

Es ist bei dieser Tendenz der Negation zum Prädikat hin verständlich, wenn sie in den verschiedensten Sprachen enge Bindungen mit dem Verb eingeht. Man kann dieses "Hineinwachsen" der Negation in das Verb z.B. im Marathi verfolgen: Für "er machte nicht" wird gesagt *nā karī* oder *karī nā* mit Vor- oder Nachstellung der Negation. Von einer 3. Person *karī nā* kommt man für die zweite zu zwei Typen: *karī-nā-s* 3. Plur. *karī-nā-t* "sie machten nicht." Im zweiten Fall ist die Negation zwischen Stamm und Endung getreten. Eine ähnliche Tendenz scheint es nur zu sein, die dem engl. Hilfsverb *do* zu seiner obligatorischen Verwendung in den fragenden und negativ antwortenden Sätzen verholfen hat.

Diese ständig vorhandene Anziehungs-Tendenz zwischen Negation und Verbum scheint mir nun auch verständlich zu machen, wie man die Negation geradezu als syntaktischen Akzent gebrauchen kann, um das Verb herauszuheben: Eine Frage kann ich positiv oder negativ stellen. Positiv: *Trinken Sie ein Glas Bier?* Dies kann Satzfrage sein, aber auch Wortfrage, wenn nämlich der Akzent auf *Bier* liegt, im Gegensatz z.B. zu *Wein*. Entscheiden lässt sich das bloss mit der

Intonation. Oder dann, wenn ich die Frage negativ stelle: *Trinken Sie nicht ein Glas Bier?* Hier ist eindeutig das Prädikat, d.h. Verb in Frage gestellt. Es ist dies eine Spielart der Erscheinungen, welche bei den Romanisten unter dem Namen "mise en relief" bekannt sind. Zugleich spielt noch ein Faktor mit: Der Fragesteller erwartet jedenfalls durch die negative Frage eine positive Antwort: *ja*. Mittelst des negativen Sachverhalts wird gleichzeitig der konträre evoziert.

Im Lateinischen leitet enklitisches *-ne* die Fragen ein, bei welchen nicht ein Fragepronomen steht: z.B. *Et heri cenaristisne?* Die naheliegendste Erklärung dieses *-ne* scheint mir trotz allem durch die Identifizierung mit der Negation gegeben, und der Sinn dieses Gebrauchs lag eben ursprünglich darin, aus der indifferenten Frage eindeutig eine Prädikats- oder Satzfrage zu machen.

Kontradiktorische Bedeutung der Negation ist in manchen Fällen beobachtet worden. Wir glauben, dass diese stets vorhanden ist.

Eine ganze Reihe von Adjektivbegriffen können nur durch negierte Konträrbildungen ausgedrückt werden. Das Baltische scheint hierfür eine besondere Vorliebe zu haben. Es liegt ferner nahe, hier auch die stilistisch ausgewertete Erscheinung der sog. Litotes anzuschließen. Wenn ich sage: *das ist nicht schlecht*, so will ich mit dieser negativen Aussage die konträre evozieren, ja sogar betonen.

Am deutlichsten zeigt sich das binäre, delimitierende Wesen der Negation im syntaktischen Zusammenhang. Frz. *Je suis parti avant qu'il ne soit arrivé.* Die Negation, die man oft als "überflüssig" bezeichnet hat, zu Unrecht, wie mir scheint, delimitiert hier konträre Aussagen der Form: *je suis parti — il est arrivé.* Wir finden die Negation ferner bei Vergleichen, und zwar nicht nur bei komparativischen: *il est plus riche que je ne pensais* sondern bei regelrechten Äquativen, z.B. im Altindischen: RV I 16, 5 *gaurō ná trsitáh piba* "trinke wie ein durstiger Büffel," wo die Negation geradezu in der Funktion von "wie" steht. Man kann sich bei solchen Vergleichen, überhaupt bei der binären Funktion der Negation, fragen, zu welchem Teil des Bindens sie eigentlich gehört. Wenn z.B. durch die Form *nullus* stets zwei Konträrbegriffe präsent gemacht werden: "keiner - einer," wieso sollte da nicht unter Umständen *nullus* selbst zu der Bedeutung "einer"

gelangen? Dies ist tatsächlich passiert im Gemeinslav. und abg. *ne-kuto* wörtlich "nicht-wer," d.h. "jemand" und anord. *nekkverr*. Es kann sich also hier bei der Negation unmöglich um eine Verneinung im beigebrachten Sinn, d.h. eine Reduktion bis auf Null handeln. Umgekehrt nehmen Formen für "irgendein" die Bedeutung "kein" an, voran dt. *kein*, welches ein nicht-negierter Ausdruck ist so gut wie poln. *zadny* "wünschbar, beliebig" > "keiner," ferner frz. *pas*, *point*, *personne*, etc. All diese Ausdrücke haben ihre Konträrbedeutung deshalb angenommen, weil sie ursprünglich in engster Verbindung mit der Negation standen.

Schliesslich erinnere ich noch an die Ausdrücke *Un-Menge*, *Un-summe*, gr. ἀπαρθέεντος — rein, jungfräulich, mit scheinbar pleonastischem ἀ-. Eine *Un-summe* ist nicht eine nicht vorhandene Summe sondern im Gegenteil eine sehr grosse Summe. Der negierte Sachverhalt weist mit Nachdruck auf das Konträre hin.

H. SEILER

J'ai été assez étonné de lire dans certaines réponses que la négation, en portant sur un élément, lui donnerait le sens contraire. Je suis d'avis que la négation exprime l'annulation ou l'absence. Par exemple: "Mon père n'est pas ici" exprime l'absence du père. Dans "elle n'est pas jolie, mais elle n'est pas laide non plus," on exprime que la beauté est nulle, et que la laideur est nulle; si la négation exprimait le contraire, la phrase "elle n'est pas jolie" signifierait "elle est laide"; mais on a soin de dire qu'elle n'est pas laide.

Si l'on compare les trois adjectifs "moral-immoral-amoral," on voit que "immoral" exprime le contraire de "moral," mais que "amoral" est la négation de "moral" et de "immoral."

Ce qui incite à confondre contraire et négation, c'est que parfois on emploie la négation pour éviter le contraire. Par exemple, il n'est pas aimable de dire "Il est impoli"; on atténue l'affirmation en disant "Il n'est pas poli"; ce faisant on recourt à une figure de style; au delà du sens exact des mots, "il n'est pas poli" on en suggère un autre: "il est impoli." Mais on n'a pas le droit de confondre le sens exprimé et l'arrière-pensée qui n'est pas exprimée.

Sur ce même chapitre de la négation, je voudrais faire une petite remarque.

Monsieur Ellis a mentionné les négations doubles. On sait qu'en vieil anglais, il y avait parfois trois ou quatre négations dans une même proposition, et dans le peuple on entend aujourd'hui encore des phrases comme celle-ci "I won't say nothing to nobody." Ce que je voudrais faire remarquer, c'est qu'une pareille phrase obéit à une tendance générale très connue, à savoir l'accord.

Prenons par exemple la phrase française "Les cinq généraux seront là." L'idée du pluriel y est exprimée d'abord par l'adjectif numéral cinq, ensuite par le pluriel *les*, le pluriel *généraux* et le pluriel *seront*. Cette quadruple expression du pluriel est superflue; mais elle nous est habituelle. Nous appelons accord cette sorte de contagion qui s'étend inutilement à plusieurs mots.

C'est une autre forme d'accord que nous avons lorsqu'une proposition contient deux ou trois négations superflues.

E. BUYSENS

We can scarcely consider meaning in negation without consulting the form in which it is expressed. As with all other linguistic phenomena, we must proceed in the first instance from form to meaning. It has been observed that in various languages there is a close correspondence between affirmation and negation and that the latter is formally a modification of the former. At its simplest this modification involves the introduction of a particle, say, like the "empty words" *buh* "not," *moh* "don't" of Chinese (Gwoyeu) or the *ne* of Russian and Lithuanian, into an affirmative sentence, which thereby becomes negative. The nasal sonant (*m*, *n*) or its oral counterpart (*b*) need not figure in such a particle, although it often does. Norwegian and Swedish have *ikke/icke*, Arabic has *la*, Estonian *ei*, Albanian (Geg) *s'*, and Basque *ez*. The same particle may be used in all constructions, as in Slavonic and Baltic, or there may be two particles, as in Chinese, Finnic, and Ancient Greek, to serve grammatical refinements. The particle, moreover, may be reinforced, as in French, and the reinforcement, originally affirmative, may come to be the

bearer of negation (e.g. Fr. *pas mal*). These are all familiar phenomena, but there are certain others which present negation in a less familiar light. I have in mind here variation in the case of the direct object of the verb according to whether the verb is affirmative or negative. This feature is shared by Indo-European and Uralian languages in North-Western Europe: it is found, for instance, in Baltic and Slavonic, where the object of a negative verb appears in the genitive case, and in West Finnic, where it appears in the partitive (cf. Russ. *ja ne vižu sveta* "I don't see any light" with Latv. *es neredzu gaismas* and Est. *ma ei näe valgust*). It is, as it were, a concordance and concord, aiming at a maximum of emphasis on negation. West Finnic too, in its more conservative sources, presents us with the mechanism of the negative verb (cf. Finn. *en-es-ei-emme-ette, eivät*, lit. "I, thou, he/she, we, you, they not"). Here negation is shown to be closely bound up with the notion of the predicate, although, as examples from the same group show, it can be bound up equally well with subject and object. There appear to be marked differences of form which the negative idea invests according to which part of the sentence and even according to which part of speech it is associated with (cf. Est. nominal *eba-, mitte-* with verbal *ei, ep, ära*). We are led from an investigation of these data to postulate the presence of two notions in negation — that of "absence" in the nominal and that of "denial" in the verbal part of the sentence. In both instances we have one term of a clear dichotomy.

So long as we confine ourselves to the facts of language we shall have no need to discuss the philosophical problems of existence and non-existence, truth and falsehood, which, like other extralinguistic concepts, can do no more than obscure the linguistic issue. This remains grounded in the correlation of form and meaning, the approach to which must obviously be "grammatical."

LONDON

W. K. MATTHEWS

The two questions, both of the attachment of the negative particle and of its position in the sentence (when this is one of general negative content), arise in a special form in those languages which have a relatively free word-order, such as

Sanskrit or Greek. Where is the negative particle to be placed? A problem of meaning is also involved here, because there is a tendency for the word, or phrase, immediately after the negative to be regarded as expressing the idea which is particularly denied.

It appears that in the older forms of Indo-European, the regular place for the sentence-negative was at the start of the sentence. This is what we should expect, since the start was the place of most emphasis, and negation is an emphatic notion. So in Sanskrit, where the work of Delbrück, Speyer and Thommen on word-order has shown that in the oldest prose *na* was habitually initial, and only occasionally found later, preceding the verb. Delbrück sought to distinguish these two positions, as those of sentence-negative and verb-negative respectively, on grounds of meaning, as if the latter negated the verb alone, and not the whole sentence: here, however, he was mistaken, for the difference is no more than one of emphasis.

In Greek also the initial place for *οὐ* is most favoured in the early language, accounting for 70% in Homer.¹

The initial sentence-negative serves as a sign-post, to show that the statement which follows is denied as a whole. The second word in such a sentence is not necessarily the element (if any) which must especially be denied.

But later stages of both Sanskrit and Greek show a change. This is due to the tendency, to which Jespersen drew attention, for the negative particles to be weakly stressed, and even to become proclitic, because another word in the same sentence receives the strong sense of contrast instead. We are familiar with this in the phonetic weakening of negative particles, but it can be seen also in the development of word-order. In Sanskrit later prose, the habitual place of *na* is after the start, and preceding the verb: it is only occasionally initial, and also appears in other parts of the sentence, not before the verb. In Greek there was a similar, but not identical, movement of *οὐ* away from the start. So in Herodotus initial sentence-negatives are only 40% of the total; Plato 44%; Thucydides 43%. But the majority of the negatives which are displaced from the start go no further than to second place — which simply means that another word, for emphasis or greater interest (since the negative is essentially a colourless word),

¹ Statistics are based on extensive samples.

has slipped into its old place. In some authors the second-place negatives even outnumber the initial. It is much less common to find the negative later, preceding the verb.

Possibly the Greek use of the double negative, where the same particle *οὐ* is found both initially and later in one sentence, may have its origin in the blending of two originally distinct positions. This usage should be kept distinct from cases where the negative reappears in a different form, as in English *I did not see nobody*.

A. C. MOORHOUSE

Assuming that, for a certain given language, a list of words (or morphemes) can be exhibited which contains all "negation-words," e.g. for English, the words "no," "never," "nobody," etc., and the morphemes "dis-," "un-" etc., one might, if this should turn out to be fruitful, designate sentences containing one or more of these words as "negation-sentences." It makes however no sense to say that such sentences express (or do not express) negations (in the sense of significate). It can easily be shown that such a way of talking is self-refuting. "John disappears" would have to be treated as expressing a negation (because of the "dis"), and "John vanishes" as expressing a position (to use this term in an unusual but clear sense); but these two sentences are synonymous, hence the distinction of negation and position on the signification-level is untenable. In its place comes the symmetrical relation of incompatibility. "John is in good health," and "John is not in good health" are, (under certain assumptions) incompatible sentences; one of them contains a negation-word, the other does not, yet it makes no sense to say that the first expresses a position, the other a negation. One might as well have used "John is sick" and "John is not sick" in their stead (at least approximately so). To insist on the positive character of what is signified by "John is in good health" would be again a very unhappy adoption of antiquated metaphysics, this time going back to Aristotle. To regard sickness as absence of health (but not vice versa!) is bad metaphysics, which no linguist should feel under any obligation to accept.

The sentence of vulgar English "I won't say nothing to nobody" certainly contains three negation-morphemes, if "-n't," "nothing" and "nobody" occur in the mentioned list of English negation-morphemes. Whether this is the most elegant way of describing the situation or whether it might not be preferable to have perhaps only *one* English negation-morpheme, say "not", which when combined with "anybody" yields "nobody," and regard now the mentioned sentence as containing *this* negation-morpheme only once, viz. as a so-called component, is not a question for me to decide in this place. Whether the omission of two consecutive negation-morphemes in a sentence of a given language yields always, or sometimes, or never, a sentence that is logically equivalent to the first, is an interesting empirical question. But formulations like "double negations cancel (or do not cancel) each other" should better be avoided, since they may be too easily interpreted as statements on the significate-level, for they make no sense whatever.

Y. BAR-HILLEL

Here are some facts which I think worth considering in regard to the problem of meaning in negation. Some aspects of the matter have been mentioned in the preliminary reports, and also at this meeting; others, so far, have been neglected.

1. In Spanish, as in other Romance languages, intonation plays an important rôle. A mother punishing her child would say: "*Toma, para que vuelvas a llegar tarde*" where the meaning of the sentence is exactly the opposite of the intention of the speaker. In the same way: "*No tiene dinero*" can have, according to the intonation, two quite opposite meanings.

2. This last example has nothing to do with lexicalisation, (French: *rien, personne*, Sp. *nada, nadie*), a development still going on in modern Spanish where expressions such as *en mi vida, en absoluto*, standing without any negative particle give to the sentence a negative meaning. *En mi vida he estado en Francia* is, accordingly, a negative statement, where *en mi vida* takes the place of the negative particle, i.e. before the verb.

3. The preposition *en* is at present becoming more and more associated with negative statements, especially in

beginning a sentence: *En todo Madrid hay hombre más feliz, en diez años no escribió nada*, but *durante diez años escribió sin parar*.

4. Spanish has two sets of forms for the imperative:

sing. lava
pl. lavad

sing. no laves
pl. no lavéis

reflexive:

sing. lávate
pl. lavaos

sing. no te laves
pl. no os lavéis

E. LORENZO

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SECTION A

GENERAL LINGUISTICS

- (4) What questions of general linguistic theory are raised by the study of numeration and of classifiers? What bearing have these questions on the categories of numerals and determinatives?

Chairman: PROFESSOR G. DEETERS

Rapporteur: PROF. W. E. COLLINSON

Recorders: MR. P. B. SALMON
MR. I. RICHARDSON

For Preliminary Reports see pp. 61-74 above.

GENERAL INSTRUCTIONS

These instructions are intended to guide the student in the study of the subject of the course. They are not intended to be a substitute for the text or the lectures, but rather to supplement them.

1. The student should read the text carefully and thoroughly.
2. The student should attend the lectures attentively.
3. The student should take notes on the lectures.
4. The student should study the notes carefully.

These instructions are intended to guide the student in the study of the subject of the course.

INTRODUCTION BY W. E. COLLINSON

In regard to the study of numerators and of classifiers, I have to confess that they are a bit outside my line of country and I must leave the discussion to the experts. Since I wrote my somewhat meagre commentary I have been struck by several facts in regard to numeration which have not been mentioned. One is the discussion which has been going on in Norway in regard to the cardinals, especially the position of the word for the second digit in numbers like 25 and yet, to the outsider at any rate, the Norwegian system is simplicity itself compared with Danish. Another is the use, at least in writing, of a cardinal expression instead of the ordinal in dates and titles. I wonder how far writing is here exerting an influence on speech. Then there is the manner in which a whole number fraction is expressed: in German, *anderthalb* has apparently survived side by side with *ein einhalb*, but *drittehalb* etc. have largely gone out of use. Finally, it would be interesting to see just what areas of Europe are characterised by the clock-time expressions for *half past the hour* as *half of the next hour* e.g. *halb zehn* for "half-past nine" (or Lancashire students persist in using *half ten* for half of the next hour i.e. "half past ten"). A dissertation on the linguistic indication of clock-time might be most valuable, at least from the cultural point of view.

In regard to "classifiers" there would appear to be a *general* and a *specific* problem. Do languages like the Bantu which have an elaborate system of classification which permeates the whole grammatical structure have (1) specific determination or indicators (say when German and Latin have to do with referential pronouns in three genders) and (2) a special set of numeral forms to count each separate class? On the other side a language like Chinese which does not label each expression with the mark of its class-category makes use of certain classifiers with its numerals like our *a hundred head of cattle*. I suppose one interesting problem is the extent and range of such classifiers.

I have raised a few hares for you to chase if you feel so inclined and in any case I have come here to learn and not to teach.

W. E. COLLINSON

The contribution by Mr. Halliday and myself, on classifiers and determinatives, aimed to show the development, in one type of language, of the classifier from numerative function to other grammatical functions, while remaining as separate words, a live category in the language. We may contrast with this the possible state of affairs in the Indo-European family, where, if the suggestion to Dr. W. S. Allen is true, that the class of IE determinatives like *-bh-*, which appear to have a meaning involving physical attributes similar to what is found in numerative classifiers, represents remnants of a nominal classificatory system parallel to that of languages like Bantu and others, then the further development of the classifier has been in a purely lexical direction ending in absorption in the stem.

J. ELLIS

The linguistic derivation of Latin ordinals from Latin cardinals includes that a given ordinal is a lexical species of a given cardinal. Thus the opposition *secundum/tertium* and so on involves lexically the opposition *duo/tria* and so on. But what may be the additional element which distinguishes a given ordinal from a given cardinal? The ordinals *prius* the "first of the two" and *secundum* the "last of the two" have in common that they design "one of two limits," but are distinguished from each other by the opposition "initial limit"/"final limit." Thus Latin cardinals are characterised by (1) objectivity, (2) quality, (3) number and its determinants, but Latin ordinals also by (4) delimitation, which determines number. Besides *primum* "the first of a number" in general, *postremum* the "last" are distinguished by (5) initiality/finality as determinants of the generic feature delimitation. Finally (6) two-ness distinguishes *prius* from *primum*, and *secundum* from *postremum*.

One may call *primum* a pro-superlative and *prius* a pro-comparative. Such a superlative as *optimum* is characterised by the "first of a number of goodnesses," the comparative *melius* by the "first of two goodnesses," but the so-called positive *bonum* is characterised by "goodness as a unit." So it involves *unum*, which consequently is to be called a pro-positive. Finally *postremum* and *secundum* are the contrary of pro-superlative and pro-comparative.

The group *ante horam secundam* designs the hour "before the last of two hours." Consequently we have to interpret the following utterance of Caius Julius Cæsar: *is dies erat ante diem quintum Kalendas Apriles*¹ in this way: "It was the first of five days of which the beginning of April was the last" or literally: "It was the day before the beginning of April, being the last of five days," — the *Kalendae* may have designed originally "calling up the full moon, the Ides." The current re-interpretation "four days before the first of April" is but a case of the substitution in chronology of a modern pattern of thought and language for an old one. Logically it is the substitution of the relation between the last of a group of one and the same group. Compare with the French notions *dans huit jours*, *dans quinze jours* the English notions *in a week*, *in a fortnight*. Linguistically it is the substitution of cardinals for ordinals.

To sum up, a Latin ordinal is a numeral which is characterised by delimitation, a Latin cardinal is a numeral which is not. Cardinals may be substituted for ordinals, for example *unum* in the group *unum . . . , aliud . . . , tertium*. But *aliud* "other of a number" in general and *alterum* "other of two" are characterised by negative indication, they are negative demonstratives. Peculiar cases are *medium* "middlemost" and *solum*. The ordinals are characterised by active delimitation, but *medium* is characterised by passive delimitation. Finally *quotum* is a pro-ordinal. Compare English *the how-manyeth*.

As in Eskimo the so-called ordinals and cardinals are lexical alternants of numerals, the Latin opposition between ordinals and cardinals is conventional, that is to say: super-individual but not universal.

PIER ERINGA

The suggestion was made by Dr. Ellis and myself in our contribution that the problems involved in the study of numeration and classifiers include that of the evolution of classifiers in their semantic function once the numeral is not abstracted. As an example of a language in which the numeral is abstracted from the classificatory system I will mention

¹ *Commentarii belli Gallici* I, 6, 4.

Worora, of the North Kimberley division, Western Australia (see the studies by Capell and Elkin in *Oceania* VIII, 1, 2 1937-8). The Worora noun presents a four class system (Class I being male and associated concepts, Class II female &c., Class III nouns ending in *-u -i -a*, Class IV nouns ending in labial consonant) with concord extending to adjectives, numerals and some pronouns including the incorporated pronoun object. Only the numerals one, two, three are found, two and three being formed by the addition to the numeral one of the dual and trial suffixes; thus the Class I numerals are *iaruŋ*, *iaruŋandu*, *iaruŋuri*, and these become in Class II, with the Class II prefix *nya* and double concord, *nyaruŋ*, *nyaruŋandinya*, *nyaruŋurinya*. A sentence exemplifying concord is *woraliny-andu ny-ambulij-inyandu ny-aruŋandinya atsu nyi-nin-andu ya:d ny-eye:ri-nandu* "two blind girls sat begging," explicitly "girl-two female-blind-two two female-sat-two female-begging-two." Classification and enumeration are here two aspects of the same process.

In some other languages classifiers are incorporated in the numeration system but the classifier is itself a noun with concrete signification. Thus in Bwaidoga, a Melanesian language of the d'Entrecasteau Archipelago, the numerals are *seana*, *luge-na*, *toi-na*, *laufuli*; for the numeration of coconuts the forms are *ifweyana*, *ifolugana*, *ifotana*, *fuliseana*, where *ifo* "portion of husk by which coconut is carried" is the classifier and *fuliseana* is four-one, one unit of four (see Jenness and Ballantyne: *Language, Mythology and Songs of Bwaidoga*, New Plymouth (N.Z.), 1928).

This state of affairs may be contrasted with the complete abstraction of the numeration and emergence of the classifier in a distinct function, that is as a distinct word-class, for instance in Malay. Here the classifier, which assigns the noun to a general category based on a subjective assessment of its visual form, is still part of the system of numeration; it is used only with numerals, but the numeral is now fully abstracted. The classifier follows the numeral and the complex numeral-classifier may precede or follow the noun without difference of meaning (except with the numeral one, which in front of the noun has the special, now non-numerative sense of "a certain"). Because of Malay word-order the classifier is distinctive in such pairs as *sa-chawan kopi* "a cup of coffee," *sa-biji chawan kopi* "a coffee cup."

In Thai the classifier is so far distinct from the system of numeration that it is used not only with the indefinite numerical categories "several" and "how many?" but also after the deictic words "this" "that" and "which?". There is however a syntactic difference, of word-order: the classifier follows the numeral (including indefinite numbers) but precedes the deictic words which thus occupy the normal position of the qualifier. (The numeral "one" can also follow the classifier with the sense "a certain.") The complex containing the classifier follows the noun.

The Cantonese use of classifiers is similar to that of Thai except that in addition to their use with numerals, "several" "how many?" and "this," "that," "which?", classifiers occur with personal pronouns to indicate possession (*O gan fonq* "my room") and combined solely with the noun in a sense comparable to that of English definite article plus noun (*gan fonq* "the room"). The complex containing the classifier precedes the noun; thus while in Malay it is free as to position, in Thai and Chinese it regularly takes the position of qualifier.

Thus in modern Chinese the numeral forms with certain other words a distinct class of qualifier in that it requires a classifier to concretise it. That the function of the classifier is no longer to enumerate is shown by its use with non-numerating qualifiers; but whereas in Canton it may still be said to classify, since it individualizes the referent as a member of a species or class (some objects e.g. "letter (= epistle)" forming a class of one), in Pekingese the classifier not only no longer enumerates; it has ceased almost to classify. Although the grammatical employment of the classifier in Pekingese is similar to that of Cantonese the tendency in the former is for all classifiers to be replaced by the general, semantically unmarked, classifier *go*. *Go* can replace any classifier, but it cannot replace the grammatically identical partitives, collectives and quantitatives (belonging to the same word-class). Its function is thus to express the category "individual" without reference to species; it enters into a two-fold opposition (*a*) marking the individual against part of an individual or collection of individuals, (*b*) marking a species made up of individuals against one that is not (*igo rhen* "a person," *ikyn rhen* "a group of people"; *ibei shui* "a glass of water," where *bei* cannot be substituted by *go*).

The category of classifier must be set up for each language under consideration. The question is whether one can in this way throw light on the historical evolution of the classifier; whether the different languages studied in their present states in fact represent stages in the evolution of the classifier from and out of the system of numeration, via the separation of the functions of classification and enumeration resulting in the complete separation of the classifier from the numeration system, to its appearance in Pekingese as a mere functional element, marking the individual.

M. A. K. HALLIDAY

What I have to say is hardly a contribution to the general question. But, as the use of numeral classifiers in Japanese seems to differ from that in other languages of which I have read descriptions, I hope you will allow me to describe the Japanese system as it exists in the modern language.

The numeral classifiers in Japanese are suffixes which fall grammatically into the same class as the suffixes for weights and measures. In our practical teaching we lump them all together as "units." One way of explaining this is to say that Japanese nouns all behave as if they were the names of substances, not of individual things. Things, that is to say, are not by Japanese grammar countable: you cannot say literally "two pins," but only "pin two-stems" (or perhaps one should say "pin-stuff two-stems"). It is only the units which are countable.

The compound words consisting of numeral prefix plus unit suffix behave syntactically in the same way as indefinite measuring words such as the equivalents of "all," "much," and "a little". But the class is still wider; most words which are the equivalents of English adverbs also behave in the same way. These words in Japanese function, generally speaking, as both adverbs and nouns, just as "tomorrow" does in English. But it seems true to say, of the numeral plus unit at least, that the adverbial use is the normal one, since this use is not only the commonest, but the noun use carries with it certain extra implications which mark it as more specialised than the adverbial one. Typically, then, the numeral plus unit functions as an adverb, signifying the

extent of the action named by the verb. The equivalent of "I got two pins from my friend" is *Tomodati kara, pin o, ni-hon, moratta*; literally, "Friend-from, pin-stuff, to the extent of two stems, got."

I do not know how old this adverbial use is, but the preference for it over what is more or less clearly a noun use seems to be recent. Since there is little doubt that the whole system of classifiers in Japanese has been much influenced by Chinese, it seems particularly interesting that it should have developed what is apparently a quite different method of working.

One word should be added about the word "classifier." Not only is it scarcely needed in describing Japanese, since, as I have said, there is no grammatical distinction between "classifiers" and other "units," but it is somewhat misleading, because most nouns are not, strictly speaking, classifiable according to the unit by which they may be measured. When talking of two pins, for instance, *hutatu* is used as well as *nihon* — *huta*, like *ni*, meaning "two," and *tu* being used in connection with almost anything inanimate. (*Huta* is native Japanese; *ni*, Sino-Japanese. Both sets of numerals are used up to 10, generally native numerals with native "units" and Sino-J. with Sino-J. From 11 upwards the Sino-J. numerals are used, except as regards *tu*, with all "units." *Tu* is dropped entirely for "x objects" where x exceeds 10; it has, that is to say, a zero alternant.) If we take it as equivalent to "object", we may say that "pin-stuff to the extent of two objects" is used as well as "pin-stuff to the extent of two stems."

I hope it will be understood that I am speaking throughout about a certain part of the Japanese language, not about the Japanese mind. "Pin-stuff" is only a desperate expedient to suggest in English the grammatical predicament of the Japanese word *pin* as concerns counting. Moreover, counting and measuring are expressed by different words, and pins are counted. Probably few Japanese have argued the matter out to the conclusion that they count, as I maintain, as if they were measuring.

F. J. DANIELS



SECTION A

GENERAL LINGUISTICS

In the absence of Professor Mezger his report was presented by Professor Whatmough, who called attention to several features which Professor Mezger especially wished to emphasise or correct, notably the contribution that the conceptual dictionary might make to mutual understanding between communities of divergent language-habits.

- (5) Conceptual dictionaries and dictionaries of usage; their nature and use.

<i>Chairman:</i>	PROFESSOR W. VON WARTBURG
<i>Rapporteur:</i>	PROFESSOR F. MEZGER (Report read by Prof. J. Whatmough.)
<i>Recorders :</i>	MR. K. NORTHCOTT MR. P. B. SALMON

For Preliminary Reports see pp. 75-89 above.

THE HISTORY OF THE

In the history of the world, there is no more important event than the discovery of America. This discovery, which took place in 1492, opened up a new world of opportunity and wealth for the European powers. It also led to the colonization of the Americas and the eventual development of the United States.

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INTRODUCTION BY W. VON WARTBURG

Es ist seltsam, dass der strukturelle Gedanke sich den verschiedenen Teilen der Sprache gegenüber so ungleich rasch durchgesetzt hat. Die Betrachtung der Laute und dann auch der Formen fand unter der Wirkung von Trubetzkoy durch die neuen Gesichtspunkte eine Neugestaltung. Das Lexikon hingegen wurde kaum berührt, obschon auch für das Lexikon die Ansätze bei Saussure nicht fehlen. Und doch ist auch der Wortschatz ein in sich geschlossenes Ausdruckssystem, in dem ein Teil den andern bedingt und ausserdem das Ganze als die sprachliche Widerspiegelung des Weltbildes gelten kann. Wenn diese Aufgabe weniger lockte, so lag das wohl daran, dass der Wortschatz verglichen mit anderen Elementen der Sprache eine ungeheure Ausdehnung hat und fast nicht zu überblicken ist.

Es ist also nicht die Frage, ob in einer einzelnen Sprache ein solches System des Lexikons festgestellt werden kann. Es ist vielmehr die Frage, ob es möglich ist, ein System von sprachlichen Begriffen aufzustellen, das auf die verschiedenen Sprachen, auf die verschiedenen Sprachperioden angewendet werden darf. Die Aufgabe, die sich in diesem Sinne zuerst bietet, ist diejenige, eine Sprache zu einer bestimmten Zeit in ihrem sprachbegrifflichen Aufbau zu zeigen. So haben es seit vielen Jahren junge Romanisten unternommen, das Vokabular einzelner Autoren oder einzelner Gegenden zu bestimmten Zeiten darzustellen (Chastell, Wace etc.) Das bedeutendste und erfolgreichste Werk in diesem Sinn ist nicht aus unseren Reihen hervorgegangen; es hat einen Diplomaten zum Verfasser: Casares. Die Darstellung des Wortschatzes französischer Autoren verschiedener Zeiten nach dem gleichen begrifflichen Aufbau hat gezeigt, dass wir zu einer Vergleichung des lexikalischen Systems verschiedener Epochen und verschiedener Sprachen gelangen können, und das ist die Frage, die wir heute zu diskutieren haben, ohne dass diese dadurch vergewaltigt werden. Wenn es gelingen sollte, ein solches System zu finden, wäre der Gewinn ungeheuer. Denn es wäre damit eine Methode gefunden, die lexikalische Struktur der verschiedenen Sprachen einander vergleichend gegenüberzustellen.

W. VON WARTBURG

Nachdem Herr Mezger in seinem *Preliminary Report* die Forderung nach einer neuen, wirklich wissenschaftlichen Anordnung der Elemente eines Wortschatzes begründet hat, möchte ich mir erlauben, auf einen ausführlich gegliederten Vorschlag eines derartigen Aufbaus hinzuweisen. Dieser Vorschlag ist soeben unter dem Titel "Begriffssystem als Grundlage für die Lexikographie" erschienen*, und einige Exemplare liegen für die Teilnehmer des Kongresses zur Einsichtnahme auf. Die Autoren, Wartburg und sein früherer Leipziger Assistent, Hallig, jetzt Dozent an der Universität Göttingen, haben in aller Stille und mit den Unterbrüchen, die der Krieg bedingte, zwanzig Jahre daran gearbeitet. Ich möchte kurz erzählen, wie die Idee eines solchen Systems und dieses selber entstanden sind.

Bekanntlich hat die Romantik mit der Humboldtschen Auffassung von der Sprache als einem lebendigen Organismus die moderne wissenschaftliche Sprachbetrachtung begründet. Es ist deshalb nicht verwunderlich, dass schon im Laufe des 19. Jahrhunderts immer wieder Stimmen laut wurden, welche eine wirklich wissenschaftliche Erforschung des Wortschatzes forderten und mit aller Deutlichkeit betonten, dass dies nur dann möglich sei, wenn die unwissenschaftliche alphabetische Anordnung aufgegeben und der Wortschatz organisch, d.h. vom Begriff her erforscht werde. Ihre Worte verhallten, wenn nicht ungehört, so doch unrealisiert; d.h. eine Realisierung des begrifflich geordneten Wörterbuchs vollzog sich, aber sie kam von einer ganz andern Seite her und entsprang rein praktischen Zwecken, dem Bedürfnis nach dem Finden des passenden Ausdrucks. Dem ersten solchen Werk, dem *Thesaurus of English Words and Phrases* von Peter Mark Roget von 1852, welches ausdrücklich den praktischen Zweck betont, folgte bis in die modernste Zeit eine grosse Zahl von ähnlichen Werken in den verschiedensten Sprachen, als eines der letzten und besten der *Diccionario ideológico* von Casares. Die Situation, welche das 19. Jahrhundert kennzeichnet — auf der einen Seite die Erkenntnis der Unzulänglichkeit der bisherigen Wörterbücher, die Forderung nach einer wissenschaftlichen Lexikographie auf einer systematisch-organischen Grundlage, auf der andern Seite eine lediglich praktischen

* Rudolf Hallig und Walther von Wartburg, Begriffssystem als Grundlage für die Lexikographie. Versuch eines Ordnungsschemas. *Abhandlungen der Deutschen Akademie der Wissenschaften zu Berlin. Klasse für Sprache, Literatur und Kunst.* Jahrgang 1952, Nr. 4. Akademie-Verlag-Berlin 1952. 140 Seiten.

Bedürfnissen gerecht werdende Realisierung — hat ihre tieferen Gründe in der Gesamtentwicklung der Sprachwissenschaft. Diese war in einer ganz andern Richtung orientiert: sie war beschäftigt mit der historisch-vergleichenden Grammatik. Eine entscheidende Wendung trat erst um die Jahrhundertwende ein, und zwar durch zwei Forscher, welche der Sprachwissenschaft in weitestem Rahmen neue Aussichten eröffneten, Jules Gilliéron und Ferdinand de Saussure. Die Beiden kamen von verschiedener Seite und gingen in verschiedener Richtung. Gilliéron ging aus vom zunächst vom Laut her konzipierten Sprachatlas, entdeckte dann aber an Hand seines Materials die Dynamik des lexikalischen Geschehens; Saussure ging aus von allgemein sprachtheoretischen Erwägungen und kam zur Konstituierung der synchronischen Sprachbetrachtung. Die Interpretation der Sprachkarten führte Gilliéron zur Sprachgeographie und die Sprachgeographie öffnete ihm den Blick für völlig neuartige Probleme. Das Interesse verlagerte sich unter der Wucht seiner sprachgeographischen Arbeiten auf die Wortforschung.

Dass das Wort nicht wie im alphabetischen Wörterbuch isoliert im Sprachganzen drinsteht, sondern durch mannigfache Fäden organisch mit seiner Umgebung verknüpft ist, blieb nicht mehr nur eine durch die Logik gewonnene Erkenntnis, sondern wurde zu einer lebendigen, an einer Fülle von praktischen Einzelfällen erprobten Erfahrung.

Fast gleichzeitig mit Gilliéron wirkte Saussure bahnbrechend in einer andern Richtung. Der im 19. Jahrhundert das Feld beherrschenden historischen Sprachwissenschaft stellte er mit unerbittlicher Schärfe und Konsequenz die synchronische Sprachbetrachtung als gleichberechtigten Partner gegenüber. Für Saussure war die Sprache ein Ausdruckssystem, dessen einzelne Glieder ihren Wert nur durch das gleichzeitige Vorhandensein anderer Glieder erhalten. Nur die synchronische Sprachbetrachtung, so behauptet Saussure, sei in der Lage, die Sprache als Gesamterscheinung zu erfassen, während die diachronische Perspektive nur einzelne Ereignisse sehen lasse. Trotz der Einseitigkeit der Saussureschen Auffassung, die später einer Korrektur bedurfte, brachte die Genfer Schule die Sprachwissenschaft einen entscheidenden Schritt vorwärts. Sie erwies die Berechtigung der synchronischen Sprachbetrachtung und machte Ernst mit der

Erforschung der Sprache als eines Ausdruckssystems, dessen einzelne Teile sich gegenseitig bedingen. Die Wirkung blieb nicht aus. Charles Bally veröffentlichte schon 1909 ein Begriffssystem, welches, allerdings noch auf Roget fussend, zum ersten Male über eine lediglich praktische Verwendungsmöglichkeit hinaus einen wissenschaftlichen Anspruch erheben konnte.

Von verschiedenen Seiten also, von der Sprachgeographie, von der Wort- und Sachforschung einerseits, von der Saussureschen Synchronie andererseits, stiess man vor zur Begriffsordnung des Sprachganzen.

Unterdessen ging der Kampf um die Gestaltung des Wörterbuches weiter. 1921 wendet sich Franz Dornseiff scharf gegen die "Einsargung" des Wortschatzes in einem alphabetischen Wörterbuch. Er fordert eine durchdachte Klassifizierung der Begriffe, ein gestuftes Weltbild, ein Schatzhaus der Sprache anstelle eines lexikalischen Haufens. Im *Deutschen Wortschatz* von 1933 versuchte Dornseiff dieses Schatzhaus aufzubauen, ein prinzipiell bedeutsames Unterfangen, mit wissenschaftlichem, nicht mehr nur rein praktischem Ziel. Seine wissenschaftlichen Ansprüche übersteigern sich sogar bis zur Forderung einer vergleichenden Ideengeschichte auf Grund seines Begriffssystems. Als Grundlage für die lexikalische Arbeit der Zukunft ist Dornseiffs Klassifizierung allerdings trotz der hohen Ansprüche kaum brauchbar; dazu fehlt ihm schon die überzeugende Grundkonzeption der Einteilung.

Eine Weiterführung der grundlegenden Zielsetzung kam von romanistischer Seite. Ausgehend von Gillieron und Saussure veröffentlichte Wartburg 1931 einen programmatischen Artikel über "*Das Ineinandergreifen von deskriptiver und historischer Sprachwissenschaft*." Darin wies er nach, dass beide Betrachtungsweisen sich auf einer höheren Ebene zusammenschliessen, dass schon Gillieron die trennende Mauer durchbrochen und das Ineinandergreifen von Statik und Dynamik "mit unübertrefflicher Schärfe" dargetan habe.

Nur eine Verbindung der beiden Betrachtungsweisen kann einen umfassenden Einblick in das sprachliche Geschehen vermitteln; dies bedeutet, dass die historischen Veränderungen stets in ihrer Auswirkung auf das Sprachganze, auf das synchronisch zu betrachtende System überprüft werden müssen.

In den "Betrachtungen über die Gliederung des Wortschatzes und die Gestaltung des Wörterbuchs," *Mélanges Bally* 1939, führte Wartburg seine Gedanken weiter und zog entscheidende Konsequenzen für die Erforschung und Darstellung des Wortschatzes. Die Sprachgeschichte wird zur Strukturgeschichte, welche das sprachliche System in seiner Entwicklung erforscht. Damit eröffneten sich für die Sprachwissenschaft eine Fülle neuer Möglichkeiten.

Gestatten Sie mir, diese ganze Entwicklung an einem geometrischen Bilde zu verdeutlichen, das, ich möchte dies von vorn herein betonen, alle Mängel eines Bildes aufweist und der sehr viel komplizierteren Sprachwirklichkeit nicht gerecht werden kann. Die historische Sprachwissenschaft des 19. Jahrhunderts verfolgte eine Fülle von Einzelphänomenen in ihrer historischen Entwicklung: sie war linear, eindimensional. Dieser Entwicklungsstufe entsprach auch das alphabetische Wörterbuch, welches das einzelne Wort als Individuum behandelte und es gewaltsam aus dem Sprachganzen losriss.

Die Genfer Schule erkannte in der Sprache ein synchronisches System. Sie kam zu einer flächenhaften, zweidimensionalen, jedoch unhistorischen Schau. In paralleler Entwicklung bekam nun selbst der durch das Wort zurückgedrängte Laut plötzlich einen neuen Aspekt und neue Entwicklungsmöglichkeiten. Ausgehend von Trubetzkoy begann die Phonologie den Laut nicht mehr isoliert, sondern als Teil eines Struktursystems zu betrachten. So kam auch die Lautlehre zur flächenhaften, zweidimensionalen Betrachtung.

Diese zweidimensionale Schau wurde gleichzeitig, vor allem ausgehend von den Sprachatlanten, durch die onomasio-logische Betrachtungsweise gewonnen, jedoch, einen Schritt weiter gehend, in der Einzelzelle bereits vertieft zur dreidimensionalen Schau. Jede einzelne historisch-onomasio-logische Arbeit stellt eine solche Einzelzelle dar, die in ihrer organischen Entwicklung dargestellt wird. Hierher gehören auch allerdings von einer andern Fragestellung, der Sprachinhaltsforschung und dem Feldbegriff ausgehend, die Arbeiten der Trier-Schule.

Wenn wir nun vorhin feststellten, dass wir am Beginn einer grossen neuen Entwicklung stehen, so bedeutet dies nichts anderes, als dass wir nicht mehr nur in der Einzelzelle, sondern im Bereich des Sprachganzen übergehen müssen zum raumhaften, zum dreidimensionalen Schauen. Die raumhafte,

dreidimensionale Betrachtung des Sprachganzen ist nichts anderes als die Strukturgeschichte des Gesamtsystems. Erst diese wird die Trennung von Diachronie und Synchronie überwinden und der synchronischen Sprachbetrachtung Saussures den tieferen Sinn verleihen.

Eine parallele Entwicklung bahnt sich in der zunächst zweidimensionalen Phonologie an. Es zeigt sich hier, wie sehr diese ganze Entwicklung in den Denknöten und Schwierigkeiten unserer Zeit bedingt ist, da unabhängig von einander dem Lexikologen und dem Phonologen das gleiche Ziel aufgeht. Diese ist in jüngster Zeit ebenfalls zum dreidimensionalen Schauen übergegangen. Eine ähnliche Entwicklung ist u. a. auch auf dem Gebiet der Wortbildung zu beobachten.

Aus dieser neuen Schau der Dinge ergeben sich für die zukünftige lexikalische Forschung weittragende Konsequenzen.

1. Zweck eines wissenschaftlichen deskriptiven Wörterbuches muss in Zukunft sein, das Ausdruckssystem einer Sprache in seiner begrifflichen Gliederung darzustellen. Da dieses System als Ausdruck des jeweiligen Weltbildes sich von einer Epoche zur andern modifiziert, muss
2. jede (in sich möglichst geschlossene) Epoche gesondert dargestellt werden. Diese je nach der Sprache andere Zeiträume umfassenden synchronischen Querschnitte ergeben zusammen das deskriptive Gesamtwörterbuch.

Zu diesem synchronischen, deskriptiven Gesamtwörterbuch muss eine doppelte dreidimensionale historische Darstellung des Sprachganzen treten:

1. eine historische Darstellung nach Wortfamilien. Beispiel das *Französische Etymologische Wörterbuch*.
2. eine historische Darstellung des Wortschatzes nach Begriffen. Diese Darstellung der Gesamtentwicklung nach Begriffen wird einen Oberbau bilden, der zusammenfasst, was im deskriptiven, nach Perioden geschichteten Gesamtwörterbuch in breiter Darstellung räumlich getrennt lebt, und dieser Oberbau wird erst die krönende Synthese von synchronischer und diachronischer Betrachtungsweise bilden.

Vor dem Hintergrund dieser Aufgaben der Zukunft gewinnt die Frage nach einem Gliederungsprinzip plötzlich eine zentral

wichtige Bedeutung. Es galt ein Gliederungsprinzip zu finden, welches, auf einer überzeugenden und einheitlichen Grundkonzeption beruhend, das Sprachganze organisch auszugliedern, welches jedem Ding seinen Platz im Gesamtsystem anzuweisen und sich den dauernden Veränderungen des Weltbildes leicht anzupassen vermag. Vor diesem Hintergrund der allgemeinen Entwicklung erhebt sich das neue Begriffssystem von Wartburg/Hallig, welches, zugleich von den neuen theoretischen Erkenntnissen und von der praktischen Erfahrung aus einer jahrzehntelangen Beschäftigung mit dem Wortschatz herkommend, eine neue Lösung sucht, und zur Diskussion stellt.

Die in den bisherigen Begriffssystemen vorgeschlagenen Lösungen können nicht befriedigen, weil sie entweder eine unorganisch willkürliche Einteilung geben — die primitivste Form davon ist das synonymische Wörterbuch mit einer alphabetischen Anordnung der Begriffsgruppen wie z.B. Barcia's *Sinónimos castellanos* — oder weil sie nach einem philosophischen System wie Roget (nach Hume) oder nach der Einteilung der Wissenschaften wie Dornseiff klassieren.

Ein philosophisches Prinzip hatte schon Dornseiff abgelehnt.

Aber auch seine Einteilung nach wissenschaftlichen Kategorien ist ganz entschieden abzulehnen, und zwar deshalb, weil, wie wir gleich sehen werden, diese Einteilung sich über die sprachliche Realität hinwegsetzt, und Dornseiff selbst war in seinem *Deutschen Wortschatz* gezwungen, sein Einteilungsprinzip an manchen Stellen aufzugeben. Die Folge davon war ein undurchschaubarer Kompromiss. Er wollte sich nicht von der wissenschaftlichen Einteilung frei machen, trotzdem er selbst z.B. die *Classification des sciences* von Naville sehr richtig mit der Begründung ablehnte, dass das Leben nicht in Gegenstände von Wissenschaften aufgeteilt sei. Damit berührte Dornseiff den entscheidenden Punkt. Eine wissenschaftliche Einteilung ist nur möglich und sogar notwendig auf dem Gebiet der Berufssprachen, auf dem Gebiet der Technik und der Wissenschaft selbst. Aber es ist eine unbestrittene Tatsache, dass die Berufssprachen sich um die Allgemeinsprache *herum* gruppieren und dass der Durchschnittsmensch einen äusserst kleinen Teil der berufssprachlichen Terminologien kennt, nämlich den Teil, mit dem er in Berührung kommt, der in seinem Leben eine Rolle spielt. Jedermann

kennt z.B. die Wörter Auto, Steuerrad, Reifen usw., aber nur der Automechaniker kennt die unzähligen Teile des Getriebes. Eine wissenschaftlich technische Einteilung des Begriffssystems würde bedeuten, die Allgemeinsprache mit einer erdrückenden Fülle technischer und wissenschaftlicher Termini zu überschwemmen, die ihr zu 90% fremd sind. Dies würde zu einer Verfälschung der Sprachwirklichkeit führen, welche die Bemühungen um eine organische Wortforschung illusorisch machen würden. Selbstverständlich gehören auch die wissenschaftlichen und technischen Ausdrücke zur Sprache; auch die Berufssprachen müssen ihren Platz im Begriffssystem finden, aber an der Stelle, an welcher sie in der sprachlichen Wirklichkeit stehen, nämlich unter einer speziellen Rubrik der wissenschaftlichen und technischen Terminologie, d.h. das Begriffssystem muss dem tatsächlichen Verhältnis zwischen Allgemeinsprache und Berufssprachen Rechnung tragen.

Die Konsequenz dieser Erkenntnis bedeutete, ein Einteilungsprinzip zu finden, welches ausgeht vom "begabten Durchschnittsindividuum, dessen Weltbild durch die sprachlich bedingten vorwissenschaftlichen Allgemeinbegriffe bestimmt ist und das mit naivem Realismus die Welt und die Menschen betrachtet." Welch gewaltiger Unterschied zwischen der wissenschaftlichen und der volkstümlichen Begriffsbildung besteht, musste z.B. Jaberg bei der Arbeit am Italienischen Sprachatlas erleben. "Wie weit werden Wiesel und Hermelin, Maus und Ratte unterschieden" schreibt er in den *Sprachwissenschaftlichen Forschungen und Erlebnissen*, und wo ist die Grenze zwischen der Heuschrecke und der Grille zu ziehen? Und dabei stellt er fest, dass die Wissenschaft einige hundert Grillen und Heuschreckenarten kennt! Ein Begriffssystem, welches als Grundlage für die Lexikographie gelten will, muss somit notwendigerweise vom volkstümlichen vorwissenschaftlichen Begriffsgut ausgehen. Nur wenn man sich entschlossen hat, die Allgemeinsprache entsprechend der Sprachwirklichkeit ins Zentrum zu stellen, ist es möglich, das "Weltbild" der Sprache, das jedem Menschen mit der Erlernung seiner Muttersprache vermittelt wird, zu erfassen. Nur dann ist es möglich, der Humboldtschen Erkenntnis, dass die Sprache nicht nur der Äußerung und der Mitteilung dient, sondern darüber hinaus eine Zwischenwelt geistiger Inhalte

aufbaut, Rechnung zu tragen, d.h. das Ausgehen von der volkstümlichen vorwissenschaftlichen Begriffsbildung bildet die notwendige Voraussetzung für die organische Wortforschung der Zukunft.

Unser Überblick war notwendig, um die Bemühungen Wartburgs und Halligs um ein neues Begriffssystem verstehen zu können. Es wuchs heraus aus den Impulsen, die zu Beginn des Jahrhunderts von Gilliéron und Saussure ausgegangen waren und aus einer jahrzehntelangen lexikalischen Forschungsarbeit.

Abgesehen von der volkstümlich vorwissenschaftlichen Grundkonzeption der Einteilung, unterscheidet sich das neue Begriffssystem von allen früheren Versuchen dadurch, dass es nicht Wörter und Bedeutungen ordnet, sondern Begriffe, die nur deshalb durch die Wörter einer Einzelsprache symbolisiert sind, weil Begriffe nun einmal nur durch das Mittel der Sprache ausgedrückt werden können. Das Ideal wäre eine Klassifizierung der Vorstellungsinhalte, welche nicht an die Sprache, d.h. notwendigerweise an eine Einzelsprache, in welcher die Wörter mit einzelsprachlichen Nebenvorstellungen verknüpft sind, gebunden wäre. Schon 1894 hatte Hermann Paul darin die Hauptschwierigkeit gesehen: "Diese Vorstellungen," schreibt er, "dir wir zunächst als etwas vom sprachlichen Ausdruck Unabhängiges aufstellen müssten, lassen sich als solche gar nicht mitteilen, es ist nur eine indirekte, bereits an die Sprache gebundene Mitteilung möglich." Es stellt sich also die Frage, ob man mit Hilfe von sprachgebundenen Wörtern nicht-sprachgebundene Vorstellungsinhalte, d.h. Begriffe klassieren kann. Wir können diese Frage bejahen, weil wir gelernt haben, zwischen Bedeutung und Begriff zu unterscheiden.

Bedeutungen sind sprachgebunden; sie haben fließende Grenzen. Klar begrenzt werden sie erst durch die Sprechsituation, durch den syntaktischen Zusammenhang. Aber aus den Bedeutungen heraus kann sich "ein deutlich umgrenzter Kern entwickeln, der sich vom Mutterboden der Bedeutung abhebt, sich als fassbarer Gehalt dem Bewusstsein einverleibt und dann auch eine vom Wort losgelöste selbständige Existenz führen kann, mit dem Wort zwar verknüpft bleibt, aber nicht mehr mit ihm verschmolzen ist," d.h. "es trennt sich von der Bedeutung des Wortes der sachlich,

in sich nach eigener Gesetzlichkeit bestimmte Begriff." Ich kann mich mit dieser sehr knappen Formulierung begnügen, da im Vorwort zum neuen Begriffssystem und in der sprachphilosophischen Literatur der letzten Jahrzehnte ausführlich davon gehandelt wird.

Der Unterschied zwischen Begriff und Bedeutung wird sofort deutlich, wenn wir den Sprachgebrauch betrachten: wir können von der Bedeutung eines Wortes sprechen, aber nicht vom Begriff eines Wortes, sondern nur vom Begriff, den ich mit einem Wort verbinde, d.h. die Bedeutung ist mit dem Worte eng verknüpft, die zum Begriff verdichtete Bedeutung hat sich vom Wort gelöst (das Beispiel ist von Steinsel). Deshalb verhalten sich Begriff und Bedeutung sehr verschieden in bezug auf die Konstanz: die Bedeutung wandelt sich, der Begriff, einmal von unserem Bewusstsein als Gegenstand erfasst, wandelt sich nicht oder zum mindesten nur sehr langsam. Begriff und Bedeutung verhalten sich zudem verschieden zum Ich: Bedeutungen werden erlebt und können nur umschrieben werden, Begriffe werden "gewusst," selbst wenn sie schwer definierbar sind (jederman kennt z.B. den Begriff "Tisch" — eine Definition dieses Begriffs dürfte jedoch nicht leicht fallen).

Diese beiden Komponenten des Begriffs, die "relative Konstanz" und die "intellektuelle Erfassbarkeit" ermöglichten es Wartburg und Hallig, ihr Ordnungsschema auf Begriffen aufzubauen. Somit klassiert das neue Begriffssystem nicht Ausdrucksmittel wie alle bisherigen Werke, sondern Vorstellungsinhalte. Die Tatsache, dass das neue Begriffssystem sich der französischen Sprache bedient, wird damit nebensächlich; sie hindert nicht, dass die hinter den nationalen Wörtern stehenden Begriffe übernational sind. Die Gliederung ist auf weite Strecken naturgegeben, da, wie Stenzel in seiner Sprachphilosophie betont, die gegenständliche Vorstellungswelt den Völkern weitgehend gemeinsam ist. Wäre es anders, müsste der Plan eines übernationalen Begriffssystems von vornherein aufgegeben werden. Es liegt somit zum ersten Male der Versuch eines Begriffssystems vor, welches als Basis für die lexikalische Forschung verschiedener Sprachen — nicht nur einer Einzelsprache — dienen kann, weil es nicht Wörter, sondern Begriffe, klassiert.

In einer ersten Form war dieses Begriffssystem schon seit 1934 der Wissenschaft zugänglich, weil in diesem Jahre die

ersten so gegliederten Arbeiten aus dem romanischen Seminar Leipzig hervorgingen (Heidel, Heilemann, Runkewitz u.s.w.). Die entscheidende von den Autoren konzipierte Dreiteilung in Universum, Mensch und Beziehungen zwischen Mensch und Universum hat keine Änderung erfahren. Hingegen wurde von hier aus das ganze System neu durchdacht, ergänzt und verfeinert, sodass "sich alles zu einem gefügehafte Ganzen zusammenschliesst." Es ist erfreulich, dass auch Herr Mezger, der sich ebenfalls seit langen Jahren mit diesen Problemen beschäftigt, zu einer sehr ähnlichen Gliederung gekommen ist.

Was das neue Begriffssystem will, ist lediglich — und dies ist sehr viel — jedem Einzelteil seinen festen Platz im Gesamtgefüge der Sprache zuzuweisen. Dieses System mit Leben zu füllen, ist die Aufgabe der zukünftigen lexikalischen Arbeit. Es ist ein synchronisches System und dient in erster Linie der Synchronie. Ohne sich in wirklichkeitsferne, philosophische und ideengeschichtliche Spekulationen zu verlieren, ist sein höchstes Ziel jedoch, der organischen Verbindung von Synchronie und Diachronie in einer Strukturgeschichte des Gesamtwortschatzes den Weg zu bereiten.

KURT BALDINGER

My original suggestion, which you will find in the Preliminary Reports, was prompted by the vagueness of the definitions given in so many dictionaries, whether unilingual or bilingual, comprehensive or merely of synonyms. How often have we not looked up a word in a dictionary and found ourselves no wiser than before! Sometimes this is caused by the lexicographer's satisfying himself too readily with a definition which, though accurate as far as it goes, does not go far enough (e.g. Wyld's definition of *oleaster*), but it may even be that on close examination a superficially plausible definition is really completely inaccurate. Sprach-Brockhaus does not give *oleaster*, but its definition of *oleander* (*seidenblättriger, rösenähnlich blühender, giftiger Zierstrauch*) is slightly off the point in every respect mentioned.

Abstract terms may even defeat definition completely. I will simply quote the essential definitions of *long* given by the N.E.D.:

A I. With reference to *spatial measurement*.

1. *Great* in measurement from end to end. Said of a *line*, of *distance*, a *journey*; also, of a portion of *space* or a material object, with reference to its greatest *dimension*.
2. Having *extension* from end to end.
3. Having the *length* much greater than the *breadth*; *elongated*.

II. With reference to *duration*.

5. . . . Having a great *extent* from beginning to end.
6. Of a period of time . . . Having great extent in duration.

III. That which has continued or will continue in action over a *long* period.

We have still not defined *long*, and much chasing of the various terms used to define it in the above definitions will get us no further. In fact the only way we can define *long*, or whatever the equivalent word may be in our own language, is by a symbolical representation. This symbol could be a line (since *length* is the one positive quality of a line) but I suggest that the oblong is probably better, since it enables us to go on and make diagrams for many other abstract words, including non-idiomatic uses of prepositions, without making any real changes in our basic diagram.

The particular value of such diagrams (even when they are strictly speaking unnecessary — as for *wide*, given *long*) is that before we can construct them we must clarify our ideas and must be quite sure what we mean by the particular combination of letters or sounds which make a word. We are in fact expressing ourselves in mathematical terms — but this time geometrical. We are working out essential patterns, free from the hindrances of individual languages, and in the construction of conceptual dictionaries it is most important that we should work on essential patterns, and not automatically assume that those of our own language are necessarily universal.

I will mention two particular examples of this. It is almost a common-place that the power of generalisation comes only late, and that primitive languages often have many words for individuals of a class and none for a class as a whole. Arabic has many words for different kinds of humped swan-necked yellow riding-animals but none which simply means humped swan-necked yellow riding-animal — *jamal*, from

which we have our word *camel*, being a particular kind of male camel. English, similarly has no word expressing children of the same parents (except, I believe, anthropological *sib*) for which German has *Geschwister*. If we reduce either of these classes to diagram form the similarity becomes overwhelmingly obvious and our class and therefore our arrangement of our conceptual dictionary become clear.

Another very good example appeared in several of the contributions to the Preliminary Report. Most conceptual dictionaries have been based upon a series of opposing ideas (A and anti-A — "good" and "bad") or opposing ideas plus a neutral idea (A, non-A, anti-A — moral, amoral, immoral). Now these oppositions may not be the same in all languages. The Englishman's reaction to *long* is to say *short*, whereas the Frenchman is apparently far more likely to answer *wide* (Fr. *large*). These oppositions are in fact determined by non-logical considerations — rhyme, alliteration, assonance, rhythm — and are unreliable as a guide to the formation of a system, as is shown by the fact when we turn our English *long* into *length* we now have *breadth* as its opposite, but this does not mean that, because English now agrees with French, this is the correct pair of opposites to use as the basis of our system. If however we attempt to reduce our terms to their essence, by means of a diagram we find that in Fr. *long/large* and Eng. *length/breadth* we are opposing two dimensions (if, indeed, opposites can be at right angles!) whereas in *long/short* we are opposing quantities in the same dimension, length in this particular instance. We are now in a position to make our choice — or make no choice at all, if we consider both oppositions equally valid. But by making our geometrical analysis we have at least made quite certain that we are not led astray by accidental peculiarities of our own language. Furthermore, and finally, I would suggest that, wherever the nature of the word permits it, we should make such an analysis even though it may seem unnecessary. We all of us here know at least two languages to a greater or lesser extent, and most of us more, but we all, with very few exceptions, think in Indo-European, and even West European, and our conceptual system is bound to be influenced by West European linguistic habit. We are much more likely to reach a universally applicable system if we can remove its construction as far as possible from that sphere of linguistic habit.

G. MELLOR

Monsieur B. E. VIDOS fait des réserves concernant les dictionnaires idéologiques et préfère les dictionnaires basés sur l'ordre alphabétique.

(B. E. VIDOS)

I feel forced to answer the question put just now by Mr. Vidos concerning the utility of an ideological dictionary.

For a long time I have used the one published by Julio Casares, and I think it is a very useful work, not only for the general public which Mr. Vidos questions, but also for translators, writers and other specialists.

The fact that, having been published after the Spanish Civil War, it has already reached the 3rd reprinting — I think about 15,000 copies altogether — allows us to dismiss the question whether such a dictionary would be of any utility to the public. Clearly it is!

E. LORENZO

A propos du "Dictionnaire" de Casares, M. MANUEL DE PAIVA BOLÉO, professeur de l'Université de Coimbra, demanda la parole pour signaler deux dictionnaires idéologiques en langue portugaise: l'un publié en 1936, à Porto Alegre (Brésil), qui a pour auteur un brésilien, Carlos Spitzer (*Dicionário analógico da língua portuguesa*); l'autre, d'un auteur portugais, Artur BIVAR en cours de publication, depuis 1948 (*Dicionário geral e analógico da língua portuguesa*).

M. Paiva Boléo insista sur l'importance des services que rendent les dictionnaires idéologiques dans l'élaboration de travaux de caractère onomasiologique.

Quant au fond même du problème en question, l'opinion du professeur de Coimbra est que les dictionnaires idéologiques ne sauraient remplacer les dictionnaires d'usage, mais que les uns sont le complément des autres.

(M. DE PAIVA BOLÉO)

J'ai trouvé dans le rapport — par ailleurs excellent — de M. Mezger une conception abstraite de la langue que je voudrais combattre. Elle se retrouve resumée dans cette phrase-ci:

" Conceptual dictionaries will provide an exact and complete representation of the system of concepts of a language " (p. 85).

Il m'est impossible de considérer la langue comme un système de concepts; notre système de concepts constitue l'armature de notre pensée; quant à la langue elle n'est qu'un système de signes destiné à évoquer les concepts. A première vue, cette distinction paraît un peu subtile, voir superflue; mais j'espère vous montrer par quelques exemples qu'elle a une importance pratique.

Prenons les deux substantifs français *fer* et *acier*. Pour les métallurgistes et les chimistes, le mot *fer* désigne un corps simple, que l'on trouve rarement à l'état pur, tandis que le mot *acier* désigne un composé de *fer* et de *carbon*. Mais pour le public français en général, le mot *fer* désigne l'acier non trempé, et le mot *acier* l'acier trempé. On constate donc que les mêmes mots peuvent évoquer des concepts différents chez les différents individus, on ne peut pas dire que les mots *fer* et *acier* représentent certains concepts de la langue; ce ne sont que des signes et le système des concepts qu'ils évoquent peut varier d'un individu à l'autre.

En conclusion il n'est pas possible de considérer qu'un dictionnaire conceptuel représente exactement le système conceptuel d'une langue; au delà de la langue il y a plusieurs systèmes conceptuels.

D'autre part, je ne vois pas le moyen de partir des mots pour aboutir aux concepts, comme c'est dit implicitement dans le rapport. Ce sont les faits et non les mots qui font naître les concepts en nous. Par exemple, à l'école le maître montre aux enfants trois pommes, trois livres, trois fenêtres, etc., et amène ainsi les enfants à concevoir le nombre trois. Il peut dès lors associer le signe *trois* à la notion. Mais sans les faits, l'enfant serait incapable de concevoir le nombre trois; le mot à lui seul est impuissant.

Voilà pourquoi un dictionnaire conceptuel ne peut pas nous fournir une image du système conceptionnel des individus. Le dictionnaire ne peut être qu'un tableau des mots que nous employons pour parler des concepts; nous devons avoir appris à connaître les concepts autrement.

Cette remarque vaut pour la question 4 de la section C, " Quelles conclusions concernant les systèmes culturels et

sociaux peuvent-elles étre tirée des données purement linguistiques fournies par les langues indo-européennes?"

En resumé je ne mets pas en doute la valeur des listes opposant systématiquement les mots; mais je ne puis pas approuver que l'on confonde les signes et les idées.

Je ne désire plus qu'ajouter une remarque d'un ordre plus pratique. Les dictionnaires conceptuels ne portent guère que sur les mots isolés, alors qu'il arrive couramment qu'on exprime une idée au moyen d'une périphrase, par exemple, l'idée d'*interdiction* est le plus souvent exprimée en français par la périphrase "*Tu ne peux pas faire ça.*" Il serait hautement souhaitable que tout procédé soit classé.

E. BUYSENS

It is probably for the sake of completeness, that we find on page 82 of the *Preliminary Report* amongst other languages also the Semitic languages. But since fellow-workers in this field are scarcely represented at this Congress, I would have liked to touch upon some problems confronting the student of Semitic lexicography. He works on a family of languages of the utmost importance for the history of culture, but still has not got much more of scientific value than a dictionary of Biblical Hebrew, a Syriac dictionary and some native Arabic dictionaries. To him who is still dreaming of simple comprehensive dictionaries of Accadian and Arabic, the request (p. 83) that such a dictionary should have by all means the essential quotations "and for certain categories all quotations" of a dead language, seems fantastic.

But instead of speaking upon the general problems of the Semitic linguist in this respect, I prefer to use the minutes allotted to me, in order to acquaint you with some of the problems which confront us in our work on Hebrew lexicography in the Hebrew University of Jerusalem and in the Academy of Hebrew language; and, if I be allowed, the way I have tried to lay out the dictionary of modern Hebrew usage, on which I am working.

Though there has been much innovation and a great influx of terms during the past decades, most modern European languages have a long tradition and possess accepted — sometimes popular — terms in the various fields of technology,

medicine, etc., etc. In so far as one is allowed to speak of our European-American civilisation, the objects to be named are, in spite of all the differences between the different languages, much the same, and in any case, not so different as the scholar might be inclined to assume as an *a priori* starting point.

This is an asset as well as a liability: an asset, since there is much that has already been done during the past generations; a liability, since very many terms should be changed, but by the very nature of inertia tend to linger on.

The conceptual dictionaries compiled in the last two decades on behalf of the Hebrew Academy differ in this very important respect: they try on the one hand to revive terms already minted two or three thousand years ago, which have lasted to our days in a literature which has been thought to appertain to a dead language. On the other hand, the dictionaries are full of artificial creations — sometimes loan translations — from Hebrew and Semitic roots and patterned on usual or unusual Hebrew patterns. The whole reservoir, however, of Latin and Greek, whence terms in modern Indo-European languages are coined, is practically closed.

But since the mode of thought and the contents of modern Hebrew have been so profoundly influenced by loan translations and by the whole of European civilisation, it seems that ultimately the conceptual dictionary of modern Hebrew — and the same holds true, *mutatis mutandis*, of Arabic — will not differ basically from dictionaries of Indo-European languages.

As far as the dictionary of usage is concerned, it is clear that all the suggestions laid down in the preliminary report cannot easily become a reality even in the dictionaries of great and wealthy nations, let alone a nation whose language is, paradoxically, in *statu nascendi* after a history of 3,500 years. Thus the somehow structuralistic suggestions on p. 84 and p. 86 — though perhaps systematically valuable — seem to me impracticable even in languages like English, German and French.

Be this as it may — I have tried to combine in the lay-out of my dictionary practical efficiency with the most scientifically important information — as follows:

The reader will find besides the definition (verbal or non-verbal) one or two — but not more! — quotations, illustrating the possibilities of each usage, the word class (or classes)

as far as practically important, the syntactical peculiarities (not all usages in general — especially the ways of joining prepositions) and the so-called synonyms and antonyms (in their shading — as far as possible). Very important to me are: the most usual qualifiers and verbs (with nouns), and objects (with verbs), and an indication as to the period of the language from which the expression in question has been taken (Old — Middle etc.) or whether it is a recent innovation or loan translation. Furthermore, the layer within the modern language (slang, colloquial, standard, literary, etc.) and its customariness are to be indicated.

Against what has been said on p. 87 — in a language such as modern Hebrew, at least — lexicography must be slightly normative in indicating the correctness (relative or absolute) of the expression.

It might be objected, that this would be stretching the boundaries of a dictionary too far. But also any syntactical information was once thought to be completely outside the scope of a dictionary, whereas even in our days the incorporation of most of the essential facts of syntax into the dictionary has been suggested. To me it seems that we should avoid hard and fast rules as to the boundaries between grammar and dictionary. Professor Buyssens has said in his contribution (p. 86), that the proposed dictionary should "*combler le fossé existant entre la grammaire et le dictionnaire.*" Some systematisers might find it nasty and disturbing: but there are no clear boundaries, and a certain amount of "double" information of overlapping is not only unavoidable but even most desirable, and the user will get the benefit of it. Is this not important enough?

H. H. GOTTSTEIN

Prof. E. OTTO wirft die Frage auf: Wenn die Begriffswörterbücher von den Bedeutungen auf die Begriffe zurückgehen, wie weit sind dann die Ergebnisse der neueren Ontologie (N. Hartmann, Th. Litt, P. Tillich) zu berücksichtigen?

(E. OTTO)

In der Vollsitzung am gestrigen Vormittag habe ich über die grundsätzlichen Besonderheiten der Fachsprachen gegenüber der Gemeinsprache berichtet.

Diese Besonderheiten, kurz zusammengefasst in der Vorherrschaft des Begriffes, bedingen entsprechende Besonderheiten in der Gestaltung der Fachwörterbücher. Auch die Klärung dieser Fragen der Darstellung steht im Arbeitsprogramm des Technischen Komitees ISO/TC 37 "Terminologie," welches ich in meinem Bericht erwähnt habe.

Für die theoretische und lexikalische Behandlung der Gemeinsprache war früher die begriffliche Bedeutung meist nur ein weniger wichtiges Anhängsel zur Form der Wörter, seit einigen Jahrzehnten aber nehmen immer mehr Sprachforscher die Bedeutung der Sprachbestandteile zum Ausgangspunkt ihrer Betrachtungen. Je mehr diese Entwicklung fortschreitet, um so mehr Nutzen wird auch die Sprachwissenschaft aus den methodologischen Erkenntnissen ziehen können, die in der umfangreichen terminologischen Lexikographie gewonnen worden sind. Die meisten Besonderheiten der Fachwörterbücher, von welchen ich jetzt zu sprechen habe, werden daher wahrscheinlich nach und nach aufhören, Besonderheiten zu sein.

3.1 *Die Definition.* — Aus der Tatsache, dass es in der Fachsprache vor allem auf die genaue Bedeutung ankommt, folgt, dass in einem vollwertigen Fachwörterbuch jeder Benennung eine "Definition" beigefügt werden muss.

In dem "Questionnaire," der zur Vorbereitung dieses Kongresses verschickt worden ist, ist folgende Frage aufgeworfen: "What is the desirable balance between descriptive and historical contributions to a comprehensive dictionary? Is there need at present for more descriptive work?"

Die Antwort eines Terminologen kann nur lauten: Allein die Definition ist für die Fachsprache wichtig. Die Etymologie kann von diesem Standpunkt aus nur als gelehrtes Ornament, allenfalls als Merkhilfe gewertet werden. Der an der Bedeutung Interessierte kann nicht oft genug gewarnt werden, in der Etymologie eines Ausdruckes eine Art Definition zu sehen. Denn dann ist die Etymologie irreführend und störend. Wird dagegen jede Wortstelle durch eine kurze Geschichte der Bedeutung der betreffenden Benennung ergänzt, dann ist das eine nützliche Unterstützung der Definition, und auch die Etymologie kann ohne Nachteil in eine solche Anmerkung eingebaut werden.

Die Notwendigkeit des Definierens gilt, wie gesagt, in erster

Linie für Fachwörterbücher. Aber es wäre falsch, daraus etwa zu folgern, dass Definitionen in der "Gemeinsprache" fehl am Platze wären. Keineswegs! Denn auch die Gemeinsprache besitzt keinen Begriff, für welchen nicht irgendein Fachgebiet (oder mehrere gleichzeitig) zuständig ist, wenn auch sehr viele Begriffe der Gemeinsprache anders abzugrenzen sind als die der betreffenden Fachsprache. Da es in der Gemeinsprache aber keineswegs allein auf die begriffliche Bedeutung ankommt, sondern auch in hohem Masse auf Wortfügungen, auf Grammatik, auf Gefühlsworte und Redensarten, hat ein gutes Wörterbuch der Gemeinsprache neben Definitionen auch Anwendungsbeispiele notwendig.

Leider sind die allermeisten Übersetzungswörterbücher der Gemeinsprache in begrifflicher Hinsicht so ungenau, dass ich mir schon vor Jahrzehnten angewöhnt habe, auch für die Gemeinsprache nur einsprachige Definitions- und Bildwörterbücher zu verwenden, wie z. B. den kleinen Larousse und den Bilder-Duden. Aber diese begriffliche Unzulänglichkeit ist kein Wesenszug der Gemeinsprache, sondern nur der meisten Wörterbuchverfasser. Sie ist eine Folge davon, dass die begrifflich unanfechtbare Durcharbeitung auch nur des Wortschatzes eines Taschenwörterbuches eine so gewaltige Aufgabe ist, dass ihr ein durchschnittlicher einzelner Wörterbuchverfasser weder geistig, noch wirtschaftlich gewachsen sein kann. Selbst die bekanntesten Definitionswörterbücher der Gemeinsprache sind begrifflich nicht so genau, wie sie es könnten und sollten. Ich kenne nur eine einzige, dafür um so rühmlichere Ausnahme: *The Modern Library Dictionary of the English Language* (1. Auflage: New York 1947). Allerdings haben an diesem einbändigen Werk 350 Personen mitgearbeitet, und das Absatzgebiet umfasst mehrere Hundert Millionen Menschen.

Viele Fachwörterbücher enthalten auch Bilder. Solche Bilder können bei manchen Begriffen ein nützliches Hilfsmittel sein, um dem Benützer des Wörterbuches rasch eine ungefähre Anschauung zu geben. Es muss aber mit allem Nachdruck betont werden, dass Bilder Definitionen nicht ersetzen können. Und zwar deshalb nicht, weil sie den Abstraktionsgrad nicht erkennen lassen, welcher den darzustellenden Begriffsumfang abgrenzt.

3.2 *Die Reihenfolge der Wortstellen.*—Begriffe definieren heisst, sie gegen verwandte Begriffe abgrenzen. Wenn man also

einen bestimmten Wortschatz definieren will, dann muss man damit beginnen, dass man alle zugehörigen Begriffe "nach" ihrer "Bedeutung ordnet." Für den Benutzer eines Fachwörterbuches könnte eine solche Anordnung schon zur Hälfte Definitionen ersetzen. Handelt es sich vollends um ein mehrsprachiges Fachwörterbuch, so verlangt schon die zwischensprachliche Begriffsvergleiche eine solche semantische Gruppierung der Begriffe.

Bei der Gemeinsprache ist die Anordnung eines Wörterbuches nach der Bedeutung eine seltene Ausnahme, die man dann etwa "Begriffswörterbuch" oder "dictionnaire idéologique" nennt. Bei den Fachwörterbüchern aber hat sich die systematische Anordnung in den letzten Jahrzehnten als allein zweckmässig "durchgesetzt."

Für die semantische Ordnung der Gemeinsprache sind verschiedene Systeme aufgestellt worden. Diejenigen von Roget und Dornseiff sind bekannte Beispiele für theoretisch gut durchgebildete, sprachorientierte Begriffssysteme. Keines von den Begriffssystemen dieser Art aber hat in der praktischen Lexikographie — sei es in der einsprachigen oder in der mehrsprachigen — Anwendung gefunden. Für solche Zwecke sind vielmehr eigene Begriffssysteme geschaffen worden, z. B. in "Heckers Wortschatz" oder im "Bilder-Duden." Aber auch von diesen Systemen hat keines sich so tief in die Gehirne einer grösseren Anzahl von Menschen eingegraben, dass dadurch etwa eine Art gemeinsamer Denkeleise entstanden wäre.

Anders in der internationalen Sprachnormung. Auf diesem Arbeitsgebiet hat man sich kurz vor dem zweiten Weltkrieg dahin geeinigt, alle Fachwörterbücher nach der Internationalen "Dezimalklassifikation" (DK) zu ordnen. Dieses System umfasst gegen 100000 Begriffe. Es wird in der technischen Literatur und in Betriebsbüchereien in grossem Umfang verwendet, und es wird durch internationale Vereinbarungen ständig weiterentwickelt. Die Dezimalklassifikation erhebt nicht den Anspruch, ein Erkenntnismittel zu sein, aber ihre Begriffsgliederungen sind für viele Menschen in vielen Ländern zu einer Art innerer Sprache geworden. Und das ist für die internationale Begriffsangleichung das wichtigste.

Die Dezimalklassifikation ist ursprünglich nur als ein Hilfsmittel für Bibliothekare geschaffen worden, und sie ist

daher in erster Linie auf fachliche Bedürfnisse zugeschnitten. Das war aber kein unüberwindliches Hindernis, auch die 7000 häufigsten Begriffe der Gemeinsprache in sie einzugliedern, wie es in den letzten Jahren geschehen ist. (Als Häufigkeit der Begriffe wurde dabei ein Mittelwert aus den vier am meisten verbreiteten europäischen Sprachen genommen).

Die stürmische Entwicklung der technischen Fachsprachen bringt es mit sich, dass ein buchförmiges Wörterbuch heute nicht mehr auf dem Laufenden gehalten werden kann. Kaum ist ein Fachwörterbuch erschienen, müssen schon neue Begriffe nachgetragen werden. Aus diesem Grunde geht die neueste Entwicklung der fachlichen Lexikographie dahin, jede Wortstelle in ein eigenes Feld zu drucken und 8 oder mehr solche Felder auf einer Buchseite nebeneinander zu stellen. Diese Felder können ausgeschnitten, gelocht und als "Zettel" in Spezialordnern aufbewahrt werden. Wer für praktische Zwecke besonderen Wert darauf legt, kann diese Zettel sogar nach dem Alphabet ordnen.

3.3 *Die lexikalischen Zeichen.*—Die begriffliche Schärfe, die in den Fachsprachen unerlässlich ist, macht lexikalische Darstellungsmittel notwendig, die in den Wörterbüchern der Gemeinsprache unbekannt sind.

So findet man oft in mehrsprachigen Wörterbüchern, auch in solchen mit Definitionen, "nebeneinander zwei Benennungen" für denselben Begriff angegeben, die etwa durch das Wort "oder" verbunden sind. In einem solchen Falle weiss der Benutzer nicht, ob die beiden Benennungen Synonyme sind oder ob sie zwei verschiedene Unterbegriffe des gegebenen Begriffes ausdrücken. Es bedarf erst einer zusätzlichen Unterweisung, d.h. eines konventionellen lexikalischen Zeichens.

Ein besonders schwieriges lexikographisches Problem bilden in Fachwörterbüchern die, wie ich sie nennen möchte, "stilistischen Pseudosynonyme." Z. B. gelten der Definition nach folgende Gleichsetzungen:

voiture (franz.) = Wagen

auto (franz.) = Kraftwagen (Unterbegriff von Wagen)

Im Satzzusammenhang aber könnte es möglich sein, dass das französische Wort "auto" weit häufiger nicht durch "Kraftwagen," sondern durch das kürzere Wort "Wagen" wiederzugeben ist, und umgekehrt. Das wäre dann nur eine

stilistische Gleichsetzung, die in Wirklichkeit keine Gleichsetzung der Begriffe, jedenfalls nicht der Abstraktionsgrade ist. Wie soll nun diese häufige und bequeme stilistische Gleichsetzung dem Benutzer eines Wörterbuches vermittelt werden, ohne dass man ihn dadurch hinsichtlich des Begriffsumfanges irreleitet?

Das Problem "Auto-Wagen" tritt besonders häufig in romanisch-germanischen Wörterbüchern auf. Denn es entspringt einer "Denkverschiedenheit" dieser beiden Sprachfamilien, nämlich der Vorliebe der germanischen Sprachen, mit weiteren Begriffen zu arbeiten als die romanischen. Diese Vorliebe der germanischen Sprachen wieder hängt eng mit der ihnen eignen Fähigkeit zusammen, Wörter nach Belieben zusammenzusetzen. Diese Denkverschiedenheit macht sich auf allen Wissensgebieten in grossem Umfang bemerkbar, und zwar auch bei den naturbedingten Begriffen, d.h. etwa bei den volkstümlichen Namen der Pflanzen und Tiere. So ist im Deutschen die höhere Akstraktionsstufe "Schnecke" (französisch "gastéropode") volkstümlich, im Französischen aber sind es ihre Unterbegriffe "Nacktschnecke" ("limace") und "Weinbergsschnecke" ("escargot"). Man muss also im Satzzusammenhang meistens "Schnecke" durch einen der beiden Unterbegriffe "limace" oder "escargot" übersetzen, und umgekehrt, obwohl das der Definition nach eigentlich falsch ist.

Die Anlage von Fachwörterbüchern nach der begrifflichen Verwandtschaft bringt es mit sich, dass in gewissen Abständen immer wieder eine Anzahl von Unterbegriffen auf einen gemeinsamen Oberbegriff folgt. Die Zusammenschau wird nun ganz bedeutend erleichtert, wenn die nachfolgenden "Unterbegriffe" durch ein besonderes Zeichen "kenntlich gemacht" werden. Werden den einzelnen Wortstellen die Nummern der Internationalen Dezimalklassifikation beigelegt, dann erkennt man die Unterbegriffe an der grösseren Stellenzahl. Aber auch schon ein fatter Punkt kann denselben Zweck erfüllen.

Das Bestreben, in einem Fachwörterbuch den Stammbaum der Begriffe möglichst klar sichtbar zu machen, bringt noch manche andere lexikographische Probleme mit sich. Sie alle können nur durch ein wohldurchdachtes, vereinbartes System der "Instrumentation" gelöst werden.

Der Stammbaum eines einzelsprachlichen Begriffsfeldes

kann ebenso wie ein menschlicher Stammbaum überhaupt nur durch "zweidimensionale," also flächenhafte, Anordnungen sichtbar gemacht werden. Sollen darüber hinaus die Stammbäume von zwei Sprachen miteinander verglichen werden, so ist dazu schon ein dreidimensionales Gebilde, also ein geometrischer Körper (oder dessen perspektivische Darstellung) notwendig.

4. Die sprachwissenschaftliche Terminologie.

Auf den früheren Internationalen Linguistenkongressen ist wiederholt über die Terminologie der Sprachwissenschaft verhandelt worden. Auf dieses Spezialproblem jetzt näher einzugehen, ist kein Anlass.

Auf einen Umstand aber möchte ich doch aufmerksam machen: Die mehrmals erwähnte Internationale Dezimalklassifikation (DK) enthält auch eine umfangreiche Abteilung über Sprachwissenschaft. Auch diese Abteilung wird ständig weiterentwickelt. Es wäre zu empfehlen, dass solche Sprachwissenschaftler, die an der internationalen fachlichen Zusammenarbeit — und insbesondere an einer Vereinheitlichung der sprachwissenschaftlichen Terminologie — interessiert sind, in stärkerem Masse als bisher auf die Weiterentwicklung der DK-Abteilung "Sprachwissenschaft" Einfluss nehmen.

Am Montag Nachmittag hat der CIPL-Ausschuss für die Terminologie der Sprachwissenschaft eine Sitzung abgehalten. Dort wurde beschlossen, die Möglichkeiten einer Mitwirkung beim weiteren Ausbau der DK-Abteilung "Sprachwissenschaft" zu prüfen.

EUGEN WÜSTER

Wozu ein vergleichendes Wörterbuch des Sinnwandels? (Ein Wörterbuch semasiologischer Parallelen)

Seit langem ist in der Sprachwissenschaft die Forderung anerkannt, dass die Annahme jedes Lautwandels durch ein Lautgesetz gerechtfertigt sein muss, wenn es gilt, Wörter in geschichtliche Beziehung zueinander zu setzen. Bestehen aber zwischen Wörtern, die sich lautgesetzlich auf eine Urform zurückführen lassen, mehr oder weniger grosse Sinnunterschiede, so hat man die Forderung nach Erforschung sinngeschichtlicher Gesetze weit später zwar gestellt, aber im

allgemeinen unterlassen, konsequent und systematisch zu verlangen, dass angenommene Sinnwandlungen Gesetzen des Sinnwandels entsprechen, vor allem wohl wieder deshalb, weil, von bestimmten allgemeinen Linien abgesehen, solche Gesetze nicht genügend bekannt waren. Es bestehen zwar übersichtliche Zusammenfassungen von Arten der Entwicklung des Wortsinnes (eine der neuesten ist Kronassers *Handbuch der Semasiologie*, Heidelberg, 1952), aber eine empirische Sammlung von Sinnentwicklungsfällen auch nur für einen Sprachzweig, einen Sprachstamm fehlt. Eine solche will auch das *Dictionary of selected synonyms in the principal Indo-European languages, a contribution to the history of ideas*, Chicago 1949, von C. D. Buck und seinen Mitarbeitern nicht sein, obwohl es eine grosse Tat zur Förderung semantischer Arbeiten ist. Und doch wäre eine Sammlung von Sinnentwicklungsfällen und -Parallelen nicht nur für viele Fragen der Wortforschung und der Semantik sehr nötig und förderlich, sondern auch kultur- und geistesgeschichtlich aufschlussreich.

Die äussere und innere Lage zu rekonstruieren, in der ein Ausdruck geschaffen wurde, muss immer das Ziel der Wortforschung sein. Dazu wird zunächst gefordert, dass die formale Struktur des Wortes analysiert und klar ist wie auch die Beziehungen seiner Teile zu Teilen anderer Wörter; sodann ist der Sinn, d.h. zunächst Nennwert (Meinung), Nebenvorstellungen, Gefühlston, soziale und geographische Verbreitung des Wortes möglichst vollständig zu beschreiben; auch seine Ableitungen können für die Bestimmung seiner Herkunft, und seines Deutewertes ("Grundbedeutung") Wichtiges enthalten, z.B. das lat. Kompositum *sūmo* enthält den alten Nennwert der Wurzel EM, den das Simplex *emo* nicht mehr hat; ebenso die Verbindungen des Wortes im Satz, sein Stilwert, das Sinnfeld mit den Mitbewerbern des Wortes: so kommt man immer näher an die Wortschöpfungssituation heran, und Bedingungen und Triebkräfte seiner Entwicklung werden klar. Für die Forderungen an ein etymologisches Wörterbuch und für die semantische Terminologie s. F. Slotty, Besprechung des Vgl. Wörterbuches von Walde-Pokorny IF 51, 143 ff.

Aber auch dann muss noch der Deutewert und somit der Weg zum Etymon noch nicht erkennbar werden. Z.B. nach eingehender Untersuchung des betr. Sinnfeldes im Ahd. sagt J. Trier in seinem *Deutschen Wortschatz im Sinnbezirk des*

Verstandes, 1931, S. 66 A 2 über ahd. *ferstēn*, as. *farstanden*, ae. *understandan*: "Herkunft und Sinn des Präfixes, zugrundeliegende Metapher und Beziehung sind dunkel." Oder in der begrifflich tiefgehenden philologischen Untersuchung B. Snells *Die Ausdrücke für den Begriff des Wissens in der vorplatonischen Philosophie*, 1924, wird als Deutewert von CYNÍHMI "das Wort mit aussprechen" erschlossen, was nicht zutrifft, wie ich in meiner Prager Dissertation S.IIIff. gezeigt habe. Hier kann nur der Vergleich mit anderen Fällen der Benennung des Begriffes das möglich hohe Mass von Wahrscheinlichkeit dafür ergeben, wie diese Ausdrücke, deren Geschichte teilweise verdunkelt ist, benannt worden sind. Ich habe bereits in meiner Dissertation dargetan, dass vor allem wegen der Wortanatomie, wie sie z.B. Persson durchführte, die Möglichkeiten, ein Wort zu deuten, ungeheuer gestiegen sind, es aber an einem Leitfaden fehlt, um die richtige zu finden. Hier ist der Vergleich des anzunehmenden Entwicklungsweges mit historisch klaren Wegen, die bereits Wörter gleichen Nennwertes nachweislich gegangen sind, von grösstem Wert, wie sich aus zahlreichen Beispielen aaO S.III ff. ergibt. Der Sinnwandelvergleich, wie ich diese Methode nenne, wird ferner noch wichtiger, je weiter wir uns in "die Zeit vor der Völkertrennung" wagen, in der natürlich die Gültigkeit der uns bekannten Wortbildungsgesetze immer mehr abnimmt. Dagegen wiederholen sich die Metaphern, mit denen viele Begriffe benannt werden, immer wieder, ohne dass grammatistische Struktur und Typus oder genealogische Verwandtschaft einen Einfluss darauf hätten, wie ich bereits auf Grund von mehr als 18000 Sinnwandlungen sagen kann, die ich bis 1945 gesammelt habe. Auch in Fragen der Sprachmischung und zwischensprachlichen Beeinflussung wie der Lehnübersetzung ermöglicht uns der systematische Sinnwandelvergleich, ein höheres Mass von Wahrscheinlichkeit zu erreichen.

Andererseits gibt es Unterschiede im Gesamtbild der Sinnentwicklung des Wortschatzes mehrerer Sprachen, ja schon der Sondersprachen und Mundarten. Während sich einerseits z.B. die Metapher "Geschlecht, Geborene" für "Volk" in nach Typus, Abstammung, Ort und Zeit verschiedenen Sprachen findet, kommen bestimmte Metaphern in Isoglossen oder Iosemen vor, die nur bestimmte Sprachgemeinschaften untereinander verbinden. Bis jetzt habe ich eine angesichts

der Gesamtzahl verschwindend geringe Zahl von Metaphern gefunden, zu denen keine Parallelen vorliegen. (Ausgeschlossen habe ich Eigennamen als Individualbezeichnungen). Aus den Verschiedenheiten lässt sich wiederum die Einstellung der Sprachgemeinschaft zu dem benannten Gegenstand ablesen, da sich dessen Benennung nach einem Merkmal in der Sprachgemeinschaft jeweils nur dann durchsetzen konnte, wenn dieses Merkmal sie auf den Gegenstand unfehlbar hinwies, also für sie wesentlich war. Somit ist jede derartige Benennung eine Aussage über geistiges Verhalten der Sprachgemeinschaft.

Der Aufbau des Wörterbuches

Ausgehend von meiner vergleichenden Untersuchung "Die altindischen Ausdrücke für Aufmerken, Wahrnehmen und Erkennen, sinngeschichtlich dargestellt und erklärt" (I. Teil "Die im Altind. neu geschaffenen Ausdrücke usw. . . ." Dissertation Prag 1934, Maschinenschrift, Photokopie entleihbar vom Slavischen Institut der Universität Heidelberg) habe ich zunächst den Wortschatz der geistigen Tätigkeiten im Indischen erfasst und seine Neubildungen mit den Parallelen anderer idg. Sprachen verglichen, sowie mit solchen aus dem Semitischen, Finnougr., den afrikan. und indones. Sprachen, wie auch chinesischen und japanischen Fällen. Das Bild, das ich so von der Benennung der geistigen Fähigkeiten gewann, projizierte ich in die Zeit zurück, in der idg. Ausdrücke wie G'EN "erkennen," und MEN "denken," K^wEI "merken" u.a.m. geschaffen wurden. So gelang es mir, bei fast allen betreffenden ererbten Ausdrücken des Ai. eine Etymologie aufzustellen, die semantisch nicht auf individuellen Mutmassungen beruht. Als ich diese Methode versuchsweise auf andere Gebiete anwandte, konnte ich eine Fülle von Etymologien aufstellen bei Ausdrücken, die bis dahin als ohne Etymologie oder dunkel bezeichnet werden mussten. Obwohl die bald darauf einsetzenden Verhältnisse es mir unmöglich machten, die Ergebnisse zunächst zu veröffentlichen, konnte ich inzwischen in mehreren Fällen, wo Etymologien der betreffenden Wörter von anderer Seite geboten wurden und sich bewährt haben, feststellen, dass sie ebenfalls semantisch mit Sinnwandelparallelen unterbaut waren.

Ich habe meine 1930/1931 — also etwa gleichzeitig mit Buck — begonnenen Sammlungen alphabetisch geführt und diese

Ordnung vorläufig beibehalten, auch in der jetzigen teilweisen Rekonstruktion von etwa 8200 Einträgen nach dem Verlust der Sammlungen von mehr als 18000 Einträgen zugleich mit meinen übrigen Manuskripten infolge angeblicher Konfiskation als deutsches Eigentum in der Tschechoslowakei. Das Wörterbuch enthält in Stichwort jeweils die deutsche übliche Bezeichnung des Begriffes, wo nötig, mit einem verdeutlichenden Zusatz. Dann folgen Belege zunächst aus den idg. Sprachen in einer festen Reihenfolge unter Berücksichtigung auch des Neuind., Neupers., Armen., Heth., Tochar. (soweit möglich), Alban., Bulg. und Sloven., die bei Buck nicht berücksichtigt sind. Bei den einzelnen Sprachen werden auch nach Möglichkeit Wortprägungen der Dichtersprache, wie z.B. bestimmte Epitheta, der Volksdichtung, der Sondersprachen, und die verbreitetsten mundartlichen Benennungen angeführt, soweit diese zugänglich sind. Bei jedem Ausdruck wird kurz die soziale und geographische Verbreitung, sein Gefühls- und ästhetischer Wert ersichtlich gemacht. Bei jedem Ausdruck wird der Nennwert, der ja bereits im Stichwort steht, nicht mehr angegeben, ausser wenn begriffliche Unterschiede bestehen, wohl aber wird der Deutewert, also das Merkmal jedes Ausdruckes diesem beigesetzt, z.B. Stichwort "Hahn" (*gallus domesticus*); zugleich Verweis auf "Hahn" — "Männchen bestimmter Vogelarten." Der Deutewert von dt. *Hahn* und germ. *hana* ist "der Sänger," er erhält eine Kennziffer z.B. 1a, 1; lit. *gaidys*, Deutewert ebenfalls "der Sänger," erhält Kennziffer 1a, 2. Gr. 'HIKANÓC, Deutewert "Frühsingender," erhält Kennziffer 1b, 1; afr. *chante-cler*, Deutewert: "Sänger der Frühe," erhält Kennziffer 1b, 2; hebr. *tarnegól* "Schreier" 1c, 1; dt. *Wetterprophet* "tempestatis propheta" Kennziffer 2a, 1 usw. usw. Der erste Teil der Kennziffer läuft durch alle Sprachen durch und zeigt an, welche Deutewerte gemeinsam sind und welche einander nahestehen. Untereinander näherstehende, aber nicht identische Deutewerte werden durch a, b, c usw. unterschieden, erhalten aber dieselbe erste Kennziffer. Der zweite Teil der Kennziffer gibt die Häufigkeit der Benennungsweise, bezw. des Deutewertes, oder des Merkmales an. Nach den idg. Sprachen folgt das Sem., vertreten durch Akkad., Hebr., Arab.; das Finnougr., vertreten durch Ungar. und Finn.; das Türkische; das Kaukasische, vertreten durch Georgisch; das David.,

vertreten durch Tamil und Kanar.; dann Chines.; Japan.; ein Vertreter des Indonesischen; Suaheli und Quechua.

Wer nur Idg. treiben zu dürfen glaubt, wird sich an diesem Buch überzeugen können, ob die Stimmen der Völker wirklich so sehr voneinander abweichen.

Der aufzunehmende Wortschatz betrifft

- I. das menschliche Wollen, Fühlen, Denken
- II. das organische Leben
- III. Erscheinungen, Eigenschaften und Zustände der Natur
- IV. die menschliche Gesellschaft, Kultur, Religion.

Unter "Denken" werden auch die Sinnwandlungen bzw. Metaphern behandelt, die zu Ausdrücken des Beziehungsdenkens wie Adverbien, Präpositionen, Konjunktionen geführt haben, was natürlich auch der vgl. Formenlehre und der vgl. Syntax zugute kommt, ebenso die Pronomina. Dieses Wörterbuch der sinngeschichtlichen Parallelen (zum Terminus "Sinngeschichte" vgl. Sloty S. 145 aaO) wird durch seinen Aktionsradius und seine Fragestellung nach dem Merkmal der Gegenstände eine Vorarbeit zu einer Geschichte der Begriffe sein.

Der Fortgang und Abschluss dieser Arbeit hängt zunächst von den materiellen Bedingungen ab. Doch lassen die Stimmen, die diese Unternehmen begrüßen, hoffen, dass sich auch Faktoren zur Förderung dieser Bedingungen finden.

NACHTRAG. Erst nach der Niederschrift dieser Zeilen erhielt ich von Prof. Meriggi den freundlichen Hinweis auf seine Aufsätze "Sulla semantica," *Archivio Glottologico Italiano* 25 (1934), 65-103, und "Sugli avverbi di tempo," *Scritti in onore di Alfredo Trombetti* 1937, 235-285, deren erster den Sinnwandelvergleich in seiner ganzen Wichtigkeit würdigt und als unentbehrliches Arbeitsprinzip bei der Wort- und Sinnforschung (auch in Morphologie und Syntax) fordert, während der zweite besonders die methodischen Folgerungen anwendet und deren Richtigkeit an zahlreichen schwierigen Beispielen eines von der Wortforschung vernachlässigten Sinnfeldes glänzend dartut. Diese in gegenseitiger Unabhängigkeit aufgetretene Uebereinstimmung zwischen dem oben Gesagten und Prof. Meriggis Ausführungen kann als Beweis für die Notwendigkeit unserer Folgerungen gelten.

JOHANN SCHRÖPFER

CONCLUDING REMARKS

Zuerst möchte ich festhalten, dass an dem hohen *praktischen* Wert der Ordnung des Wortschatzes nach Bedeutungen nicht gezweifelt werden kann. Eine Uebersicht über die gesamten Möglichkeiten, welche die Sprache zur Darstellung eines Sachverhaltes zur Verfügung stellt, eine Abgrenzung derselben untereinander, mit Berücksichtigung der stilistischen Werte und der Situation, in denen jeder Ausdruck angebracht ist, kann bei der alphabetischen Darstellung nie erreicht werden. Das Alphabet ist unentbehrlich für das Register, mit dessen Hilfe jeder Ausdruck rasch greifbar gemacht werden soll; darüber hinaus hat es keinen Wert. Selbstverständlich muss eine solche Ordnung des Wortschatzes alle Arten der Darstellung eines Sachverhalts berücksichtigen, ob sie nun verbaler, substantivischer, adjektivischer Art seien, ob sie in einer sprichwörtlichen Redensart bestehen u.s.w. Der Begriff der Wortarten kann hier nur eine untergeordnete Rolle spielen, auch schon deswegen, weil die eine Sprache oft durch ein Adjektiv ausgedrückt, was eine andre mit einem Verbum darstellt usw.

Dass auch die rein wissenschaftlichen (philologischen, linguistischen) Zwecken dienenden Wörterbücher sich durch eine nur alphabetische Aufreihung der Wörter eines grossen, ja des besten Teils ihrer Wirkung berauben, leuchtet unmittelbar ein. Hätten z. B. die Verfasser der Wörterbücher zu Corneille und zu Racine ihren Stoff sachlich gruppiert, so würde daraus etwa für das Gebiet der Gefühlsreaktionen oder der Charakterdarstellung der ganze Gegensatz zwischen den beiden Dichtern und den von ihnen repräsentierten Generationen herausspringen, während bei alphabetischer Aufreihung die Art und Weise wie die Wörter sich gegenseitig abgrenzen und wie sie sich zusammenfügen, nicht erfasst werden kann.

Was den *wissenschaftlichen* Wert der Bemühungen um das Auffinden einer innern Ordnung des gesamten Wortschatzes anbetrifft, sind, wie schon zu Beginn dieser Sitzung bemerkt, zwei Fragen zu unterscheiden: 1. hat ein gegebenes Vokabular wirklich ein Ordnungsprinzip in sich; 2. hat dieses Prinzip von einer Sprache zur andern, von einer Periode zur andern genügend Verwandtschaft, damit man allen Sprachen und allen Perioden ein gemeinsames System zugrunde legen kann?

Die erste Frage ist zweifellos mit ja zu beantworten. Dieses Prinzip ist das sprachliche Korrelat des Weltbildes, das der Durchschnittsmensch in sich trägt, wobei zum Weltbild auch das Gewoge seiner Gefühle und Wertungen, seiner Wünsche und seiner Träume, gehört, und wobei zu beachten ist, dass bei jedem Menschen etwas Individuelles bestehen bleibt. Die zweite Frage zu beantworten ist Sache der durch die heutige Sitzung eingeleiteten Diskussion. Ihr einen festen Ausgangspunkt zu liefern haben Hallig und der Vorsitzende in zwanzigjähriger Arbeit das Begriffssystem geschaffen, das heute publiziert vorliegt; das wäre nicht geschehen, wenn nicht beide fest davon überzeugt wären, dass eine gewisse Naturgegebenheit diese Ordnung rechtfertigt. Dass ihr nicht der Charakter der Willkürlichkeit anhaftet, geht auch daraus hervor, dass Herr Mezger, der — wie Herr Whatmough uns gesagt hat — ebenfalls schon seit langen Jahren diesen Problemen nachgeht, zu der gleichen Grundeinteilung gekommen, ist, wie wir. (1. Universum, 2. Mensch, 3. Mensch und Universum). Selbstverständlich muss das Gefüge einer solchen Begriffsschau so locker wie möglich sein und darf nicht zu sehr ins Einzelne gehen, damit der Eigenart der verschiedenen Sprachen nicht Gewalt angetan wird. Der Gewinn, den die Sprachwissenschaft aus einer solchen gemeinsamen Grundlage ziehen würde, wäre sehr gross; sie würde die Vergleichbarkeit der verschiedenen Systeme gewährleisten und würde damit auch erlauben, ihre Besonderheiten und die sprachlichen und kulturgeschichtlichen Vorgänge beim Umbau des Wortschatzes einer Sprache von Periode zu Periode zu erfassen. In diesem Sinne zielt auch das *Französische Etymologische Wörterbuch* über die Darstellung von Artikeln hinaus auf eine Strukturgeschichte des gesamten galloromanischen Wortschatzes hin, wofür dann die einzelnen Artikel, wie sie im alphabetisch geordneten Teil des Wörterbuches vorliegen, das Material vereinigen.

W. VON WARTBURG

SECTION B

COMPARATIVE LINGUISTICS

- (1) What special problems arise in the comparative study of languages without a history? What special methods are applicable in such fields?

Chairman: PROFESSOR B. BLOCH

Rapporteur: PROFESSOR M. GUTHRIE

Recorders: MR. D. N. MACKENZIE
MR. I. RICHARDSON

For Preliminary Reports see pp. 91-99 above

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Chairman: Professor B. Bloch
Rapporteur: Professor M. Gutwirth
Reviewers: Mr. D. N. Mackenzie
Mr. I. Richardson

For Preliminary Reports see pp. 91-99 above

CAUSES DES ERREURS DANS LA RESTITUTION DU KAREN COMMUN

Dans un article du *B.S.L.* (t. 42. pp. 103-111) en 1946, j'ai fait une restitution du Karen commun, en comparant les dialectes Pwo et Sgan d'une manière formelle, en tenant compte de tous les mots dont la correspondance des tons et des initiales était régulière.

Le résultat a été la restitution d'une langue à deux tons, à trois séries d'occlusives, à nasale et liquides sourdes: *hl*, *hm*, *hn* à occlusives finales. M. Luce, de la School of Oriental and African Studies, a recueilli des dialectes Karen de montagne, qui ont conservé les nasales et liquides sourdes, et les occlusives finales, mais qui sur deux points contredisent mes restitutions.

Pour un phonème représenté par γ dans les dialectes actuels, j'avais restitué η et $h\eta$. M. Luce ne trouve pas de nasales mais *h* ou *r*, il faut donc restituer *r* et *hr* et poser l'évolution: $r > R > \gamma$. Les mots ayant *r*: *run* "forge," *mæron* "variole" dans les dialectes actuels doivent être des emprunts plus récents.

De même les initiales *py-*, que j'avais maintenues pour la langue commune, sont représentées par *pl-* dans les dialectes archaïques; il faut donc restituer *pl-* et admettre l'évolution *pl- > py-*. Les mots en *pl-* que j'avais donnés pour Karen commun, tel *play* "bouteille," *Klot* "fourreau" sont donc des emprunts récents.

Cet exemple montre comment dans les langues sans histoire connue, la restitution comparatiste et structurale doit s'accomplir de l'étude des emprunts.

A. G. HAUDRICOURT

I feel that it is perhaps an accident of the history of our science that the question before us has been put in the form given: It implies that for languages without a history there are special problems, problems in addition to those applicable to the study of languages possessing a recorded history.

I shall not repeat here my views on the analytical procedures involved in comparative linguistics, which I have briefly stated in the *Preliminary Reports*. I merely say that I claim that

these procedures hold for the comparison of any languages regardless of records.

I should say rather that the shoe is on the other foot, and that just because our predecessors started their studies with recorded languages we should not lose sight of this fact. Indeed it is in the study of recorded languages that the special problems have arisen, and it is these that have given rise to that noble and complicated subdivision of our discipline, usually termed "philology".

The special problems to which I refer are:

(1) The analysis and statement of the system on the basis of incomplete, inexact, and often misleading raw data. Our scribes, as so many so painfully know, are often miserable informants or recorders.

(2) The investing of this system with life — i.e. our phonetic notions of a dead language always depend on our knowledge of what at least one live language sounds like.

To such extent as we can vocalise a Hittite text, we do so ultimately on the basis of our knowledge of the phonetic values of phonemes in such languages as English, German or Russian on the one hand and Arabic and perhaps traditional Hebrew on the other.

(3) The use of these props for enriching our reconstructions by providing either (a) intermediate stages; or (b) additional extrapolation points. I say props advisedly, since the records have in the past often saved us temporarily from an embarrassment to which the foggiest of the method should rightfully have brought us.

E. R. HAMP

Professor Guthrie has presented to us in his report the different aspects of our question. I should like to call your attention to the following two points:

(1) The difference between the methods to be used in the comparative study of the IE languages and those to be used in the other fields is, after all, not so great as it might be deduced from the formulation of our question. Only in the especially favoured situation we have in IE research, the small methodical difficulties have lost their importance. However, if

our description of the mutual IE relations is going to be exhaustive, we shall meet with the same difficulties as in the other fields. In IE too, we have to deal with both genetic and typological relationship. The former type of relationship is well known to us in IE, Semitic and Ural-Finnic research; as to the latter type, I am thinking of such facts as the converging development of periphrastic tenses (or perhaps aspects?) in most of the European languages: *he has been*, *il a été*, *on byl*, and then, especially of the typological relations in the European vocabulary caused by the influence of Greek and Roman civilization.

(2) The study of IE, its results and its methods, maybe, can serve as a model for a comparative study of the other languages. Here we have to deal with genetic and typological relationship; the difficulties and dangers, however, are much more serious. Therefore, our methods have to be thoroughly improved. In this connexion I shall call your attention to the necessity which consists in the establishment of regular relations of content-units, just as such relations have been worked out in the field of expression (so-called sound-shift). For this purpose we must work out a method resulting in an exhaustive analysis of content-units.

As an illustration of my idea I shall give the following example: T + CGANCp¹ an ordinary Danish, German, Greek or Sanskrit adjective with its theme (central pleremes) and its implicit morphemes (marginal pleremes). Comparing it with an ordinary English adjective, a T + CCp, we can, now, register a shift (or as I prefer to say, a relation) of content-units, in the same way as, long ago, certain relations between expression-units were registered:

Expression	Content	
Skr. <i>návaḥ</i>	Germ. NEU ²	T + CpGANC
Engl. new	Engl. NEW	T + Cp C

It seems to me that such — as I hope — precise methods will be useful for comparative work both in the field of IE research and in the study of languages without a history.

JENS HOLT

¹ Concerning the symbols CGANCp I beg to refer to J. Holt, *Rationel semantik* (pleremik). *Acta Jutlandica* XVIII, 3. Aarhus 1946.

² The capitals indicate the content theme.

Certain special features of language structure and language history increase the difficulty of demonstrating linguistic kinship. Such features include, for example, extremely short meaningful elements, and any reductive phonetic changes which may have sloughed off parts of original elements or neutralized old phonemic distinctions. However, I believe and here attempt to show that no such features ever actually prevent the establishment of common origin, provided only that one makes proper use of quantitative as well as qualitative criteria of relationship. The idea of testing linguistic correspondences against a calculated index of chance convergence was first used, to my knowledge, by Björn Collinder.¹ My own approach is similar in principle though different in detail. It has given good results in resolving a number of moot problems of remote common origin in the American Indian field, a few examples of which I shall present here by way of illustration. Needless to say, the same general principles apply everywhere in linguistics.

Chinook, spoken along the lower course and mouth of the Columbia River in northern Oregon and southern Washington, is a polysynthetic language with a high degree of morphemic reduction. Many important elements — including verb stems as well as pronominal and other grammatical affixes — are made up of a single consonant or vowel. This is certainly the ultimate of linguistic brevity. Moreover, Chinook has a noteworthy grammatical peculiarity, which one might expect to limit linguistic comparison, in its systematic use of sound-imitative constructions where other languages use verb stems; thus, "make lp" is the normal expression of "to boil." Does the existence of extremely brief elements and of sound-imitative substitutes for stems prevent the discovery of genetic relationships? No, for Edward Sapir was able to suggest long ago the distant connection of Chinook with the California Penutian languages, with various languages of Oregon, and with Tsimshian of British Columbia.² Although Sapir never published more than scattered bits of his evidence

¹ La parenté linguistique et le calcul des probabilités, *Språkvetenskapliga Sällskapets i Uppsala Förhandlingar* 1946-48, pp. 1-24.

² Edward Sapir, A Characteristic Penutian Form of Stem, *International Journal of American Linguistics*, vol. 2, pp. 58-67, 1921. Some specific lexical comparisons are included in L. S. Freeland, The Relationship of Mixe to the Penutian Family, *IJAL*, vol. 6, pp. 28-33, 1930, and in Sapir and Swadesh, Coos-Takelma-Penutian Comparisons, *IJAL*, in press.

for the Penutian connections of Chinook and Tsimshian, it is possible by quantitative and sampling methods to rediscover the proof and to see even further possibilities of genetic connections.

Disregarding the glottal stop, whose occurrence is positionally limited, Chinook has 27 phonemes: *i u a p p' t t' ɕ [ts] ɕ' c [tʃ] c' tʃ tʃ' k k' q q' s ʃ ʔ* (voiceless spirant) *x ɣ m n l*. All of these are used for meaningful elements, except that this applies only imperfectly to the contrasts of glottalized versus non glottalized, sibilant versus shibilant, and palatal versus postpalatal. In these three instances the contrasts are employed to indicate diminutive-augmentative symbolism as well as to show primary lexical contrast. To simplify our calculations while erring only on the side of conservatism, let us disregard one phoneme in each symbolic pair, therefore leaving out of our count *ʃ ɣ p' t' ɕ' c' tʃ' k' q'*. This leaves 18 phonemes. On this basis, let us calculate the chances of accidental convergence between Chinook and other languages and language families. When we have done so, we will apply the test to the pronominal affixes, first person *n*- and second person *m*-, and then to a general list of vocabulary elements.

Turning to other North American languages, we find that the number of phonemes varies from less than 20 to over 40 and that, even allowing for limitations in the lexical importance of some of the contrasts, one never need go below fifteen as a basis for calculating convergence. If we assume for the moment that all phonemes are of approximately equal frequency in the meaningful elements of each language, there would be one chance in fifteen of a given item being expressed by a given phoneme — provided that it is marked (wholly or principally) by one phoneme. Actually, *n* tends to be more frequent than other phonemes, so that the chances of an American language using *n* for the first person is somewhat greater than one in fifteen, but it is probably not more than one in ten on the average. The phoneme *m* is usually less frequent than *n*, and we may reckon the abstract probability of *m* in the second person at one in fifteen. The chances of having both *n* and *m* in these two functions is the product of $1/10$ and $1/15$, in other words one in 150.

Where a language has different forms of the personal markers in different specific functions — possessive, intransitive sub-

ject, transitive subject, object, indirect, independent — this of course multiplies the chances of convergences. In fact, in a series of languages each having five different pronoun forms, every other language could be expected by sheer chance to have at least one *n*-form for the first person. Therefore, we must narrow our test down. For languages having more than one pronominal form, let us take only one, the intransitive subject in the independent declarative mode; where this has more than one form, we may take the one used with the largest number of stems.

The first comprehensive genetic classification of American languages north of Mexico, that of Powell published in 1891, recognized 57 separate language families. Some of these families consist of a number of languages, some of only one. For our purposes, each of Powell's families is a unit. For the ramified groups, we must try to reconstruct the protoforms of the pronouns. At this stage, there are some uncertainties, either because the data are unavailable or unclear or because comparative study is not sufficiently advanced, but in most cases we are able to register a set of pronouns for each family. If we do so, we find that at least 20 have *n* in the first person, and at least 19 have *m* in the second person. Ten have both of these signs. These figures far exceed pure chance, which would be $57 \times 1/10$ or about 6 for first person *n*, $57 \times 1/15$ or about 4 for second person *m*, and $57 \times 1/150$ or about one chance in three of having even one language with both markers. From this, we can conclude at once that the Powell classification has separated genetically connected entities. While Powell, for practical reasons, considered only languages north of Mexico, there are languages in Mexico and farther south which could just as well be added, since the high ratio of *n* and *m* continues.

Seven of the ten groupings having both *n* and *m* are ones which Sapir subsequently classed as Penutian in the broad sense, while at least four Penutian languages have different pronouns in one or both persons in the function we are considering. This is not surprising, since the original Penutian system could have been replaced by other forms. By the same token, the non-Penutian language families with *n* and *m* pronouns may conserve an ancient common feature which ties them together with Penutian at a time level more ancient than

Penutian itself. If this should be the case, Sapir's Penutian, Azteko-Tanoan and Hokan-Coahuiltecan would be brought together into a very ancient genetic unity whose time-depth would be two or three times as great as that of Indo-European or Indo-Hittite, and greater than that of Hamito-Semitic. On such an ancient level, Powell's 57 stocks were surely represented by no more than a dozen linguistic entities, and it becomes highly probable that the *n-m* marking of the pronouns was not an accident but due to common origin. That is, three in ten is 45 times greater than the chance expectation ($10 \times 1/150$). However, there may be a certain risk in deducing common origin merely on the basis of two pronominal elements, and we cannot now examine all the other evidence bearing on the relationship. Therefore, let us leave this perspective as one strongly suggested by quantitative considerations and pass on to a more detailed examination of the place of Chinook with reference to its presumable congeners in the Penutian phylum.

According to Boas,¹ "The Chinookan stock embraces a number of closely related dialects . . ." From Boas' statement and an inspection of the material, it would seem that we are dealing with a single language with mutually intelligible variants. It has no close relatives. As already indicated, its pronouns suggest that it belongs in Sapir's broad Penutian grouping. I propose here to check this hypothesis with two Penutian languages chosen because of their geographic separation from Chinook — Yokuts of California and Tsimshian of British Columbia.

For the purpose of testing an hypothesis of distant kinship, it is well to avoid any far-seeking selective procedure, such as culling out of the vocabularies a few prize exhibits which are phonetically close and whose meanings are alike or can somehow be made to fit, for such similarities can be found between any two languages in the world. Instead, I propose a rigid though simple sampling approach. Using the generalized test list of basic vocabulary items which I devised for my studies of lexical statistics and rate of change,² I list the equivalent

¹ Franz Boas, *Chinook*, pp. 559-667, *Handbook of American Indian Languages*, pt. 1, Bulletin 40, Bureau of American Ethnology, 1911. Also see Boas, *Tsimshian*, *ibid.*, pp. 283-422, and Stanley S. Newman, *Yokuts Language of California*, *Viking Fund Publications in Anthropology* 2, New York 1944.

² See Lexico-Statistic Dating of Prehistoric Ethnic Contacts, *American Philosophical Society Library Bulletin*.

elements in each of the three languages in so far as available lexical data permit. The choice of entries is made according to fixed procedural rules which have nothing to do with finding the maximum number of cognates: For each item in the test list, one sets down the most common equivalent expression, preferring independent or relatively independent forms to grammatical affixes, everyday words to technical vocabulary, current expressions to archaic ones. In those cases where there is doubt as to which of two words to list, the item is omitted and disregarded in calculation. In the present case, we are handicapped by the unavailability of dictionaries, and can only partially fill the test lists. However, the omissions are conditioned by what happens to be mentioned in the grammatical discussions and not by any factor of purposely choosing elements to support a theory of genetic relationships.

As between Yokuts and Chinook we are able to supply 125 pairs of words corresponding to the test items. Of these 13 are phonetically close enough to be considered probable cognates, namely:

I	<i>na³</i>	<i>naika</i>	float	<i>hogan</i>	<i>-xun</i>
thou	<i>ma¹</i>	<i>maika</i>	live	<i>xoo</i>	<i>xanati</i>
we	<i>na'an</i>	<i>ncaika</i>	mother	<i>no'om</i>	<i>-na'a</i>
you	<i>ma'an</i>	<i>mfaika</i>	person	<i>yokoç</i>	<i>i-kala</i>
blow	<i>poosu-</i>	<i>pu-</i>	this	<i>ki</i>	<i>xik</i>
cut	<i>k'it'i-</i>	<i>-kittq'uþ</i>	wife	<i>mokiy'</i>	<i>-mkf</i>
drink	<i>'ugun-</i>	<i>-qamst</i>			

For Chinook and Tsimshian we find eleven agreements in 70 pairs, as follows:

you	<i>mfaika</i>	<i>nism</i>	not	<i>nkst</i>	<i>niki</i>
cut	<i>-kittq'uþ</i>	<i>-q'uç</i>	see	<i>-kl, -kft</i>	<i>ka'a</i>
drink	<i>-qamst</i>	<i>aks</i>	stand	<i>-tx</i>	<i>hilk^w</i>
hair	<i>-qfu</i>	<i>qis</i>	sun	<i>-'attaχ</i>	<i>tlugs</i>
mother	<i>-na'a</i>	<i>-nuχ</i>	wife	<i>-mkf</i>	<i>naks</i>
person	<i>i-kala</i>	<i>kat</i>			

Between Yokuts and Tsimshian there are eleven agreements in 110 pairs, including six items already shown (you, cut, drink, mother, person, wife) and in addition:

bone	<i>ç'iy</i>	<i>ç'ip</i>	tongue	<i>talxaþ</i>	<i>tilχ</i>
fat	<i>heexa³</i>	<i>hix</i>	name	<i>hoyoo-</i>	<i>hwa</i>
know	<i>hudu</i>	<i>hulax</i>			

The agreements in all three languages are six among 70 test items.

Although we began this study with apprehensions that the reduced character of elements in Chinook would force us to deal with many one-phoneme elements, the actual test proves that such brief elements are proportionately very few, and that for the most part we are dealing with elements made up of two phonemes or more. In terms of the morpho-phonemic structure of the languages under consideration, each of the two phoneme positions is almost completely free, that is any phoneme can be the first of the two and any of them can be the second, with only a few of the original sequences blurred by positional coalescences. We can therefore calculate the pure chance convergences by squaring $1/15$, giving 0.45 as the index of chance agreements. The actual percentages of probable cognates, however, is 10% for Chinook-Yokuts and Tsimshian-Yokuts, 16% for Chinook-Tsimshian, exceeding the chance expectation by 22 and 35 times. This demonstrates that common origin and not chance unites these languages.

The best proof of the genetic relationship we are testing is to be found in the three-way correspondences. For, if there is one chance in 225 of finding agreements between languages A and B and a similar ratio between languages B and C, then the chance expectation of A-B correspondences coinciding with B-C correspondences is the square of $1/225$, or one chance in 50,625. The six agreements we have found in 70 comparable sets exceeds chance expectation by some thousands of times.

The lexico-statistic method is capable not only of providing a test of relationship but also of giving an index of the time-depth. By using a rate of retention, which has been calculated from historically known cases and which has proven to be relatively constant at about 81% retention per 1,000 years, one can convert any percentage of common elements in the test list to centuries and millenia. Our Penutian figures when so converted, indicate that Chinook has been diverging from Tsimshian for over 4,000 years and from Yokuts for over 5,000 years. Of course, these figures have to be regarded as very tentative because of the limited data available for our study, but it is at least clear that the languages are very distantly related. To take an example by which to gauge it, we may take the relationship of modern English and Russian,

which show about 25% agreement in the same test list. Thus, it is clear that Chinook and Tsimshian are much farther apart than English and Russian.

Our examination of Chinook comparative linguistics raises some side questions.

Do we eliminate structural comparison and phonetic laws from the discipline of comparative linguistics? Of course not. If our method has any validity, then vocabulary correspondences of the type indicating genetic unity will be found to correlate with structural agreements of the type which are found in languages having a common origin. If we have omitted structural comparison from the present essay, it has been only in order to present the sampling method of lexical comparison in greater relief. However, as Sapir has already pointed out,¹ there are strong structural points of contact among the Penutian languages generally; Chinook he characterized as an independent polysynthetic development on the basis of an analytic development of Penutian but with vestigial features bearing out its relationship to the group. In evaluating such a characterization, one must bear in mind the enormous time lapse separating Chinook from the other Penutian languages and also the fact that it is geographically cut off from the others by intervening non-Penutian languages. One might also note how far apart structures may move in historically known cases.² Thus, the structural concordances between Russian and English are not too great even though these languages are in time-depth probably much closer together than Chinook and Tsimshian and even though they are separated from each other geographically only by other Indo-European languages.

As for phonetic laws, they are by no means disregarded in our procedure even in the present preliminary approach to the problem. Thus, when we compare Chinook *-tx* and Tsimshian *hitkw*, both meaning "stand," we have in mind the fact that Chinook has no *h* except in sound-imitative words and that it lacks the distinction between labialized and non-labialized

¹ Central and North American Indian Languages, *Enc. Britannica*, 14th edition, 1929.

² The idea of learning to understand prehistoric phenomena by close examination of historic cases is recommended by Marcel Cohen (*Langues chamito-sémitiques et linguistique historique*, *Scientia* 1951, p. 311) in the following terms: "La conclusion . . . c'est que la tâche la plus importante de la linguistique historique pour l'approfondissement de l'étude du langage est d'étudier toujours mieux les voies de l'évolution dans les périodes où on bénéficie du maximum de lumière historique."

palatals. An original *hitk^w* would necessarily have had to develop, with loss of the vowel, at least to *-tk*. From this to *-tx* is then a small step of sound shifting, or the original might have been *hitx^w* with Tsimshian having shifted the spirant to stop. While some uncertainty must remain until the supposed sound shifts have been fully worked out, nevertheless our comparisons on the whole seem to be well founded. It seems unquestionable that most if not all our comparisons will be eventually confirmed by phonological patterns of correspondence. If some of our comparisons should prove unjustified, we may expect them to be counter-balanced by associations which we now reject but which may turn out to be valid in the light of sound shifts yet to be discovered.

Is it possible that some other factor than common origin may account for the similarities found? Might general sound-imitative tendencies account for the agreements? What about borrowing? Sound imitation may enter into such an agreement as Yokuts *poosu-*, Chinook *pu-* "blow" and might even play a minor role in one or two other agreements, as Yokuts *k'i'ti-*, Chinook *-kittq'up* "cut," but it cannot account for all of the agreements and cannot invalidate the inference of historical connexion we have drawn. As for the supposition of lexical borrowing, I have dealt with this problem at length in another paper, "Diffusional Cumulation and Archaic Residue as Historical Explanations."¹ In that paper, I pointed out that loans are found primarily in the cultural vocabulary rather than in the basic vocabulary, that for items such as are contained in our test list they are only a tiny percentage, and that loans do not accumulate indefinitely but are subject to replacement in the same way as the native vocabulary. In the present case, recent borrowing is precluded by the wide geographic separation, and even more remote diffusion has to be rejected because of the absence of common items of cultural vocabulary. Even if diffusional contacts went back 2,000 years, one would expect to find patent cases of common culture words. If borrowing took place over 2,000 years ago, then such words as had penetrated the fundamental vocabulary would have been cut into by subsequent replacements until less than half would remain of any agreements which then existed between the source language

¹ *Southwestern Journal of Anthropology*, vol. 7, pp. 1-19, 1951.

and the borrowing language. At this rate, Tsimshian and Chinook would have had some 35% of loan agreements in their fundamental vocabulary 2,000 years ago. If one notes that French and English have only five loan agreements in 200 items of the test vocabulary (Eng. *count, flower, mountain, vomit, river*) after 2,000 years of continuous contact, that is between Germanic-English and Latin-French, then it becomes obvious that 35% is a completely impossible figure and there is not the remotest chance that the Chinook-Tsimshian agreements are due to borrowing.

Is it possible that Chinook is not as difficult a case with regard to its reduced elements as some others which may be encountered in other areas? This question can only be answered by actually calculating the differentiations made in each language by reference to the languages with which one proposes to compare it. It is not sufficient simply to imagine a hypothetical case. For example, if one wished to invent a language whose structure makes remote comparison impossible, one could suggest one whose longest meaningful element is a single syllable made up of a consonant and a vowel, whose consonant system consists of only five phonemes, whose vowels undergo mutations which makes it impossible to distinguish one lexical element from the other on the basis of the vowel; there would then be only five distinctive phonetic complexes possible and the sheer chance ratio of agreement would be 20%. But it is obvious that such languages do not and cannot exist, simply because the communicative function of language in human society requires much higher differentiative possibilities.

What of the obscuring effects of sound changes? Can these be so great as to prevent linguistic comparison? Again the question must be answered on the basis of actual languages and not on invented suppositions, for the role of language in conveying meaning between the older and the younger generations precludes any fantastically abrupt leaps. Sweeping reductions may affect some phonemes in some positions, but they cannot transform a language entirely in a single generation. As to the possibility of a series of changes over a hundred generations which entirely obscure relationships, this can be concluded only if concrete instances are found.

As a check on the conclusions drawn from Chinook, let us examine another Indian language in an entirely different area.

characterized by few phonemes and by extensive reductional changes in the stems.⁸ Atakapa, a now extinct language of Louisiana, has 13 consonants and 4 vowels. The typical stem is CVCV, CVC or simply CV. When Atakapa is compared with distantly related Chitimacha, one finds that CVC stems of one language often correspond to stems which have obscured the final in the other.—To estimate roughly the field of chance between these two languages, it seems safe to multiply 13 for the number of initial consonants by 4 for the number of vowels by 5 for what is left of the differentiating power of the final CV. This gives 1 in 260 as the ratio of chance. Even if one multiplies $13 \times 4 \times 4$, the ration is 1 in 208, only slightly higher than that which we used for Chinook. From such examples as Chinook and Atakapa, which were purposely selected because they seemed to put the comparative possibilities to a difficult test, I tentatively conclude that in the most unfavourable cases of comparison, involving two languages without ancient written records and showing extensive phonetic reduction, the possibility of effective comparative work continues strong for some five or six thousand years. In favourable cases, especially where the original language is to-day represented by many ramified off-shoots, admitting the reconstruction of intermediary stages, it should be possible to penetrate time-depths twice as great even without the help of historical records. I have recently completed a study demonstrating the common origin of the Mosan stocks, where the time-depth seems to be about 9,000 years.

MORRIS SWADESH (read by J. ELLIS)

It should be emphasised that the comparative study of languages without a history is a very recent extension of linguistic studies. Most of the principal language groups which have formed the subject of earlier studies, have had some

⁸ See Swadesh, *Phonologic Formulas for Atakapa-Chitimacha*, *IJAL*, vol. 12, pp. 113-132, 1946. Some important details of this treatment need to be corrected, especially the reconstructed vowel system, which is too complicated (e.g. a form like Atakapa *hul-* "to sift," corresponding to Chitimacha *hayi-*, is from **hari-*, with the first vowel assimilated to the second, and does not require the reconstruction of a separate vowel phoneme). The proof of relationship in this article is based on proportion of presumable cognates to total available vocabulary (see p. 113) in place of the simpler and better controlled sampling method subsequently developed. The sampling method is used in my recently completed and still unpublished paper, *Mosan as a Problem of Remote Common Origin*, as a means of clinching the case built up by structural, phonological and lexical evidence.

historical background, even though this may not have been sufficient to establish upon a satisfactory historical basis the origins of languages within a group. For instance, though history has not preserved the parent of the Teutonic or the Slavonic groups, comparativists studying the languages of either group do possess some historical records, which may or may not provide support for any results derived from the inductive approach.—But languages with no history at all, like the Bantu languages, provide no evidence for checking results which must remain inferential. The circumstances may call for a return to the attitude prevailing in grammatical scholarship before the French Revolution. At least for Bantuists, it may have to be taken for granted that evolutionary classification of Bantu languages is not possible. Bantuists will then return to the attitude towards the study of languages shared by grammarians for more than two thousand years, accepting the fact that the Bantu languages exist, but without probing into the origins of their diversity.

Meanwhile the experience of the Bantuist in the comparative field must, to a very large extent, be a repetition of what has gone before in linguistic studies. The doctrine of organic evolution may influence his approach. Meinhof's attempt at reconstruction of Ur-Bantu roots is an example of this. Yet even if Meinhof's work had been completely satisfactory and comprehensive on the linguistic level, it could have no certain historical value. One is reminded of the question put by Von der Gabelentz in his book *Sprachwissenschaft* (1891):

"If we could retrace our steps for a moment to the presumptive root-stage of language, should we be entitled to say that it is the first, and not perhaps the fourth, or seventh, or twentieth in its history?"

Yet Meinhof's emphasis upon the relationship of Bantu languages is not to be disregarded. The Bantuist may be forced to reject the use of the term *related* in the evolutionary sense, in reference to the languages of the Bantu field, but he can certainly retain its use within the limits imposed by the contemporaneous nature of his material.

The problems confronting the linguist studying languages without a history are, in the nature of some of the difficulties, not unlike those facing the scholar concerned with the origins of human speech. It is not forbidden to either to make excursions into prehistory, provided that he recognises that in pre-

history there are no recognisable stopping-places denoting time and place. This proviso is sometimes overlooked by Africanists who extend their excursions, in seeking the origins of African languages, not only into prehistory, but also into some other neighbouring continent or country.

Previous comparative work on languages with at least some historical background has followed the method of comparison rendered effective in establishing the Latin parentage of modern French, Spanish, Portuguese, Italian and Rumanian. The almost continuous historical record of what has happened to Latin has, of course, established the Latin parentage of these languages as an historically verifiable fact. According to this method, word-similarity, grammatical behaviour and consistent differences between words of corresponding meaning have been the most important sign-posts along the historical road. In the relationship of languages without a history, however, it cannot be taken for granted that the same sign-posts will lead us in the right path to prehistory. In the comparative study of Bantu languages, for instance, it is not word-similarity which provides us with the clearest directive, though Greenberg makes an almost exclusive use of this method of comparison in his attempt to classify African languages. Neither does grammatical behaviour help us so long as its criteria for the purpose of comparison remain unspecified. Within the Bantu complex of languages, consistent differences between words of corresponding meaning are a more important clue to probable relationship. This is, in fact, the only practical starting-point so far discovered for any ultimate excursion into prehistory. There may be others but they can be established only when the grammatical, structural and syntactical data have much wider reference than they have at present. Regularity of sound-correspondence does by itself, in the Bantu field at least, suggest linguistic unity, but not necessarily unity of origin. Before unity of origin can be established, the linguistic unity suggested by regularity of sound-correspondence must be checked by reference to other comparative features which only an accumulation of grammatical, structural data may disclose. While leaving the door open for subsequent evolutionary classification, comparative Bantu studies exclude it for the present, and may perhaps have to exclude it altogether. Until the door is shut, we do well to suspend judgment on the subject of origins.

LYNDON HARRIES

In a comparative study of languages without a history special attention should be given to the following principles and hypotheses:

(1) Phonemes, the smallest functional units of a language, are not permanent entities, but a product of the phonematization of previous free variants. Each phoneme performs its function because of the presence, in the system of the language concerned, of a specific phoneme-constitutive series of pairs of signs; the b/m pair of phonemes, for instance, results from the fact that English has a constitutive series bad-mad, boss-moss, bind-mind, bean-mean, etc. Phonematization as well as the semantic differentiation of previous free variants takes much time. The analysis of the semantic relations between previous free variants, allows us, in combination with the length of the constitutive series, to make a rough estimate of the phonematization period. For the purpose of reconstruction the previous free variants take the place of the later pair of phonemes for the period before the phonematization in question. In this way we must try to arrive at a working hypothesis for the gradual reconstruction of previous phonemic systems, but the results will be partial only and only reliable to some extent.

It should be taken into account that in primitive language spontaneous development of new signs through differential fixation of free variants of signs is of great importance, correlative to the rareness of loan-words, using loan-words being the easier way towards amplification of the language. In languages without a history the number of free variants is greater than with us, owing to the lower general frequency rate of speech in the corresponding speech societies. The best way to study the extent of the spontaneous development of new signs is to select a few words of exceptional structure, as peculiarities of the word form may allow us in such cases to reconstruct — to some extent — the group of previous free variants.

(2) As the frequency rate of features of morphology tends to be higher than the frequency rate of signs, and as a high frequency rate is a preservative factor, affinity between languages is recognized more easily in the field of morphological structure than in the vocabulary. In the Indonesian area, the central features of morphology in Javanese, Tagaly (Philippine Islands) and Malagasy (Madagascar) are still closely akin,

though the vocabularies are rather different. On the basis of comparison of morphological features the area should be divided into subdivisions in order to facilitate investigations in the field of phonetic change.

(3) Dictionaries of Indonesian languages are often insufficiently reliable, as they do not distinguish between signs and free variants. If a native informant is confronted with two free variants, and he is expected to explain the difference between them, the fact of the question being put is in itself a stimulus to differentiation. Such differentiations under the strain of the moment belong to the domain of *parole*, and do not enter the *langue*. The authors of many dictionaries do not observe the difference between *parole* and *langue* and therefore give a wrong picture of the actual situation, and provide us with a bad starting-point for comparative research.

(4) Each real addition to a linguistic system, especially in the field of morphology, improves its function as a means of understanding and is, therefore, an acceleration factor. The growth-speed in language is intimately connected with the efficiency of the system. So the time-scale in reconstruction work must show a gradual slowing-down of evolutionary processes, correlative to the complication of the linguistic structure.

C. C. BERG

Inhalt der Bemerkung: Behandlung einer Sprache, nämlich des Deutschen, mit Geschichte "wie wenn sie keine Geschichte hätte."

Dabei führt die rein synchronische Betrachtung von selbst zu einer gewissen Geschichte. Es zeigen sich verschiedene Schichten, die in ihrem Bau nicht zusammenpassen und die sich am besten deuten lassen, wenn man sie als in verschiedenen Zeiten entstanden denkt.

Beispiel: im Deutschen	konjugierbare	)	
	deklinierbare	)	Wörter
	unveränderliche	)	

(a) Nun bei den Konjugierbaren nur eine Wortart, sehr klar geordnet (*Verb*).

(b) Bei den Deklinierbaren vier oder fünf Wortarten (Substantiv, Adjektiv, Artikel, Pronomen, Numerale).

(c) Bei den Unveränderlichen noch weniger möglich, eine Ordnung aufzustellen (gleiches Wort, z.B. "zu" als Adverb, Präposition und Verbal-Konjunktion, in "er gab es zu," "zu ihm," "er versuche zu sprechen").

Beste Denkmöglichkeit, diese merkwürdige "Unstimmigkeit" zu erklären: diese Schichten sind nicht gleich alt. Die klarste, best organisierte ist die jüngste; die uneinheitliche Gestalt der andern weist darauf, dass sie umgebaut, umgedeutet, vielleicht sogar mehrfach umgebaut worden sind. Vergleich mit einer Kirche, an der mehrere Jahrhunderte gebaut haben und die daher verschiedene "Stilschichten" zeigt.

So eine Möglichkeit, eine oft sehr alte Geschichte aus dem heutigen Zustand herauszulesen — wobei freilich immer scharf festzuhalten, dass das nicht historische Sicherheit gibt, sondern nur eine mehr oder weniger grosse, wenn auch vielleicht sehr grosse Wahrscheinlichkeit.

H. GLINZ

Before calling on the Rapporteur to summarize the discussion, I ask your indulgence for some comments of my own.

It seems to me shocking that in the sessions of this Congress that I have attended, and above all in the present session, the names of Edward Sapir and Leonard Bloomfield have not been mentioned. To discuss the question that occupies us here without reference to the work of these men in the comparative study of American Indian languages is to run the risk of abrogating the scientific character of linguistics; for it is surely an essential trait of any science that the results are cumulative, that each generation of workers takes up the task where the preceding generation left it. The scholar who enters upon such a discussion as ours without using the accumulated results of past research as his point of departure may be accused of reducing linguistics to a mere branch of speculative philosophy.

Of course it is true that special problems arise and special methods are needed in the comparative study of unwritten languages — or let us say, rather, in the comparative study of any given set or group of unwritten languages. But this is true also, and in the same way, for any set or group of languages whatever, written or unwritten, ancient or modern.

Comparative work in Bantu or in Malayo-Polynesian or in this or that family of American Indian languages will inevitably differ in many details of method from work in Indo-European; but in exactly the same way, and for exactly the same reason, comparative work in Germanic (say) will differ in many details of method from work in Slavonic or Celtic or Romance. If an unwritten language presents special problems to the comparativist and requires him now and then to devise special methods, it is not because the language is unwritten, but because it is a language.

What needs rather to be emphasized is the importance of carrying on the study of unwritten languages within the framework of assumptions that have been tested and found useful in other fields — the importance and wisdom of treating unwritten languages as nearly as possible in the same way as those written languages from whose intensive study the principles of the comparative method have gradually emerged. The point is illustrated in a well known anecdote about Bloomfield's work in Central Algonquian, a group of related languages spoken in the Great Lakes basin of North America. Having reconstructed for Proto-Central-Algonquian a series of consonant clusters with *k* as the second member, each one fully attested in dozens of sets of corresponding forms in the separate languages, Bloomfield found that there remained just one set — the morpheme for "red" — which could not be referred to any of his reconstructed clusters. In the separate languages, the words for "red" contain no cluster that is not found also in other words; but the correspondence from language to language is unique. Instead of resorting to analogy, that *deus-ex-machina* for troublesome cases, or talking vaguely about "primitive languages" and their convenient refusal to follow the rules of human speech, Bloomfield paid his unwritten languages the compliment of treating them in the same way that he would have treated those of the Indo-European or any other stock. If the neogrammarian hypothesis of regular phonetic change is valid anywhere, he argued, it must be no less valid in Central Algonquian than in Germanic; if the assumption of regular change has been fruitful in one field, there is no reason to expect less of it in another. Accordingly, he applied the comparative method as it had been developed by Indo-Europeanists, and reconstructed a Proto-

Central-Algonquian morpheme for "red" with a unique cluster. The reconstruction was published in 1925 (On the Sound System of Central Algonquian, *Language* 1.130.56, esp. 147-52). Three years later he was able to report that his prediction had been verified: in the dialect of the Swampy Cree, a Central Algonquian language not previously studied, the morpheme for "red" does in fact contain a cluster with *k* that is found in no other word of the language (A Note on Sound Change, *Language* 4.99-100). For a penetrating exposition of Bloomfield's view, see Edward Sapir, The Concept of Phonetic Law as Tested in Primitive Languages by Leonard Bloomfield, *Methods in Social Science* 297-306 (Chicago, 1931), and Charles F. Hockett, Implications of Bloomfield's Algonquian Studies, *Language* 24.117-31 (1948).

It has been said that languages long written provide, in their ancient monuments, a check upon the results of comparative work: that the comparativist, having reconstructed earlier linguistic stages, can test the validity of his reconstructions by comparing them with the languages actually preserved in written documents. So far as I know, this is to some extent true in Romance. There it is indeed possible to confront the Vulgar Latin of one's reconstructions with the Classical Latin of Roman literature — a different dialect to be sure, but near enough to serve as a check. For other fields the statement seems to me false. It would be true only if the linguist — the Indo-Europeanist, for example — based his reconstructions wholly on a comparison of present-day languages: English, French, Gaelic, Russian, and so on, and if he looked at the earlier stages of these languages, as they appear in documents, only after he had finished his task. I cannot recall having ever heard of an Indo-Europeanist foolish enough to work in this way. Instead, knowing that written records can give him a free ride for part of his journey into the remote past, the linguist uses each language in the oldest form that is accessible to him. Note especially that he uses the language in this oldest form not as a check upon his results, but as part of the raw material from which those results are eventually to be derived. Reliance on ancient sources where they are available is like reliance on the speech of old people (in preference to that of children) where they are not: each is a useful device, but neither is a way of checking one's results.

Much that has been said here to-day seems to me cogent and worth discussing. But I must turn aside from the main question before us to enter a protest against the repeated use, in the proceedings of this Congress, of the term "primitive languages." As I find it used in the *Preliminary Reports* and as I have heard it from the platform, it reflects the romantic notion of our ancestors, long since exploded, that all languages spoken by savage peoples are somehow basically alike, and that all of them are basically different from more respectable or more elegant languages like our own. From the irresponsible statements about "primitive languages" that I have heard and read here, I infer that those who made them have never themselves had any contact with a language to which they would apply this label. The scholar who speaks about the concrete vocabulary of American Indian languages, as compared with the greater abstractness of words in English and French, can only be repeating what others have written, since an hour's personal inspection of almost any native American language would convince him that such a comparison is rubbish. The young man who writes that "primitive" languages (apologizing for the term, though not sufficiently, by putting it in quotation marks) "change so rapidly that correspondences are obscured in any case" has been taught to prefer an easy popular generalization, however baseless, to the plain results of experience. To propagate this poisonous nonsense before an audience of men who regard the study of language as a sober and significant employment, is to insult one's colleagues. To accept these echoes of dead superstition without protest is to deserve the insult.

BERNARD BLOCH

SUMMARY BY MALCOLM GUTHRIE

Before attempting to summarize the various points that have been made, I should like to express my agreement with most of what our Chairman has said. This topic is not a new one, but it was surely important that it should be included in the programme of a Congress such as this, if only because many of the basic principles are often overlooked by those working in some of the more exotic fields.

Although some of the speakers have referred mainly to specific points in certain fields of comparative study, there has been clearly discernable a common desire to place on a firm foundation the different excursions into prehistory that are being made. Even when a given hypothesis appears very reasonable however it is essential to bear in mind that its conclusions must never be asserted as facts.

Above all it has become very clear that nobody would wish to claim for this kind of comparative study any peculiarity or special status. The principles involved are similar, whether in a well-established field with some historical records or in a group of languages where nothing whatever is known about earlier states. Indeed what we have been discussing is not some obscure subject but merely a branch of the major discipline of comparative linguistics, which derives its special quality from the special nature of the facts it has to handle.

SECTION B

COMPARATIVE LINGUISTICS

- (2) What contributions to comparative linguistics are to be sought from the two groups of disciplines and techniques broadly termed (a) phonetics, (b) semantics?

Chairman: PROFESSOR G. DEVOTO

Rapporteur: PROFESSOR J. FOURQUET

Recorders: MR. D. N. MACKENZIE
MISS R. WALLBANK

For Preliminary Reports see pp. 101-111 above.

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As early as 1884 Brandes showed that the result of applying the general results of Indo-European Comparative linguistics to Indonesian languages was not very satisfactory. Instead of exceptionless phonetic laws Brandes found a more or less chaotic situation. This has been explained in several ways, which are not relevant to the discussion of this afternoon. Structural linguistics, however, has provided us with an explanation which I should like to submit to your judgment.

Phonemes are virtualized within the limits of a phonetic capacity which is the same for all normal human beings, as babies learn the language of any milieu in case of early change. The number of phonemes, however, is different from language to language. So the intrinsic variability of an average phoneme too is different from language to language. Each word consists of phonemes in a given order, and the total number of words of a given structure cannot be higher than the product of the numbers of phonemes which are allowed in the positions concerned. So the potential vocabularies of two different languages must be different, if the sets of phonemes are different. Moreover, the virtualization of the lexical capacity is different from case to case. If we call "waste space" the lexical capacity of a language which is not used for the purpose of language, it follows that the structure of waste space is characteristic for each given language. Now waste space furthers the intrinsic variability of a word to become actual variability, if the frequency rate of this word is sufficiently low. In European languages with a generally high frequency rate and relatively little waste space a shift in the realisation of phonemes tends to result in an exceptionless phonetic law, but if the frequency rate is low and much waste space is available, then the high variability of the words may prevent the phonetic law to be exceptionless. This is why in Indonesian comparative linguistics phonetic change is much less regular than in Indo-European comparative linguistics. So I am inclined to think that comparative research work yields different results in accordance with the general conditions, as far as waste space and frequency rate are concerned, in different linguistic areas, and a new task of comparative

" comparative linguistics " is to find out the correlation between the structure of a language, in the first place its phonemic pattern, and the workability of the theories developed in the Indo-European area.

It has been said that the general result of dialect geography is a sceptical attitude towards the applicability of the theory of regular phonetic change. In my opinion dialect geography has shown that phonetic change is a process in which the factors speed and duration must be taken account of, whereas the neo-grammarians have over-looked these factors when studying cases of fully accomplished phonetic change. Speed and duration too are dependent on the general structural conditions of a given language and its speech community.

So for different reasons structural linguistics and especially the comparative study of phonemic patterns and their implications have widened the sphere of research in Comparative Linguistics and must lead us towards a differentiation of methods especially adapted to a given field of research.

C. C. BERG

Je me propose de soutenir cette thèse: les comparatistes ont toujours fait de la phonologie — sans le savoir. Ils en ont fait bien avant que le cercle de Prague eût donné au mot *phonologie* le sens que nous connaissons, celui de l'anglais *phonemics*, et que N. S. Troubetzkoy eût écrit les *Grundzüge*; la linguistique comparative est amenée par sa nature même à faire de la phonologie; elle ne peut pas ne pas en faire; mais jusqu'à présent, elle l'a mal faite, faute de prendre conscience de la nature des éléments sur lesquels elle opère; elle en a fait occasionnellement, quand certaines difficultés l'y contraignaient.

Dès lors, on ne peut s'empêcher de poser la question: le comparatiste est-il prêt à se servir des notions singulièrement plus rigoureuses qu'ont dégagées et que dégageront les spécialistes de la phonétique fonctionnelle et structurale? est-il prêt à s'en servir systématiquement?

Voici deux exemples, parmi des dizaines, l'un pris dans le domaine de l'étude comparative de l'indo-européen, l'autre à celui du germanique.

(1) Les gutturales des diverses langues indo-européennes proviennent-elles de deux gutturales (symboles $*k^w$ et $*k$), ou de trois (symboles $*k^w$, $*k^1$, $*k^2$)? Meillet a pris parti pour *deux*, en montrant que le point de départ de l'évolution indo-iranienne était que $*k$ avait pris une forme différente selon que la voyelle qui suivait était vélaire ou palatale. Ses adversaires ont invoqué quelques cas où $*k^1$ et $*k^2$ auraient existé devant la même voyelle. Sans le dire, on recourait au critérium qui sert à distinguer si l'on a affaire à des variantes d'un même phonème, ou à deux phonèmes, capables d'entrer dans une opposition significative.

L'alsacien de Sélestat, comparé à l'alsacien de Mulhouse, offre le même problème: tous deux ont les consonnes ξ et x , chuintante et constrictive vélaire; mais il y a trois séries de correspondances: Sél. ξ : Mul. ξ ; Sél. x : Mul. x ; Sél. ξ : Mul. x (ex. Sél. [riš], Mul. [rix] (riche)). L'ancienne grammaire comparée aurait posé trois consonnes au stade initial, par ex. $*\xi$, $*c$ (palatale), $*x$ (vélaire) et enseigné que Sélestat avait confondu $*\xi$ et $*c$, et que Mulhouse avait confondu $*c$ et $*x$.

Le phonologiste observe qu'à Mulhouse l'opposition significative entre $/\xi/$ et $/x/$ existe en toute position, devant toutes les voyelles; à Sélestat, elle existe devant *a*, *o*, *u*, mais elle est neutralisée devant *i*, *e*: en face de [riš] un [rix] est impossible.

Or une évolution phonétique peut neutraliser une opposition significative dans certaines positions; elle ne peut pas en créer une ex nihilo. C'est donc Sélestat qui a innové, et Mulhouse qui conserve l'état ancien. Il n'y a jamais eu trois phonèmes, qui se seraient opposés deux par deux; l'état intermédiaire (largement attesté) offre deux phonèmes: $/\xi/$ et $/\varsigma/$, $x/$ le second affecté d'une variante palatale et d'une variante vélaire. A Sélestat, la variante palatale $[\varsigma]$ s'est confondue avec $[\xi]$, d'où la neutralisation (Aufhebung) devant *i*, *e*.

Posé en termes de phonologie, le problème des gutturales indo-européennes est immédiatement résolu dans le sens de Meillet, alors qu'il a fait couler des flots d'encre, et que les trois gutturales hantent encore beaucoup de précis de grammaire comparée de l'indo-européen.

(2) Le prégermanique avait les voyelles *i* et *e*, les langues germaniques telles que le v. h. a., le v. saxon, le v. angl., le v. isl. les ont aussi, mais $*i$ prégermanique est devenu tantôt *i*, tantôt *ē*; et $*e$ prégermanique est devenu tantôt *i*, tantôt *ē*.

On a d'abord admis, sur le témoignage du gotique, que **i* et **e* indo-européens s'étaient confondus en *i*, puisque cet *i* avait pris la forme ouverte *ē* devant *a*, *e*, *o*. Mais certains dialectes, notamment le v. angl. donnent l'impression qu'au moins devant *u* **i* et **e* indo-européens ne s'étaient pas confondus en *i*.

Les longues discussions sur ces deux voyelles tournent autour d'un fait qui, pour le phonologiste, est familier: l'opposition entre les phonèmes **i* et **e* a été progressivement neutralisée par ces faits d'assimilation à distance que sont *Umlaut* et *Brechung*, la forme de neutralisation étant une forme palatalisée (*i*) devant palatale, ouverte (*ē*) devant voyelle ouverte. Il y a un reste, assez faible, de positions où l'opposition subsiste. Il faut continuer à poser deux phonèmes, sauf en gotique, où il y a eu coalescence, évolution toute différente.

Je conseillerais donc au comparatiste d'acquérir les habitudes — je dirais presque les "réflexes" — du phonologiste. Cette discipline se ramène à deux points essentiels.

(1) Se demander si le "son" (*Laut*) sur lequel on raisonne est une variante combinatoire, un phonème susceptible de s'opposer à tel autre, ou une forme de neutralisation, où cette opposition est abolie. Un *i* qui s'oppose à un *ē* n'est pas le même élément qu'un *i* seul possible, à l'exclusion de *ē* (*i* de neutralisation). Les correspondances qu'on attendra seront différentes; deux variantes combinatoires (par ex. *Ich-* et *Ach-Laut*) correspondront à un seul élément "primitif" (*k*), une forme de neutralisation à deux phonèmes.

(2) Caractériser un phonème non seulement par ses qualités phonétiques (occlusive, sourde, aspirée, forte etc.) mais par ce que j'appellerai son comportement.

Le phonéticien se contente d'enregistrer que *s* français et *s* allemand (*Bühnendeutsch*) sont "identiques" (constrictive sifflante sourde). Le phonologiste sait que *s* allemand n'existe pas à l'initiale (où la sonore [z] est seule possible), que l'opposition *s:z* n'existe en allemand qu'entre voyelles, et seulement après voyelle longue. La conséquence est qu'un Allemand du Nord, qui possède, au dire du phonéticien, *s* et *z* "français," est incapable de prononcer *il se rase* autrement que [ilzeras].

Ces différences de comportement traduisent une histoire antérieure des phonèmes /s/ et /z/ profondément différente en

allemand et en français, et convenablement interprétées, mettent le comparatiste en garde contre beaucoup d'hypothèses fausses qu'il serait tenté de faire.

On a admis un substrat (celtique) commun au français, au néerlandais et à l'alsacien, en se fondant sur le fait que dans ces trois langues *u* est passé à *û*; or l'analyse phonologique montre qu'en français, il s'agit de phonèmes entièrement différents. En français, il s'agit d'une évolution de l'opposition entre [u] et [ø] fermé, qui prend finalement la forme [y] : [u] (français *mur*, *tout*); en alsacien, il s'agit d'une évolution de l'opposition entre [u] et [y], qui prend la forme d'une opposition entre [y] et [i]; le nom de *Mulhouse* (Mülhusen) se prononce [mil(h)ys]. En français, [u] et [ø] sont seuls affectés. En alsacien ancien, il s'agit d'une palatalisation générale des vélaires (d'où les formes *hüs*, *stüb*, *bröt*, *hölz*, *köufe*, *güet*). Toute l'hypothèse de substrat devient sans objet.

La première et la seconde mutation offrent l'exemple d'un passage de *k* (devenue une aspirée *k^h*) à une constrictive gutturale (x): on en a conclu que c'était le renouvellement du même phonème — Le phonologiste observe que [x] haut allemand n'existe qu'après voyelle tandis que [x] germanique existaient en toute position. Les correspondances néerl. *koken*: h. all. *kochen* et lat. *caecus*: got. *haihs* illustrent immédiatement la différence. Il s'agit en v.h.all. d'une évolution conditionnée, produite par la voyelle, en germanique d'une évolution inconditionnée: la nature du phonème est nécessairement différente.

La difficulté est qu'il manque actuellement, comme intermédiaire entre la phonologie, comme uniquement sous la forme descriptive (synchronique) et la linguistique comparative, qui est par définition une science historique (évolutive) un intermédiaire: à savoir une phonologie évolutive, une science de l'évolution des relations dans lesquelles un phonème est engagé du fait de sa fonction distinctive. C'est sur ces relations précisément que se fonde la méthode comparative, qui part d'équations de vocabulaire.

Provisoirement, le comparatiste initié à la phonologie est obligé de se faire sa phonologie évolutive, en se transposant en termes de phonologie la description des évolutions phonétiques qui ont été bien étudiées par les phonéticiens et qu'il connaît bien.

C'est le prolongement de la méthode employée par Troubetzkoy dans les *Grundzüge*, et qui consistait à construire les systèmes phonologiques de langues bien décrites par les phonéticiens.

Les tentatives de ce genre que j'ai faites dans le domaine germanique m'ont pleinement convaincu de la fécondité du point de vue phonologique. L'habitude de faire des tableaux des faits de variantes combinatoires, de neutralisation, non seulement mène à rectifier des erreurs, mais à découvrir des faits nouveaux, dont on se dit que les phonéticiens auraient dû les voir depuis longtemps, et qui ont pourtant échappé à leur attention. Elle a une valeur heuristique.

De plus, pour décrire une situation, une évolution, deux mots (par ex. "neutralisation par palatalisation (Umlaut)" et "ouverture (Brechung)") suffisent là où les manuels alignent de complexes tableaux de correspondances et d'évolutions combinatoires.

Je suis resté jusqu'à présent dans le domaine de la description et de la caractérisation des faits, où ce qui a été découvert en termes de phonologie peut être traduit en termes de phonétique, et inversement.

Il y a, au delà, un vaste domaine, où la phonologie apporte quelque chose de nouveau, qui ne concerne plus la description méthodique des faits, mais l'étude des facteurs d'évolution.

Elle pose le problème de savoir si la structure des systèmes phonologiques n'est pas une cause d'évolution, une force dont la composition avec d'autres forces détermine la direction de l'évolution.

L'analyse phonologique des évolutions donne l'impression que des systèmes trop chargés, où les "marges de sécurité" des phonèmes sont trop petits (par ex. entre *e*, *ë*, *ā* en m.h.a.) tendent à s'alléger par une série de coalescences (il n'y a plus qu'un *e* bref en allemand); que des systèmes mal équilibrés tendent à déplacer des phonèmes pour rétablir l'équilibre (l'alsacien rétablit l'équilibre entre ligne palatale et ligne vélaire par le passage de *ā* à *a* et *ë* à *a*). C'est le domaine dans lequel se meuvent mes collègues Haudricourt et Juilland, et que je me propose aussi d'aborder.

On peut aller plus loin encore, et invoquer comme facteur d'évolution un changement dans la réaction psychologique du

sujet: M. Kurylowicz propose d'expliquer l'évolution d'une opposition de sonorité du type *p:b* en une évolution d'aspiration du type *ph:p* (point de départ des "mutations") en disant que la sonore, de plus en plus fréquente dans le vocabulaire i.eur. a cessé d'être sentie comme le terme marqué: ceci a entraîné le développement de l'aspiration, qui faisait de l'autre terme, *p*, un terme "marqué." Il faudrait un nombre suffisant d'exemples pour vérifier cette hypothèse, en elle-même légitime, et cela suppose en dernière analyse que le terrain ait déjà été déblayé par la réunion d'une masse d'exemples d'évolutions caractérisées phonologiquement, et parmi elles, d'évolutions où la pression du système phonologique apparaîtrait nettement comme une force motrice.

Nous avons donc devant nous la perspective de travaux qui ne consisteront pas à glaner quelques épis dans un champ déjà moissonné, mais qui mèneront à faire du neuf. Et, au moins dans le domaine du signifiant, il me semble que cela promet à la linguistique comparative, après la période de grande activité des *Grundrisse*, une seconde jeunesse.

J. FOURQUET

If one reviews studies in tonal languages in different parts of the world, and examines the problem of tonal and non-tonal languages in general, one may be led to the conclusion that certain developments in the direction of a greater precision are required in the scheme of the subdivision of languages into tonal and non-tonal languages in comparative linguistics. The view has been fairly generally held that languages should only be described as tonal if syllabic tones with semantic value are found in them, and that languages which do not come up to this requirement, should be regarded as non-tonal.

Dr. Kenneth Pike, in his treatise on tonal languages, has given a more precise description of the problem, and has carefully distinguished languages in which syllabic tones are correlated with stress and quantity from those in which they are not.

I should like to proceed from Dr. Pike's views as my foundation, adding my own — possibly ambitious — superstructure. I have, in particular, been induced to follow this line of thought

by the existence of languages with indisputable and independent syllabic tones which however appear to have very little semantic value or none at all. Such languages do not well fit in with the present classifications. As examples I may quote one or other of the Bantu languages, e.g. Mawiha, and above all some of the languages of New Guinea.

Dr. A. Capell, in his article "Two tonal languages of New Guinea" points to the existence of languages of this type in New Guinea and in Northern Australia, and describes their tones as "ornamental." He also states that with the Bukawa' language which is one of the two languages described in his article and which has indisputable syllabic tones, he was unable to find examples of tones with semantic value in his material. This does not yet prove that such cases do not occur at all in this language, but makes it rather likely that the occurrence of tones with semantic value is rare in Bukawa'.

I may, therefore, suggest the following grouping:

(1) Real tone languages. In such languages the tones (a) are inherent parts of each syllable and not parts of the phrase melodies; (b) have no independent meanings of their own; (c) are contrastive; (d) are unchangeable or change according to definite principles; (e) are not correlated with stress or quantity. I do not however want to make it an essential requirement that the tones have semantic value, and am inclined to subdivide the real tone languages into (a) real tone languages with semantic tones, (b) real tone languages with — predominantly or exclusively — ornamental tones. Examples of (a) are for instance West African languages such as Efik, Ewe, Yoruba etc., Sino-Tibetan languages etc. For (b) Bantu languages such as e.g. Mawiha, and languages of New Guinea and North Australia may be quoted. It remains a task for further research to elucidate how far, in the latter languages, certain types of sentence rhythm may partly be responsible for some of the tones found in them, and it may perhaps be found that one or the other of them would more correctly be classified with the group which I am going to mention in the fourth place.

It may be noted to the last of the five requirements mentioned above that in a number of real tone languages there is evidence that their tones have originally been due to certain characteristics of the syllables such as voiced or voiceless initials etc.

Such languages, of which Chinese, Panjabi and Yabem in New Guinea may be quoted as examples, may nevertheless be regarded as real tone languages, since these phenomena are purely historical and often even difficult to trace down in the present-day forms of the languages, whereas in the cases of correlation of tone with stress or quantity we observe the co-existent and contemporary, not historical appearance of two phenomena.

(2) Quasi tonal languages. With such languages, the first four of the five requirements mentioned above are valid as is the case with real tone languages, but the syllable tones are correlated with stress or quantity. This is for instance the case with Serbo-Croat, Swedish, Norwegian, Lithuanian etc.

(3) Non-tonal languages. In such languages no syllable tones are met with, only intonation and meaningful phrase melodies are found.

(4) A category between the quasi tonal and non-tonal languages. In such languages, there is a definite basic intonation pattern which is modified in numerous but regular ways by the sequence of stresses. The syllable tones resulting from this are not correlated syllable tones as is the case with the syllable tones in quasi tonal languages, but are modifications of the intonation, and only the mutual relation of the successive tones in certain tonal patterns is well-defined, whereas the relation of the tonal patterns themselves to the tones of the adjoining syllables is variable. Many, if not all, Turkic languages belong to this category, but the existence of further languages of this type is likely, perhaps in New Guinea, and perhaps also elsewhere.

STEFAN WURM

Liaison between semantics and comparative linguistics has been considerably strengthened in recent years. There is, however, one major difficulty: some of the specialized and highly complex techniques of semantics are still in an experimental stage and are being worked out in branches of linguistics far removed from the usual field of the comparativist. Yet these techniques are potentially more valuable for linguistic reconstruction than many of the time-honoured categories of semantic change: extension, restriction, deterioration and

the like. I shall try to illustrate this point by three brief examples; the first of these refers to the history of single words, the second to the interaction of words, the third to the structure of the vocabulary.

(1) Although the search for semantic "laws" has proved a mirage, certain general *tendencies* of semantic development have been established with a fair measure of precision. A set of such regularities has been ascertained, by statistical methods, in the sphere of transfers from one sensory domain to another, commonly known as *synaesthetic* transfers. It has been found, for example, that such metaphors originate mostly in the lower reaches of the sensorium and move towards the higher reaches: transfers from touch to sound or sight are therefore more common than those in the opposite direction. Should these tendencies be confirmed by subsequent investigations, they might provide useful pointers to the comparativist. Faced with a word applicable, within one language or in several cognate languages, to more than one sensory domain, he might be able to reconstruct the starting-point and filiation of these meanings in the light of the general tendencies governing synaesthetic transfers. It need hardly be stressed that no certainty can be achieved by this method: all that we may expect is a greater or lesser degree of probability which, under favourable circumstances, may amount to quasi-certainty.

(2) Much work has been done in the last quarter of a century on the various types of *semantic analogy*: changes of meaning due to the influence of one or more associated words. Best known among these are the processes of *calque* or loan-translation. Analogical contacts between words of the same language are less obvious but perhaps even more significant. They may arise between synonyms: if a given word acquires a new meaning, one or several of its synonyms may follow suit. Similar influences are also at work between antonyms and between words belonging to the same sphere of thought, the same "semantic field." How this principle can be applied to comparative data has been shown by the founder of the "field theory," Professor Trier himself, in a series of studies on the terminology of the "human ring," an ancient form of assembly, in Germanic and Indo-European. Many important words such as *tale*, *number*, *rhyme*, *father* etc. have been traced back, more or less convincingly, to this rudimentary social

institution. It is easy to see the connections of this method with the well known "law of expansion" established by Sperber: spheres of thought which bulk large in the interests of the community will provide similes and metaphors for the description of less prominent spheres.

(3) The third example is rather in the nature of a caveat. It is common knowledge that the speech of uncivilized races is rich in concrete, particular terms but poor in generic ones. To give but one illustration, in Cherokee there is no general word "washing," only a series of specific expressions for washing one's head, face or hands, washing clothes, dishes, meat etc. (Jespersen). The same peculiarity has been observed at so many different points, stretching from Polynesia to the North Pole, that it can safely be regarded as a fundamental law of human development. Now it is fairly clear that many of the languages reconstructed by comparativists were spoken by precisely such communities. Paradoxically enough, however, the starred meanings which we assign to their hypothetical words, or roots, are only too often vague and general. This is of course methodologically inevitable: in Dr. W. S. Allen's words, such meanings are merely "a kind of highest common factor or lowest common multiple" (*TPS*, 1950, p. 1) to which no linguist would attach any reality. They are mere working hypotheses: they synthesize the information available regarding the meaning of reconstructed forms. Nevertheless the comparativist should always bear in mind the paradoxical situation which compels him to describe in hyper-abstract terms the hyper-concrete vocabulary of a "primitive" community.

STEPHEN ULLMANN

LES CALQUES ET LA METHODE COMPARATIVE

Dans le raisonnement étymologique classique, base de la méthode comparative, la phonétique continue à avoir le pas sur la sémantique, malgré les exhortations d'un Schuchardt par exemple. Cette prépondérance de l'élément phonétique ne provient pas de ce que nous connaissons le plus souvent les causes ou même seulement les modalités exactes des changements phonétiques. Loin de là. Si nous affirmons que — sauf

influence troublante d'une cause particulière — tout *a* tonique libre du latin est devenu *é* ou *è* en français moderne, ce n'est pas parce que nous aurions découvert le pourquoi de ce développement: les romanistes, eux surtout, en discutent encore. Ni comment cette évolution a eu lieu: est-ce par voie de diphtongaison ou par simple fermeture? Les romanistes en discutent également encore. Non, l'affirmation se base sur la constance avec laquelle, dans un nombre suffisamment élevé de cas constatés, *é* ou *è* français correspondent à *a* libre tonique du latin; c'est à dire que l'affirmation se base sur une série de corrélations, appelée autrefois "loi phonétique" et que, aujourd'hui, certains préfèrent nommer "tendance phonétique."

Or, pour ramener un mot à son "étymon", ce sont ces lois phonétiques qui constituent le facteur déterminant; l'élément sémantique n'intervient en général que comme facteur supplémentaire. Malgré quelques résultats de systématisation non négligeables obtenus, nous n'avons pas encore réussi à ramener la masse des changements sémantiques à des lois qui puissent être comparées aux lois phonétiques.

La subordination de la sémantique par rapport à la phonétique et la résistance qu'offre la masse des phénomènes sémantiques aux efforts pour les réduire à des lois, tiennent sans doute à des causes diverses. Je me suis demandé — et ce sera la portée de cette intervention — si une des causes ne serait pas une certaine mésestime que nous aurions eue pour le phénomène du calque. Je me suis demandé si une étude plus poussée et surtout plus systématique des calques ne permettrait pas d'enregistrer des progrès intéressants.

Certes, il y a longtemps que les linguistes connaissent le phénomène de l'emprunt de la seule signification, appelé *calque* en français, *Abklatsch*, depuis Bally, en allemand, *translation loan word* en anglais. Et notamment depuis le "Troisième Congrès International des Linguistes" de nombreuses études ont été consacrées à ce sujet, sujet d'actualité puisque, aujourd'hui, la radio et les agences de presses créent journellement des calques: nous les voyons, pour ainsi dire, naître sous nos yeux. Mais il semble quand même que l'on considère le calque encore souvent comme une sorte de phénomène linguistique aberrant, exceptionnel. L'étude des lois phonétiques n'a fait des progrès importants

qu'après que les linguistes s'étaient mis à trier systématiquement les emprunts et à les séparer des mots indigènes, dits "populaires." Est-ce que, peut-être les calques ne sont pas appelés à avoir, dans la sémantique, la place occupée, dans la phonétique, par les emprunts?

Rappelons brièvement en quoi consiste le rôle perturbateur des calques pour les recherches sémantiques. Le sémanticien — et presque chaque linguiste est sémanticien à son heure — ayant constaté un phénomène d'ordre sémantique pour un mot d'une langue déterminée et voulant en rendre compte, sera facilement enclin à chercher la raison suffisante de ce phénomène dans cette langue et dans la communauté qui la parle. Or, si l'on a affaire à un emprunt de signification, la raison suffisante de ce phénomène sémantique, c'est à dire l'ensemble des conditions qui lui ont donné naissance, ne se trouvera pas entièrement dans la langue de calque. Là — l'impulsion étant venue d'ailleurs — ne se trouvera qu'une cause partielle. Une partie des conditions — souvent même les facteurs les plus importants — résideront dans la langue qui avait servi de modèle et dans la communauté qui parle cette langue. Par conséquent, si le sémanticien, sans se rendre compte du fait de l'emprunt sémantique, fournit quand même une "explication" du phénomène, ou bien ce sera quelque chose de très général et de vague, ou bien quelque chose de précis, mais alors de faux.

Qu'il soit permis de citer un exemple qui m'a paru particulièrement instructif: l'origine de quelques emplois du mot latin *disciplina* et de ses dérivés romans. *Disciplina* possède, comme *discipline*, outre les valeurs les plus connues: *doctrine*, *science* d'autres valeurs: *punition*, *flagellation*, et même *massacre*, *carnage*. Comment faut-il expliquer celles-ci? Parmi les nombreuses réponses données à cette question recueillons-en trois, toutes puisées chez d'excellents travailleurs.

M. Gamillscheg, dans sa *Französische Bedeutungslehre*, parue l'an passé, explique le rapport entre *discipline* et *châtiment* par le fait que: "la masse du peuple considère l'enseignement comme une punition."

Le regretté wallonisant Jean Haust dans ses *Etymologies liégeoises*, rend compte de la valeur de *carnage*, *massacre* qu'il trouvait pour *discipline*, par le fait que: l'idée de

flagellation, châtement corporel, engendre aisément celle de coups portés à l'ennemi."

M. Rheinfelder enfin, dans sa *Kultsprache und Profansprache in den romanischen Ländern*, explique la valeur de *flagellation* à partir de la *disciplina militaris* de l'armée romaine; celle de *massacre* par le fait que "un grand malheur s'appelle souvent au sens figuré un châtement."

On conviendra que des explications de ce genre sont monnaie courante dans la plupart de nos dictionnaires et manuels étymologiques. Comparons maintenant la solution qu'on peut retirer de l'étude de M. Marrou sur *Disciplina* et *Doctrina* et surtout de celle du théologien George Bertram sur *Le concept d'éducation dans la Bible grecque* et que nous résumerons très brièvement.

Disciplina qui était déjà un équivalent usuel pour rendre le grec *paideia*, l'est également dans les cas où, dans la version des Septante, *paideia* servait à traduire le substantif hébreu biblique *musar*. Or, ce substantif *musar* signifie en premier lieu: "la punition dans un but éducatif."

Ainsi *musar* sera en particulier la punition infligée par Iavhé à son peuple, surtout pour le corriger du péché d'idolâtrie. Au livre des Nombres nous lisons que cette "leçon punitive" a coûté la vie à 24,000 fils d'Israël. Voici donc déjà l'idée de *carnage*, *massacre*.

Puis *musar* sera: la punition infligée par un père à son fils au moyen de verges, comme au premier livre des Rois. Voilà donc l'idée de *flagellation*.

Finalement on trouve, plus rarement, *musar* avec la valeur de: *éducation*, *enseignement*.

A la lumière de ces données, les solutions antérieures proposées pour les valeurs "spéciales" de *disciplina* s'avèrent insuffisantes, sinon fausses. Pourquoi? Pour deux raisons: nous trouvons ce cumul de sens dès l'hébreu où il provient de circonstances historiques concrètes, en l'espèce: des conceptions particulières de la théocratie juive d'avant notre ère. Puis le fait que *paideia* ne connaît ces valeurs qu'à partir des versions grecques de la Bible comme *disciplina* seulement à partir des versions latines: c'est à dire qu'il y a eu calque et non création autochtone, spontanée, ni en grecque, ni en latin. Et surtout pas en français qui a pris le mot avec toutes ses significations au latin.

La question qui se pose maintenant est de savoir si le nombre des calques non reconnus est grand. Or, ici il faudra d'abord tenir compte du fait que le calque est souvent masqué. On a déjà fait remarquer que, à la différence de l'emprunt, du moins en général, rien dans la forme du calque ne dirige l'attention vers la langue d'origine. Au surplus, les lexicologues s'attachent en général au vocabulaire d'une langue (le français, l'italien) ou d'un groupe de langues de même famille, (les langues germaniques, les langues slaves, etc.). Nécessairement ils seront moins familiarisés avec des langues d'une autre famille qui, toutefois, pourront fort bien avoir été imitées dans tel ou tel cas. Et, facilement, ces calques là ont pu échapper à leur attention. Ensuite il y a la difficulté — à laquelle je voudrais m'arrêter un instant — de distinguer entre une série de calques et une série de créations sémantiques, autochtones, spontanées, que nous appellerons ici polygenèse sémantique.

Afin de mieux situer ce problème et mieux marquer son importance, je citerai un exemple qui a fait couler un flot d'encre. Il s'agit de nouveau d'un mot fort connu: l'origine du pronom français *on*, *l'on*, correspondant à l'allemand *man*, néerlandais *men*. Tandis que l'origine formelle de ce mot ne présente pas plus de difficultés que tout à l'heure *disciplina* (c'est le latin *homo*), nous n'avons pas encore réussi à éclaircir complètement l'origine sémantique.

On se rappellera qu'Antoine Meillet, dans son article: "Comment les mots changent de sens?" avait commencé par expliquer l'origine du français *on* et de l'allemand *man* comme provenant de la structure de certaines phrases négatives, interrogatives ou conditionnelles, telles que *Nul homme n'est venu*. Il songeait donc à une polygenèse.

La polygenèse est également adoptée dans une étude du romaniste néerlandais Weerenbeck: "*Le pronom on en français et en provençal*," laquelle, publiée en 1943, n'a peut-être pas retenu toute l'attention qu'elle méritait, ne fût-ce par sa documentation abondante. M. Weerenbeck est d'avis d'ailleurs que cette polygenèse de *on*, *man* etcetera s'explique par ce que ces mots, dit-il, "font partie d'un ensemble de faits linguistiques qui appartiennent à un fond psychologique et linguistique, commun peut-être à toute l'humanité."

Antoine Meillet, plus tard, a changé d'opinion et s'est rallié

à ceux qui attribuent l'origine du français *on* à l'influence du germanique *man*, ce qui est combattu à juste titre dans un article très fouillé du *F.E.W.* de M. von Wartburg. Mais, dans ce cas, comment expliquer le germanique *man* et le néerlandais *men*? Par l'influence du français avec Brøndal ou par l'hypothèse gréco-latine de Schrijnen? Le problème se complique d'ailleurs du fait que M. Weerenbeck, pourtant partisan, en général, de la polygenèse, a cru pouvoir constater que du moins pour l'ancien italien *uomo* il y a lieu de songer à une influence du français, donc au calque.

Nous constatons donc que des auteurs différents (et nous sommes loin d'avoir cité tous les nombreux savants qui s'en sont occupés) — et même quelquefois le même auteur à des moments successifs — expliquent le même phénomène par des causes opposées, les uns songeant à une polygenèse, les autres à des calques. Cela montre bien qu'il s'agit d'un problème qui dépasse le cas particulier. Rien que pour cette raison il vaut la peine d'essayer de trouver un point de vue plus général qui nous permette d'y voir plus clair.

En outre, le problème est important à un autre égard. Il est, certes, intéressant de rechercher le rapport éventuel entre sémantique d'une part et le *niveau culturel* de l'autre et, par exemple, de se demander si la création du pronom *on* indiquerait un degré plus élevé dans le pouvoir d'abstraction. Mais alors il n'est point indifférent de savoir si l'apparition, dans une langue, de ce pronom est due à une création autonome ou à l'imitation d'une autre langue.

Comment peut-on reconnaître la polygenèse sémantique? Par définition elle est le fait que des phénomènes sémantiques semblables se produisent dans plusieurs langues indépendamment. Elle est, puis, la conséquence de ce que les hommes parlant des langages différents mais se trouvant dans des circonstances analogues peuvent réagir linguistiquement de la même manière sur ces circonstances analogues et le font en effet souvent.

Ainsi, quelle que soit leur langue, les hommes vivent dans le temps et dans l'espace. On ne pourra donc pas légitimement s'étonner de voir que, dans des langues différentes, indépendamment les unes des autres, des expressions d'espace servent pour indiquer le temps. Si donc, à côté du français *sur ce* (= après cela) on trouve en néerlandais un équivalent formel *daaroφ*

ayant la même valeur temporelle que *sur ce*, il peut très bien s'agir d'une polygenèse de signification.

Ainsi encore, les hommes ont appris à constater que l'effet suit la cause (on a même parfois créé un dicton pour avertir que tout ce qui suit n'est pas effet: *post hoc non propter hoc*); il ne peut donc paraître légitimement étonnant si — dans des langues différentes et indépendamment — telle expression indiquant la succession va indiquer l'effet, la conséquence. Songeons d'abord au mot *conséquence* lui-même, puis au français *par suite*, néerlandais *bijgevolg* comme à l'allemand *folglich* et à l'anglais *followingly*.

Mais est-ce que l'inverse est vrai également? Si, en soi, rien ne s'oppose à ce que les formes citées aient été créées les unes indépendamment des autres, est-ce que l'on peut conclure qu'elles l'aient été? M. Weerenbeck, à propos de l'origine de *on*, attache grande importance au "parallélisme tellement parfait" dans tant de langues, pour prouver la polygenèse. Mais contre cet argument il est possible de citer tout de suite un cas pour lequel on trouve dans au moins autant de langues un parallélisme non moins parfait: je veux dire l'hébreu *chinnom*, cumulant les sens de *gratuito*, *frustra*, *immerito*, et qui a été calqué dans le grec *dorean*, le latin *gratis*, le slave *darom*, *darmo*, l'ancien français *en pardons*, l'allemand *vergebens*, le néerlandais *vergeefs* et dans d'autres langues.

En effet, le propre du calque est d'imiter de fort près l'original qu'il calque. Il suffit par conséquent qu'un calque se répète un certain nombre de fois, pour que nous obtenions ce "parallélisme parfait et répété" qu'on voudrait donner comme la caractéristique de la polygenèse. C'est à dire que ce critère est insuffisant.

On pourrait essayer de trouver un autre critère pour distinguer polygenèse sémantique et calque. Dans la polygenèse, par définition, nous avons affaire à un phénomène sémantique qui peut se répéter sur plusieurs points ou à plusieurs moments de notre globe; il s'agit, par conséquent, de quelque chose de relativement général.

Ainsi, on appelle en France une personne très avare un *Harpagon* et, en Italie, sans doute indépendamment, *Néron* une personne fort cruelle. Mais quand vous trouverez dans n'importe quelle langue l'équivalent linguistique de *éminence grise* pour exprimer la même notion complexe que ces deux

mots accolés ont en français, je suis sûr que vous songerez tous au calque et non à la polygenèse. Pourquoi? C'est que, en français le groupe des mots *éminence* et *grise* avec la notion qui s'y rattache est le résultat de circonstances historiques particulières telles que les chances pour qu'elles se répètent sont infimes, pour ne pas dire nulles.

De ceci on peut, semble-t-il, tirer la conclusion suivante: Tandis que la polygenèse ne peut avoir lieu qu'à propos de quelque chose d'assez général, les calques, en revanche, *peuvent* imiter des nuances très particulières. A cet égard, en effet, rien n'empêche certes la série des pronoms *on*, *man* etcetera d'être des créations spontanées. Mais rien n'empêche non plus qu'ils aient pu être des calques. Ce critère s'avère donc également insuffisant.

Il vaudra mieux commencer par l'autre bout et se demander: Peut-on reconnaître le calque? Le calque est un emprunt de signification et, en principe, il peut être localisé dans le temps et dans l'espace. En principe, on peut déterminer l'époque, la voie et la direction de cet emprunt de signification.

Qu'est-ce qui nous oblige à voir dans le cumul des trois sens: *gratuito*, *frustra*, *immerito* qu'on trouve dans une série de mots un calque de l'hébreu *chinnom* et non une polygenèse? Est-il exclu, en soi, que ce cumul se soit produit, spontanément en plusieurs langues? Non, cela n'est pas exclu. Surtout pas pour les mots, la plupart, ou ce cumul est lié à une forme pour *don* ou pour *donner*. Ce que je reçois *en don*, je le reçois *pour rien* et on me le donne *pour rien*. Or, à partir de *pour rien*, il est facile de voir ce qui unit ces trois adverbes, quand on songe aux trois phrases suivantes: (1) Il me l'a donné *pour rien* (*gratis*); (2) j'ai travaillé *pour rien* (*frustra*); (3) il m'a battu *pour rien* (*immerito*).

Malgré cela, nous sommes contraints de voir dans ce cumul un calque, du fait que dans toutes les langues qui connaissent ce cumul, excepté l'hébreu, il ne se présente jamais avant la version biblique de cette langue; du fait qu'il se présente régulièrement dans ces versions bibliques aux endroits notamment où l'hébreu a *chinnom*; et qu'il s'y présente depuis. Le calque est attesté grâce à ces correlations constantes. Pour le roumain, langue romane, nous avons d'ailleurs une confirmation intéressante. La forme *în dar*, *în zadar* ne provient pas du latin, mais du slave, ce qui correspond avec

le fait que les premières Bibles roumaines ont été traduites précisément sur la version slave.

Puisque l'existence d'un calque, dans des cas pas trop défavorables, peut être démontrée, on trouvera les polygénèses par élimination des calques. Il y aura donc avantage à poser, en principe, pour chaque phénomène sémantique la question préalable du calque, exactement comme nous posons, en principe, pour chaque phénomène phonétique, la question préalable de l'emprunt. Cela n'a pas été fait systématiquement jusqu'ici. Il est donc probable que pas mal de polygénèses présumées se révéleront être des calques.

De toute façon il paraît bien que le total des calques entrés dans des langues telles que celles de l'Europe doit être fort élevé. Les recherches faites jusqu'ici en ont d'ailleurs révélé un certain nombre déjà, notamment ceux provenant de l'antiquité gréco-latine. Etant donnée la grande influence exercée par la philologie classique sur la linguistique, cela ne peut guère nous étonner. Mais, pour parler d'un domaine que nous avons plus particulièrement étudié et auquel nous nous sommes permis de puiser la plupart de nos exemples, on est loin d'avoir repéré tous les emprunts de signification faits par les langues romanes et par les autres langues d'Europe aux différentes versions de la Bible. Un merveilleux instrument de travail, par ailleurs, le *Thesaurus linguae latinae* nous en cache beaucoup par suite du fait qu'il range sous une même rubrique les emplois cueillis chez les auteurs profanes et ceux remontant à la Bible. Mais il y a d'autres veines à ouvrir. Signalons ici seulement l'étude récente du savant finnois M. Öhman qui a mis en lumière combien d'emprunts de signification faits à l'ancien français se trouvent en ancien néerlandais et en ancien haut allemand.

Ce qui nous manque c'est des répertoires de calques. On en a pour les emprunts; on n'en a guère pour les emprunts de signification. Dans la Section de ce Congrès consacrée à la Linguistique Générale, on discute de la forme que devront prendre des dictionnaires dits conceptuels. Il serait utile d'y faire entrer aussi les calques.

Pour finir, nous voudrions maintenant indiquer brièvement comment l'étude des calques pourra compléter, sinon corriger, la vieille méthode comparative, même du côté phonétique.

Les calques permettront d'établir, à côté des stemmas étymologiques traditionnels, à base phonétique, du type:

latin: *patrem* > italien: *padre*, français: *père*, provençal *paire* etcetera, des formules étymologiques à base sémantique, du type mentionné:

(gratuito)
 nexus de sens(frustra) réalisés dans hébreu *chinnom*, grec
 (immarito)
dorean, latin *gratis*, slave: *darom*, *darmo*, roumain *în dar*,
în zadar, ancien français *en pardons*.

Si, dans le stemma étymologique classique, les constantes sont d'ordre phonétique, ici en revanche, les constantes sont d'ordre sémantique et consistent dans la répétition régulière, dans une série de langues, — même là où les formes ne sont pas généalogiquement apparentées — de certains groupes de significations.

Le raisonnement étymologique étant au fond une forme du calcul des probabilités, le degré de vraisemblance d'une étymologie croît en même temps que le nombre de facteurs de comparaison qui entrent en jeu. Or, le stemma sémantique — exactement comme le stemma phonétique — augmente le nombre de facteurs de comparaison, et cette fois du côté sémantique. Grâce à quoi, il pourra nous mettre à même de résoudre des problèmes étymologiques dans des cas où le nombre de facteurs phonétiques est insuffisant.

Ainsi, l'adverbe italien archaïque *indarno*, signifiant en vain, a été fort longtemps censé être un cas désespéré, pour lequel de nombreuses étymologies avaient été proposées.

Tant qu'on ne considérait ce vocable italien qu'au point de vue phonétique on ne pouvait le relier qu'à un ancien français, assez peu attesté: *en dar(t)* tout aussi énigmatique. L'explication d'après laquelle *indarno* serait un dérivé de *dare* (donner), contaminé par son synonyme *in vano*, n'était qu'une solution entre tant d'autres.

Mais dès qu'on se rend pleinement compte de l'existence du stemma sémantique précité *gratuito-frustra-immarito* tout change. En effet, pour pouvoir admettre n'importe laquelle des autres solutions proposées pour *indarno*, il faut en même temps et nécessairement admettre que — alors que dans une dizaine de langues il existe — et cela par suite d'une série de calques — un lien entre un terme pour *frustra-en vain* et un mot pour *don-donner*, précisément en italien, où ce lien existe aussi, il serait dû au hasard. Cela est si invraisemblable

que, par là, les autres étymologies, qui d'ailleurs avaient été établies sans tenir compte des cas analogues dans les autres langues, sont écartées.

En appliquant le stemma sémantique il ne faut évidemment pas perdre de vue que l'expression née de calque fait dorénavant partie de cette langue; qu'elle va entrer dans ce que M. von Wartburg, bien que dans un ordre d'idées quelque peu différent, a appelé d'une formule heureuse: "die innere Verknöpfung innerhalb einer Sprache." Cette expression pourra ensuite subir des évolutions phonétiques et sémantiques: c'est à dire qu'il se greffera des phénomènes particuliers sur le phénomène général qu'il faudra tenir distingué.

Distinguer entre phénomène général et phénomène, particulier, dans le cas des calques bibliques, voudra dire en premier lieu: faire une distinction entre les différentes versions de la Bible. Car, quoi qu'il en soit de leur contenu, au point de vue linguistique ces versions sont loin d'être équivalentes.

Quand on compare à quoi correspond dans la *Vulgate* ou dans la *Vetus latina* le substantif grec *paideia* et le verbe *paideuein* dont nous avons parlé tout à l'heure, on ne rencontre plus un substantif et un verbe, mais une profusion de formes. A quoi cela tient-il? Sans doute au fait que, si le grec possédait le verbe *paideuein* à côté de *paideia*, le latin classique, en revanche, n'avait pas de verbe **disciplinare* à côté de *disciplina*. Les traducteurs, tout en rendant, le plus souvent, *paideia* par *disciplina* ont, en général, reculé devant la création d'un néologisme **disciplinare* — qu'on trouvera, bien que rarement, et ont traduit *paideuein* par *castigare*, là où le verbe grec signifiait *punir* et par *erudire* là où il signifiait *enseigner*. Cet état de choses est à la base du développement ultérieur de ce groupe dans les langues romanes.

Le stemma sémantique rendra même possible de trouver un point de départ méthodologique dans des cas autrement insolubles précisément en permettant de distinguer, dans un calque, entre le phénomène général et le phénomène particulier à une langue.

Empruntons ce dernier exemple à l'ancien français:

Nous trouvons en ancien français un verbe *abomer*, ou *abosmer*, quelquefois: *abo(s)mir*, signifiant surtout *détester*, mais dont le participe passé, de loin le plus fréquent, signifie *triste*, *abattu*. Il y a ensuite un substantif *abome*, *abosme*,

signifiant: *horreur*, notamment *horreur* pour *l'idolâtrie*, et aussi: *dégoût*, *nausée*.

Les *s* de *abosmer* rendant difficile l'étymologie de *Diez abominari* on en a imaginé un grand nombre d'autres, dont aucune satisfaisante. Le grand phonéticien Antoine Thomas a même construit comme une mosaïque: *ad* + *bosma* (= poitrine) + *are* = *abosmer* = attrister.

En désespoir de cause — les étymologies insolubles semblent s'attirer dès qu'il y a un élément formel en commun — on a rapproché d'autres mots. Notamment — en partant de la valeur *nausée* — de certains formes *bomi*, en dialectes de langue d'oïl; signifiant *vomir*. Et aussi d'autres mots dialectaux modernes, même du français suisse *s'embaumer* = *se casser la figure*!

Or, l'étude des calques bibliques permet de construire le stemma suivant: le nexus sémantique, assez hétérogène à première vue, (1) *idolâtrie*, (2) *horreur pour l'idolâtrie*, et *horreur* en général; (3) *dégoût*, *envie de vomir* se retrouve en grec *Bdeluttomai* latin *abominari* allemand *grauen*

bdelugma abominatio Greuel

néerlandais *gruwen*
 gruwel

Puisque *abominari* peut donner *abomer* en français comme **alluminari* a donné *allumer* le stemma nous donne un point de départ ferme pour isoler d'abord les formes où *abomer* et *abome* ont les valeurs du phénomène général.

Au point de vue phonétique ce *s* n'est pas assez fort pour infirmer ces correspondances dans au moins quatre langues: il faut considérer sa présence comme un phénomène particulier, c'est à dire français, et l'expliquer comme tel, ce qui n'est d'ailleurs pas trop difficile.

Ayant ainsi allégé ce fouillis de formes et surtout ayant disposé du sens *nausée*, nous voyons qu'il n'y a plus aucun lien entre les formes *bomi* = *vomir* et *abo(s)mer*. Il s'agit ici d'un phénomène absolument à part: le changement de *v* initial à *b* dans un certain nombre de dialectes français, dont il faudra évidemment essayer de rendre compte.

Finalement il reste un certain nombre de formes dialectales modernes qu'on peut maintenant renvoyer aux dialectisants pour plus ample étude.

Dans ce cas comme dans celui d'*indarno* et on pourrait en

ajouter d'autres, le stemma a contrebalancé et corrigé dans une certaine mesure la prépondérance de l'élément phonétique. En résumé, si ce que nous venons de dire est exact, l'étude plus systématique des calques:

(1) permettra de reconnaître comme emprunts de significations des pseudo-polygénèses sémantiques;

(2) rendra possible de poser les problèmes sémantiques dans la langue où elle se posaient vraiment et, par là même, de mieux les résoudre;

(3) permettra, finalement, de contrebalancer dans une certaine mesure, grâce au stemma sémantique, la prépondérance de l'élément phonétique sur l'élément sémantique.

J. ENGELS

SECTION B

COMPARATIVE LINGUISTICS

- (3) In what sense and to what extent is comparative grammar of value in descriptive linguistics? Can a more comprehensive grammatical method be developed in traditional comparative linguistics?

Chairman: PROFESSOR W. K. MATTHEWS

Rapporteur: PROFESSOR L. HJELMSLEV
(Report read by Professor H. J. ULDALL)

Recorders: MR. D. N. MACKENZIE
MR. W. E. SKILLEND

For Preliminary Reports see pp. 113-118 above.

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Chairman: Professor H. K. Matthews

Reporter: Professor L. Hjalmarsson

(Report read by Professor H. J. Ullrich)

Secretary: Mr. D. V. Mackenzie

Mr. W. E. Sullivan

For Preliminary Reports see pp. 113-116 above

The questions, as they stand, are highly ambiguous and cannot be properly answered without a preliminary clear distinction between the very different meanings which may be attributed to the terms involved. The contributions received (contributions by T. Bolelli, E. Buyssens, J. Ellis, M. A. K. Halliday, and Eric P. Hamp) clearly reflect these ambiguities. It may safely be stated that there would have been no ambiguities, or at least that the ambiguities would have been considerably less in evidence, if "traditional comparative linguistics," which is expressly included in the questions, were not so obviously guilty of a misleading and confusing use of the term "comparative grammar," which has nothing to do with present-day linguistics and may actually be considered as being out of date, particularly at an International Congress of Linguistics.

(A) If by comparative grammar is understood a morphological and syntactic (or, briefly, a morphemic) typology of languages, and if by descriptive grammar is understood the morphemic description of one, synchronic, *état de langue*, it seems obvious that the two must go hand in hand and that they presuppose each other. The same would hold good of phonemic typology and phonemic (idiosynchronic) description, and of other possible branches of linguistics such as, e.g., analysis of the lexical content, and the study of external linguistics (relations between language and culture, etc.), which might all conveniently be comprised under the heading "descriptive linguistics." The answer by E. Buyssens is to this effect, and I take it that any linguist will be prepared to subscribe whole-heartedly to his insistence on the necessity and utility of a comparative method in synchronic grammar (as in linguistics in general, and in science in general). The very instructive contribution by Ellis-Halliday tends also to this point, with emphasis on the distinction between the two points of view which in the present Report are called (A) and (B).

(B) If — as particularly suggested by the wording "traditional comparative linguistics" — *comparative grammar* is to be taken in the somewhat obsolete sense of *vergleichende*

Grammatik, thus restricting it to mean not the general study of linguistic types, but the genetic study of language families, then the question seems to be a repetition of one of the questions discussed at the Sixth International Congress of Linguists, viz. that of the relations between "synchrony" and "diachrony." In effect we should not consider the debate on this subject as closed; but in order to reach a solution, or at least an understanding, it will be useful to recur to the contributions made at the Paris Congress and earlier Congresses, and, more generally, to refer to the present status of linguistics as a science, and not to go back to "traditional" notions like those advocated, say, by Brugmann and (partly) by Saussure, respectively.

Considering the present status of linguistics as a science, it seems, in effect, natural to consider linguistic genetics as a special case within language classification and to distinguish between two different kinds of classes of languages: families and types. A linguistic family is defined by a net of relations (or *correspondances régulières*, as Antoine Meillet would have it) between contextual variants of the elements of expression in one language and contextual variants of the elements of expression in another language (language being here understood as *état de langue*); the elements of expression may be either phonemes (cf. particularly the contribution of Hamp) or graphemes (as in ancient Indo-European, where the relations in question were first recognised, and for which the method of comparison was first set up, by Rasmus Rask); in most actual instances they are graphemes; this remains immaterial to the method. A linguistic *type*, on the other hand, is defined by relations between categories in one language and categories in another language. In both cases, we are dealing with classes of languages, or with systems of systems. In both cases we are necessarily dealing with comparison. This comparison is necessarily a comparison between *états de langue(s)*; it premises a separate description of each of the *états de langue(s)* involved in the comparison, and this description must be given according to a uniform method, which has to be laid down by language theory, which in its turn is founded on observation and comparison, but which emancipates itself from observation by establishing a set of formal definitions and a general calculus. In all this, linguistics simply shares the general fate of any science.

The two kinds of classification are mutually independent, in so far as languages (*états de langue*) belonging to one and the same family may belong to different types, and languages belonging to one and the same type may belong to different families. A mother-language and a daughter-language (say, e.g., Latin and French) belong by definition to one and the same family; their relation is that of continuation (change, evolution), which is a selection whose selecting (premising) terminal (or variable) is the daughter-language, and whose selected (premised) terminal (or constant) is the mother-language. A continuation is thus in itself a pure relation within a system of systems; it is, however, manifested in time and space, like all other observable facts, and its substance has to be described in terms of time and space. The main task of general genetic linguistics must be to establish general laws predicting and precluding changes from one type to another (cf. particularly the contribution by Bolelli).

LOUIS HJELMSLEV

The wording of the questions posed here for discussion implies the existence of limitations on the use of "comparative grammar," which is presumably synonymous with the "comparative-historical method," in descriptive linguistics and suggests that this method is insufficiently "comprehensive". We are asked whether the time factor has validity in describing language in an arrested state, or, in other terms, whether the diachronic and the synchronic approach can be combined in the same investigation. There is, as we are all aware, a school of linguistic thought which rejects such a synthesis outright. If we are adherents of this school our answer to the two questions requires no elaboration. If we are "traditionalists" we shall consider the role of "comparative grammar" in descriptive linguistics to be both paramount and material, we shall deny pure descriptive linguistics anything but a practical value, we shall be disinclined to admit that our method requires to be made more comprehensive, and we shall be content at most with correcting its imperfections under the pressure of new material and the new horizons which this inevitably opens up. It is no doubt true

that we cannot conceive of language as static, except as an abstraction, but it is also obvious that such an abstract conception of language is needed in order to make a linguistic system capable of being defined, not only for practical but for theoretical purposes. The intrusion of a sense of flux into a presentation of the "facts" of language at a particular moment in its development, although more consonant with reality than its exclusion would be, must nevertheless be disturbing and could be confusing. Saussure's separation of the descriptive and historical aspects of language seems to me to be altogether desirable, for it ultimately implies a discrimination between two types of mind — the reporter's and the historian's. The reporter-linguist gives his detailed, comprehensive picture of a language at a given time to all appearance without being in the least aware of the time factor, although this, in terms of years, can be very considerable. The historian-linguist is less concerned with the apparently simultaneous comprehensiveness of his colleague's picture than with such of its details as point to change when compared with similar details in earlier and perhaps later pictures of the same language. This awareness of change is the prerogative of historical linguistics.

The description of a language is not a hard and fast thing, except when it is conceived as belonging to a given fashion (e.g. "traditional," "functional," etc.). It is in the nature of mind to "discover" and to "imitate," advance theories, and to adhere to them, sometimes with dogmatic tenacity, for as long as there is no awareness of flux and fashion. Historical perspective however helps to limit the validity and comprehensiveness of our methods and ranges them in a chronology of development.

Adverting to our questions again, we realise after a little thought that the comparative-historical method, belonging as it does to an out-grown and unfashionable phase of linguistics, can offer the descriptive study of a language at the present time, when "diachronic" and "synchronic" are the rigid terms of an opposition, no more than certain pointers of a very general kind. But these pointers have a high moral and scientific value: they show that the qualities of perseverance, industry, clarity, accuracy, and fair-mindedness are indispensable to the linguistic worker. They also show that

hypothesis is not a substitute for fact and ingenuity for truth. We become aware of these pointers from a study of the defects as well as of the merits of "traditional linguistics." The presence of defects naturally engenders the idea that the comparative-historical method requires to be perfected. And this brings us to our second question, which accepts "traditional comparative linguistics" as a valid method of linguistic study and envisages the possibility of improving it. Here, quite inadvertently no doubt, the time factor reappears.

If we accept the comparative-historical method, as our question means us to do, we must give a definite answer, whether positive or negative. The answer will emerge from a consideration of the interrelations of the traditional and the modern, the less fashionable and the fashionable. We have seen that modern descriptive linguistics can learn valuable scientific and moral lessons from the traditional comparative-historical method. In the same way the "discoveries" of phonology (and its allophone "phonemics"), of glossematics, of "morphophonemics," and of the rest can enrich traditional linguistics with perspectives won from new points of vantage. But in any case, whichever method the linguist adopts, the light that must guide him in his researches, especially in moments when, either involuntarily or of choice, he cannot see the wood for the trees, is the light of truth. He will not see this light if he is content to pursue ingenuity, subtlety, and complexity. This is a warning which can hardly be ignored in our time — an age when involution is at a premium in the creative arts and the making of ingenious cross-word puzzles a satisfying mental exercise.

W. K. MATTHEWS

What historical linguistics can learn from synchronic linguistics is the structural approach. In the synchronic description of languages we deal not with isolated features, but with patterns. Actually the idea of structure is nothing new in linguistics: the existence of morphological patterns was recognised a long time ago — by the classical grammarians, in fact. Nevertheless, we still find people handling diachronic problems in terms of the evolution of features and not of patterns.

The tendency is especially conspicuous in the phonological field (cf. the treatment of Italian phonology by G. Rohlfs, which is excellent in so many respects). People seem to think it enough merely to state that Latin *ū* > French *u*. Those who do demand an explanation are usually content with a Celtic substratum. A proper causal explanation must consider the phonological patterns of all the stages and languages involved in the change, as well as the morphological and lexical patterns in so far as these may have a bearing upon phonological evolution.

A. MARTINET

Je constate avec regret, mais non sans un certain soulagement, que je n'étais pas le seul à ne pas comprendre la question que nous sommes cependant réunis ici pour discuter. Les contributions précédentes montrent, que le sujet dont nous traitons de facto, est celui de la relation des linguistiques synchronique et diachronique. A ce sujet je désirerais souligner l'importance d'établir des descriptions synchroniques détaillées des états passés d'une langue.

Les descriptions synchroniques que l'on donne de l'état actuel d'une langue se distinguent, en général, par une louable précision et essaient de décrire jusqu'au moindre détail le système de la langue en question. Des recherches diachroniques devraient en principe pouvoir retracer le développement de chacun des faits ainsi enregistrés. Pour ceci il faudrait connaître les états antérieurs de la même langue. Or, il arrive que trop souvent, on se contente de retracer les antécédents phoniques des morphèmes actuels, sans se soucier des rôles qui leur étaient dévolus dans les systèmes linguistiques antérieurs. Qui pis est, on admet a priori que leur emploi n'a pas changé et que seules leur formes ont subi des transformations. Or, il se peut que le morphème en question ait été utilisé différemment à une époque plus ancienne que ne l'est son "descendant" actuel; c'est-à-dire qu'en fait il s'agit de deux morphèmes différents. Pour que la linguistique diachronique soit autre chose qu'un jeu abstrait de linguistes, il faut qu'elle soit basée sur une série de descriptions synchroniques dignes de ce nom.

D. SINOR

I will first defend myself and then attack myself.

(1) I could not answer the first of the two questions because the definition of language implies that a particular language is conventional, i.e. supra-individual, but not universal. On the one hand, all linguistic elements common to users of a particular language constitute its grammar. On the other hand, we cannot state that a particular fact belongs to a particular language, unless we can prove that the fact is non-universal, which means that we have to compare the language in question with other languages. (The history of a language involves the succession of a number of systems. Accordingly a description of that succession involves comparative grammar in the modern sense.) We may, therefore, substitute "comparative" for "descriptive" linguistics in the first question which will read as follows: "In what sense and to what extent is comparative grammar of value in comparative linguistics?" But I can find no answer to such a question.

(2) In the Ionic of Homer there is no semantic difference between $\lambda\acute{\upsilon}\sigma\omicron\mu\epsilon\nu$ and $\lambda\acute{\upsilon}\sigma\omega\mu\epsilon\nu$. The external opposition represented by ω/ω , has in this case no semantic function. Nevertheless, that opposition is conventional ("phonological"), as the external opposition, represented by $\lambda\acute{\upsilon}\sigma\omicron\mu\epsilon\nu/\lambda\acute{\upsilon}\sigma\omega\mu\epsilon\nu$, does not occur in the Greek of the third century of our era, in which the first syllable of $\lambda\acute{\upsilon}\sigma\omicron\mu\epsilon\nu/\lambda\acute{\upsilon}\sigma\omega\mu\epsilon\nu$ is stressed, whereas in Classical Attic and Ionic the *mora* immediately preceding its *paenultima* bears the pitch accent. As the semantic opposition $\lambda\acute{\upsilon}\sigma\omicron\mu\epsilon\nu/\lambda\acute{\upsilon}\sigma\omega\mu\epsilon\nu$ in Classical Attic occurs in neither the Ionic of Homer nor in the Greek of the third century of our era, it is a conventional opposition. And now I shall apply the method of the "historische vergleichende Grammatik" in the old-fashioned sense of the term (for I like to reconstruct Classical Attic as others like to reconstruct Indo-European). It would be odd, if in a language such as Classical Attic, being historically between two other languages such as Homeric Ionic and the Greek of the third century of our era, there had been a future tense unknown to the other two. In fact, Classical Attic $\lambda\acute{\upsilon}\sigma\omega\mu\epsilon\nu$ is characterised by actuality ("present tense") and by a particular aspect, but not by "future tense," like $\lambda\acute{\upsilon}\sigma\omicron\mu\epsilon\nu$ and $\lambda\acute{\upsilon}\sigma\omega\mu\epsilon\nu$, between which it stands. The aspect of $\lambda\acute{\upsilon}\sigma\omicron\mu\epsilon\nu$ is momentaneous like the aspect of $\lambda\acute{\upsilon}\sigma\omega\mu\epsilon\nu$, but the aspect of $\lambda\acute{\upsilon}\sigma\omicron\mu\epsilon\nu$ is durative. The mood of $\lambda\acute{\upsilon}\sigma\omicron\mu\epsilon\nu$ is indubitative, like the mood of $\lambda\acute{\upsilon}\sigma\omicron\mu\epsilon\nu$ but the mood of $\lambda\acute{\upsilon}\sigma\omega\mu\epsilon\nu$ and $\lambda\acute{\upsilon}\omega\mu\epsilon\nu$ is dubitative.

PIER ERINGA

It will be convenient to take questions B (2) and (3) together. I think that each of these questions should be, first of all, more clearly formulated. What is meant by "comparative grammar?" In point of fact, we have two comparative grammars — the one which studies heterogeneous language families to arrive at conclusions about the general development of language, and the one which is concerned with a particular language-family, i.e. with allied languages. The first I should designate as the comparative grammar of general linguistics, and the second as the comparative grammar of allied languages.

It is not difficult, in my opinion, to show that the aims and methods of both are not identical. The aims of the comparative grammar of general linguistics are to discover the general laws of the development of language, and through this, perhaps, the origin of language. A sound knowledge of the structural-functional features of a language (i.e. the system of the language and its use of words in sentences) is necessary, in the first place, for this grammar, whereas knowledge of its phonological and morphological-semantic features is secondary. The aims of the comparative grammar of allied languages, on the other hand, are quite different. It is important to show here how individual languages have evolved from common languages, as well as what prompted this development and what hindered it. Such things as can prove a common origin most easily and convincingly and show the lines of development from the common language to the separate languages are obviously paramount. Phonetical-phonological and morphological-semantic properties, are the most significant for them, and only in the second place the structural-functional properties, which are not so easily changeable and which, being common to the whole language-family, are in general applicable to all its individual members. It would seem, in view of what has been said, that in framing question (2) one had in mind the comparative grammar of allied languages. As for the perfection of methods for the study of the comparative grammar mentioned in item (3), I think that they relate to the comparative grammar of general linguistics, which still stands in need of elaboration. One can see from the history of our science that this is correct. The comparative grammar of Indo-European languages mostly emphasized phonetics (phonology) and morphology (in the broad sense of the word),

giving syntax second place, whereas current general linguistics is mostly, and quite rightly so, evolving in the structural-functional direction. It should not, of course, neglect, as it sometimes does, the phonological-morphological features of the language-families studied by the comparative method.

A. BELIĆ

I have been working for some years, first with Professor Westermann and lately with Professor Tucker, on the classification of African languages, i.e. languages "without a history," including both languages which are fairly well known and others which have been almost, if not entirely, unknown until now.

We began by following more or less the "pigeon-hole" system, but have become increasingly aware of the dangers referred to by previous speakers. As a result, we found ourselves continually increasing the number of pigeon-holes. In fact, we have found that what is needed is less a pigeon-hole than a card-index system. You do not start with a pigeon-hole and fit things into it, but you discover a phenomenon and start a new section of the card.

M. BROWN

The diachronic and synchronic theories of the development of comparative grammar, as discussed here, must also be tested by an historical perspective of the oldest known sites of social groups. E. Herzfeld and O. S. Menghin thought that cultures first arose in highland regions and gradually descended to the plains. In the hills of Armenia, India or Elam-Persia, the spoken word and the parts of speech have developed in the same way as among peoples of the same regions settled on much lower ground. In the Taurus, the writer observed that native Armenian cattle-farmers, who until recent years had lived isolated, had no notion, even if they were literate, of future tense and subjunctive mood, and except the genitive and instrumental, no idea of nominal declension; yet they could correctly sing or recite the liturgy in Classical Armenian. Furthermore on a steep peak, about 12,000 feet high, two

groups of natives, at a distance of about 400 yards from each other down the mountain, could hear one another clearly, but could not understand one another without manual gestures.

Under these circumstances, it seems that comparative grammar is of little value in descriptive linguistics.

If the Indo-European words in cuneiform Hittite are derived from — or of common origin with — the Khurrian (the Tushratta letter, glosses in the Kikkah text) it may be reasonable to assume that, the Khurrian-Chaldean (Urtian) on the one hand, and Kassite-Sanskrit-Avestic, on the other, had a synchronic development, but, it would seem, only in vocabulary. In so far as the Eastern group of Indo-European languages is concerned, it does not seem that grammar is more important for linguistic research than vocabulary; whereas north of this Indo-European belt, the group of Caucasian languages, which are still little known, seem to present a diachronic development. If we suppose that Lawazantiya¹, mentioned in Hittite texts, is identical with modern Lazistan, and that the language of Pala, meagrely attested in Hittite records, was the dialect spoken in that region, the language of the Laz people would seem to be the oldest type of Caucasian. Like their ancestors in the Ancient East (cf. the item *Lazica*, in Pauly's *R.E.*), the Lazes today are daring and skilled navigators. It must be assumed that in the course of centuries, Laz navigators carried their language along the coasts of the Caucasus to the Georgians, Mingrelians, Circassians, Abkhazians, and other tribes inland. Although there is very little evidence to substantiate this view, the Caucasian group of languages may be taken as a diachronic growth of comparative linguistics.

A. SAFRASTIAN

¹ Cf. A. Goetze: *Kizzawatna and the Problem of Hittite Geography*. Yale University Press, New Haven, 1940, p. 71 ff. and footnote 289.

SECTION B

COMPARATIVE LINGUISTICS

- (4) Are there areas of *affinité grammaticale* as well as of *affinité phonologique* cutting across genetic language families?

Chairman : PROFESSOR C. REGAMEY

Rapporteur : PROFESSOR A. MARTINET

Recorders : MR. P. B. SALMON
MR. W. E. SKILLEND

For Preliminary Reports see pp. 119-130 above.

THE HISTORY OF THE
COMPARATIVE LINGUISTICS
BY
J. H. GREEN
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INTRODUCTORY REMARKS

The term "affinity," as it is used in such phrases as "area of linguistic, phonological or grammatical affinity," is non-committal in the sense that it does not imply any particular mode of diffusion whereby the observed similarities could be explained. In his 1936 Copenhagen report, Roman Jakobson very carefully avoided favouring or excluding any type of causal explanation. Consequently the use of this term especially suits the convenience of those who are confident that the bulk of synchronic descriptive evidence will suffice to convince all linguists of the existence of such areas.

Yet most linguists have retained a legitimate interest in diachronic processes. Evidence of synchronic similarity is felt to be little more than raw material for diachronic investigation. Traditionally whatever similarity cannot be explained genetically is ascribed to chance. If this similarity is considerable and structurally integrated, the traditionally inclined linguist may be disturbed but will not be convinced until he has been given or has found an explanation.

The non-linguistic explanation according to which linguistic similarity is ultimately due to protracted living in the same habitat, without any necessary implication of socio-linguistic contact, will hardly be found acceptable, except perhaps in lexical matters. In any case, this is an hypothesis which will be hard to prove or disprove, because, in the long run, two peoples living in the same corner of the world cannot help establishing contacts.

Here, as well as in genetic matters, linguists should first of all look for *linguistic* causes, which they are well equipped to handle, before they bother about climate, altitude, latitude, and the like.

Linguistic explanations of affinity belong to two types:

- (1) the mutual influence of the two languages in contact, with or without predominance of one of them;
- (2) the influence exerted by a third language, be it a substratum, a superstratum, or an adstratum language.

As soon as causes, of whatever nature, are adduced, the whole problem of linguistic affinity becomes a part of the vast domain of linguistic convergence which is, or at least should

be, one half of dynamic linguistics, the other half of which is, of course, linguistic divergence, an aspect which, for about a century, was believed to exhaust the task of linguists.

So far, linguistic research has hardly been affected by the Saussurian conception of linguistic evolution as determined by the conflicting requirements of "*l'esprit de clocher*" and the force of intercourse. Even though the *Stammbaum* pattern is to-day generally pooh-poohed, many linguists continue in practice to operate with linguistic evolution as an unending branching off. The wave theory is still widely looked upon as a theory and nothing else whereas linguistic divergence is known to be a hard fact. Little is known so far about the geographic spread of linguistic change; it seems clear only that various types of spread should be distinguished. Such a spread is generally considered likely only in homogeneous areas, i.e. when it takes place from one dialect to another dialect of the same language. In other words innovations could only spread through areas where mutual understanding is secured, if not from one end of the domain to the other, at least between neighbours and without any break. A language boundary is normally one of the breaks between areas of mutual understanding. One appears where contact, loose or close, is established between populations that were not in contact before. Mutual understanding is not necessarily excluded at a language border, or, at least, it may be restored if the two speech forms in contact are genetically closely related. In such a case, no one would doubt that changes could spread even across such a border. But if the two languages thus put in contact are not related or are only distantly so, mutual understanding can only be secured by some measure of bilingualism. Our problem becomes thus: how far and how can bilinguals transmit linguistic forms and changes from one community to another? This, in my opinion, is one of the central problems of our science.

So far bi- or plurilingualism has been widely considered as an exceptional, abnormal situation belonging, almost, to linguistic pathology. Two facts have often been overlooked:

(1) a general bilingualism i.e. one affecting all or at least most of the members of a community may be an unstable situation, but it is an extremely wide-spread one. In modern France practically all the peasant population of the southern

half of the country is bilingual, not to speak of Brittany, Flanders, Alsace, and a good many Romance speakers in the northern half;

(2) in a community where only a few people are bilingual, those are, as a rule, the ones endowed with prestige (cf. mediæval England), i.e. the ones who are likely to be imitated in linguistic as well as in other matters.

All stratum theories, as soon as they cease to be sheer romantic speculation, boil down to a research into the effects of bilingualism on linguistic evolution.

Thus any study of the dynamics of linguistic affinity will ultimately merge with that of the effects of bilingualism.

It is of course clear that the first part of our task remains the collection of language material pointing to areas of linguistic affinity. The most serious difficulty to be conquered initially results from the dearth of linguists equally at home in the two or more genetically distinct domains that have to be investigated. Yet team-work should afford a solution, provided co-ordination can be secured.

A possible approach would be the preparation of a linguistic atlas of a domain, lying across one or several linguistic boundaries. The Balkans would be a linguistically favourable ground. But the current political situation would probably make any Balkan project impracticable. Since the effects of convergence have to be opposed to those of divergence, it is of great importance that the languages investigated should belong to genetic groups whose history is well known. Therefore I would suggest as a field of research a strip of land parallel to the coast of the Channel and that of the North Sea, approximately 150 miles wide, extending from the Seine to the Weser. Lest the questionnaire should become unwieldy, lexical problems should be left aside and the investigators should concentrate upon the phonological, morphological, and syntactic patterns. Historical perspectives should by no means be neglected.

The *enquête* should not be made with a view to collecting alleged proofs of affinity exclusively. At every point, a detailed picture of the structure should be drawn whereby the comparative importance of genetic and non-genetic factors could be assessed. This would imply a very large questionnaire, and might make it advisable not to plan an atlas, but a detailed *enquête* carried out in a limited number of points.

A. MARTINET

Professor Ernst Lewy, der leider nicht hierher kommen konnte, hat in seinem *Bau der europäischen Sprachen* an der Hand von Strukturskizzen der einzelnen Sprachen den Nachweis geführt, dass sie sich trotz verschiedener genealogischer Zugehörigkeit zu drei grossen Gruppen zusammenschliessen, einer westeuropäisch-atlantischen, einer zentralen und einer östlichen. Das Englische, das ursprünglich als germanische Sprache zu der zentralen Gruppe gehörte, ist auf den Britischen Inseln unter dem Einfluss des Keltischen in die atlantische Gruppe übergegangen. Diesen Einfluss des Keltischen habe ich seit 1938 in den *Indogermanischen Forschungen* 56 und 57 und in der *Anglia* 66 aufgezeigt. Er erstreckt sich auf Lautliches, Wortbildung und vor allem auf Syntax und besondere Wendungen. Im einzelnen sehe ich ihn in dem frühen und völligen Verlust unbetonter Silben, der zu kurzen Wörtern geführt hat; in der Bildung der reflexiven Fürwörter; in der Entwicklung des sogenannten Gerund mit all seinen syntaktischen Besonderheiten; der progressiven Form des Verbs; der Verwendung von "to do" in Fragen und verneinten Sätzen und zum Nachdruck; des uneingeleiteten Relativsatzes und der Endstellung der Präpositionen in diesem und in anderen Relativsätzen; der Hervorhebung eines Satzteils mit *it is . . . who* etc.; die Neigung zu nominalem und attributivem Bau des Satzes, aber zu statischem Satzbau; in der Wortfolge *there + Verb + Subjekt* (*there are some people coming to dinner tonight*); in dem klassifizierenden unbestimmten Artikel beim Prädikatsnomen; in der Verwendung von Nebensätzen in Abhängigkeit von Präpositionen (*there was such a contrast between what went on inside (the house) and what went on outside*); in dem ausgedehnten Gebrauch des attributiven Possessivpronomens; in der Formung der angehängten Frage: *you see that, don't you?* (und umgekehrt); in der allmählichen Steigerung *better and better, more and more*. Das sind die wesentlichsten Punkte.

Verschiedene Parallelen dazu bieten Spanisch, Französisch, Niederländisch, Dänisch-Skandinavisch. Soweit nicht auch hier direkt keltischer Einfluss vorliegt, handelt es sich nach meiner Ansicht um Fortwirken der hamitischen Sprachschicht, die nach Morris-Jones vor allem Pokorny für die Ausprägung des Inselkeltischen herangezogen hat und die Freiherr von Eickstedt anthropologisch nachgewiesen hat.

Im Englischen treten die genannten Erscheinungen etwa gleichzeitig, seit dem 13. Jh. auf, soweit sie datierbar sind.

Ich behaupte nicht, dass der Einfluss des Keltischen *allein* die englischen sprachlichen Erscheinungen hervorgebracht hat; oft mögen andere Bedingungen mitgewirkt haben; aber er ist nach meiner Überzeugung überall *entscheidend* gewesen.

W. PREUSLER

The degree of structural affinity among Chinese, Thai and Malay seems sufficient to arouse interest. It is impossible, without a detailed analysis, to estimate this degree of affinity relative to that obtaining between (say) two New Chinese dialects; here I merely wish to note similarities in the grammatical structures of Chinese (Pekingese), Thai (Siamese) and Malay.

Each of the three languages consists almost entirely of words entering freely into syntactic combinations and without formal distinction of word-class (of which a certain number, considerable in Chinese, small in Malay, are "bound," that is, restricted to combination with certain other words); together with a small number of auxiliary elements, of which Malay nominal and verbal affixes are distinguished by their greater number. Word-order is fixed, subject preceding predicate and the verb its object; the qualifier occurs in fixed position relative to what it qualifies (preceding in Chinese, following in Thai and Malay).

All the languages are characterized by the existence of the unmarked category in the noun and verb (nouns unmarked as to number, e.g. Chinese *ren* "person, people," contrast the personal pronouns; verbs as to temporal category). All make use of classifiers with nouns with numeral (and, in Thai and Chinese, certain other) qualifiers; classifiers form one word-class with partitives and collectives. In all the predicative adjective is verbal; Malay alone, however, requires no copula with substantive predicates. There is no separate word-class of prepositions; spatial (and temporal) relations are expressed by the combination of a verb (in Malay, auxiliary element) giving the relation (motion to, towards, rest at, &c.) with a noun giving the location (front, inside, &c.).

There is no special form for questions of either type, which follow normal word-order; the use of alternative questions (formed by negative "or not?") is common to all. There is a special negative word with sense "not yet" (not in Pekingese, but in other Chinese dialects as Cantonese). There are no negative or indefinite pronouns, the interrogative words being used in combination: "no-one" = "who? . . . not."

A comparable degree of similarity in grammatical structure could be shown for Annamese, and languages of the Mon-Khmer family. Among the languages here considered there are of course significant differences (especially in word-order: the position of the qualifier and of the classifier). There are also similarities in detail which are striking (e.g. the presence of a generalized object with verbs of the type *eat*, *sleep*, *walk*). There are also lexical similarities, such as the highly specific nature of the concrete verb, (e.g. the many different words for "to cut").

Among the items mentioned, there are some shared by only two of the three languages. No item however is common only to Chinese and Malay. The suggestion that there is here an area of *affinité grammaticale* is not a new one; nor am I offering an interpretation. I wish only to suggest that taking into account the variations of grammatical structure among languages in general, and among some genetically related languages, the degree of affinity here warrants our consideration.

M. A. K. HALLIDAY

Are there areas of *affinité grammaticale* as well as *affinité phonologique* cutting across genetic language families?

"Phonological affinity," as revealed, say, by Trubetzkoy's comparison of palatalisation in the genetically diverse Russian and Mordvin, is a synchronic datum such as can be multiplied by reference to the phonological systems of many other geographically adjacent languages of different origin. There is nothing however to be gained at this stage by pursuing such comparison beyond relatively restricted geographical limits. We have not sufficient organised data and cannot delimit areas of phonological affinity without such data. It is therefore idle to seek the origins of such phenomena by tracing them

back to the existence of substrata or to any other plausible, but hypothetical causes. The task of recording dispassionately must precede the "illumination" of a "theory."

"Grammatical affinity" has been less studied and therefore appears as a more tenuous phenomenon. But its existence could be reasonably postulated, even if instances of it had not yet been recorded. Such instances however are familiar to readers of Sandfeld's *Linguistique balkanique* or to those who have noticed the presence of a formally defined "narrative mood" (a paradigmatically expressed *oratio obliqua*) in genetically diverse Estonian and Latvian (cf. Est. *ta olevat tulnud* "he is said to have come" with Latv. *viņš esot atnācis*), which have no formal parallels in adjacent languages, for Finnish and Lithuanian use variable participles and German has an auxiliary verb (e.g. *er soll gekommen sein*). This, like the Balkan examples of grammatical affinity (cf. the presence of a similar, but more elaborate construction in Bulgarian and Turkish) may be examined as a fact of linguistic convergence, or call it what you will. It should be observed however that in doing so we are merely giving the phenomenon a name which is not altogether adequate, because it deliberately selects and emphasises one of several contributory and variable functions.

The existence of a particular grammatical concordance between languages of unrelated types which are separated by considerable distances need not necessarily point to grammatical affinity in the geographical sense (cf. the "ergative construction" as I have discussed it in my forthcoming article "The Ergative Construction in Modern Indo-Aryan"), but the occurrence of a parallel set of refinements within such a concordance between adjacent languages, as in the "ergative construction" of Nepali and Tibetan, (viz. the presence of the construction outside the sphere of the past participle), does point to such affinity.

Our areas of affinity, whether phonological or grammatical, will serve as useful working hypotheses so long as we do not venture to define them too closely in a geographical sense. We are not in a position yet to delimit isoglosses and we should studiously avoid doing so until the presence of substantiated facts assumes this shape.

W. K. MATTHEWS

I think the question posed in this session is in reality a question on the "*parenté linguistique*." Fundamentally, when we speak of linguistic relationship we do not forget the biological metaphor according to which languages are living organisms. But, as has been rightly pointed out, linguistic relationship is only a case of cultural relation (Terracini). This cultural relation can be remote, and then signifies a common origin, or it is simply a case of cultural contact or even adaptation. Most of our modern European languages, or Greek and Latin, are tongues relatively immune to external influence. Borrowings, semantic adaptations, are not sufficient to let the languages lose their special physiognomy. But if we consider the relationship between French and English we see well what *affinité grammaticale* and even *affinité phonologique* mean across the frontier between Romance and Germanic languages.

I would refer to two cases of such affinity. The first, one of *affinité grammaticale*, is taken from South American languages. The most different languages have sometimes the same possessive prefixes, as it is possible to see on the diagram. It would be possible to continue this enquiry farther all over the American continent, and to extend this affinity.

The other case, an *affinité phonologique* emerges from the similar features observed by Gomez-Moreno for Iberian and Basque: no initial *r*, no *f*, no *v*, no *muta cum liquida* group . . . Or from the phonological similarity between Spanish and Basque: the same five vowels, the same *rr*, no *v*, an equal proportion of vowels and stops and so on.

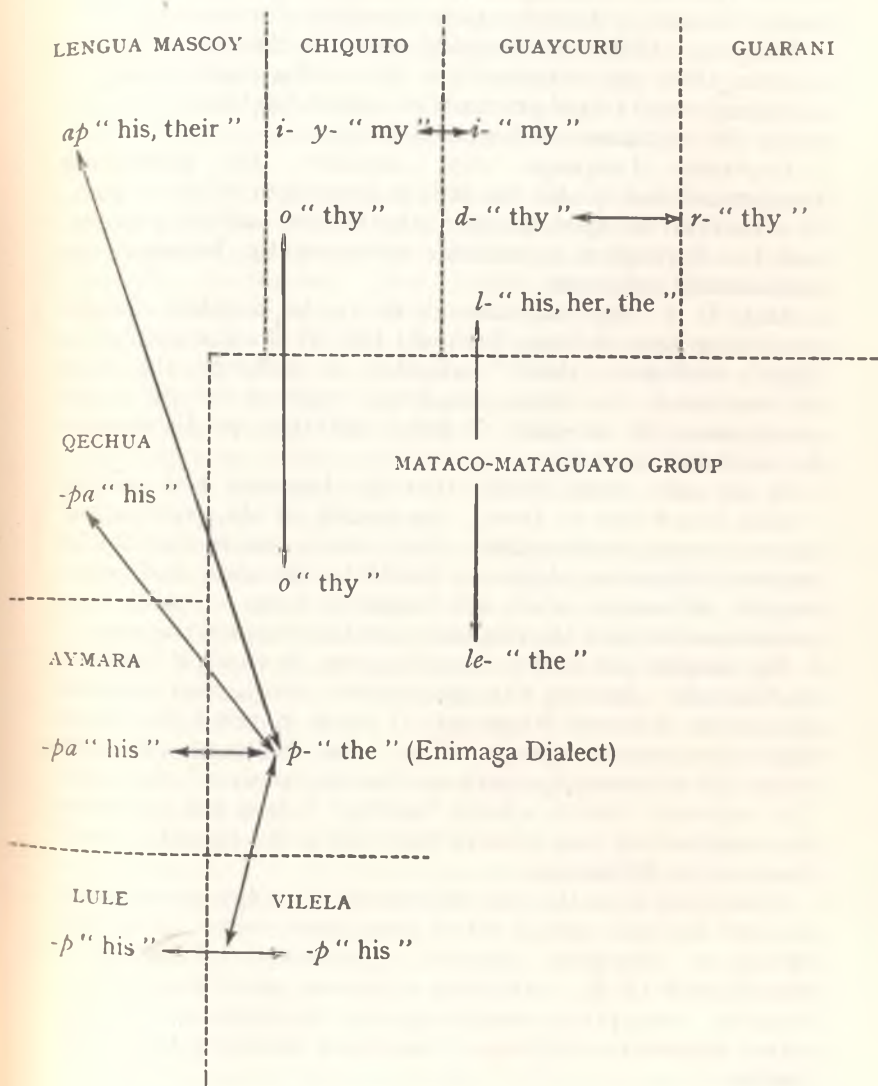
The conclusion to be drawn from this is that these affinities are very likely to cut across the most diverse languages. Professor Ernst Lewy has pointed out the common features of the so-called "Atlantic Languages." I think such vicinity and culture groups are more likely to be formed as we go on to more primitive conditions.

I propose to insert between the "*Elementarverwandschaft*" pointed out by Schuchardt, and the genealogical relationship, a type of prehistorical relationship, corresponding to a time when unilingualism was not yet established. Beside the different types of relationships we have the cultural interpenetration of languages, which makes it possible to speak of a European type.

ANTONIO TOVAR

Map to show the "*affinité grammaticale*" of some South American languages not genealogically interrelated.

Wandering possessive prefixes (or suffixes)



The problem under discussion is similar to the old and much debated question of "mixed" languages. The result of that discussion was rather ambiguous. On the one hand it was stated that all languages are "mixed," and on the other hand, that such a thing as a mixed language does not exist.

Long ago Whitney (*Language and the study of language*, London, 1868, 199) expressed the opinion that such a thing as a language with mixed grammatical apparatus has never come under the cognizance of linguistic students.

Jespersen (*Language*, 215) considers this affirmation exaggerated and quotes the Balkan languages as an example. It is however an open question whether the suffixes, prefixes, and the syntactical agreements instanced by Jespersen are grammatical influences.

And, if I may be allowed to quote another opinion concerning this problem, I should like to draw attention to Sapir's statement that "Language is probably the most self-contained, the most massively resistant of all social phenomena. It is easier to kill it off than to disintegrate its individual form."

If we agree with Meillet that the language is a system "dans lequel tout se tient," the mixing of the grammatical systems seems inadmissible. One should clearly distinguish between influences which are facts of civilisation and grammatical influences, which are linguistic facts. I think that a confusion between the two categories has obscured the issue.

My remarks will refer to a specific area, in which it has been traditionally admitted that grammatical isoglosses cut across genetically different languages. I have in mind the South East European languages. The term Balkanic philology labels the relationships between the languages of that area. The regretted Danish scholar Sandfeld Jensen has presented the results of his researches in that field, in his masterly work: *Linguistique Balkanique*.

Proceeding from the concordances in the languages of South Eastern Europe, special terms have been coined: *Philologie balkanique* (Sandfeld Jensen), *Sprachbund* (Trubetzkoy), *Spracheinheit* (Jokl). All these terms are meant to express a linguistic conception concerning the development and the actual structure of a group of languages belonging to various families.

However, the studies published so far do not support the existence of a type of mixed linguistic structural system. I share the caution of the rapporteur that "the whole problem still awaits an exhaustive and authoritative treatment."

In his report, Mr. Martinet says further that the linguists of the Prague School have pointed out the existence of areas of "*affinité phonologique*." I should like to draw attention to the fact that this attempt of the School of Prague has so far been unsuccessful regarding the area with which I am concerned.

In 1933, B. Havranek, based on Sandfeld Jensen's, *Linguistique balkanique*, made an attempt to establish the phonological system of the Balkan languages (*Zur phonologischen Geographie. Das Vokalsystem des balkanischen Sprachbundes*) and *Archives Néerlandaises de Phonétique Experimentale* VIII-IX, pp. 119-125; *Proceedings of the International Congress of Phonetic Sciences*, La Haye, 1933). He states that "in den Balkansprachen existiert ein im Grunde identisches Vokalsystem." But the Balkanic phonological system of Havranek does not include the Greek vocalic system and eliminates some Serbian dialects. On the other hand it includes the Ukrainian.

The arbitrariness of the phonological approach to the vocalic systems of the Balkanic languages has been shown, in the same year, by the Polish balcanologue M. Malecki (*Systemy wokalne języków balkanickich*, see *Sprawozdania P.A.U.*, 1933, vol. 38, nr. 8, pp. 3-7; also *Bulletin int. de l'Acad. Pol. des sc. et des lettres*, 1933, pp. 156-160) who demonstrates that such a thing as a common system of the Balkan languages does not exist. He does not deny that these languages have in common a dynamic stress and have no vocalic quantities.

In conclusion I think that there is no clear evidence yet of grammatical, or phonetic elements spreading across genetic boundaries of this particular area and entering the structure of genetically different languages. There are lexical and syntactic influences due to common cultural influences, or parallel developments due to the same attitude of the human mind towards necessities of linguistic expression.

In support of this last point I should like to record a remark made the other day in one section of this congress by Sir Alan

Gardiner, who drew attention to the fact that in Old Egyptian the negative form of a verb in the imperative demands a kind of imperfective verbal aspect. (See Sir Alan Gardiner, *Egyptian Grammar*², p. 262: "do not hear may, at the start have signified *do not be (in) hearing* . . . (the second part) being analogous to an adverbial predicate. It is possible . . . that the negational complement is a survival of the 3rd pers. m. of the active old perfective, become stereotyped and invariable for all persons and numbers in this particular use.") Now, something similar exists in other languages, e.g. in Rumanian where the verbal form of the negative imperative is an infinitive, different from the affirmative imperative form. Certainly there is no connection between Rumanian and Old Egyptian. Greek and Latin showed also a certain dislike of using the negation with a perfective verb (See J. Vendryès, *La comparaison en linguistique* (Bull. de la Soc. de Ling., 49, nr. 124, 1946, p. 11)). In Russian we have similar constructions: *zastreljati ego*.

Such parallel developments, I would be inclined to explain in agreement with the rapporteur: "by the psycho-biological substratum common" to the human mind, or if you prefer Schuchardt's term: *Elementarverwandschaft*."

The contiguous languages of South Eastern Europe have common cultural and lexical influences, but whether they have also common overlapping grammatical features, due to grammatical or phonetical isoglosses cutting across genetic language families, remains to be shown.

G. NANDRIŞ

The mention of *affinité phonologique* and grammatical affinity cutting across genetic language families makes one immediately think of the old problem of the inter-relation of the Uralian and Altaic languages, which, very many years ago had been recognized to show remarkable structural similarities in a number of features. The question of their genetic relationship has, however, not even yet received its final settlement.

If, at the same time, the problem of the genetic inter-relation of the sub-groups of the Altaic languages is taken into consideration, it will be found that even the genetic relation between the two most similar sub-groups, i.e. the Turkic

and Mongolian languages, had been regarded as not really proved until quite recently, and is still not accepted by everyone.

Whether or not one may recognize the existence of genetic relationship between these two language groups, it is nevertheless notable that the geographical conditions of the area in which these languages are and were spoken, as well as the extreme mobility of the speakers in times of peace and even more so in times of war greatly facilitated a very far-reaching mutual influence as between the languages in question.

The great movements and expansions of the tribes of the steppe, like those of which history provides us with so many examples, might have greatly contributed to assimilating languages to each other which originally were much wider apart, and might have been partly responsible for giving rise to the phenomena of *affinité phonologique* and grammatical affinity found in the present day languages.

Another example may be taken from a linguistic area whose languages have only in recent years been partly and often very preliminarily recorded by linguists in the interior of the large island of New Guinea.

It may be noted that, as soon as the majority of the languages of a certain area of the island become more or less known in their main features, the first attempt to divide them into language groups can only be undertaken on the lines of obvious grammatical affinity and *affinité phonologique*. This by no means implies that a number of languages put into one group are linked together by genetic relationship.

The amazing multiplicity and diversity of languages in New Guinea, together with the complete absence of any linguistic historical data make it as a rule very difficult to establish genetic relationship between two or several languages, if in this one leaves out of account the Melanesian languages spoken in certain coastal areas and on the adjacent islands, which are of Austronesian stock, at least in a strong stratum.

It is fairly commonly found in the interior of New Guinea, that adjoining languages have very different vocabularies but, as Dr. A. Capell states in his study on the distribution of languages in the central highlands of New Guinea, their grammatical features do "recur with almost monotonous regularity from language to language." It is not unlikely

that quite a number of such languages are, in reality, genetically unrelated, and that their grammatical affinity may be due to some common substratum. Future research may perhaps contribute to elucidate these problems.

STEFAN WURM

Dans toutes les contributions à la discussion de cet après-midi l'affinité grammaticale entre les langues non apparentées a été traitée du point de vue de *contact* entre les langues, soit qu'il s'agissait de l'appartenance à un même "*Sprachbund*," soit à l' "*Elementarverwandschaft*," c.à.d. à une sorte de substrat psychologique commun. M. Halliday, dans sa contribution, suggère encore une autre explication, en se demandant "whether the general homogeneity of material culture would tend towards a uniform linguistic structure (p. 126 des "*Preliminary Reports*"). Seul, M. Matthews a mentionné un autre genre de ressemblance, due tout simplement à l'évolution indépendante des structures analogues par suite de l'existence, dans les langues en question, des conditions semblables. Je tiens à souligner ce fait, car il nous montre que les similitudes de structure entre les langues non apparentées ne doivent pas toujours être attribuées à un contact. Il nous permet également d'étudier certaines lois générales de la formation des systèmes linguistiques qui font que les mêmes conditions amènent des résultats convergents jusqu'en détails.

Je suis heureux que M. Matthews a fait cette observation au sujet de l'ergatif, car moi-même j'ai été amené à des conclusions semblables par l'étude de cette construction. J'ai présenté mes conclusions dans la contribution aux "*Preliminary Reports*." N'ayant pas suffisamment de place j'ai dû me limiter à un exposé dogmatique et abstrait. Permettez-moi de le compléter par quelques explications et par des exemples.

La structure qui nous intéresse ici se présente de façon très claire en tibétain (pour les détails je renvoie à mon article "Considérations sur le système morphologique du tibétain littéraire" *Cahiers Ferd. de Saussure*, 1947-48). Le verbe tibétain est nominal tant au point morphologique que sémantique. Il ne diffère du nom que par la faculté de

distinguer des modes, des aspects (duratif et perfectif) et — pour les verbes transitifs — des diathèses: subjective, désignant l'action accomplie par l'agens, et objective, désignant l'action subie par le patiens. Les aspects et les diathèses sont étroitement associées et leur combinaison donne les nuances de temps. En effet, l'action passée de l'agens est en même temps l'action présente du patiens: "*Le bois coupé par l'homme*" = "*l'homme a coupé le bois*" (perfectif) ou "*le bois est toujours coupé*" (duratif).

L'opposition sujet-objet est remplacée en tibétain par l'opposition agens-patiens qui sont les compléments du verbe occupant la place centrale dans la proposition. Tous les deux sont exprimés grammaticalement par le même cas sans particule qui n'est ni le nominatif ni l'accusatif, mais l'*absolutif* dont la fonction est déterminée par la diathèse du verbe central. Exemples:

1. Verbe intransitif (qui n'a que la diathèse subjective).
yab gsun — "le père parle," littéralement: "par rapport au père (absolutif) il y a action de parler."
Ici l'absolutif ne peut désigner que l'agens.
2. Verbe transitif: (a) diathèse subjective:
dpyod-pa-po dpyod-byed "l'examineur examine" = "par rapport à l'examineur il y a action d'examiner."
L'absolutif désigne l'agens.
(b) diathèse objective:
bum-pa dgan "on remplira le vase" = "par rapport au vase, il y aura le fait d'être rempli." L'absolutif désigne le patiens.

L'unique fonction de l'absolutif est d'indiquer le nom auquel se rapporte *directement* l'action ou l'état exprimés par le verbe. Le syntagme qu'il forme avec le verbe est indépendant de toute détermination ou subordination, — je l'appelle "*syntagme ouvert*."

Par contre pour exprimer les rapports supplémentaires entre le verbe et ses compléments, les langues de ce type utilisent les *tagmes déterminatifs* (compléments déterminant le verbe). A la catégorie de ces relations plus spécialisées appartient l'expression de l'agens du verbe objectif ou du patiens du verbe subjectif.

Éhos bstan-to "la Loi a été enseignée" — syntagme ouvert, le verbe étant objectif, l'absolutif désigne le patiens.

sañs-rhyas-kyis *chos bstan-to* " la Loi a été enseignée par le Bouddha " par rapport à " la Loi il y a le fait d'avoir été enseignée par le Bouddha. " L'agens *sañs-rgyas-kyis* " par le Bouddha " est exprimé ici par l'ergatif, un cas déterminatif.

Et vice versa pour la diathèse subjective:

dbul-po-la 'cho-ba " nourrir le pauvre " il y a action de nourrir pour le pauvre. Ici le verbe étant en diathèse subjective, le patients doit recevoir la marque d'un cas déterminatif, en l'occurrence du datif (particule *ra*).

Or, ce type structural n'est guère limité au tibétain. A part le fait qu'il se retrouve de façon presque identique en plusieurs langues caucasiennes dont la parenté avec le tibétain me semble, malgré les suggestions de Bouda, complètement exclue, il apparaît dans de très nombreuses familles linguistiques sûrement non apparentées et n'appartenant pas à un même "*Sprachbund*." Il s'établit indépendamment, lorsque certaines conditions sont remplies. Parmi ces conditions, la plus importante est l'absence du *verbum finitum*. Le verbe fini, une catégorie grammaticale qui n'est guère aussi répandue comme on le pense, est basé sur le rapport asymétrique et non ambigu du sujet-objet. Mais sitôt que, même dans nos langues indo-européennes, nous exprimons l'action ou l'état par une forme nominale, l'ambiguïté apparaît: *Mutterliebe*, *amor patris* — dans les deux cas la valeur du complément n'est plus claire et il est nécessaire de préciser s'il s'agit du sujet ou de l'objet, de l'agens ou du patients. Dans les langues du système en question cette précision supplémentaire n'est pas nécessairement exprimée par l'ergatif. Les langues à l'ergatif ne présentent qu'un cas particulier d'un système plus vaste qui exprime ces nuances par la combinaison des diathèses verbales et des syntagmes ouverts et déterminatifs. L'exemple le plus caractéristique est fourni par l'esquimaux qui n'opère qu'avec trois cas exprimant les relations les plus générales: absolutif, déterminatif et déterminé. Si le verbe nominal est intransitif, il a la valeur subjective et son complément est l'agens. Tous les deux sont en absolutif:

nanoq tikiḡoq " l'ours vient " = " quant à l'ours, l'action de venir " — syntagme ouvert. Tout syntagme déterminatif est exprimé en esquimaux par le procédé suivant: l'élément déterminé est au cas déterminé, le complément déterminant est au déterminatif. Par ex. le rapport du génitif: *nanuḡ amia* " la peau de l'ours. "

On retrouve la construction identique lorsqu'il s'agit d'exprimer le rapport entre le verbe transitif objectif et son agens.

piniartup nanoq takuvâ "le chasseur voit l'ours" = "quant à l'ours (*nanoq* — absolutif), il y a le fait d'être vu par le chasseur" (le verbe nominal *takuvâ* est au cas déterminé, l'agens *piniartup* est au cas déterminatif).

Le temps ne me permet pas de donner plus de détails sur ce système et je renvoie au travaux lumineux du Professeur L. L. Hammerich dédiés à ce problème.¹ Je renonce également à la présentation de constructions très semblables et sûrement indépendantes en adigé et kabardi, mais je voudrais donner un petit exemple tiré des langues indonésiennes, dans lesquelles jusqu'à présent personne n'a pensé à chercher une structure semblable. Pourtant, à mon avis, cette interprétation explique la structure indonésienne bien mieux que les schémas appliqués jusqu'ici. Le malais p.ex. n'exprime le rapport de détermination que par des procédés tactiques. Le déterminant suit le déterminé (*orang utan* "homme de la forêt"). Son verbe est nominal et distingue plusieurs nuances d'aspect et de diathèse. Ainsi à côté de *tutup* "fermeture" nous avons *menutup* "action de fermer" et *di-tutup* "fait d'être fermé." Or, nous pouvons traduire la phrase "Ali ferme la porte" de deux façons différentes dans lesquelles la séquence des mots dépendra de la diathèse du verbe. Le syntagme déterminatif sera formé par le verbe et le complément qui le suit. Si la diathèse est subjective le complément suivant le verbe désignera le patients:

Ali menutup pintu "Ali ferme la porte" = "Quant à Ali, il y a fermeture de la porte".

Avec la diathèse objective la séquence est inverse:

pintu di-tutup Ali "la porte est fermée par Ali" = "quant à la porte, il y a fermeture par Ali."

Nous retrouvons le jeu déjà connu des oppositions entre le syntagme ouvert et le syntagme déterminatif. Cette structure est obscurcie en malais et surtout en javanais par la confusion des diathèses, par ce que le Professeur C. C. Berg appelle "averrechtse interferentie,"² mais je ne peux pas discuter maintenant ces détails.

Les structures générales des langues que je viens de présenter

¹ The Cases of Eskimo, *Internat Journal of American Linguistics*, vol. XVIII.

² *Indië's talenweelde en Indië's taalproblemen*, Batavia 1939.

sont très différentes. Pourtant les constructions concernant le verbe et ses compléments agens-patients coïncident de façon tellement précise que la ressemblance ne peut pas être fortuite. On sait que l'école de Marr s'est occupée spécialement de la structure ergative en y voyant la caractéristique d'une étape évolutive du langage, dépendant des conditions sociales et économiques des peuples qui parlaient ces langues. A certains égards cette explication coïncide avec la suggestion de M. Halliday que j'ai déjà mentionnée. A mon avis cette interprétation est indéfendable. Preuve en est que la construction en question peut apparaître dans les langues dont la structure générale est très différente où elle coexiste avec les constructions du type indo-européen ou sémitique. Ainsi nous la retrouvons en géorgien, mais seulement avec les formes verbales correspondant à l'aspect perfectif. Ici, c'est probablement une survivance qui s'est maintenue seulement là où cette construction était nécessaire. Plus intéressant est le cas des langues indoaryennes modernes, par ex. du hindi. La structure générale est ici indo-européenne, mais le passé accompli est exprimé par une construction correspondant exactement au cas que nous avons étudiés. Surtout la construction impersonnelle du type: *mai ne us laṛkī ko dekhā* = "a me illam puellam visum est" est exactement la même qu'en tibétain, en caucasien etc. Le verbe est central, l'agens est exprimé par l'ergatif (part. *ne*), le patients par le datif (part. *ko*.) Ce cas est intéressant, parce que la nouvelle construction s'est superposée au type structural indo-européen. La raison en est indubitablement le remplacement dans ces constructions du verbe fini par des participes, des formes nominales qui rendaient nécessaire la précision supplémentaire de l'agens et du patients et la perte du "sujet" grammatical. Un même problème s'est posé aux langues romanes ou germaniques (temps composés du verbe); mais on a évité la nécessité du changement de structure par l'emploi de deux auxiliaires *avoir* et *être* qui maintinrent le verbum finitum et assumèrent le rôle de la distinction des diathèses.

CONSTANTIN REGAMEY

The existence of linguistic affinity in the non-genetic sense is proved by the existence of at least one such group of languages, the Balkan group. (In the opinion of the late Dr.

Bachtin this is the only such group in Europe, though there may be a case for affinity to a lesser extent say between English and Celtic languages.) I do not intend to catalogue the Balkan features, or exhibit the various combinations of Balkan languages showing particular features. All this can be found in Sandfeld Jensen's "*Linguistique Balkanique*," if nowhere else. I wish merely to comment on some questions raised in recent discussion of the subject, such as in Prof. Nandriș's masterly exposition of the Rumanian language published in *Slavonic Review* No. 74, December 1951. In my printed contribution I sketch how the special modal use of original Perfect in certain Balkan languages has features too specific to be mere coincidence, at the same time as showing an extent (in the Balkan group and within each language) partly determined by previous formal resources. Now I shall discuss evidence to similar effect about one or two other of the "Balkan features."

The postposed article of Rumanian, Bulgarian and Albanian is also found in the Scandinavian languages as well as in Russian dialects. Is this evidence that its occurrence in these three languages does not constitute part of Balkan affinity? In an article in *Archivum Linguisticum* (Vol. III, pp. 23-37), "The suffixed Article in North Germanic," which appeared since the paper of Professor Nandriș to which I have referred was delivered, Mr. M. L. Samuels has shown that the cause of the Scandinavian construction involves the phonetic process of loss of pre-tonic syllables. It has been suggested (see Samuels, p. 27, n., referring to Pușcariu, *Zeitschrift für romanische Philologie*, LVII, p. 240 ff.) that a phonetic cause is also responsible for the different word-order in Rumanian and Western Latin (a question regarded by Prof. Jopson as crucial in this matter) namely "dynamic stress," "the increase of stress on the beginning of words." This does not, however, appear to be at all the same phonetic phenomenon as the Scandinavian loss of pre-tonic syllables (nor does Mr. Samuels commit himself to equating the conditions in the two cases). I therefore think the Scandinavian phenomenon can play no part in our deciding about the cause or causes of the Balkan one.

The formation of the future tense by "will," common to all the "pre-Turkish" Balkan languages, as we may term the

principal sub-division of the Balkan group in the widest sense, is said to be found also in Chinese as well as in English, showing "a general attitude of mind." Yes, but the attitude of mind seems to be of wider application in these Balkan languages than in English or Chinese, at any rate, since the English is restricted by the co-existence of the other most common Germanic future auxiliary "shall," and the Mandarin *iao* cannot, for example, be used in plain future sense in subordinate clauses like "I think he will come," in Mandarin *wo siang ta xui lai*, literally "can come." It has been postulated that the mediaeval and modern Greek future construction with $\theta\epsilon\lambda\omega$ etc. $v\grave{a} > \theta\grave{a}$ represents an attitude of mind connected with the ending of classic Greek civilization by the Hellenization of the eastern world. However this may be, it seems clear that mediaeval Greek retained sufficient cultural predominance to impose the construction upon the other "pre-Turkish" Balkan languages.

J. ELLIS

The root of the matter lies in bilingualism. It has therefore surprised me that no linguist has taken up this problem in a systematic way although bilingualism in operation may be studied only a few hours' travel from Paris or London. I am thinking of the Celtic speaking countries. I have drawn attention to this some thirty years ago but have not myself had the possibility of taking up the work. In the Celtic countries you can follow how the influence works. In Léon in Brittany the Breton stress and vocalic system is introduced into French:

arive = *arriver*

vêra = *terra*

Breton vowel qualities and sandhi rules are applied to French.

Morphological influence is also very striking, usually by translation of compound morphological forms, cf. Anglo-Irish *I am after doing it* = I have done it. In rare cases morphological elements are taken over directly. An example is the English plural *s* which has been added to some Welsh words in need of a plural form. Bilingualism has certainly been of great importance especially in the less developed societies, such as those to which Prof. Tovar has drawn attention. Examples from Australia are well known. In European

history there are many cases. We know that there have been bilingual groups in Ireland during the Viking period and there must have existed also a sort of *lingua franca*. In mediaeval Europe we must reckon with the influence of mediaeval Latin and how it was pronounced.

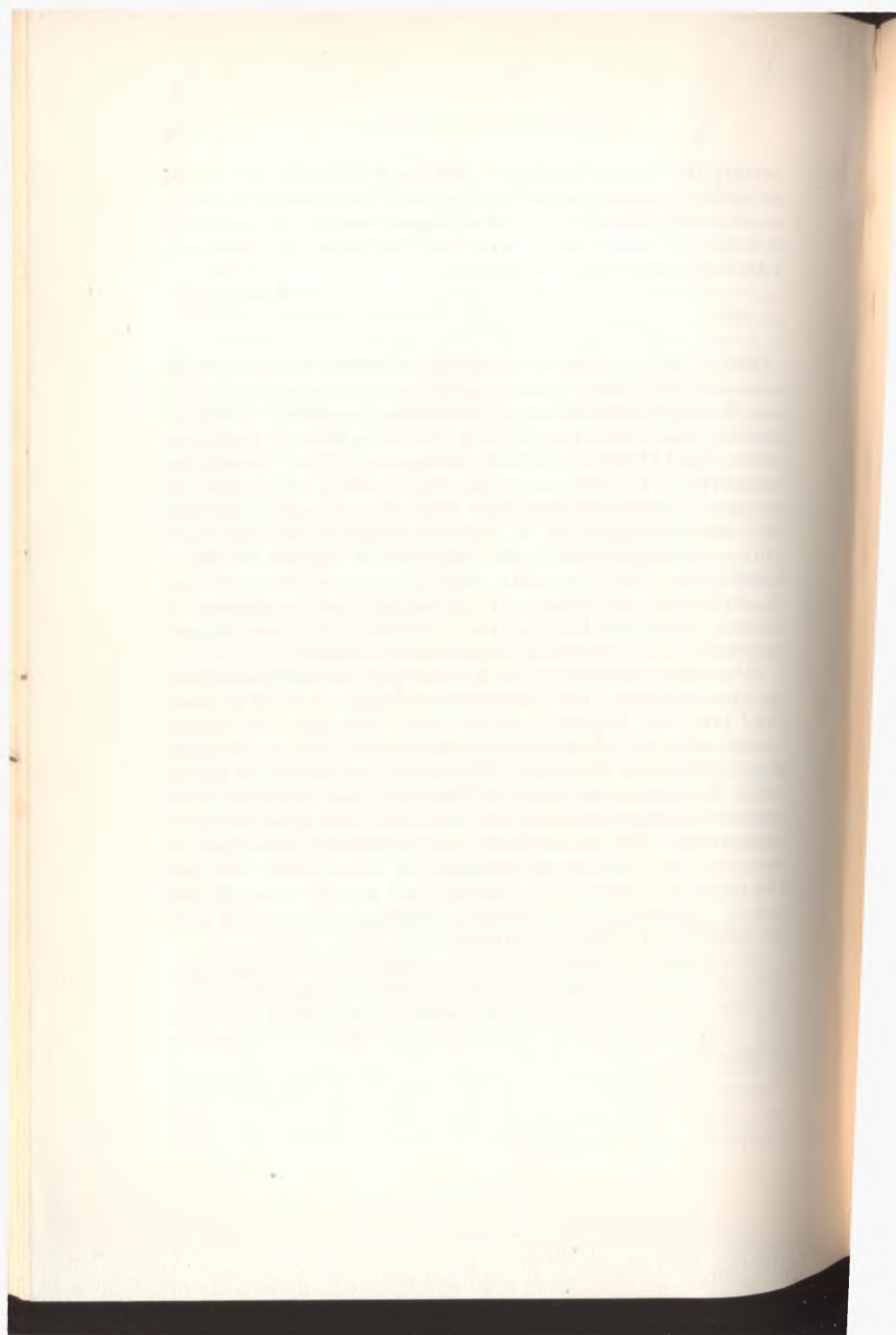
A. SOMMERFELT

May I stress again the necessity of trying to account for whatever similarity, phonological, morphological, or other, may be found on both sides of a linguistic boundary? I should like my final words here to be a plea for a form of linguistics which would transcend mere description. By "descriptive linguistics" I mean not only the practice of synchronic analysis, a practice currently favoured in many quarters, but also linguistics as it was conceived of by the neo-grammarians and is still practised by their modern followers. To this day many linguists, among both synchronicists and diachronicists, are reluctant to go beyond mere statements of directly observable facts such as "English *p* and *b* are distinct phonemes" or "French *u* corresponds to Latin *ū*."

Of course evolution can be *described* just as static conditions can be described. But describing evolution does not exhaust the task of linguists. Such early attempts at causal explanation as the substratum theory were apt to discredit all efforts in that direction. They were propounded at a time when the structural nature of language was generally overlooked, a circumstance that was bound to vitiate all explanatory endeavours. The substratum was presented as a *deus ex machina*, an autonomous principle of explanation, whereas the situation giving rise to a substratum action is nothing but one of a large variety of bilingual situations all of which may be submitted to direct observation.

Structural linguistics has, in the course of the last decades, devised new tools for the investigation of linguistic reality, and the time has come when we should try not only to describe the how's but also to determine the why's of linguistic phenomena.

A. MARTINET



SECTION C

THE COMPARATIVE PHILOLOGY OF THE INDO-EUROPEAN LANGUAGES

- (1) The laryngeal hypothesis and the theory of phonemes in
Indo-European
and
(2) Is there still a root-determinative problem in Indo-European?

(Combined meeting)

Chairman: PROFESSOR L. R. PALMER

Rapporteur: PROFESSOR J. WHATMOUGH

Recorders: DR. P. F. GANZ
MR. M. A. TANNER

For Preliminary Reports see pp. 135-145 above.

CHAPTER I

THE CONSTITUTION OF THE UNITED STATES OF AMERICA

OF THE CONSTITUTION OF THE UNITED STATES OF AMERICA, AND THE HISTORY OF ITS DEVELOPMENT, FROM THE FIRST CONVENTION TO THE PRESENT TIME.

BY

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THE UNIVERSITY OF CHICAGO PRESS, 1895.

In opening the discussion Professor Whatmough said that the laryngeal theory could not now be turned aside by mere denial. Scholars who rejected it were in the position of those who rejected relativity or modern physical theory. Not only the prediction of *H* by Møller, by de Saussure, and by Cuny, but also (ever since the confirmation of the prediction, furnished in the actual Hittite text), new and convincing explanations of old difficulties have strengthened the theory beyond dispute, e.g. the agreement of Lithuanian (') and Lettish (^) accentuation with the presence of *H*, as in Skt. *bhāvītum*, Lith. *būti*, Lett. *būt*, i.e. **bheweH-*, and Latin *plēnus*, Lith. *pilnas*, Lett. *pilns*, i.e. **peleH-*; or the recently stated rule (Hoenigswald *Language* 28, 1952, 182-185) that in the initial group *sHv-* (*v* = vowel), the laryngeal is lost (Lat. *anus*, Hitt. *hanas* but Skt. *sāna-*, Lat. *senex*, i.e. *hen-*: *sHen-* whence *sen-*, exactly like *τέγος* : *στέγος*).

Hence Lith. *širdi*, Lett. *siŗdi* (acc. sg.) and Greek *κηρ* (where the long vowel is significant) point to

**keHrd-*

But in *-Hrd-* which element is the "determinative" so-called?

We have, in fact, to deal not simply with Benveniste's formula

CvC(+vC)

but also with a very common pattern (as a special variety of it)

CvSHC

(consonant, vowel, sonant, laryngeal, consonant) e.g. in Latin *genitus*, which suggests a different basis of analysis in such groups as **bheiydh-*, **pleHdh-*, **steHp-* (*πείθω*, *πλήθω*, *στήπαται*), viz. a basis of what phonematic distribution and function is permitted, namely in IE consonant clusters (*-iydh-*, *-Hdh-*, *-Hp-*), instead of the old-fashioned assumption of a meaningless "determinative" (*-dh-*, *-p-*). In effect, this would remove both glides and diphthongs from the IE phonematic (not phonetic) pattern, and so accord with modern theory, and at the same time reduce the total number of IE phonemes to 32.

J. WHATMOUGH

The Laryngeal Theory, as Professor Whatmough has stated, has passed the stage of hypothesis and must be accepted or rejected in principle as it stands. From my personal point of view it ranks as metalinguistic and thus stands on the border-line between philosophy and science. Professor Whatmough and other supporters of the theory feel that the time has come for a radical reconstruction or supersession of the theory of the neo-grammarians in the light of Laryngeal discoveries. The argument for this extension of the Laryngeal Theory must ultimately rest upon the possibility of relating it to existing phonemic material. If it is said that the processes described are incapable of repetition or of being paralleled, the foundation of philological research is undermined by a reversion to *a priorism*.

The phenomena investigated in connection with the Laryngeal Theory, variations of vowel-length and vowel-quality, vowel and consonant coloration, consonant aspiration and the like do, however, appear in living languages. Laryngeal influences are not as a rule suspected and other explanations are, in most cases, forthcoming. The time has now come to test the theory, so to speak, in the field. Dr. Hammerich has found an example in Hindustānī and there are problems of literary use such as the Persian doublets *la'l / lāl* ruby, which appear to be explicable by a laryngeal theory. The *e*-coloration of *a* by *h* in several Neo-Indian languages, e.g. *rahnā* = *rehnā* in Hindi corresponding to Gujarātī *rahevum*, *rehavum* to remain, deserves further investigation. The optional spelling of initial *a* as *a* or *ā* in Marāṭhī, the fluctuations of quantity and quality of *i* and *u* in Gujarātī, the compensatory lengthening of short vowels on the disappearance of one of two conjuncts also merit enquiry and Pischel's account of Prakrit, which is situated in the middle of a diachronic sequence of languages would repay a fresh examination.

It is unlikely that the Gujarātī alternance of *s* in the standard literary dialect with *h* in the domestic dialect would yield any results from the laryngeal point of view. It might indeed be found to be a factor in the abandonment of the laryngeal explanation of the passage of Indo-European **s* to the Greek rough breathing and the Persian *h*.

It is doubtful whether the Laryngeal Theory can go much

further in the direction of prediction than the theory of the neo-grammarians, but if prediction is possible, it is likely to be the most fruitfully exercised upon new and abundant material.

ALFRED MASTER

In der Diskussion bemerkt Prof. Dr. ERNST FRAENKEL (Hamburg), dass er keineswegs ein prinzipieller Gegner der Laryngalthorie ist. Nur muss bei Etymologieen von Wörtern, die ehemaligen Laryngal aufweisen, stets mit exakter philologischer Methode gearbeitet werden. Man muss ferner bei Ansatz von Wurzeldeterminativen sehr vorsichtig sein. Sonst verliert man leicht den Boden unter den Füßen. Redner weist auch darauf hin, dass es ausser den Wurzeln mit normalem *e*-Vokalismus solche mit *a*-Vokal gibt. Schon de Saussure hat auf Fälle wie lat. *caecus*, got. *haihs* aufmerksam gemacht, in denen der *a*-Vokalismus mit der Bedeutung körperlicher und geistiger Defekte Hand in Hand geht. Aber es gibt noch andere Beispiele; vgl. die Wurzeln **bhau-* griech. *φάε* "leuchtete auf," *φάος* "Licht", die Specht mit dem Verbum *existentiae* ai. *bhāvati*, *bhūti-*, griech. *ἔφυν* etc. in Zusammenhang bringt, ferner griech. *ναέτωρ* = ποταμός, *νάμα* < **náma* "Quell", ai. *snauti* fließt, tröpfelt usw.

(E. FRAENKEL)

I agree completely with our rapporteur Professor Whatmough in demanding a structural view when dealing with the question of laryngeals in Indo-European, but I add that we should not omit the phonetic point of view. That is to say that we should not propose any theory which cannot be supported with knowledge of how the sounds treated behave in living languages. On the other hand, there is no reason to believe that the ancient stages of Indo-European should only present such sounds as are found later on in the IE languages: the later and known history of IE languages clearly shows how greatly sound patterns can change in course of time.

I should like to clear one point which I brought forward in my answer to the question. It is very likely, as shown in a recent paper by our old master Holger Pedersen, that the

enigmatic lack or scarcety of *b* in IE points to a pre-IE sound-shift, since not *b*, but *p* is a sound which is very apt to disappear. Taking into account that different scholars have proposed to explain the IE *bh*, *dh*, *gh* as originating from previous P T K (the emphatic, i.e. laryngealized occlusives, well-known from the Semitic languages), I would propose the following pattern of pre-IE occlusives:

pre-IE P T K b d g (p) t k (with *p* having disappeared at least in certain positions, e.g. initially).

Hence: IE P T K p t k (b) d g (the P T K perhaps being neutral in regard to the opposition voiced/unvoiced).

And then in (1) Hittite, with its characteristic delaryngealization — the substitution of the weaker laryngeal articulation by the stronger velar articulation making just the preservation of the phoneme *H* possible in Hittite — we have fusion of P T K with the other occlusives, so that the Hittite pattern is: p t k b d g

but (2) in the other IE languages (which I like to call the Centum-Satem languages), we have: bh dh gh (b) d g p t k.

To establish this, it is not necessary to take into account the fact that IE had not one, but three series of velars (in my opinion the efforts to reduce these 3 series have not been successful), but in another aspect these are very important. IE had *k^h* *k^a* *k^u*, but Centum has only *k* and *k^u*, Satem has only *k^h* and *k*. Hittite, on the other side, reflects all three series, *k^h* becoming *h* or *k* (under certain conditions), *k^a* being preserved as *k*, and *k^u* being preserved as *k^w* (Cf. my paper Laryngeal before Sonant, *Det Kgl. Danske Videnskabernes Selskabs Historisk-Filologiske Meddelelser*, XXXI, 3, Copenhagen 1948, p. 56). When this is taken together with the many other extraordinary singularities of Hittite, I venture the challenge: those who will contend that Hittite is a Centum language, have to prove it. It is not the other way round, that those who contest Hittite being a Centum language, have to prove their negative assertion.

Mr. Crossland has rightly pointed to the laryngeal before *w* being treated differently in Greek and in Sanskrit, but the structural point of view then demands researches into the treatment of the laryngeal before all sonants.

I was glad to hear that Mr. Master, who really knows Hindustani and the other New Indian languages, could approve

of my short allusion to the very interesting laryngealized vowels of Hindustani, which in my mind can help us greatly towards a better understanding of what the vocalic forms of the IE laryngeal may have been like. I do not know, if we have, in living languages, any examples of different laryngeals altering the quality of the following vowel into *e*, *a*, or *o*. Until such evidence is produced, I should recommend scepticism towards such a theory. If we do not believe in such a theory, there is no reason for assuming more than one laryngeal consonant in IE (how it may have been in pre-IE I do not know). But there are good reasons to believe that IE had three vocalic laryngeals, partly living on in the Centum-Satem-languages, but of course not in the delaryngealizing Hittite language.

LOUIS L. HAMMERICH

Comme l'a mis en valeur le rapporteur, les récents développements en matière de phonologie indo-européenne et notamment la théorie des laryngales, tendent à réduire le nombre des phonèmes qu'on postule pour la langue commune. Des trois ordres de "gutturales", deux tout au plus subsistent; des quatre séries d'occlusives, celle des sourdes aspirées a vécu; à celle des sonores on dénie une labiale; on élimine d'une façon ou d'une autre le *ǵ* et le *ǵ* de Brugmann. En supposant trois laryngales, on maintient l'effectif des sonantes aux six brèves traditionnelles et l'on réduit à un seul celui des phonèmes vocaliques. La récente tentative de Borgstrøm aboutit même à supprimer tout phonème vocalique puisque la voyelle n'apparaît plus que comme l'accompagnement automatique de chaque consonne. Elle élimine également tout trait prosodique pertinent. Le total des phonèmes atteint donc à 20.

Ce total est bas. Mais l'espagnol d'Amérique n'a guère plus d'unités distinctives, et la plupart des parlers polynésiens en ont moins. Ce qui pourrait faire douter de la validité de ce total c'est le fait que, ne comportant que des consonnes, un tel système ne connaîtrait que 20 syllabes différentes. Les dissyllabes possibles dans la reconstruction de Borgstrøm qui correspondent aux racines de Benveniste n'atteindraient qu'un total de 400 qu'il faudrait réduire à un peu plus de 300

si l'on tient compte du fait que certains phonèmes s'excluent mutuellement (par. ex. on ne connaît pas de succession homomorphématique des types *tVt tVgh*). Or les langues directement observables n'utilisent effectivement en général qu'un pourcentage assez restreint des combinaisons phonologiques théoriquement possibles. A supposer que l'indo-européen d'une certaine époque ait utilisé même 30% de ces combinaisons, cela ne ferait guère plus qu'une centaine de racines distinctes par la forme. Notons qu'en espagnol le nombre de combinaisons dissyllabiques possibles doit dépasser 4000 simplement pour le type CVCV qui n'est qu'un des types possibles à côté de CCVCV, CVCCV, etc. Un de mes étudiants, Aert Kuipers, a présenté une théorie phonologique du Tchérkesse où il réduit l'inventaire vocalique à un phonème. Mais, en ce faisant, il enfle démesurément l'inventaire des consonnes qui comporte désormais de 70 à 80 unités.

De tout ceci on ne doit pas conclure qu'on se trompe lorsqu'on élimine du tableau des phonèmes indo-européens bon nombre de ceux qu'y avaient placés nos prédécesseurs. Il y a dans tous les cas d'excellentes raisons pour procéder à ces retranchements. On ne devra pas non plus conclure que Benveniste a tort de postuler des racines réduites à deux consonnes. Voici ce que suggèrent à mon sens les considérations qui précèdent : à la date où la racine de Benveniste peut avoir été une réalité synchronique et plus encore à celle où l'apophonie n'avait pas encore joué, le système phonologique indo-européen devait connaître des distinctions phonologiques éliminées par la suite. On pensera par exemple aux phonèmes d'articulation complexe posés par Benveniste en remplacement des interdentaires de Brugmann. Il y a eu peut-être deux ordres différents de tels phonèmes, les uns sans, les autres avec labialisation. Des considérations d'équilibre structural pourraient conduire à présenter des suppositions, comme celle d'un ordre complet de trois phonèmes sifflants, qui resteraient toutes gratuites tant qu'on n'en pourrait trouver des traces dans les langues historiquement attestées. En pratique on retiendra la conclusion qu'on ne peut connaître le système des unités distinctives de l'indo-européen commun aux différentes étapes de son développement, même sous une forme algébrique et non phonétique.

En ce qui concerne les laryngales, il est certainement d'une

bonne méthode d'essayer de faire coïncider le nombre de celles qu'on postule avec les besoins réels de la comparaison et de s'efforcer de réduire ce nombre. Mais ce qui est hautement recommandable dans le cadre d'une activité strictement comparative, ne l'est plus du tout lorsqu'on vise à une reconstruction, c'est-à-dire à la restitution aussi fidèle que possible d'une réalité passée. Tant que l'on emploie des signes comme $\alpha_1, \alpha_2, \alpha_3$, qui résument des correspondances, on doit viser à l'économie. Dès que l'on utilise des symboles phonétiques comme γ , etc., se pose la question de la vraisemblance phonétique et phonologique.

ANDRÉ MARTINET

To economize on time, I should like to address myself first to question C (2) and then to spend what time remains on question C (1).

There most certainly is a root-determinative problem in Indo-European, but I should maintain that it is not essentially different from other pre-IE problems: it is merely a problem of the chronological depth and relationship of old-layer suffixes. "Root-determinative" is a misleading term because it gives a unit-name to something without structural status as such.

In all known languages, living or dead, at a given time there are to be found affixes (or members of a class of morpheme sequences) that are more — or less — productive, more — or less — frequent, more — or less — fundamental to the pattern of the class to which they belong. Each affix has at a given time a history (what earlier structure-points did it occupy?) and a descriptive status (what does it now do?); the answers to these two questions may have no apparent connexion — it is quite possible for the accident of phonological fusion to produce a new working element. (Vrddhi in Sanskrit is an easy illustration).

We should therefore not be at all surprised to find in IE a trail of such suffix-classes in descending orders of productivity, ranging perhaps to the vanishing point. Because, however, we are reconstructing over a great time span (at least 4,000-4,500 years, say) we must beware of our myopia. It is easy to confuse chronological layers at such a distance, and as one recedes in time such chronological differences tend to take the

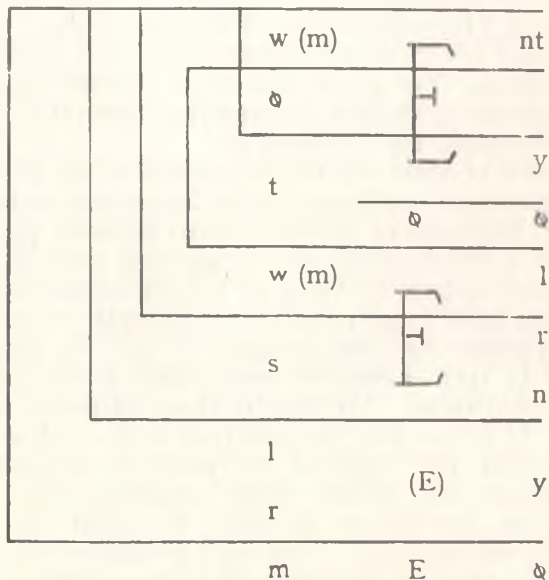
appearance of a uniform grey blur. We must also remember that as the time-lapse is increased the total number of items retained is diminished, so that we are looking at a smaller, and statistically less satisfactory, sample of the total roster of that time, the segment that for a variety of reasons happened to be preserved, and not always with its original structural value. There must be a theoretical point at which we can no longer recover enough of the pattern to recognize it. We must never — and this holds for phonology just as well — misinterpret our necessarily smaller sample at greater time-depths as meaning that the language had a smaller total stock of such structure points, or of any structure points, than it does now. Therein lies the error of recurring reconstructions of alleged one-vowel systems of great simplicity, and the like; no one denies the possibility of such a system; but, if true, it must be arrived at by means other than a simple addition of the features that have been preserved over great time-spans.

I should like to suggest here another, and perhaps fruitful, approach to the problem of old-layer suffixes: phonological correlations with classes of morphological elements. I do not refer to any possible "phonaesthetic" considerations. Greenberg has very interestingly dealt (*Word* 6.162-81. 1950) with restrictions of occurrence between certain classes of phonemes and certain morphemes (verb bases) in Semitic and Egyptian. Now if one inspects the productive nominal suffixes of Hittite listed by Sturtevant (*Comparative Grammar*², New Haven, 1951, 68-81), there seems to be a near-correlation between phoneme-class and occurrence/non-occurrence in suffixes. The following schematic table, expressed in IE (IH) reconstructions, will serve to show compactly the occurring suffixes in terms of their component phoneme sequences. To read the table, start at the left and proceed to the right using every adjacent possibility, within or across lines, but without entering a smaller box.

It will be noted that *s occurs with a limited distribution and that of the stops only dentals occur; the rest of the non-syllabics are all sonants. To what extent such phonemic restrictions affect various classes of affixes and are of significance for the status and history of earlier formations might be a subject of useful enquiry.

I now turn to question C1, on which I should like merely to

make a few general remarks in the time at my disposal. There can be no doubt that the laryngeals are here to stay, and no present-day accounts of IE phonology can pretend to adequacy without taking them into account. By that I mean a complete reinterpretation of the Brugmann-Meillet roster of sounds,



(E = gradational vowel. ø = zero.)

and not merely adding the phonemes on as an appendage (see further on this W. Lehmann, *Proto-Indo-European Phonology*, Austin (Texas) 1952, p. 5 and passim). One good reason for reconstructing laryngeals is the inescapable fact that two such phonemes are attested in the Anatolian languages and can be accounted for in no methodologically acceptable way other than by reconstructing separate antecedent phonemes for them.

But even without Hittite the assumption of certain phonemes — it matters little what we might have called them — would

have proven inevitable in the long run. Language is nothing without patterning. And the pattern alone, the symmetries, the congruences, and the economies of statement thus introduced, would have forced acceptance of the basic scheme first seen in outline by the acute and penetrating mind of de Saussure so many years ago.

The question then arises: How many laryngeals? In a note in the current *Transactions of the Philological Society* (1952) I have expressed my view that Hittite gives direct evidence for three laryngeals. For given dialects of IE the number of laryngeals present as distinct phonemes at a given time depends on the chronological layer referred to.¹

A good deal of effort has been expended in the attempt to argue the presence, or absence, of the laryngeals on the basis of phonetic likelihood or of their alleged phonetic properties. Although it is of advantage for tracing their subsequent fate in the several dialects to know all we can about them, and hence to gain some rough notion of the phonetic value of their major allophones, for their recognition and the systematic description of their structural status there is no "phonetic problem" whatsoever. We identify these entities by patterning alone. It is true that the structural units that we set up are more than just captions for observed cross-language correspondences, as Meillet would sometimes have them. That they are abstractions is beside the point; so are the phonemes of any language. You can't pronounce a phoneme; you can only utter an allophone of a given phoneme. But however "real" and valid we may regard our reconstructions to be, we can never put on our IE caps and claim to talk some "real" snatches of IE.

ERIC P. HAMP

(a) It is clear that Hittite had at least one phoneme written with the cuneiform signs which were used in Mesopotamia to represent the velar or post-velar fricative of Akkadian, and that at least in the great majority of cases any Hittite sound

¹ When I made these remarks orally I alluded to the possibility that there were either three or four laryngeals in the parent language. Since then I have identified in Albanian the fourth laryngeal (Sturtevant's /h/), that is, one additional to the three I refer to above in Hittite; I am now preparing the clear evidence for this discovery for publication. There is consequently a clear answer now to the present question: Since no one has claimed the need for five or more, there were indubitably four laryngeals in the parent language.

written with such signs which occurs in a word of IE origin represents an IE phoneme. The conclusion reached by E. Sapir and the late Professor E. H. Sturtevant that Hittite had two phonemes written with the *h*-signs is accepted by most hittitologists, but method demands that it should be considered provisional, since no full statistical analysis of the orthographies of Hittite words which contain medial *-h(h)-* has been published. Since *-h-* is rare after *-a-* in Hittite words, while *-hh-* occurs after *-e-* only when it represents the first consonant of a verbal inflexion, (e.g. in *te-eh-hi*, "I place"), it seems reasonable to equate Hittite *-h-* and *-hh-* with the first and second "laryngeals" of the system proposed by J. Kuryłowicz. (See *Transactions of the Philological Society* 1951, pp. 92-5, 99-109).

It seems unlikely that IE had more than two "laryngeals." The conclusion that it had two whose disappearance in IE languages other than Hittite and closely-related languages of Asia Minor was accompanied by no change in the quality of adjacent vowels rests on the inadequately-supported theory that all IE words began with a consonant, a corollary of which is that a "laryngeal" had disappeared or was not represented graphically at the beginning of Hittite words of IE origin the cuneiform orthographies of which begin with a sign for a vowel or for a syllable beginning with a vowel. Even if this theory is accepted, the phonology of Hittite words of IE origin which contain *h(h)* can be explained without the assumption of two "laryngeals" with the characteristics of *H₁* (Kuryłowicz' = *ɶ₁*), if it is assumed that *e* did not change in quality when adjacent to *H₁* in pre-Hittite, and that Hittite *-h-* and *-hh-* represent allophones of *H₁* which originally occurred next to front and back vowels respectively, and which may have acquired phonemic status before the period in which the Hittite texts were written. This explanation involves assuming that the *a*-vocalism of the singular forms of Hittite *nahmi*, "to fear," is analogical, based on that of the plural forms, and this seems improbable, although the original distribution of the different grades of gradation-vocalism undoubtedly did undergo considerable disturbance in Hittite. Pairs of semantically equivalent forms showing original and analogical vocalism are certainly to be found in the corpus of Hittite texts. (Cf. *op. cit.*, pp. 107-8 and ff.).

(b) Three comments must be made on Professor Hammerich's communication in the *Preliminary Reports* (pp. 143-5).

The conclusion that one or both of the IE sound-classes represented by Hittite *h(h)* were laryngeal rather than velar seems unjustified at present. It would be reasonable if it had been demonstrated that the change [e] > [a] occurs much more frequently in living languages in sound-groups containing a laryngeal than in groups containing a velar.

The phrase "Hittite *p, t, k*" is ambiguous. It is certain that, whatever orders of stops existed in Hittite in the historical period, they were not normally distinguished in writing by the use of different series of signs, as stops of the different orders of Akkadian were in texts written in Mesopotamia in the second millennium B.C. A widely-held opinion is that corresponding Hittite stops of different orders were normally distinguished by the use of single or double orthography when they occurred in intervocalic position, that they were sporadically so distinguished when in other medial positions, and that they were not differentiated when initial or final. Professor Hammerich should have indicated whether he accepts this opinion or not, and whether, therefore, his "Hittite *p,*" for example, means "the Hittite labial stop normally written double in intervocalic position, etc." (Cf. *op. cit.*, pp. 125 ff.).

If he accepts the theory, a generally-accepted equation, Hittite *ne-pi-iš* "sky": Skt. *nabhas-*, is difficult to reconcile with his conclusion that IE *bh, dh, gh* > Hittite "*p, t, k.*"

R. A. CROSSLAND

LARYNGALES INDO-EUROPÉENNES EN DEHORS DU HITTITE

Une des découvertes les plus sensationnelles dans le domaine de la linguistique indo-européenne est certes la parenté du hittite avec notre famille de langues et spécialement la présence dans cet antique idiome de trois sortes de laryngales.

L'existence insoupçonnée de ces phénomènes rend compte de divers faits dans le développement du proto-indo-européen. Malgré cela, même par induction, on n'eût pas restitué ces aspirées dans la langue-mère si on ne les avait pas vues écrites

dans les textes hittites. Depuis lors, Pedersen¹ les a observées en lycien et récemment, Palomé² en a trouvé d'intéressantes traces en arménien.

Si osée que soit notre affirmation, nous croyons pouvoir prouver qu'une autre langue encore les avait conservées, tout au moins à l'initiale.

Notre prétention engendra d'autant plus de scepticisme que l'idiome en question n'a pas jusqu'ici été admis dans la liste des langues de notre famille.

Il s'agit de l'étrusque pour lequel dans une communication lue au congrès des Orientalistes à Istambul en 1951, nous avons revendiqué une origine proto-aryenne en nous basant sur un nombre imposant de ressemblances grammaticales et lexicologiques. Nous avons depuis lors continué nos recherches et, grâce à la longue liste de mots étrusques publiée récemment par Stoltenberg, augmenté considérablement le nombre de nos correspondances et constitué un tableau des correspondances phonétiques entre la langue des Tyrrhéniens et le plus ancien vocabulaire de l'indo-européen. La démonstration dans la forme complète sera publiée en novembre dans un numéro de l'*Antiquité Classique*.

Du reste, le problème dont nous parlons a changé d'aspect depuis qu'il est apparu que le lycien et le lydien sont proto-indo-européens et surtout depuis que dans un ouvrage récent de notre collègue et ancien élève, le Prof. van Windekens, il a été prouvé qu'à l'époque pré-hellénique un idiome indo-européen, par convention appelé "pélasge," avait existé dans cette partie de la Méditerranée.

Or, toutes ces langues, dans la mesure où nous les connaissons montrent de frappantes ressemblances avec l'étrusque: génitifs en sifflante, locatifs avec *thi* ou *di*, accusatifs en *an* ou *ani*, copule enclitique: *ke*, parfait en *ke*, divers pronoms en commun, etc.

Il faut donc reprendre la question et tâcher de procéder systématiquement. Quand on recherche le genre de transformations phonétiques que l'étrusque a vraisemblablement infligées à l'indo-européen primitif, il est naturel de se baser sur les transformations opérées par l'idiome tyrrhénien dans les mots qu'il a empruntés au grec ou au latin.

¹ *Lykisch und Hettitisch*, Copenhague, 1945.

² Réflexes de laryngale en arménien. (*Mél. H. Grégoire II*, 539 sqq.).

* On se rend compte alors de ce que

1° les occlusives sourdes se transforment en aspirantes

2° les occlusives sonores se transforment en occlusives sourdes

3° les occlusives aspirées sonores se transforment en sourdes ordinaires

4° consonne devant consonne s'efface

5° *w* entre voyelles se durcit en *b* ou *p*, etc.

(transformations assez semblables à celles du pélasgique)

Un fort accent abrège les mots, tandis que l'adjonction de suffixes abondants tend à les allonger.

En appliquant aux éléments réellement étrusques les mêmes lois, nous avons eu la surprise de constater que 8/10 au moins des mots présentés par les étruscologues (sans préoccupation de parenté) s'expliquent par l'indo-européen sans recourir à des rapprochements forcés.

Nous espérons que notre démonstration sera convaincante mais dès à présent, il est établi que la parenté étrusco-indo-européenne est devenue suffisamment probable pour que l'on tienne compte du fait fort intéressant sur lequel nous devions en ce jour attirer l'attention.

L'application des lois phonétiques indiquées ci-dessus nous avait, comme nous l'avons dit, procuré la surprise de pouvoir expliquer un très grand nombre de mots étrusques par les racines indo-européennes.

Une autre surprise nous attendait. Alors que notre travail avait donc été curieusement aisé pour les voyelles ou pour des occlusives, nous n'arrivions pas à trouver de parenté pour les termes ayant *h* comme initiale. Nous étions persuadé que cet *h* venait d'une gutturale mais cette hypothèse ne se vérifiait pas.

Nous fûmes alors frappé par le fait que étr. *hanth* signifiant "précédent" ressemblait étonnamment à hitt. *hant*, "à la tête, premier", lequel offrait précisément aussi en lycien avec une *h* dans *χ̣ntoweta* "commandant." Nous avons alors pris en main la liste des mots hittites commençant par *h* telle qu'elle est donnée dans le *Hettitisches Elementarbuch* de Joh. Friedrich et nous ne fûmes pas peu étonné de constater que 9/10 de ces mots pouvaient sans difficulté être rapprochés de mots étrusques ayant environ la même signification.

Même si l'une ou l'autre de ces identifications venaient à

être mise en doute ultérieurement, il n'en reste pas moins que la liste en est éloquente et que l'on ne peut parler ici de hasard.

Ceci est d'autant plus vrai qu'un troisième étonnement nous attendait. Les difficultés que nous avions éprouvées à propos de l'*h* se renouvelèrent concernant le *z* étrusque. Croyant d'abord a priori que l'étrusque serait comme le lydien une langue du type dit: *satem* nous pensions que ce *z* correspondrait comme en thrace (*Semele* = russ. *zemplja*) à l'i.e. **ghdhom* à une gutturale, mais ici encore notre attente fut trompée. Nous nous rappelons alors que l'étrusque transformait *Diomedes* en *Ziomedes* et nous nous demandâmes si *z* ne correspondrait pas peut-être à une dentale suivie de *y*.

Or cette hypothèse fut heureuse car elle nous permet de voir dans étr. *zipana* "déesse," un indo-eu. **dyeu-ana*, puisque *w* intervocalique devient en étrusque *p* (exemple étr. *kepen* [= *kew-eno*] pour i.e. **kewi* — "prêtre," d'où sanscr. *kavi-*, lydien: *kavēs*, etc. étr. *ape*, i.e. **awo*, lat. *avus*.)

De même étr. *zin* "forme, objet travaillé" put être mis en relation avec i.e. **dhya* — "être habile" (sanscr. *dhīra* "artiste").

L'étrusque *zikh* "écrire" put être ramené à cette même racine et partant devenir parent du grec *sēma* "signe."

Il en fut de même de l'étr. *zil*, *zilāt* (avec le suffixe étrusque *l*) désignant un haut magistrat, une sorte de consul, qui s'interprète donc comme "surveillant" (comp. le *caveant consules*).

Il n'était pas défendu dès lors de se demander si dentale + *w* n'aboutirait pas aussi à une assibilation (comme c'est le cas pour *tw* en grec).

Or, l'étr. *zeri* "purification" put dès lors être hypothétiquement ramené à i.e. **dhwes-ro*, ce que en faisait un parent proche du lat. *februa* (**dhwes-r-wā*) et de même l'étrusque *zus* "boeuf" devint parent du lat. *bestia* (**dwes-dhyā*).

L'étr. *zūk* "faire une offrande" et l'étr. *zakh* "faire une libation" purent être apparentés à gr. *thuēlē* comme venant tous de i.e. **dheu* + suffixe.

étr. *zen* "mort" s'identifiait avec gr. *thanein* de rac. **dhwen*.

étr. *zir* "mort" se dérivait aisément d'i.e. **dhwei* "mourir."

Qui plus est, l'étr. *za*, *zal* "deux" qui semblait défier toute comparaison avec *duo*, devenait, au contraire de la sorte le résultat phonétique attendu.

Cette dernière étymologie, faut-il le dire, est de la plus grande importance quant aux rapports entre les nombres étrusques et les nôtres.

Une quatrième surprise fut, enfin, pour nous de constater qu'ici encore l'étrusque avait un système phonétique semblable à celui du hittite.

hitt. *zik* "toi" remonte à *twe-ge*.

hitt. *zila* "oracle" et *zašri* "rêve" se ramènent à *dhyâ* "avoir une vision".

hitt. *zah* "frapper" et *zahhai* "combat" sont parents de i.e. **twei* "frapper," "tuer".

hitt. *zanu* "cuire" serait par *dhw-an-u* de rac. **dheu* "faire fumer".

hitt. *zana* "jour" remonterait à l'i.e. **dyano-* (russ. *děň'*, lat. *nun-dinae*).

hitt. *zappenai* "dégoutter" peut être parent de **dheu* "couler" (sans. *dhavati*).

hitt. *zenna* "finir" serait parent de i.e. **dhwen* "s'éteindre, mourir."

Ce parallélisme éclaire donc l'origine obscure jusqu'ici de *z* en hittite et nous procure une correspondance de plus entre l'étrusque et cette langue antique.

Cette coïncidence et surtout celle de l'*h* initiale rend l'explication par le hasard plus qu'improbable. La seule objection serait que l'étrusque ne puisse a priori et en aucun cas être indo-européen, mais précisément ces rapprochements et les très nombreux autres que nous avons énumérés dans nos tableaux prouvent le contraire et l'histoire étonnante et imprévue de l'*h* et du *z* apportent une confirmation inattendue et impressionnante à notre thèse.

Certes, il s'agit ici d'un travail de pionnier. Il faudra identifier encore beaucoup de mots étrusques et examiner en détail nos rapprochements. Il faudra aussi tâcher d'expliquer comment à une si grande distance l'un de l'autre, les deux plus archaïques idiomes de l'indo-européen ont conservé tant de similitudes.

Il n'en reste pas moins que nous sommes confronté avec une problème de grande portée qu'on ne peut plus renoncer à examiner.

h étrusque et *h* hittite:

étrusque	hittite
<i>han</i> précéder	<i>henne</i> conduire
<i>hanu</i> guide	<i>hanna</i> conduire
<i>her</i> dominer	<i>har</i> tenir
<i>hur</i> tuer une victime	<i>hur</i> tuer d'un coup
<i>her-m</i> maîtriser	<i>har-k</i> posséder
<i>hur</i> tuer	<i>hur-kel</i> violence
<i>hamphe</i> matin	<i>hamp-šant</i> printemps
<i>hils</i> propriété, bien	<i>hela</i> terre, domaine
<i>het</i> mort	<i>hat</i> dessécher
<i>hal</i> offrir	<i>hališšiya</i> dédier
<i>hus</i> enfant	<i>haššatar</i> progéniture
<i>hup-nina</i> tombe	<i>hupuvai</i> récipient (?)
<i>hukh</i> (titre de prêtres)	<i>huek</i> exorciser
<i>hil-kwa</i> temple	<i>hil-aš</i> portique (?)
<i>hat</i> vouloir	<i>hat-reššar</i> commander

PHONÉTIQUE DE L'ÉTRUSQUE

étr.

A. *Occlusive sourde thak* toit: i.e. (s) *teg* — lat. *tego*

aspirée ou	<i>nekh</i> mourir: i.e. <i>nek</i> — lat. <i>necare</i> , <i>nex</i>
spirante	<i>nefs</i> petit-fils: i.e. <i>nepot</i> — lat. <i>nepotem</i>
	<i>thut</i> communauté: i.e. <i>touta</i> — lat. <i>totus</i>
étr. lat. <i>fenestra</i> :	i.e. <i>penth-</i> gr. <i>pontos</i> sans. <i>panthan-</i>

B. *Occlusive sonore*

occlusive sourde

<i>tin-si, tinia</i> Jour, Zeus	: i.e. <i>din</i> — lat. <i>nun-dinae</i> , russ. <i>děn'</i>
<i>tiv</i> lune	: i.e. <i>diw</i> — gr. <i>Pan-di(w)a</i> , lat. <i>Diana</i>
<i>tur</i> donner	: i.e. <i>dō</i> + <i>ro</i> gr. <i>dōron</i>
<i>tap-ra</i> offrande	: i.e. <i>dâp</i> — lat. <i>daps</i>
<i>celi</i> froid	: i.e. <i>gel</i> — lat. <i>gelu</i>
<i>tat</i> mesurer	: i.e. <i>dâ(i)</i> sans. <i>dâti</i> , <i>dâyate</i>
<i>rak</i> clair	: i.e. <i>areg</i> gr. <i>argos</i>

C. *Aspirée sonore*

occlusive sourde

<i>kav-in</i> libation	: i.e. <i>gheu</i> gr. <i>kheô</i> , <i>khoê</i>
<i>lut-ni</i> libre	: i.e. (e) <i>leudh</i> gr. <i>eleutheros</i> , lat. <i>liber</i>
<i>tek</i> faire	: i.e. <i>dhê</i> + <i>k</i> gr. <i>tithêmi</i> ; lat. <i>facio</i>
<i>tesin</i> loi	: i.e. <i>dhê</i> gr. <i>teth-mo</i>
<i>itus</i> ides	: i.e. <i>aidh</i> gr. <i>aithra</i> "ciel clair"
<i>tu-l</i> verser	: i.e. <i>dheu</i> "couler" – sans. <i>dhavati</i>
<i>tu-r</i> fumer	: i.e. <i>dheu</i> – sans. <i>dhûma</i> – lat. <i>fumus</i>
<i>Cort-ona</i>	: i.e. <i>ghorto</i> – sans. <i>grh</i> , gr. <i>khortos</i> , lat. <i>hortus</i>

D. *Perte de la première consonne d'un groupe*

<i>thiman</i> broche	: i.e. (s) <i>tig</i> + <i>men</i> gr. <i>stizô</i> , all. <i>stechen</i>
étr. lat. <i>lanius</i> boucher	: i.e. <i>lagh</i> + <i>no</i> irl. <i>laigen</i> "lance"
<i>nuli</i> époux	: i.e. <i>neubhi</i> + <i>lo</i> , lat. <i>nubeo</i>
<i>skun</i> verser	: i.e. <i>sgeud</i> + <i>no</i> "jeter," all. <i>schiessen</i>

RESSEMBLANCES LEXICOLOGIQUES AVEC L'INDO-EUROPÉEN

étrusque

<i>mulu</i> il donna	: gr. <i>meilia</i> "dot, cadeau"
<i>velth</i> vivre, prospérer	: lat. <i>valeo</i> , all. <i>walten</i> "régir"
<i>usi</i> puiser	: i.e. <i>aus</i> – gr. <i>hauô</i> , lat. <i>haurio</i>
<i>ai</i> présenter	: gr. <i>ai-nu-mai</i> , hitt. <i>pa-ai</i> "donner"
<i>ar</i> faire	: arm. <i>arnum</i> "je fais" hitt. <i>ar</i> "réaliser"
<i>ep</i> posséder	: hitt. <i>ep</i> "saisir," lat. <i>ad-ip-iscor</i>
<i>ker</i> établir	: sans. <i>karomi</i> "je fais"
<i>man</i> se souvenir	: lat. <i>memini</i>
<i>sel</i> gagner	: gr. <i>helein</i> "prendre"
<i>sun</i> jouer de la musique	: i.e. <i>swen</i> , lat. <i>sonare</i>
<i>ke-s</i> gésir	: i.e. <i>kei</i> + <i>s</i> – sans. <i>cista</i> "déposé"
<i>Lasa</i> déesse d'amour	: gr. <i>lastê</i> "courtisane," all. <i>Lust</i>
<i>ais</i> dieu	: italiq. pelignien: <i>aisis</i> "aux dieux" gr. <i>aideomai</i>
<i>kepen</i> prêtre,	
plur. <i>kep-ar</i>	: lyd. <i>kaveš</i> , sans. <i>kavi</i> – (<i>kew</i> + <i>no</i>)
<i>el</i> offrir	: lat. <i>ad-ol-eo</i> "brûler sur l'autel"
<i>su-l</i> eau, lait	: i.e. <i>seu</i> "couler," gr. <i>huein</i> "pleuvoir"
<i>pai</i> miel	: i.e. <i>bhoi</i> – "abeille" – ang. <i>bee</i>

RESSEMBLANCES ENTRE ÉTRUSQUE ET HITTITE

étrusque:

an donner, offrir
en celui-là
ita celui-ci
ka celui-ci
kape récipient
mek foule
par étranger
per terre
pen aller loin
sakni saint
saneva consacré
tu-p-it juge
nane tante

Verbes essentiels

ai présenter, donner
ar faire
ep posséder
rakh prendre

hittite:

an-iur permission, fourniture
anni celui-là
edani celui-ci
kâ celui-ci
kappi récipient
mekki beaucoup de
paryan là-derrière(?)
parn jardin
penna voyager en voiture
saku-wassar juste, régulier
san-kami prêtre
tuppi document
tuli tribunal
anne mère

pa-ai donner
ar exécuter, produire
ep s'emparer de
hork retenir

A. CARNOY

The rapporteur has rightly pointed out that the problem of the root-determinatives is still very much with us and that it is now closely related to the laryngeal theory. He also expressed his view that this problem will find its final answer together with greater certainty about the laryngeals themselves (p. 137 above), and hinted that the study of consonantal clusters may prove a new and better line of attack upon the old problem. I can only subscribe to these points and will try to show that they are borne out by my own results reached in a book under preparation.

I accept with a slight modification Benveniste's monosyllabic theory of the root. As is well known, he insisted that the vocalic nucleus was always surrounded by two consonants, and thus postulated a triliteral structure for IE. Debrunner pointed out in his review of the *Origines* that this formula was

too rigid, and my own investigations have led to the conclusion that there are also biliteral roots, inasmuch as the roots that are alleged to have begun with the *e*-coloured laryngeal have demonstrably never had a laryngeal, a conclusion that was anticipated by Hendriksen from an investigation of the Hittite evidence. At the side of the triliteral roots we have thus biliteral roots also, and it will be seen that this state of affairs resembles, in name at least, Semitic where, too, we have biradicals beside the overwhelming majority of triradical roots.

As concerns the root-determinatives, their character becomes clear once we realize the implications of the monosyllabic theory. As W. S. Allen expressed himself: "If an inquiry based on this theory should produce results such as are in fact observed in attested, living languages, we might be justified in investing these formulae with rather more than an algebraic value" (*Transactions of the Philological Society* 1950, 2). In other words, we must face up to the consequence of the theory and clearly state that at one stage the Indo-European words were all monosyllables, and that the monosyllables reconstructed by us are, at least in theory, full words both semantically and phonetically, comparable to modern Chinese.

If these premises are admitted, it will be seen that the suffixed or extended forms must belong to a later age in which two independent words were combined into one new entity, thus producing the disyllables which are the general pattern of Indo-European vocabulary as reconstructed by a comparison of the individual languages. From this follows that the root-determinatives must be regarded as second members of a compound, in other words, they are full words themselves, inherited from the monosyllabic material of the earlier stage.

These a priori considerations are corroborated by the empirical results, and may be illustrated by a few random samples from my material. The well-known IE root **aus-/wes-* "to swell, stay, be" is a compound of the adverb **au-* "down" and the verbal root **es-* "to sit" recovered from Hittite but known earlier in its reduplicated form **-es-* from Gk. ἵσται and Skt. āstē. Thus **aus-/wes-* meant "to sit down, stay, dwell" and can, for the semantics, be compared with Grm. *sich niederlassen*. It may be pointed out incidentally (1) that **aus/wes-* being the two weakened forms of the compound *au+ws*, or in laryngeal symbols **a₂eu+a₁es* the assumption

of the phonetic structure $*\partial_1 es-$ for the second member would imply that *wes-* actually stands for $*\partial_1 w\partial_1 es-$ which is impossible and thus the analysis speaks in favour of our biliteral theory; (2) that the allegedly simple root $*sed-$ "to sit" is in fact an extended form of $*es-$ "to sit" which became homophonous and is not in origin identical with the root $*sed-$ "to go."

A similar explanation offers itself for the alternating root $*aur/wer-$ "flow, rain," and in nominal forms "water, rain, river" which is a compound of the adverb *au-* and the widespread root $*er-$ "go, run, flow."

A great many of the allegedly simple roots of Indo-European are in fact compounds in which both members have very much the same meaning. Thus the root $*ret-$ "run, roll" in OIr. *rethim*, Lat. *rota* etc., is derived from the aforementioned primitive root $*er-$ "go, run" but $*et-$ is not a meaningless or undefinable suffix or extension but itself a simple root $*et-$ meaning "to run." The same components appear in a reversed order in the well-known root $*ter-$ "go, cross, traverse."

Finally, I may be allowed to refer to my results concerning the origin of the IE labiovelars. These mysterious sounds are nothing else but the sequence of a guttural plus *w*. It will therefore be less surprising if I say that the IE adverb $*kwer$ "where, when" appearing in Skt. *karhi* etc. is likewise a binominal compound consisting of the original interrogative $*ku-$ and the noun $*er-$ ("earth"; the compound thus meant "where on earth?," just as *uper* is *up-er* "above (the earth)" and *ner* is *en-er* "below (the earth)."

It would be rash to hope that it will be possible to trace all suffixes to simple roots still attested in the extant remains of Indo-European vocabulary. But once attention has been drawn to the fact that the so-called root-determinatives are themselves but unrecognized complete roots, it can be hoped that many more cases will be cleared up by the concerted efforts of various scholars, than I have been able to explain.

O. SZEMERÉNYI

IS THERE STILL A ROOT-DETERMINATIVE PROBLEM IN INDO-EUROPEAN? In my opinion this question still exists and will exist as long as the question of the roots themselves is

not entirely solved. In spite of the fine works (by Benveniste and others) lately contributed towards its solution, I think that it has not yet been solved. To illustrate this, I shall state my opinion about the roots of the Indo-European languages, to which opinion I have been led by my investigations, unfortunately not yet published.

I think that in the Indo-European languages roots are conceptual words which were used with different functions in sentences (pretty much the same as full words are used in Chinese). This use of the root had its history in proto-Indo-European. That is why it is difficult to speak about one type of root in Indo-European. Differential words or particles could be added to roots with different functions. They could then coalesce into separate words. Roots with certain extensions, which were part of the word added to them, could be derived from them, and yet not change their basic meaning (cf. in Indo-European **trep-*, **trem-*, **tres-*, with the same meaning). Accordingly, these would not be root-determinatives in the proper sense of the word, but only mechanical extensions of roots, which did not necessarily give them a particular meaning.

The theory of the conceptual and realised words can be applied also here. Used thus, the roots are always closely connected as long as the meaning of the realised word in the sentence allows it. When they are separated, by the changing of forms and meanings, then the realised word with the new meaning and the new forms become the "nucleus" of both the new conceptual word and new realisations. Naturally, in such cases, a new meaning is attached to the new form of the conceptual word as well. We see here a root extension with a real determinative, but it has become a new conceptual word. Of course, it is difficult to distinguish from such root-determinatives those cases in which a root, when used in a sentence, received some determinative (particle or similar word) which would indicate its function only, i.e., without a new meaning, and in this way the determinative penetrated into the new word. It is a usual phenomenon with nouns for instance, that the realised conceptual word as the subject, receiving, in some languages, a special ending for such a purpose, penetrates, together with this ending, the unrealised or conceptual word as well.

For that reason I think that the question of root-determinatives is closely connected with the question of the roots of Indo-European languages, and that it is, to some extent, still open.

A. BELIĆ

Professor WHATMOUGH re-iterated that those who refuse to accept the laryngeals must find some other explanation for the facts, i.e. a consistent and coherent alternative theory. There was no time left for full consideration of the various papers presented, but it was pointed out that the failure of a single etymology could not now destroy the entire laryngeal hypothesis — there is a great body of evidence in its favour. Moreover, a beginning had already been made to follow out its bearing upon, e.g. Germanic (Lehmann) or Italic and Keltic (Crossland).

(J. WHATMOUGH)

SECTION C

THE COMPARATIVE PHILOLOGY OF THE INDO-EUROPEAN LANGUAGES

- (3) How far can the methods and principles of linguistic geography be successfully applied to the study of Indo-European?

Chairman: PROFESSOR W. PÉE

Rapporteur: PROFESSOR M. LEJEUNE

Recorders: DR. P. F. GANZ
MR. P. D. SOSKICE

For Preliminary Reports see pp. 147-158 above.

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(3) How far can the methods and principles of linguistic
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European?

Chairman: Professor W. P. F.
Rapporteur: Professor M. Lejeune
Readers: Dr. P. E. Goss
Mr. F. D. Bosnich

For the preliminary reports see pp. 145-155 above.

Au début de la séance M. LEJEUNE rappelle les conclusions de son rapport, qui se trouvent d'accord avec les contributions reçues. Assurément, la grammaire historique et comparée a beaucoup appris de la géographie linguistique, telle qu'elle s'est développée, notamment, sur les domaines roman et germanique. Mais les données sur lesquelles la géographie linguistique opère, et les conditions dans lesquelles elle opère, lorsqu'il s'agit de périodes historiques révolues et à plus forte raison, de périodes préhistoriques, n'ont rien de comparable à ses données et à ses conditions de fonctionnement normales. De sorte que l'usage de cette discipline, dans le domaine diachronique, doit être assorti de précautions de méthode spéciales.

(M. LEJEUNE)

PROF. DR. ERNST FRAENKEL (Hamburg) gibt in der Diskussion ausgewählte Beispiele besonders aus baltisch-slavischem Gebiet, die eine sekundäre Parallelentwicklung bekunden und daher nichts für nähere Beziehungen beweisen können. Auch andere indogermanische Sprachen partizipieren öfters an diesen, und auch in ihnen lässt sich sekundäre Entstehung dieser Erscheinungen zeigen. Redner erwähnt die Umgestaltung des alten idg. Perfekts **uoida* "ich weiss" zu einem athematischen Präsens in der Geschichte des Altindischen, Preussischen, Slavischen und Armenischen. Der *-oi*-Vokalismus, den die preussischen und slavischen Formen voraussetzen, bestätigt die Jugend dieser athematischen Präsensformen, da, falls sie alt wären, *ei*-Vokal zu erwarten wäre.

Andere Fälle unabhängiger Veränderungen sind die Ausdehnung der von der athematischen Konjugation bezogenen 1. Sg. Präs. auf *-(e)m* auf die thematische Flexion im West- und Südslavischen, die hier stärker, dort schwächer vor sich gegangen ist, lit. 2. Pl. Präs. *eiste* "ihr geht" für älteres *eite* (daher nicht mit slav., *idq* etc. zu vergleichen), lit. Präs. *einù* für ursprüngliches *eimì* und parallele Bildungen im Tocharischen und Altlatein. Die bestimmte Flexion der Adjektive im Baltoslavischen findet im Iranischen Parallelen

Auch hier liegt nichts schon Grundsprachliches vor. Der Ersatz des Gen. sg. m. n. der -o- Dekl. durch den Ablativ kann nichts für nähere Verwandtschaft des Baltischen und Slavischen beweisen, das Altpreussische weist noch die alte Genetivform auf. In den Kasus obliquus des Plurals geschlechtiger Pronomina stellen die den drei Genera gemeinsamen Formen abg. *tēmū* "diesen," preuss. *steimans*, got. *þaim* etc. einen älteren Zustand dar als die im Altindischen, den anderen baltischen Sprachen, dem Griechischen, Italischen etc. hervortretenden Verschiedenheiten von Masc. Neutr. einerseits, Fem. andererseits in diesen Kasus. Das ältere Litauische und das ältere Lettische haben noch Spuren des Ursprünglichen bewahrt. Der präd. Instrumental des Baltischen und Slavischen stellt gleichfalls eine jüngere Parallelentwicklung dar, die sich auch in anderen idg. Sprachen in Ansätzen findet (cf. lat. *aqua nive concrescit* u.s.w.).

(E. FRAENKEL)

The few remarks I wish to make concern the concepts of central and peripheral languages, and the doctrine, I might almost say dogma, that "lateral," "marginal" or "peripheral" languages of a given group are more archaic than the central languages. This dogma is, of course, based on the findings of linguistic geography. It has frequently been observed in dialect maps, for instance that of the Gallo-Romanic words for "mare," that the relicts of the Latin word *equa* persist in more remote and inaccessible localities such as the Massif Central, the Pyrenees and the Alps. The dogma to which I have referred is a generalisation from such observed distributions. Less attention has been paid to those cases where the standard language, being buttressed and supported by the written language, is more archaic than the dialects. The truth is that we are antiquarians at heart. If we in England hear one of our American cousins say "gotten," we acclaim an archaism and might add with a touch of malice "archaism typical of a colonial area." But would anyone assert that the violently creative American English is more archaic than the speech of this island?

However, let us assume that outlying dialects are more archaic than those more centrally situated. What lies behind

this geographical distribution of the linguistic facts? First the fact that speech is a form of social behaviour. We learn to speak by imitation. The linguistic symbol is first and foremost a *mimeme*. Whom do we imitate? First the members of our own family circle and our playmates and schoolfellows. But as we grow older we discover that there are other modes of linguistic behaviour which we should do well to imitate. We shall get on better if we do, we shall feel more important and seem more important. We are, in fact, induced to imitate other modes of speech endowed with greater prestige and higher social value. In a word, we have the import of the standard language on the dialect speaker. Those dialect speakers who come less into contact with representatives of the standard language will be less affected by the innovations of the standard language. Thus the geographical distributions I have mentioned are merely the linguistic precipitations of a set of social forces operating in a centrally organised state, with its capital, ruling classes and their peculiar modes of social behaviour. Their influence on the dialect speaker varies according to ease of communication and intensity of intercourse. The more remote the dialect community the slighter this influence will be.

If the centre is considered as innovating, the dialect will remain more archaic in proportion to its remoteness and inaccessibility. Such are broadly the sociological facts behind the dialect maps of the pattern I have mentioned. The interaction of standard language and dialect is a phenomenon of bilingualism in a centrally organised state. It is only in this sociological context that the concepts "central" and "peripheral" dialects have relevance for linguistics.

If we now turn to the comparative study of the Indo-European languages, we see immediately that we are faced with quite a different set of linguistic facts. I say nothing of the difficulties of reducing the different languages attested at widely differing dates to a common synchronic basis and assigning them to their correct geographical positions at a given date, say 2,000 B.C. But of those who use the concepts "central" and "marginal" in Indo-European comparative philology we are entitled to ask "with reference to what centre is Italic, for instance, more marginal than Greek?" "How is this centre considered as a germ cell of innovations?"

"What are the social processes by which these innovations spread and come to be accepted?" "Where is the Indo-European capital, the ruler, the court, the bureaucracy, the career open to well-spoken talent, and the whole complex of social forces which lie behind the geographical distributions on which neo-linguistic dogma is based?" Until these questions are given a satisfactory answer, I submit that the concepts of centrality and marginality in the context of Indo-European comparative philology are meaningless.

L. R. PALMER

I would not be so sceptical as many of my predecessors in this section. I think the geographical methods have taught the students of dead languages a great deal. It is true that in general we have not an amount of data at our disposal comparable to that in the linguistic atlases of the modern language scholars, but the geographical experience of these scholars is useful to us. So the rules given by Bartoli are in the main true, and if sometimes the results are not the right ones, the fault in this case is not that of the rules, but of their application. For instance, Bartoli has, on geographical methods, built a scheme for the relative chronology of the IE nominative endings *-os/-oi* which is not right, since he thinks that *-oi* is a case of an *area maggiore*; but after we find *-os* in Spanish Celtic, *-os* becomes a case of an *area laterale*, and then it is the older ending, *-oi* being an innovation. I can quote another case from Spanish Celtic; I have said that Celtiberic would be "Goidelic" because we find the conservation of IE labiovelar stops; but if we rightly apply geographical methods, this conservation is only a case of an *area relegata*, and only a negative coincidence, as an American reviewer of my article rightly pointed out. It is better to say that Celtiberic is "non-Britonic."

Geographical methods are applicable to our field, perhaps not only as a *méthode de présentation*, as our rapporteur says, but also as a genuine scientific method, which, like any method, however, must be used with caution.

ANTONIO TOVAR

I quite agree with our rapporteur and the other speakers who have expressed some doubts on the applicability of linguistic geography to the study of Indo-European, but I should like to underline on one side the great merits of the linguistic geography in the fields of e.g. the Romance and Germanic languages, on the other side my fear that sometimes it has not been applied in the right way. I confess that I am inclined to approve the sharp criticism which Hans Kuhn has expressed in regard to the book *Grundlegung einer Geschichte der deutschen Sprache* by one of the most eminent germanists of our time, Theodor Frings.

On the whole, I think that the application of the linguistic geography to Indo-European, e.g. by the regretted Bartoli, has been premature, because it has not taken into account — and has not been able to take into account — the facts concerning the early history of the IE peoples which have been derived, and to a still greater extent may be derived, (1) from Hittite and other languages of Asia Minor, where IE speaking peoples enter history much earlier than anywhere else: (2) from the history of the migrations of the Indo-Iranians, where also new facts are beginning to show: (3) from the well-developed archeology of Europe, and partly, too, from the (pre-) history of the Greeks; and (4) from what may still come out of researches in Southern Russia — perhaps in connection with the studies, conducted by the specialists in Finno-Ugrian languages, on the original home of these languages.

Before such knowledge is taken into consideration, it is too early to apply linguistic geography to Indo-European — but then it is also at the present time too early to criticize the applicability of the method as such. All we can be sure of is that it will neither have to deal with the *Indogermanisch* of Brugmann nor with the *Indoeuropéen* of Antoine Meillet — to name two of our most venerable masters.

L. L. HAMMERICH

Nessuno vorrà certo sospettare che i metodi della geografia linguistica, quali sono ormai consacrati soprattutto dalla filologia romanza, possano essere integralmente, e senza varianti, applicati allo studio delle lingue indoeuropee. E' ovvio che la scarsenza di materiali a nostra disposizione per simili ricerche non ci permette di compilare carte linguistiche

come quelle dell'ALF o dell'AIS. Pure la brillante messe di risultati acquisiti per mezzo dello studio delle carte linguistiche ci spinge a tentare qualcosa di simile anche nel campo della linguistica i.e.

Le difficoltà diventano troppo grandi quando noi vogliamo fare una carta del cosiddetto dominio indoeuropeo, poiché in tal caso schiacciamo in una carta sincrona testimonianze lontane fra di loro talvolta più di duemila anni e soprattutto perché decidiamo troppo sicuramente della sede dei diversi dialetti i.e. Ma la ricerca appare relativamente più facile quando si tratti di aree meno estese e meglio note. L'area greca e quella italica presentano forse in questo senso un optimum di condizioni: la frequenza del materiale epigrafico, e la sua qualità linguistica, non è certo quale noi vorremmo, ma le epigrafi più importanti sono state quasi sempre ritrovate nello stesso luogo, o almeno nella stessa regione, dove furono poste; inoltre spesso la loro datazione può essere fatta con una certa sicurezza (sicurezza che arriva all'ordine del decennio per l'Attica).

Premesso che non possiamo fare certo per la Grecia, o per l'Italia, antica quanto è stato fatto dai redattori dei vari atlanti linguistici e tenendo presente che spesso non è possibile disporre di una completa documentazione sincrona, si deve pur sempre ammettere che per determinare la diffusione di alcuni fenomeni greci sarà tutt'altro che inutile la compilazione di una serie di carte linguistiche. Uno dei risultati più notevoli sarà l'identificazione dei centri di diffusione linguistica che si sono alternati diacronicamente od opposti sincronicamente durante la storia della lingua greca. Fare una storia della lingua greca basandosi anche su queste carte, cominciando ad es. da una carta relativa alla distribuzione di $\sigma\sigma/\tau\tau$ o agli esiti di $*\bar{a}$ per arrivare all'opposizione greco-moderna fra dialetti che conservano le vocali atone o l'articolazione doppia delle consonanti e dialetti che perdono le una e l'altra, sarà un modo né inutile né errato di ripercorrere le vicende della lingua greca.

O. PARLANGELI

I am very glad to see that a note of caution is the undertone, accoustically speaking, of the contributions which have been submitted to you and this note of caution I should like, if

possible, to amplify. Suffice it to look at a map illustrating the movements of the Germanic peoples during their migrations and subsequent foundations of empire. I am thinking e.g. of the distribution of the Goths, especially the Visigoths, during the 6th century. It is almost easier to say where in the then οἰκουμένη they were not than where they were. In scattered groups of varying dimensions, from tribal units attached to the Empire of the Huns to vast realms of their own, they stretch from the steppes of Central Asia to the Pillars of Hercules. Are we e.g. to operate with these fleeting and rapidly changing movements and further bedevil the issues at stake? Yet this was an important formative period for the existing languages of Europe.

The linguistic atlases are devised for stable populations which allow themselves to be investigated at leisure without exposing the inquisitive dialectologist to the danger of being hit over the head by a roving warrior. Yet it is notoriously those languages whose speakers are not affected by the great movements of peoples which change least, as is instanced by Icelandic as compared with the modern Scandinavian languages or Lithuanian as compared with the whole rest of existing Indo-European languages. Think, on the other hand, of the partial shedding of grammar and other features that distinguish even in our days of universal education and the press Afrikaans from its parent Dutch. How much more revolutionary were such changes in those less civilized days, as it is admitted that the languages of less civilized peoples change more rapidly than those of so-called civilized races (see Jespersen's *Language*). But it was precisely such periods of strain and stress when, in historic times, the Romance languages embarked on their divergent development and we are probably not far from the truth in assuming a similar historical background for the period of separation of the speakers of Indo-European. But their geographical disposition may have changed like the picture of a kaleidoscope.

This question is closely linked to that of the vexed substratum. Like a good many other working hypotheses, it is useful if taken with moderation but can harm in an over-dose. It should never be invoked wherever we can explain things without resorting to extra-linguistic factors and can adduce the tendency of the language concerned instead. One should

definitely not, I think, take some isolated features out of the context of the entire development of a particular language and collate it with a similarly isolated fact from another. Yet this is sometimes done. Certain similar sound shifts have taken place in Germanic and Armenian, with no valid conclusion about geographical affinity emerging therefrom. As Prof. Fourquet has shown, the Germanic sound shift is the result of an inherent tendency of the phonological system, the like of which we can actually watch at present in Danish. As far as possible, I think, we should see individual linguistic facts against the background of the entire system or tendency of the language concerned and not forget, though this may sound a truism, that the basic tendencies which have resulted in their autonomization must have been dissimilar, or else the results would have been similar — which they are not. Example: IE **lugi* "lie," in German *Lüge* no phonematic effect of the vowels on the consonants, but a distant effect of the front vowel across the syllabic division, i.e. mutation; in O. Slav *lŭzi* no transsyllabic effect, but strong action of the vowels on their preceding consonants, resulting eventually in new phonemes velar *ʎ* and *ʒ* and development of the same vowels, *u* and *i*, to *ŭ* and *ĭ*, according to their inherent sonority. The tendencies are clearly divergent. But as soon as divergent tendencies arise in contiguous dialects, not controlled by the coercion of the same literary languages, as was the case in those distant times, we witness a curious phenomenon which finds a close parallel in natural science. The gaps in the scale of evolution are due to the fact that intermediate species tend to be pushed out of existence and transitory types are attracted to the more typical, more different representatives. The same thing happens with phonemes; see Prof. Martinet's article in *Word* (April 1952). But for this curious feature there would be many more transition dialects than there actually are. For this reason, too, I think, does dissimilarity in languages not preclude geographic contiguity.

H. GALTON

Peut-on appliquer à la linguistique comparative les méthodes de la géographie linguistique? A vrai dire, *non*, ce sont des méthodes d'observation directe, sur une matière illimitée. Les principes? Les enquêtes dialectales ont fait

découvrir bien des choses qu'aucun principe ne faisait attendre — ce qu'il faut introduire dans la linguistique comparative, ce sont les enseignements qu'a apportés la géographie linguistique.

Ces enseignements obligent le comparatiste à prendre conscience que chacun de ses raisonnements est grevé d'une indétermination (voir p. 155 sq.). La situation du comparatiste est comparable à celle de l'éditeur de textes, placé devant une tradition contaminée; on sait que certains éditeurs ont jeté le manche après la cognée. Comme disait au regretté Cuny un de ses amis: la linguistique fiche le camp.

Accepter la réalité telle qu'elle est, c'est du moins un courage salutaire, et la condition d'un nouveau départ, de la recherche de méthodes d'investigation plus efficaces que les anciennes.

L'expérience de la dialectologie peut, d'abord, rendre certains services limités au comparatiste: négativement, en lui permettant de ne pas s'étonner devant certains faits que la comparaison l'oblige à reconnaître: par exemple l'impossibilité de diviser la famille indo-européenne en groupes partiels (par exemple germano-italo-celtique, ou germano-balto-slave), les coïncidences entre groupes marginaux éloignés les uns des autres (italique-tocharien, celtique-indo-iranien) etc. Positivement, en lui révélant des processus qu'il n'aurait pas imaginés: formes de contamination (Adoptivformen), extension de faits phonétiques par emprunts de vocabulaire (Formübertragung) etc.

D'autre part, le comparatiste ne peut plus se dispenser de faire entrer dans la position générale des problèmes les données géographiques, là où il y en a, pour peu qu'il y en ait. Ainsi, en germanique, il est évident qu'il y a en deux aires dialectales successives, l'une avant les grandes migrations (Völkerwanderung) l'autre après, avec des rapports de contiguité très différents. Classer les dialectes germaniques anciens par affinités, sans introduire systématiquement cette distinction, n'est vraiment plus permis! Les essais de Th. Frings pour introduire l'élément spatial dans l'étude des faits que différencient les dialectes germaniques sont le commencement d'une nouvelle époque, et justifient le terme de *Grundlegung*.

Conclusions: la géographie linguistique met le comparatiste dans une situation difficile; elle lui joue un bien mauvais tour, et ne lui apporte pas, en compensation, le moyen de repartir, en

substituant à la méthode ancienne une stratigraphie linguistique, comme celles que l'on peut faire d'après les cartes d'un atlas linguistique, cartes sans lacunes de parlers synchroniques. Ici, je suis d'accord avec M. Lejeune.

J'attends beaucoup plus, pour le sauvetage de la linguistique, du perfectionnement de la phonologie évolutive, et d'une science de l'évolution des structures grammaticales et sémantiques. Il n'en reste pas moins que si ces espoirs se confirmaient, l'épreuve indispensable de l'exactitude des résultats serait qu'ils puissent être projetés sur une série d'aires dialectales successives, si schématiques et lacunaires qu'elles soient. L'élément spatial est une part de la réalité linguistique dont il est désormais impossible de faire abstraction.

J. FOURQUET

Fra i più forti argomenti di riserva nell'applicazione del metodo geografico è da tener presente in primo luogo la sinonimia. Spesso le ricerche di geografia linguistica sono condotte come se tale possibilità di sinonimia non esistesse.

Tale riserva non riguarda soltanto la linguistica indoeuropea ma anche la romanza e vale generalmente.

Le limitazioni che sono state fatte all'applicazione della geografia linguistica all'indoeuropeo vengono suggerite dalla nozione stessa di indoeuropeo. Sarebbe, ad ogni modo, da distinguere fra indoeuropeo ricostruito e le singole lingue indoeuropee prese in sé o nei loro mutui rapporti.

Pur con tutte le limitazioni di cui quasi tutti i partecipanti alla discussione hanno parlato, il concetto di centrale e di periferico può essere ritenuto utile nella valutazione dei fenomeni linguistici.

Resta inoltre acquisito che l'esperienza geografica non è stata vana ed ha contribuito a mostrare, anche nella linguistica indoeuropea, la complessità dei fatti.

TRISTANO BOLELLI

(a) It seems to be generally agreed that while the scholars who have attempted to apply the methods of linguistic geography to the study of the older IE languages have done an important service to IE comparative linguistics in pointing

out that "Indo-European" must be conceived as a group of dialects, each of which shared features with a number of others, they have tended, in their attempts to reconstruct a system of prehistoric IE dialects, to forget that special difficulties will be encountered in comparing languages used in non-contiguous areas or in different periods (cf. above, pp. 94-5, 150-1, 153). It would seem that isoglosses between historical IE languages should only be used in reconstructing the dialect-system of IE or the chronology of the prehistoric migrations of the IE-speaking peoples under the following conditions.

The only features that should be accepted as significant for the reconstruction of the dialect-system are those which could not have developed in the languages in which they occur out of corresponding features found in one or more other IE languages except as a result of some innovation which is unlikely to have occurred independently in dialects spoken in non-contiguous areas. Opinions will often differ whether a particular feature satisfies this condition, or whether a certain number of satisfactory isoglosses between historical languages is sufficient to make it probable that they derive from a single dialect or from originally contiguous dialects, but all so-called proof in comparative linguistics is a matter of consensus among a sufficient number of specialists.

The attempt is sometimes made, usually in connection with historical research, to determine by means of linguistic evidence how early the speakers of an IE dialect whose existence in the prehistoric period may reasonably be assumed lost contact with groups who spoke others. Here again only probabilities may be established. The existence of significant isoglosses between two or more historical IE languages may theoretically be due either to their having evolved from dialects which were spoken in contiguous areas in the period before the dispersal of the Indo-Europeans began, or to their having evolved from a language or group of dialects which became isolated from all others before the peoples who spoke these latter began to disperse. If historical languages share a considerable number of features, and no one of them shares many with any other related language, the probability that they evolved from a dialect or group of dialects which began to develop in isolation before most others became dispersed would seem to be relatively great.

(b) Certain of the equations which Professor G. Bonfante has advanced as evidence of close relationship between the IE dialects from which Hittite, Greek and Armenian evolved, (see above pp. 152-3), seem to show what kind of errors are likely to be made if the methods of linguistic geography are applied uncritically when the interrelation of IE languages which do not belong to one recognized subgroup, such as the Romance, is studied.

The following features shared by two or all of the three languages in question are likely to be primitive features which had been lost or replaced in other IE languages.

- (i) Genitives such as Hitt. *suppayas*, *assawas*: Gk. *πόλεως*, *πῆχους*.
- (ii) *-anki* in Hitt. numeral adverbs: Gk. *-ἄκι(ς)*.
- (iii) *-i* in Hitt. *asi*, *uni*, *eni* and in Gk. *οὔροσί* etc.
- (iv) The 3rd sg. pres. indicative active inflexion of the Hitt. *hi*-conjugation, *-i*, if it is cognate with *-ι* in Gk. *λείπει*.
- (v) *-sta* in Hitt. 1st plur. indicative medio-passive *-wasta*: *-σθα-* in Gk. *-μεσθα*.

The following may result from the adoption of proto-Greek, proto-Armenian and proto-Anatolian (or pre-Hittite) by peoples who spoke languages with similar phonetic characteristics.

- (i) Closed pronunciation of *ē*.
- (ii) Prothesis before *r-* in Greek and Armenian and lack of initial *r-* in Hittite.
- (iii) Prothesis in the acc. of the 1st sg. personal pronoun.
- (iv) Assibilation of **t* before *i* in Hitt. and its frequent change to *-σ-* before *-ι-* in Greek.

The significance of these isoglosses and of others which Professor Bonfante has noted between Hittite and Greek or Armenian or both will be discussed more fully in an article on the nature of the relationship between Hittite and the better-known IE languages which will be published elsewhere.

R. A. CROSSLAND

M. LEJEUNE, pour conclure le débat, se plaît à constater, à travers la diversité des tempéraments, l'accord qui existe entre les orateurs, sur le fond du problème proposé au congrès.

(M. LEJEUNE)

SECTION C

THE COMPARATIVE PHILOLOGY OF THE INDO-EUROPEAN LANGUAGES

- (4) How far can conclusions about cultural and social systems be drawn from purely linguistic evidence in the Indo-European languages?

Chairman: PROFESSOR A. DEBRUNNER

Rapporteur: PROFESSOR A. SCHERER

Recorders: DR. P. F. GANZ
MR. P. D. SOSKICE

For Preliminary Reports see pp. 159-170 above.



Zunächst möchte ich auf einige Gegenstände, die im *Preliminary Report* nur angedeutet wurden, noch etwas näher eingehen.

Für die Erschliessung von urindogermanischen Kultur-elementen ist das Vorhandensein einer urindogermanischen Sprachgemeinschaft die wesentlichste methodische Voraussetzung. Da es sich dabei um die Existenz des umfassendsten sozialen Systems, des indogermanischen Urvolkes handelt, verlohnt es sich, bei diesem Punkt etwas zu verweilen. Bei den immer wieder geltend gemachten Zweifeln werden in der Regel die Tatsachen übergangen, die am stärksten auf eine ursprüngliche Spracheinheit hinweisen: nämlich die zahllosen auffallend übereinstimmenden Einzelzüge in der Wortbildung und Flexion, die als Ergebnisse gegenseitiger Beeinflussung zwischen verschiedenen Sprachen einfach nicht zu begreifen wären. Ich nenne nur einige charakteristische Beispiele:

1. Wortbildungskategorien mit bestimmter Ablautstufe der Wurzel, z.B. *e*-Stufe bei den Neutra auf *-e/os*, Tiefstufe bei Verbaladjektiven auf *-to-*, *-no-*, dagegen *o*-Stufe bei den Verbaladjektiven auf *-ito-* von Kausativen;

2. *ā*-Stämme erscheinen im Vorderglied von Kompositen als *o*-Stämme;

3. gleichzeitiges Auftreten mehrerer Bildungsmittel in einer Kategorie, z.B. bei Kausativen und Iterativen *o*-Stufe der Wurzel und Suffix *-ejo-*; im Perfekt besondere Personalendungen, *o*-Färbung des Wurzelsvokals (im Singular des Aktivs) und teilweise Reduplikation; im Optativ der thematischen Verba *-oi-* und sekundäre Personalendungen;

4. Oppositionen innerhalb von Formgruppen, z.B. Gegensatz von Stosston und Schleifton bei bestimmten Kasusausgängen; Ablativ sg. bei den *o*-Stämmen auf *-ōd*, bei anderen Stammklassen gleich dem Genetiv, im Plural gleich dem Dativ; im Singular des Ind. Praes. act. der athematischen Verba *e*-Vokalismus der Wurzel, sonst Tiefstufe; *o*-Vokalismus im Singular des aktiven Perfekts gegenüber der Tiefstufe im Plural und im ganzen Medium; Optativ mit *-oi-* bei den thematischen, mit *-je-* bei den athematischen Verben;

5. Übereinstimmungen im Suppletismus: *ego* aber *mihi*, *me*; **so*, **sā*, **tod*; Heteroklitika; Superlativ *svād-iṣṭha-*, ἡδ-ιστος zum *u*-Stamm *svādū-*, ἡδύς.

6. Übereinstimmende Unvollständigkeit des Paradigmas, z.B. defektiver Pronominalstamm *e-* "der"; deponentiale Flexion bestimmter Verba, wie ai. *sacate*, gr. *ἐπειται*, lat. *sequitur*, air. *sechithir* oder ai. *sete*, heth. *kitta*, gr. *κείται*;

7. auffallende Formenbildung, z.B. Infix *-sm-* in obliquen Kasus der Pronomina; G.pl. **toisōm* (ai. *teṣām*, anord. *þeira*, asl. *těchŭ*); Nom. sg. f. lat. *quae*, osk. *pai*, apreuss. *quai*; Nom.-Akk.pl.n. lat. *quae*, heth. *kuē*, apreuss. *kai* (Akk.) "was," lit *taĩ* "das."

Solche Züge müssen in einer engverbundenen Sprachgemeinschaft ausgebildet worden sein. Man könnte dem Schluss auf ein einheitliches Urindogermanisch nur durch eine kühne Annahme ausweichen, nämlich, dass sie alle einer unbekannten Sprache entstammen, die sich als Komponente mit einer Anzahl einander mehr oder weniger nahestehenden Sprachen vermischt hätte, so, dass aus dieser Mischung die indogermanischen Einzelsprachen hervorgegangen wären. Dann dürfte man im Sondergut einer jeden die Reste ihres ältesten Bestandes vermuten, während die oben charakterisierten gemeinsamen Züge der Wortbildung und Morphologie einem einheitlichen Superstrat zugeschrieben werden müssten; für die oberflächlicheren Übereinstimmungen, etwa im Wortschatz und in der Lautentwicklung, bliebe auch die Erklärung durch Entlehnung, besonders bei alter Nachbarschaft. Allerdings müsste man den Anteil der gedachten Komponente wohl so gross ansetzen, dass die überdeckten Sprachen nur noch als Substrate anzusehen wären, und jenes Superstrat den Namen Urindogermanisch verdiente.

Bei den sozialen Systemen ist anlässlich der vaterrechtlichen Familienorganisation auf das Zurücktreten des Begriffes "Eltern" hingewiesen worden. Vater und Mutter eines Kindes sind ursprünglich nicht in einem Wort zusammengefasst, offenbar weil das Verhältnis des Kindes zu beiden nicht auf der gleichen Stufe steht (vgl. W. Havers, *Handbuch d. erkl. Syntax*, 113). Man sagt also ai. *pitārā mātārā ca* (und mit elliptischem Dual ved. *pitārā*, J. Wackernagel, *Altind. Gramm.* II, 1, 151), heth. *attaš annaš* (J. Friedrich, *Heth. Elementarbuch* I § 306 b), toch. *pācar mācar* (Sieg-Siegling, *Toch. Gramm.* § 357, 1). Ebenso alt oder älter wird die umgekehrte Reihenfolge sein, weil sie bei der beherrschenden Stellung des Vaters in der Familie nicht gut neuaufgekommen sein kann:

rigved. *mātārā-pitārā* und mit elliptischem Dual *mātārā*, Aśokainschr. Lok. *mātaripitari* u.ä (Wackernagel a.a.O.) piken. *materesh pateresh* (Dat. pl. anstelle von älteren Dualen, M. Durante in *Ricerche Linguistiche* II, 1951, 168); vgl. auch Plautus, *Capt.* 549; *Nam istic hastis insectatus est domi matrem et patrem*. (Grund mit L. L. Hammerich: Voranstellung des weniger wichtigen Gliedes, vgl. ahd. *sunufatarungo*).

Der Vokal *a* im Wortschatz der sozialen Unterschicht (Meillet) kann nicht auf vulgärer Aussprache beruhen. Sonst müsste man erwarten, dass das *a* in einer Art Ablautverhältnis neben *e*-haltigen Wörtern der gleichen Wurzeln aufträte (wie bei **wastu* "Siedlung", falls es zu **wes-* "verweilen" gehört). Tatsächlich aber stehen die Wörter mit *a* für sich allein; es sind häufig nur einzelne Vokabeln ohne etymologisches Zubehör ausser etwa ein paar Erweiterungen oder Ableitungen (so z.B. **ap-*, **aqwa* "Wasser", **agos* "Schuld", **ados* "Spelt", **andhos* "Kraut", **kaikos* "einäugig", **kapros* "Bock", **daiwer-* "Schwager"), nicht reichverzweigte Sippen wie so viele Wurzeln mit *-e/o-* oder Langvokalen (Ausnahmen etwa: **aġ-* "treiben", **aweg-* "mehren", **ak-* "scharf" und einige andere, meist mit anlautendem *a-*); in der Regel handelt es sich um Substantiva, Ajektiva, Partikeln (z.B. *aqua*, *mare*; *albus*, *claudus*, *laevos*, *scaevos*; *ab*, *aut*), also um Teile des Wortschatzes, die besonders leicht in eine andere Sprache eindringen; nur ziemlich selten sind es Verba (von Denominativen natürlich abgesehen), wie *ago*, *alo*, *augeo*, *cado*, *caedo*, *cano*, *capio*, κλαίω. Ganz fehlt das *a* bei den Pronomina, den Zahlwörtern, den Stammbildungssuffixen, und in den Endungen ist es auf das Verbum beschränkt (Med. auf *-ai*, 1. sg. Perf. auf *-a*). Es gehört also offenbar nicht zum Grundbestand des Indogermanischen.

Das erinnert an das Vorwiegen des *a* in alteuropäischen Reliktwörtern und geographischen Namen (so z.B. in der Zusammenstellung mediterraner Elemente bei Devoto, *Storia della lingua di Roma*, 42 f., und Ribezzo, *Atti del 1° Congresso Int. di Preistoria e Protostoria mediterranea*, 1950, 190 ff.). Auch das System alteuropäischer Flussnamen, dem H. Krahé in den *Beiträgen zur Namenforschung* nachgeht, zeigt dieselbe Erscheinung und zwar gerade bei den verbreitetsten Namenstämmen am stärksten. Auch wenn man die Namen, deren *a* durch illyrische Vermittlung auf idg. *o* zurückgeht, streicht,

bleiben die *a*-haltigen Stämme in der Überzahl, d.h. die Flussnamen sind zu einem ganz unverhältnismässig grossen Teil aus der durch den Vokal *a* auffälligen Schicht des indogermanischen Wortschatzes gebildet (so *Alba*, *Sala*, *Argentia*, *Alantia*, *Aventia*, *Aquantia*, *Alisontia*, *Kambantia*, *Palantia* und viele andere).

Auf die soziale Oberschicht wird wohl mit Recht das System der zweistämmigen Personennamen zurückgeführt. Bezeichnenderweise fehlt bei ihnen der *a*-haltige Wortschatz fast völlig, soweit es sich um nachweisbar altererbte Namenstämme handelt; wir finden hier Wörter wie **deiwo*- "Gott," **klewes* — "Ruhm," **kluto*-, **mē/ōro*- "berühmt," **qoitu*- "Gestalt, Glanz," **kori*, **korjo*- "Schar, Heer," **teuto*- "Volk," **rēg*- "Herrscher," **ekwo*- "Ross," **wlqwo*- "Wolf," **wesu*- "gut," **mato*- "gut" (Wz. *mā*-; kelt. *Teuto-matos*, got. *Mathe-suentha*, alam. *Gundomadus*), **peri*- "ringsum," **upo*- "unter." Mit *a* finde ich im Augenblick nur **audh(o)*- "Glück, Besitz" (ill. *Audarus*, germ. *Audoberht*), **aġo*- "Führer" (gr. *ἡγας*, mak. *Κόσας-αγας*) und ein paar keltisch-germanische wie **katu*- "Kampf." Ein auffälliger Gegensatz zu dem Wortmaterial der Flussnamen (und den einstämmigen Personennamen, die vielleicht zunächst der Unterschicht angehörten) ist also nicht zu verkennen. — Wichtig, aber schwer zu beurteilen ist das Auftreten der bei den Indogermanen beliebten Vererbung eines Namenbestandteils einerseits bei finnisch-ugrischen Völkern (H. Jacobsohn, *Das Namensystem bei den Osttscheremissen*, S.B.Ak.Berl. 1919, S.485 ff.; Hämäläinen, *Über die Namengebungsgebräuche bei den Mordwinen und Tscheremissen*, *Mittn. d. Vereins f. finn. Volkskunde* III, 1945, S. 7, 3) und auf der anderen Seite bei den Aquitanern (z.B. *Belex Belexconis* f., *Bonbelex Harbelexsis* f., *Hotarri Orcotarris* u.a.), sowohl den Iberern oder Keltiberern, deren Namen, wie U. Schmoll in einer noch ungedruckten Arbeit im Anschluss an G. Gatti (*Bollettino della Commissione Archeologica Comunale di Roma* 1908, 216; 1910, 273) nachweist, ganz ähnlich wie die indogermanischen aus meist zweisilbigen Elementen zusammengesetzt sind; vgl. unter den Namen der Turma Salluitana: *Illur-tibas* *Bilus-tibas* f., *Sosin-adem* *Sosin-asae* f., *Sosimilus* (wie Schmoll zeigt, lautgesetzlich aus **Sosin-bilus*) *Sosin-asae* f., *Beles Umar-beles* f., u.a.

Auf dem Gebiet der Weltanschauung begegnen wir in der

Sprache Spuren des Dämonismus: abstrakte Substantiva zur Bezeichnung leiblicher und seelischer Affektionen oder Zustände in ihrer ältesten syntaktischen Verwendung, in der Affekte, Krankheiten, Schlaf u.dgl. auf persönliche Wesen zurückgeführt werden (P. Kretschmer, *Glotta* XIII, 1924, 101 ff., W. Havers, *Handb. d. erkl. Syntax*, 103 ff., W. Porzig, *Namen f. Satzinhalte*, 349 f., 358 f., F. Specht, *Ursprung d. Dekl.*, 386 ff.).

Die Einreihung von Sachen unter das "genre animé" wird in der Regel als Beweis für belebte oder beseelte Vorstellungsweise aufgefasst. F. Specht postuliert sogar Beseelung für alle Begriffe der ältesten Schicht und betrachtet die Stammbildung mit *-k/g-*, *-t/d-*, *-s-*, *-i-*, *-u-*, *-n-*, *-men-* und *-r/l-* sowie die Nominativendung *-s* als Zeichen der beseelten Vorstellung (a.a.O. 290 ff., 357). Gegen den Einwand, dass diese Stammsuffixe sich auch, zum Teil sogar vorzugsweise, beim Neutrum finden, sucht er sich S. 299 ff. zu helfen, indem er jenen Animismus in die Zeit vor der Entstehung der Genusunterscheidung zurückdatiert. Aber wenn alle umgebenden Gegenstände als beseelte Mächte empfunden wurden, dann wäre ihre sprachliche Charakterisierung überflüssig gewesen. Überdies ist eine solche Geistesverfassung undenkbar, und die Religionswissenschaft ist längst von dieser Form des Animismus abgerückt (s.z.B. G. Mensching, *Allg. Religionsgeschichte*, Leipzig 1940, 26 f.). Man wird vielmehr annehmen dürfen, dass die Indogermanen unter besonderen Umständen bestimmte Gegenstände wie Steine, Bäume, Quellen, Flüsse als göttliche Mächte erlebten (vgl. Rud. Otto, *Das Heilige*). Der Machtglaube, den wir den Indogermanen zuschreiben müssen (vgl. W. Havers a.a.O. 106, und in "*Christus u. die Religionen d. Erde*," herausg. v. Fr. König, Bd. II, 726 ff.), bezog sich nur potentiell auf alle Dinge. Die Ausbildung sprachlicher Ausdrucksformen dürfen wir nur für die besonderen Gegenstände erwarten, bei denen die potentiell überall mögliche Machtgeladenheit als aktuell vorhanden erlebt und daraufhin dauernd empfunden wurde; hierher wohl die Stämme auf *-men-* und die oft besprochenen sakralen *u*-Stämme. Da es sich dabei vielfach um Neutra handelt, war die Vorstellung der Macht offenbar nicht grundsätzlich mit der der Belebung verbunden.

Die Behandlung eines Gegenstandes als Maskulinum oder Femininum bezeugt aber überhaupt nicht, dass man an seine Belebung oder Beseelung glaubte. Sie ist vielmehr nur eine

Darstellungsform. Man spricht von einer Sache wie von einem Lebewesen, man zieht eine Parallele, die sich aufdrängt, man macht sich eine naheliegende Analogie für die sprachliche Darstellung nutzbar. So gibt der Deutsche keiner animistischen Glaubensvorstellung Ausdruck, wenn er sagt: *die Uhr geht, der Ofen will nicht brennen, der Kaffee wartet auf mich, ein Ziel lockt, eine Gefahr droht*; der Engländer glaubt nicht an eine Beseeltheit des Schiffes, wenn er ihm feminines Genus beilegt; wenn im Slavischen beim Ersatz des Akkusativs durch die Genetivform Geld, Zigarren, Tänze u. dgl. den Lebewesen gleichstehen (vgl. Specht S. 293 f.), so doch nicht, weil man sie für beseelte Wesen hält oder einmal gehalten hat, sondern in mutwillig-spielerischer Analogie.

So muss es schon bei den Urindogermanen gewesen sein. Wenn sie den Zahn **ǵombhos* "Beisser" nannten, dann gewiss nicht aus animistischer Weltanschauung, sondern weil sie seine Funktion mit der Aktivität von Lebewesen in Parallele setzten; die Analogie ist noch spezieller gefasst bei **edont-* "der Esser," nach dem essenden Menschen oder Tier. Nur bei dieser Auffassung lässt sich begreifen, warum z.B. neben Gerätebezeichnungen, die als Nomina agentis gebildet sind, charakterisierte Neutra auf *-trom* stehen, warum oft für die gleiche Sache ein Neutrum und eine geschlechtliche Form nebeneinander gebraucht werden (vgl. die idg. Wörter für Wasser, Feuer und: *δῶρον* und *arbor*, gr. *κῆρα* : κεφαλή, *ἡμαρ* : ἡμέρα, *πέλαγος* : πόντος, lat. *delubrum* : *aedes*, *flumen* : *fluvius*, d. *das Meer* : *die See*): es wurde eben dem Gegenstand nicht an sich Beseelung zugeschrieben oder abgesprochen, sondern es kam auf den zufälligen Impuls bei der Schöpfung des Wortes an: ob man dabei an ein analoges Verhalten, an eine analoge Eigenschaft eines lebenden Wesens dachte oder nicht.

Mit dem Machtglauben hängt zusammen die Scheu vor dem Gebrauch des eigentlichen Namens von Wesen und Dingen, die mit gefährlicher Macht geladen sind: Namentabu, Worttabu, tabuistische Ellipse (W. Havers, *Wiener S.B.* 223, 1946, 5. Abh., *Handb. d. erkl. Syntax* 106, 239).

Zur Erschließung eines urindog. Opferwesens aus sprachlichen Beobachtungen und eines Priestertums aus der Etymologie vom "Priester" = "Umwandler" vergl. W. Havers in *Christus u.d. Rel. d. Erde* II, 735 ff.

Bei der Zeitteilung weist der Gebrauch etymologisch zusammengehöriger Wörter für Mond und Monat auf die Rechnung nach Mondmonaten.

ANTON SCHERER

Bei der Behandlung dieser Frage hat man sich bisher fast ausschliesslich auf morphologische und wortgeographische Untersuchungen beschränkt. Es ist jedoch notwendig, auch die Syntax in unsere Betrachtung einzubeziehen, ja es könnte fast scheinen, dass eine Vergleichung syntaktischer Fügungen uns zu wesentlich tieferen Erkenntnissen führen könnte, als man gemeinhin anzunehmen pflegt. Allerdings muss man sich darüber im Klaren sein, dass es noch lange dauern wird, bis uns die Anhaltspunkte für eine zutreffende Bewertung der jeweiligen Ausdrucksformen in ausreichendem Masse zur Verfügung stehen werden, um aus ihnen Schlüsse auf die dahinter stehende soziologische Struktur und auf die Weltanschauung der betreffenden Bevölkerungsgruppen zu ziehen. Man braucht das Problem jedoch nur ernsthaft anzugreifen, um zu bemerken, dass sich die darauf zu verwendende Mühe lohnt. Die Möglichkeiten einer solchen Untersuchungsmethode sollen im folgenden durch einige Hinweise über den Gebrauch des Passivs in einigen IE Sprachen erläutert werden.)

Eine Untersuchung des Passivgebrauches auf dem Gebiete der keltischen, italischen und arischen Sprachen zeigt, dass dieser mit bestimmten psychischen Erlebnisformen und Vorstellungen Hand in Hand geht, die als typisch angesehen werden müssen. Besonders aufschlussreich sind die keltischen Sprachen, die uns daher als Masstab für andere Sprachzweige dienen können.

Unter den Situationen, in denen das Passiv besonders weit um sich gegriffen hat, ist das Messen der Kräfte bei Auseinandersetzungen ursprünglich sakral-juristischen Charakters zu nennen. Dieses Fordern ist auf Seiten des Schwächeren oft mit Fasten verbunden, dessen mystischer Kraft sich nach einer Sitte, die sich bei den Kelten und Indern noch bis in unsere Tage erhalten hat, der Stärkere nicht zu entziehen wagt. Zu den eindrucksvollsten Auseinandersetzungen dieser Art ist die grosse altirische Fastenszene auf dem Berge Cruachan zu zählen, wo der Heilige Patrick mit Gott um jede irische Seele ringt, bis dieser ihm schliesslich zugestehen muss, dass er am jüngsten Tage über Irland zu Gericht sitzen darf. Die häufige Verwendung des Passivs in dem sich zwischen St. Patrick und dem Engel des Herrn entspinrenden Dialog ist typisch. Ein Beispiel:

Nach jeder erfüllten Forderung befiehlt der Engel dem

Heiligen *dingaib don Cruachan* — "geh fort von dem Berge!", Patrick jedoch antwortet jedesmal *ni dingéb* — "ich werde nicht fortgehen" und unterbreitet einen neuen Wunsch. Erst nachdem Gott auf die Intervention der gesamten Creatur auch die letzte Bitte gewährt hat, lässt sich der Heilige schliesslich herbei, dem Befehl des Engels nachzukommen und vom Felsen herabzusteigen. Er sagt: *bennacht forsinrig socherndi dorat ocus dingébthar din Chruachan* — "Segen über den gabenreichen Herrn, der gegeben hat, und es wird fortgegangen werden."¹ Mit diesem Passiv fügt er sich in die Macht Gottes.

Solche passivischen Wendungen sind nicht auf die ausschliesslich sakrale Sphäre beschränkt, sondern kehren bei mannigfacher Gelegenheit im täglichen Leben wieder und zeugen von einer Bewertung des subjektiven Faktors, die uns übrigen Europäern fremd ist. Noch heute differenziert man z.B. im Dialekt von West-Kerry zwischen einer durchaus bereitwilligen Ausführung einer Weisung und dem Sichfügen in den Willen eines Anderen, indem man im ersten Falle antwortet *raghad ann* — "ich werde hingehen," während man zum Ausdruck des widerstrebenden Gehens das Passiv *raghsfar ann* — "es wird hingegangen werden" gebraucht.

Kenner des Plautus werden sich daran erinnern, dass in ähnlichen Situationen im älteren Latein das Sichfügen in den Willen eines Partners durch *ibitur* und *abibitur* ausgedrückt wird, z.B. in der Szene *Casina* 739 ff., wo Olympio den alten Lysidamus veranlasst, in das Haus einzutreten, wo angeblich *Casina* mit einem Schwerte auf sie wartet:

Ol. *Stasne etiam? isis.* Lys. *Ego his habeo.* Ol. *Numquid est ceterum quod morae siet?* Lys. *Gladium Casinam intus habere ait, qui me atque te interimat.* Ol. *Scio. Sic sine habere: nugas agunt: novi ego illas malas merces.* Lys. *At Pol malum metuo. I tu modo: perspicito prior quid intus agatur.* Ol. *Tam mihi mea vita tua quam tibi carast. Verum i modo.* Lys. *Si tu iubes, em, ibitur tecum.*

Wir wissen zu wenig über Religion und Recht im alten Italien, um den Hintergrund eines solchen Dialoges aufhellen zu können. Ich habe jedoch keinen Zweifel, dass die Ähnlichkeit derartiger syntaktischer Fügungen auf einen engen geistesgeschichtlichen und historischen Zusammenhang

¹ Wh. Stokes, *The Tripartite Life of Patrick*, 1887, p. 120.

zwischen Kelten und Italikern schliessen lässt, und dass wir berechtigt sind, die Lücken unseres Wissens von der soziologischen und besonders der religiösen Struktur der italischen Gruppe vorzugsweise durch Heranziehung des keltischen Materials zu ergänzen.

Es ist von grossem Interesse, dass hinsichtlich der Entwicklung des Passivs die indo-iranischen Dialekte den italischen und besonders den keltischen an die Seite zu stellen sind. Es handelt sich um eine Tendenz zur Passivität, die in den ersten Ansätzen bereits im *Rigveda* fassbar ist, aber sich erst später — ganz ähnlich wie in den keltischen Dialekten — zu grosser Intensität steigert.

Man kann z.B. nebeneinanderstellen:

Ir. *do rinneadh amhlaidh sin leis* — "und so wurde es von ihm gemacht," *dorath duitiu ó Dia* "es wurde dir von Gott gewährt": Lat. *non prius Virodonicem reliquosque duces ex consilio dimittunt quam ab iis sit concessum, arma uti capiant* (Caes. B.G. III, 18, 7), *nam vos quidem id iam scitis concessum et datum mihi* (sc. Mercurio) *esse ab diis aliis, nuntiis praesim et lucro* (Plaut. *Amph.* 11 f.):

Ai. *anu te dāyi . . . anu te kṣatram . . . devebhiḥ* — "dir (Indra) wurde von den Göttern die Herrschaft zugestanden" (RV 6, 25, 8): Airan. *ārōi ʔi xšmā mazdā ašā ahurā . . . zastāištā yā nā xʔā rē dāyāt* — "gewährt (zugesichert) sind ja von euch, o Mazda, Aša und Ahura, die Handwinke (bei der Scheidung von Guten und Bösen), die uns ins Paradies versetzen" (*Yasna* 50, 5, Bartholomä, *Wb.* 184, 1686).

In den späteren Sanskrittexten wird auch genau in der gleichen Weise, wie lat. *ibo/ibitur* und ir. *raghad/raghfhar* einander gegenüberstehen, zwischen *gamiṣyāmi* und *gantavyam* (-*tavya*- ersetzt das mediale Futurum in pass. Funktion) unterschieden. Im *Vetalapañcavinsati*, das alte Volkserzählungen enthält, die in den Motiven den irischen oft sehr ähnlich sind, heisst es z.B. *mantriputreno 'ktam: adya tvayā gantavyam. tayo 'ktam: gantavyam* — "vom Ministersohn wurde gesagt: "von dir muss gegangen werden." Von ihr wurde geantwortet: "es wird gegangen werden" (Vet. 9, 4 ff.).

Über die hier angedeuteten Übereinstimmungen hinaus lassen sich eine grosse Anzahl typischer Situationen aufzeigen, in denen sich das Passiv in den genannten Sprachen ausbreitet. Besonders auffällig erscheint es, dass das Perfekt in gewissen

späteren Perioden der keltischen und arischen Dialekte infolge einer in ähnlichen Bahnen verlaufenden parallelen Entwicklung, die aus den gleichen Ansatzpunkten hervorzugehen scheint, vollkommen in Passiv gewendet ist, und dass der nominalen Ausdrucksform (im Keltischen dem passivischen Verbalnomen) ein so breiter Raum zugefallen ist.

Was die Ursachen der Entwicklung dieser Passivität anlangt, so ist das keltische Material so gelagert, dass wir darauf eine befriedigende Antwort zu geben vermögen. Wir können sie mit dem Glauben an die Allkraft *Nert* in Verbindung bringen, einer Kraft, deren Wirken gegenüber dem handelnden Menschen in vielen Situationen lediglich die Rolle eines Mittlers, eines Agents, zukommt. Dadurch treten die Wortkategorien, die besonders dem Ausdruck des Vorganges (z.B. Verbalnomen) und des Zustandes (part. perf. pass. in passivischen Wendungen) dienen, in den Vordergrund, so dass das Subjekt aus seiner beherrschenden Stellung verdrängt wird.

Wir dürfen wohl annehmen, dass auch im Italischen und Arischen ähnliche Kräfte am Werke gewesen sind, und aus den Veränderungen der Sprachen schliessen, dass *Brahman* und *Chwarenah* nicht nur ihrem Gehalte nach mit dem keltischen Begriffe *Nert* verwandt waren, sondern dass ihnen auch im Denken dieser Völker eine ähnliche Rolle zugefallen ist. So werden wir darauf geführt, dass in den oben postulierten geistesgeschichtlichen und strukturellen Zusammenhang, der die Italiker und Kelten verband, auch die Arier einzubeziehen sind. Daraus folgt dann wiederum, dass wir wenigstens zeitweise die Arier (r- Formen !), allerdings an anderer Stelle, auch historisch näher an die westlichen IE Völker heranrücken müssen.

Es dürfte wesentlich zur Stützung dieser Ansichten beitragen, dass Gelehrte wie Dumézil sich schon seit langem bemühen, die Ähnlichkeiten zwischen italischen und arischen religiösen Vorstellungen darzutun, und dass schon im Jahre 1919 Vendryès in einem noch zu wenig gewürdigten Aufsätze in den *Mem. de la Soc. de linguistique* auf die grosse Anzahl von sakral-juristischen Isoglossen zwischen dem Italo-keltischen und Arischen aufmerksam gemacht hat.

Am Schlusse mag noch angemerkt werden, dass wir nun mit grösserem Nachdruck die reiche Entfaltung des Passivs im Polynesischen und in gewissen Indianersprachen mit Kraft-

vorstellungen wie Mana und Orenda in Verbindung bringen dürfen und die Passivität auch anderer Sprachen (etwa des Baskischen u.a.) mit der Entwicklung ähnlicher Vorstellungen in Verbindung bringen dürfen.

HANS HARTMANN

The suggestions I have to make are definitely among those theses characterized by Professor Scherer as "anregende Gedanken und Möglichkeiten"; and they concern things not necessarily peculiarly Indo-European. Furthermore, they are not intended to apply necessarily to the state of affairs immediately before the break-up of IE unity. There must have been many stages before this that may still be reflected in some way in the languages if we could only see it. This notion of stages must be borne in mind in what follows; let me for the moment emphasize that we cannot necessarily know to what stage a possible phenomenon belongs.

Professor Scherer says in his printed report on the question of IE family-life that possible evidence of patriarchy or patrilocality is provided by terms for "marry" with the original meaning "take the wife home" and of patriarchy by the two meanings "master" and "husband" of **poti-*. I suggest that this reflects the last stage before break-up (of the language concerned) or at least a later one than emergence of gender. But we should note that matri- and patriarchy and -locality may be borrowed from one community to another in the course of migration and so on, and this sort of relatively temporary state will be reflected in phraseology and vocabulary whereas grammatical phenomena are likely to reflect if anything something more primitive (since vocabulary takes shape more quickly than grammar, to say the least of it).

Now what is the relation to this of the view of Schmidt and others as quoted by Professor Scherer? "Umgekehrt will man in dem Aufkommen der grammatischen Kennzeichnung des Femininums eine Folge der grösseren wirtschaftlichen und sozialen Bedeutung der Frau als erster Pflanzenzüchterin und erster individueller Bodeneigentümerin sehen." Is this thought to be inconsistent with the patriarchal or patrilocal view? In fact, I would attribute it to an earlier stage. Is woman's role thought of as greater than it had been? This

would assume a later stage than patriarchy or one within it. I have suggested that the evidence (such as it is) does not point this way. Or greater than man at the same time? But then how is this shown by grammatical distinction of the feminine? In modern usage (not only inherited grammar) we mark the feminine and identify the masculine unmarked, because of the primacy of man in modern society till now, as witness the demand that has arisen to add to "he" generic, "or she."

No, it is not just a question of the emergence of feminine gender, but of the specific form that emergence of gender takes. Our possible conclusions would not be the same if it was the masculine that derived from the collective. My possible conclusions are either one (1) or (2), or all three of the following:

- (1) gender reflects cattle-raising (Prokosch)
- (2) it reflects human relations (like Prokosch's *-bh-/-r-*)
- (3) it reflects unity of conception of (1) and (2).

First, as regards (3) (unity of conception of human and animal), there is as far as I know no direct linguistic evidence of IE totemism, though there is the possible evidence of "taboos" on certain animal-names. (The customary explanation by "taboo" is no explanation but merely a definition of the problem to be explained). Totemism itself, of course, belongs to an earlier stage than Prokosch's cattle-raising.

As regards (2), I should like to link this not only with *-r-/-bh-* but with what Dr. Gonda says about the root **sem-*, and with Brøndal's etymology of the Latin *totus*. The whole, of which Dr. Gonda's **sem-* forms part, is surely in origin¹ the human community (Germanic *þeudō* people, IE **teutā* and, according to Professor Scherer, also Latin *totus*). This community was the "tribe," as Prokosch calls it, "we" as opposed to other men, if the root *wi-* in **wīua-/*wīros* is to be identified with the pronoun "we." The community may be thought of as including ancestors (Sommerfelt's account of totemism confirms this). This, as Professor Scherer points out, is evidenced by the plural, not dual, form of words for "parents," but is not this equally possibly evidence for group-marriage at an early stage? (as parents suggested in speech the parent-generation).

Finally, to turn for a moment to the suggestions characterized by Professor Scherer as not referring so immediately to cultural systems. Dr. Gonda's suggestion about counting with pairs

¹ Cf. Prof. Sommerfelt's *La Langue et la Société* on the social origin (totemistic) of primitive linguistic categories.

applies, so he says, not only to the Indo-Europeans, but I have mentioned a specific relation which his material might have with part of the cultural outlook evidenced in IE. Similarly with the suggestion I quote about the secondary nature of Optative beside Subjunctive being associated with the emergence of prayer from primitive magic; this emergence would be a general phenomenon of human culture, but the specific linguistic form in which it would possibly be reflected in IE is not a general phenomenon of languages, and we should be left with the problem of accounting for this.

Professor Scherer calls Dr. Gonda and myself optimists because we do not devote any of our ration of space to considerations of principle. Without wishing to be accused of English empiricism, I am optimistic enough to think that the offering of specific hypotheses, however tentative, may lead, through their demolition in general discussion, to the establishment of a measure of truth.

J. ELLIS

The following note is concerned with the conclusions that can be drawn regarding the social system from the evidence in the IE languages.

It should be pointed out first of all that our inferences from partial evidence are always to be drawn with full realization of the fact that any new or hitherto unnoticed data might overthrow our conclusions. Thus it is claimed that the existence of a common word for "tribe, community, people" in the Western languages indicates a special type of organization in the West, different from that of the other peoples. In this case we can prove that the conclusion was premature. Morgenstierne indicated some years ago that the word occurs, in Persian (perhaps Iranian) also where it has the form *toda*, *tod* "heap, stack, tumulus" (NTS 12, 1942, 266); [I may add that the same word appears in the name of one of the political parties looming so large in the news of our days, the Tudeh party, which is the Persian version of the European people's parties].

While this instance is apt to drive home once more the need for great caution when drawing conclusions from our fragmentary material, there seem to be other cases where our evidence

allows us to go beyond the usual conclusions. IE **swekuros* "father-in-law" has long been recognized to be formed from two elements, the well-known **swe-* and *kuros* related to Gk. *Κύριος* "lord" etc. But while the meaning of the second member is fairly clear, the function of the first member is at first sight rather puzzling. Is it "his father"? The matter is further complicated by the existence of IE **swe-sor* where again the second member is, since Benveniste's explanation of Av. *harisi*, established as "woman"; but is **swe-sor* "his woman"? Since we know from historical and even contemporary evidence of the existence of the *zadruga-* or *Grossfamilien-*system among Indo-European peoples, it would seem more plausible to suggest that **swe-* had the meaning "Grossfamilie," and thus to **swekuros* can be attributed the plausible meaning "Herr der Grossfamilie," "the head of the clan"; **swe-sor* also becomes clear: it means "the or any woman of the clan."

After I had reached these conclusions, I was glad to see that similar views have been voiced by other scholars. Risch (*Mus. Helv.* I, 1944, 118) declared: "bhrātēr und swesōr bezeichnen nicht, wie man gewöhnlich angibt, Bruder und Schwester, sondern allgemein "das männliche, bzw. weibliche, blutsverwandte und zur selben Generation gehörige Mitglied der Grossfamilie." Similar results seem to have been reached by Benveniste who, in a lecture before the Paris Linguistic Society maintained that: "Le nom de la soeur: *swesor-* atteste deux éléments: *swe-* (désignant le cercle étroit de famille) et *sor-* (désignant l'être féminin du groupement appelé *swe-*)," see *BSL* 46, 1950, XXI. Specht also declared (*KZ* 68, 1943, 46²) that *swe-* was "alles, was zur idg. Grossfamilie gehört."

If this is true, light can be thrown on a phenomenon that has been unexplained hitherto. As is well-known, the IE possessive pronoun **sweyos*, **swos*, shows a remarkable usage in certain areas, especially Homeric Greek and Slavonic. While in other languages it refers to the third person only, in these areas the pronoun can be used of any person whatsoever; it means not only "suus," but also "meus, tuus, noster, vester" (cf. for Homer, Bolling, *Language* 23, 1947, 31-3). This usage becomes clear now. If **swe-* was the clan, then the social system was obviously based on communal, not personal ownership; everything belonged to the **swe-*, not to any member of it. The

notions of "mine, thine etc." were unknown in connection with any external property. Thus **sweyos* originally meant "belonging to the clan" and for this reason could be used in any context, referring or not to the person involved. This early promiscuity was later, after the breaking up of the clan-system over large areas, altered and it was only at this stage that **sweyos* became restricted to the third person.

OSWALD SZEMERÉNYI

Man könnte gegen die Fragestellung einwenden, dass man entweder ganz prinzipiell fragen kann, was rein sprachliche Tatsachen uns über die Kultur der Sprecher einer beliebigen Sprache sagen können, oder konkret fragen kann, was sprachliche Tatsachen in Verbindung mit allen anderen uns bekannten Tatsachen uns über die Kultur der Sprecher der angenommenen idg. Ursprache sagen können, aber nicht beides zugleich. Es ist aber festzustellen, dass die Frage allgemein im zweiten angegebenen Sinn aufgefasst worden ist.

Wer eine idg. Ursprache leugnet, sollte uns ein lebendiges Beispiel aufzeigen von einem Sprachzustand, der nicht Sprache genannt werden kann und dennoch der Ausgangspunkt von Sprachen sein kann; eine Mischsprache ohne irgend eine Basis gibt es doch kaum.

Ich möchte berichten, dass wir im vorigen Jahre in Kopenhagen ein skandinavisches Symposion über die Frage der idg. Urheimat hielten; Teilnehmer waren Archäologen und Sprachforscher; ein Bericht wird veröffentlicht werden. Alle waren sich darüber einig, dass die Urheimat nicht in Europa gesucht werden darf, wohl aber in einem Raum nördlich des Kaspischen Meeres südlich vom Ural, mit recht unbestimmter Ausdehnung nach Osten und Westen. Die allmählich bekannt werdenden Tatsachen über die Hittiter — das zu frühest geschichtlich bezeugte idg. Volk — lassen darauf schliessen, dass ihre Abspaltung spätestens im zweiten vorchristlichen Jahrtausend stattgefunden hat, und dass wir zu frühest für das dritte Jahrtausend mit einem idg. Volke rechnen können. Zahlreiche spätere Wanderungen und Ueberlagerungen haben sekundäre Berührungen einzelner idg. Völker zuwegegebracht, so dass weder Isoglossen noch Isomorpheme uns ohne weiteres

über ursprüngliche Lagerungen belehren können; nur Ueber-einstimmungen, die weder auf Berührung noch auf Entlehnung beruhen können, sind im Stande, uns über die älteste Kultur etwas zu sagen. Die notwendig anzunehmende frühe Zeit spricht für ein jedenfalls überwiegendes Steinalter; vgl. auch das doch wohl sumerische Lehnwort, das u.a. in deutsch *Erz* vorliegt.

Ohne dass man die geschichtlichen und geographischen Bedingungen scharf ins Auge fasst, haben die rein sprachlichen Tatsachen keinen Wert als Beiträge zur Kulturgeschichte des idg. Volkes. Allein etwas Positives wird doch wohl herauskommen. Die sicherste Grundlage bleibt, wie längst erkannt, die Bedeutung des Pferdes oder vielleicht noch deutlicher: des Wagens.

L. L. HAMMERICH

Ce que M. Scherer nous a expliqué dans cette année 1952 ne diffère pas beaucoup, et en tout cas non pas essentiellement, de ce qu'on lit, d'une façon élégante dans le livre de Vilhelm Thomsen, *Oldarisk Kultur* (La civilisation des indo-européens anciens) paru 1908. Ce fait nous indique clairement la tristesse des méthodes contemporaines concernant notre question.

JENS HOLT

It is very difficult to answer this question, since the necessary investigations have not been carried out for many groups of the Indo-European languages. But if we examine the significance of social environment for the vocabulary of people's languages in historical time, we shall see that it is enormous. I constantly have occasion to consider the Serbo-Croat vocabulary and I regularly meet with different social and economical environments which have left their impression on words. For instance, *sedmāk* has the meaning of a *horse* or an *ox* seven years old, a *carpet* seven yards long, a *pupil* of the seventh form at school, a *coin* of seven units, etc. The first meaning refers to a cattle-breeding environment, the second to carpet co-operatives or workshops, the third belongs to the milieu of education, the fourth to a commercial environment, etc.

This is the case with a large number of derivatives in Serbo-Croat, and it is true of other Slavonic languages also. There is therefore no doubt that older cultural and social systems must have left considerable traces in languages which, after changes in their cultural or social systems, have adapted themselves in some words or expressions to a new situation. That is why it is difficult to-day to discover these broken systems on the basis of an analysis of certain words, until whole groups of words, which would supply the missing key are identified. Kinship words in the Slavonic languages have been examined (Meillet), but not thoroughly. Again, it is still debatable whether words like proto-Slavonic *rpka* (cf. *rink-ti* in Lit.) with the meaning "collector" indicate an epoch of fruit-collecting, or proto-Slavonic **kpl'a* with a one-time meaning of "sheltered place" (now "home") indicates a period characterized by shelters (caves and the like).

These questions must go parallel with ethnography conceived in a broad sense. If linguistic evidence is drawn from social and economic units, then previous conditions as well as those still existing among the Yugoslavs (tribe, clan, co-operative communities, etc.) and the other Slavs, must be brought into relationship with former conditions. If, moreover, the study (comparative and historical) of the Slavonic languages themselves is critically and fully carried out, I think that better and deeper results will be obtained than those hitherto achieved by Šafarik, Niederle, Kadlec, Novaković, Miklosich, Schrader and others. Although for other Indo-European languages much more has been done than in Slavonic the final results can be achieved only when similar investigations into the spread of all the Indo-European languages have been precisely and conscientiously carried out. Then we shall be able to give a definite answer to the above question: to what extent the social and economic systems prevailing at different epochs in the development of Indo-European languages can be characterised, and the way in which residues of older systems were adapted to new conditions.

A. BELIĆ

In der Diskussion ergab sich, dass die Möglichkeit, aus sprachlichem Material kulturgeschichtliche Schlüsse zu ziehen, nicht ernstlich angefochten werden kann, soweit die nötige

Vorsicht gewahrt wird. Besonders muss berücksichtigt werden, dass auch Kulturzustände aus einer der Völkertrennung weit vorausliegenden Zeit in der Sprache Relikte hinterlassen haben können (J. Ellis). Schlüsse aus unzureichendem Material bleiben immer verfänglich; oft gibt es mehrere Deutungsmöglichkeiten (z.B. für die Reihenfolge "Mutter und Vater": O. Szemerényi und L. L. Hammerich). Ein berechtigter "Optimismus" wird es sich trotzdem nicht nehmen lassen, mit Besonnenheit weiterzuarbeiten und davon für die Zukunft eine Klärung der schwierigen Tatbestände zu erwarten (vgl. J. Ellis). Auch die vom Rapporteur gegebene Übersicht war nur in diesem Sinne gemeint.

Gegenüber modernen Tendenzen, die Vorstellungen von einer indogermanischen Ursprache und einem Urvolk aufzulösen, ist also die "konservative" Stellungnahme zu diesen Problemen nicht so sehr zurückgedrängt, wie es oft den Anschein hat. Wir werden uns nicht mehr eine geradlinige Weiterentwicklung der Ursprache vorstellen (H. Hartmann), und überhaupt den Wert der Erkenntnisse, die z.B. die Dialektgeographie gebracht hat, nicht gering veranschlagen, aber das Mass ihrer Anwendbarkeit auf vorgeschichtliche Verhältnisse bedarf strenger Prüfung. Es braucht kein schlechtes Zeichen für unsere Wissenschaft zu sein, wenn heutige Formulierungen, wie J. Holt feststellte, den 50 Jahre zurückliegenden ähnlich klingen.

ANTON SCHERER

SECTION C

THE COMPARATIVE PHILOLOGY OF THE
INDO-EUROPEAN LANGUAGES

- (5) What inferences can be drawn from the evidence of morphology and word-formation about the development of proto-Indo-European?

Chairman: PROFESSOR A. ERNOUT

Rapporteur: DR. O. SZEMERÉNYI

Recorders: DR. P. F. GANZ
MR. M. A. TANNER

For Preliminary Reports see pp. 171-178 above.

The following table shows the results of the experiments conducted on the effect of the various factors on the rate of the reaction.

THE EFFECT OF THE VARIOUS FACTORS ON THE RATE OF THE REACTION

The rate of the reaction was measured by the volume of gas evolved per unit time. The results are given in the following table. The rate of the reaction increases with the concentration of the reactants, with the temperature, and with the surface area of the solid reactant. The rate of the reaction decreases with the volume of the liquid reactant.

TABLE I
The effect of the various factors on the rate of the reaction

Factor	Rate of reaction (ml. gas / min.)
Concentration of reactants	1.0
Temperature	2.0
Surface area of solid reactant	3.0
Volume of liquid reactant	4.0

Questions of proto-Indo-European development have in the last two decades or so again become fashionable. This is the more gratifying as it comes after a long period of interdiction in which some scholars tried to narrow down the scope of comparative philology by proclaiming that the sole task of the comparativist was to establish correspondences without venturing to draw any conclusions as to the linguistic state they must derive from. But the publication of Kuryłowicz's *Études Indo-Européennes* and Benveniste's *Origines* showed once more that the task of comparative philology necessarily included an attempt to penetrate to those dark regions of linguistic evolution which had long seemed to be the exclusive hunting-ground of the much ridiculed Hermann Hirt. Their initiative was boldly followed up by Specht's synthesis which remains an imposing achievement whether one feels able to agree with it or otherwise.

It is therefore a sign of the times that the organizers of the present Congress should have invited the members to express their views on what conclusions can be drawn from the extant remains as to early Indo-European development. The rapporteur's task is then to try to present an overall picture of this development with special regard to nominal and verbal inflexion, but also to word-formation.

For these tasks we have two well-established positions from which to work. First, the state of affairs that can be reconstructed from the various IE languages, and which still coincides to a great extent with the picture presented in Brugmann's *Grundriss*. The second is usually not realized but is of equal importance. I mean the theory of the root as established by Benveniste, and which has gained the adherence of the great majority of scholars working in this field. Our present outlook differs not inconsiderably from that of former days.

Then, although the root was postulated, there was a certain timidity about its function. We do not hesitate any longer to take the positive view that the roots functioned as independent words (cf. Duchesne-Guillemin, *Rapports, V^{me} Congrès*, p. 17: "On ne nie plus beaucoup que les racines aient fonctionné comme des mots indépendants."). In these circumstances it must be inferred that the route of the general development

is clearly marked out: a slow evolution led from the state of monosyllabic full words to that of inflexion. An intermediate stage can also be defined satisfactorily, that of the disyllabic extended and alternating forms: such forms as *ter-m-/tr-em-*, *per-k-/pr-ek-* etc.; that is to say, the general pattern of the actually attested and employed IE radicals, can only be reconciled with the monosyllabic theory if it is admitted that the extensions are nothing else but full monosyllabic words, the disyllabic root representing a coalescence of two originally independent monosyllabic roots. It seems that these considerations provide an answer to the problem set by one of the most archaic, if not the most archaic, formative procedures, heteroclisy, thrown into sharp relief by Benveniste. If we are not prepared to accept a mystical alternation beyond our reach, we must seek to elucidate this pattern by methods widely observed in the historical periods. I have therefore recently suggested that the combination of the stems **wedōr/udōr* with **uden-* can be explained as follows: **ud-ōr* represents the coalescence of the primitive word **wed/ud* "water" with another word **ōr* "river, water," from the root **er-* "go, run, flow"; **uden*, on the other hand, is, as suggested by Hirt, a compound of **ud* with the independent word **en* "in." The amalgamation of such compounds into a unified pattern provides the originally probably meagre foundation of heteroclisy which subsequently spread to other words in which only one of the two terms of the opposition was present originally.

On the whole therefore I am not inclined to accept Specht's interpretation of the various nominal suffixes as deriving from pronominal elements or stems. To give one more example: the suffix of the *-es*-stems cannot, as suggested by Specht, be dissected into the thematic vowel and a pronominal element *s*, but represents a full word meaning "together, the whole of"; this formal analysis is confirmed by semantics, since the *-es*-stems, often paralleled by primitive consonantal stems, are admittedly collective nouns.

But if in this case we feel that the thematic vowel has been invoked outside its proper sphere, there can be no doubt that it played an important role in the later development of Indo-European. It has often been stated that the general IE tendency was to replace the athematic formations by thematic forms. This problem, especially the origin of the thematic

formations, has elicited two replies to the questionnaire. Scherer (cf. above p. 176f.) takes his departure from the assumption that the well-known type of composition in which the first member appears in its stem-form, does not imply the existence of such stems as independent words, since compounds with inflected first member are not later than stem-compounds. In his view, stem-compounds are rather determined by, and restricted to, certain categories which can broadly be defined as predicative and attributive functions, attesting a period in which agreement was not yet grammaticalized and the adjective was without case-endings and motion. This state gave rise to the thematic formations which are based on originally uninflected, adjectival agent nouns in *-e/o*; after the period of "ablaut" weakenings these forms received case-endings from the athematic and pro-nominal paradigms, but also verbal endings from the then existing only verbal type, the athematic verbs.

The same problem is attacked from a different angle by Hammerich (cf. above p. 175 f.). He insists that, although there is a clear-cut distinction between thematic and athematic formations, yet there is no difference in function. He recalls therefore the theory of earlier scholars, according to which the nom. and gen. of the singular derived from a common ancestor, and equates this syntactic state with the subordination of certain languages such as Eskimo. Now in these languages the subordination is accompanied by superordination. If one of the terms of this opposition breaks down, the other must likewise lose its original function. Since subordination broke down in IE, argues Hammerich, we may infer that the system of superordination, which now lost its function, lives on in the thematic formations which are likewise without function.

It is not my duty here to assess the merits of these hypotheses. But as I have indulged in propounding my own views concerning word-formation, I should like to add a few remarks on the question of case-endings also. In my view, the ending of the gen.sg. can be established as *-es*, which is distinct from the outset from the nom. ending. It has often been noted that the gen. is in essence an ablative. I have suggested therefore that the gen.-*es* is an independent word meaning "from away," and is, in the last resort, identical

with the suffix *-es* ("together" employed in the formation of *es*-stems. Their common origin is a root **es-* "to take" which branched off in two directions: "take away" and "take together, collect." The same word explains the ending of the nom. plural in the animate gender, the identity of which with the formans of the collective *es*-stems has already been surmised by Hirt.

We cannot and do not want to go into further detail here. But there is reasonable hope that the IE nominal inflexion will eventually yield up its secrets.

Yet we must not forget that the noun is only one of our numerous and besetting problems. No less problems are raised by the IE verb. The whole question of thematic verbal forms has already been touched upon in connection with Scherer's views. Far greater complexity puzzles the investigator when he attacks the problems of diathesis, tense, action, aspect etc. The richness of Sanskrit and Greek tempted earlier scholars to project all their varieties back into the dark ages of Indo-European. The discovery of Hittite produced a thoroughly sobering effect. Its comparative poverty, with only two tenses in the indicative, reminded scholars that in Indo-European, too, as reconstructed before Hittite, it had been noted that aorist and present action were closely connected and constituted one unit opposed to the greatly differing perfect formation. Here again, Hittite provided the basis for a better insight into the structure of the perfect which turned out to be a special present formation. From the admirable richness of varied tense-formations, Indo-European has thus fallen down to a poor level on which present and preterite seem to have been adequate tools to cover all requirements. A similar state of affairs is now again advocated by Ellis (cf. above p. 174 f.) in whose view the "ablaut" and accent variations, the thematic vowel (used for subjunctive and future) and "even the distinction of Primary and Historic endings, may go back to one distinction of immediate and remote action: *dēik-ti* "he is pointing" and *dik-ē-t* "he might point." An important question of detail is taken up by Crossland who suggests that the tense-pattern of Hittite, in particular the *a*-forms which appear both in the present and preterite, indicates a more primitive state than would appear from the employment of specialized secondary tense-forms seen in Greek and Sanskrit.

None of the answers sent in has made a direct attempt at a solution of the further problem how noun and verb are to be reconciled, whether in particular either of them has a claim to priority. Hirt, as is known, decided for a priority of the noun, Benveniste would appear to be inclined to reverse that order. It is therefore to be regretted that no one has taken up this point, and it is to be hoped that Benveniste will yet redeem his promise in the Chapter on the root to expound his views on the verb. In the meantime, it seems clear to us that the investigation of the development of Indo-European will have to be pursued along the lines indicated above and that great help will be derived from a consistent application of the monosyllabic theory, if all its implications are realised.

OSWALD SZEMERÉNYI

Die Frage nach der Entwicklung des Urindogermanischen gehört heute nicht mehr zu jenem Problemkreis, den man früher mit der Bezeichnung "glottogonische Spekulationen" abzutun pflegte. Unsere Situation ist vergleichbar mit dem Tatbestand bei einer archäologischen Ausgrabung. Ein Horizont ist in mühevoller Arbeit kreuz und quer durchsucht worden und die vielen Kleinfunde, die er lieferte, liegen wohlsortiert und etikettiert in Grundrissen und etymologischen Wörterbüchern. Unter dieser indogermanischen Sprachschicht liegt aber etwas anderes: ich möchte glauben, etwas Unerwartetes und Neues. Einzelne Vorstöße in dieses Gebiet sind schon früher gemacht worden. Sie konnten — ähnlich wie bei einer Versuchsgrabung — kein abgeschlossenes Bild ergeben.

Dieses erhalten wir vielmehr erst dann, wenn wir uns die Anschauung zu eigen machen, dass in dem speziellen Fall der indogermanischen Ursprache die Entwicklung nicht geradlinig verlaufen ist. Wir müssen die Ausprägung des indogermanischen Sprachtypus als das Ergebnis einer Umschichtung, eines Umbaus ansehen. Im System der Flexion deuten beim Nomen ebenso wie beim Verbum gewisse Anzeichen den früheren Zustand an. Dem System der acht indogermanischen Kasus (das eigentlich immer nur defektiv war) muss einmal ein Zweikasussystem vorausgegangen sein. Es standen sich

dabei ein direkter und ein indirekter Kasus gegenüber, wovon der erstere Subjekt des intransitiven und Objekt des transitiven Satzes sein konnte. (Kasus inaktivus im Gegensatz zum obliquen Aktivus, den man heute den Ergativ nennt). Der nominativischen Satzkonstruktion mit nur einem Subjektskasus steht also ein früheres ergativisches Stadium des Vorindogermanischen gegenüber. Ich hoffe die Theorie des indogermanischen Ergativs durch meinen Beitrag zum Genitivproblem (*Die Sprache* 2, 131 ff.) weiter gestützt zu haben. Es war mir dabei möglich, in der indogermanischen Endung *-sjo* zwei getrennte Morpheme festzustellen: die Ergativendung *-s* und eine possessivische Partikel *-jo*. Diese letztere spielt in der indogermanischen Wortbildung auch sonst eine nicht unbedeutende Rolle, was sich aus ihrem ursprünglichen deiktischen Sinn erklärt.

Was das Verbum betrifft, so muss ich auf meinen Aufsatz: "La voyelle thématique serait-elle un indice d'objet indo-européen" (*Lingua* 3) hinweisen. Wie schon der Titel verrät, suche ich die Frage nach der objektiven Konjugation im Indogermanischen in einem anderen Sinne zu lösen, als dies von unserem Altmeister der Indogermanistik, Hofrat Kretschmer, unternommen worden ist. Man kann zunächst die Berechtigung dieser Frage überhaupt bezweifeln. Warum sucht man, warum suchen wir eigentlich nach Überresten der objektiven Konjugation im Indogermanischen, d.h. einer Konjugation, die am Verbalkomplex nicht nur Subjektsaffixe sondern auch Objektsindices kennt. Ich glaube, dass diese Frage sich für denjenigen notwendigerweise ergibt, der im nominalen Flexionssystem einmal den alten indogermanischen Ergativ erkannt hat. Denn gerade bei den Sprachen des ergativischen Systems ist die objektive Konjugation verbreitet, ja ich möchte fast sagen: zu Hause. In der indogermanischen Konjugation ist nun die Dichotomie der Pronominalaffixe schon früher auf das Vorhandensein zweier Subjektskasus zurückgeführt worden. Im erwähnten Aufsatz habe ich festgestellt, dass die Grundbedeutung der gemeindogermanischen Verba deren Zugehörigkeit zur *-mi* bzw. *-ō* Konjugation feststeht, mit der unterschiedlichen Funktion des Ergativs bzw. des Absolutivs (wie wir den Subjektskasus intransitiver Verba nennen wollen) im Einklang steht. Der Themavokal der transitiven *ō*-Verba kann folglich nichts anderes sein als ein Objektsinfix.

Aus den *Preliminary Reports* habe ich entnommen, dass dem Themavokal beim Nomen wie beim Verbum die Aufmerksamkeit namhafter Fachkollegen gilt. Ich selbst habe in meinem erwähnten Aufsatz gleichfalls versucht, eine Brücke zum Nomen zu schlagen. Ausführlicher konnte ich manche der Fragen, die uns hier berühren, in meiner ungedruckten Habilitationsschrift *Zur Vorgeschichte des indogermanischen Verbums* behandeln.

Ich gebe zu, dass ich noch nicht alle vorauszusehenden und möglichen Einwände gegen meine Erklärung entkräften konnte. Es ist ein schwieriges Unternehmen, aus Morphologie und Wortbildung die Entwicklung des Urindogermanischen zu erschliessen. Es geht uns nicht anders als den Ausgräbern, die sich zu einem neuen Fundhorizont durchgearbeitet haben. Da und dort tauchen schon Mauerreste auf. Ich glaube, dass wir aus ihrer Lagerung heute schon das frühere System, den Bauplan, erkennen können. Aber wenn wir weitergraben, werden wir wohl noch manche Überraschung erleben.

JOHANN KNOBLOCH

It is to be assumed that the oldest, known languages made very little progress before the art of some sort of writing was discovered, and that that writing could be re-read and understood by the writer himself and his pupils. Within a well-defined geographical area, a priest-king or the leaders of organised groups could easily shape and name their Deities, even set up some sort of Pantheon, before they had invented a method of recording their names and the prayers to be offered to them.

But when it came to growing commercial exchanges and composing long Creation myths, the oldest pre-dynastic kingdoms of Western Asia must have felt that no human memory, however pious and devoted it might have been, was capable of retaining thousands of lines of sacred poems and subjects of religious worship.

Any failure on the part of priests or scribes to recite these poems at religious ceremonies, or at sacrificial feasts with absolute correctness, was an unpardonable sin.

The theogonies and Creation epics of the Ancient East must have existed in some form in the later stages of the palaeolithic.

The slow growth of social consciousness, coupled with the extension of exchange operations between tribes seem to have accelerated the process of writing in pictographs or symbols.

During the last half-century, research by a group of cuneiform epigraphists, has arrived at the conclusion that the small city-states of Sumer invented the Cuneiform syllabary. But the Sumerian language is not Indo-European; it is further believed that, not only the Akkadians and Assyrians, but also the Hurrians, the oldest inhabitants of Armenia and the periphery as well, borrowed their art of cuneiform writing from the Sumerians and that the Hurrians passed it on to the Hittites, their neighbours in the West.¹

At the present stage of research, the Proto-Hittites, the oldest known inhabitants of Cappadocia and the Halys basin, and the Hurrians, the oldest natives of Armenia, are assumed to have been non-Indo-European; the cuneiform Hittites and the Luwians are proved to be Indo-European, and writing Indo-European languages. Prof. B. Hrozný² has put forward the view that the Indo-European Hittites invaded Central Asia Minor either across the Bosphorus or through the Caucasus. Profs. E. O. Forrer and A. Goetze make the Hittites arrive from the Balkans. Basing himself on a reference to the rising sun in an old Hittite text, Prof. F. Sommer, in 1947, thought that the Hittites might have arrived from the regions of the Caspian Sea and across Lake Van.

From the outset of historical research in the last century, the theory of migrations of peoples from one continent to another has assumed the character of a Dogma. No evidence has been produced to support the theory, at least none relating to the historical time in the Ancient East. All over the region the population seems to have been small, often decimated by epidemics.

I suggest now that the Cuneiform Hittites, like the Proto-Hittites before them, were natives, who at opportune moments rose to power, extended their territory and evolved a monarchical system, as it has happened everywhere ever since.

The Hittites seem to have been an acquisitive stock, easily and willingly adopting the deities, the vocabulary, the

¹ Cf. E. A. Speiser: Introduction to Hurrian. *Annual of American Schools of Oriental Research* Vol. XX. (1940-41) f.

² Cf. for instance, *Archiv Orientalni* III (1931) pp. 281 ff. et passim. E. Forrer thought that the proto-Hittite language was kindred to the Caucasian languages, and Goetze thought that the cuneiform Hittites came from the Balkans.

onomastic, rituals, art, etc. from their more advanced neighbours, particularly from the Hurrians. A. Goetze, Joh. Friedrich, H. G. Güterbock have proved the religious, literary and artistic influences which the Hurrians had exerted over the Hittites. According to Anton Moortgat, the latter learnt the art of chariot-fighting from the former.

How can, then, these influences be divorced from the linguistic affinity of the two peoples and empires? Indeed there appear to be considerable differences between the vocabulary and the grammar of the two languages. Many of the Hurrian words, whose meaning has been established appear in Classical Armenian of the 5th Century A.D. A much larger number of Hittite words has also come into Armenian.

Cuneiform Hittite has been proved to be Indo-European; does it not follow that, except for some grammatical forms, Hurrian is a much older dialect of Indo-European? A fuller discussion of these linguistic problems will be published in full elsewhere. Suffice it to mention here the etymologies of two words, which may give an idea as to how Proto-Indo-European developed through the centuries.

(a) *auli-*, a Hittite word, which von Brandenstein¹ translates as *Hirsch* (stag); the word occurs also in Egyptian, Akkadian, Coptic and other languages. With B. Landsberger², one might think of a common borrowing from a non-Semitic language, and even surmise that the Hittite country, rich in mountains and forests, was a more probable homeland of the word, because stag, deer etc., love to roam in forests and vales (cf. A. Moortgat: *Die bildende Kunst des Alten Orients und die Bergvölker*. 1933, pp. XXX, XXXV ff.). Now *auli* has come into Armenian as *eln* "the stag." The stag and the ibex in particular, have been immortalised in an Armenian epic poem, preserved by Moyses Khorenaci, the national historian of the fifth century.

(b) *kaluti* — or *kalutiš*. A. Goetze and especially E. Laroche³, have thoroughly studied this Hurrian-Hittite word and have rendered it as "circle, divine Court." The word and its mythological significance was current among the Hurrians in the fifteenth and fourteenth centuries B.C. It is

¹ C-G v. Brandenstein: Zu den hethitischen Jahreszeiten, *Orientalia* VIII N.S. (1939) p. 78.

² *Fauna des alten Mesopotamiens*, (1924) p. 99.

³ E. Laroche: Tešup, Hebat et leur Cour. *Journal of Cuneiform studies*. II (1948) ff. 113 ff.

very probable that it came into Armenian as *galuth*, which means a "colony," "colonia." People of the same ethnic origin, speaking the same language, worshipping the same gods, come together outside their own native land and form a "colony." In a detailed study of the Hurrian pantheon, Laroche has reconstructed the divine *kalutiš* of the storm-god Tešup and his sister and consort (?) Hebat¹ in the various centres of the Hurrian world. From cuneiform texts transliterated and interpreted by himself, it appears that from small beginnings the divine couple Tešup and Hebat had gathered around themselves into "colonies", numbers of deities and goddesses together with all appropriate earthly appurtenances: thrones, stools, ovens, dinner tables (?) and other household furniture. The very idea of a complete divine "colony" comfortable enough to make room for a number of subordinate deities must have taken several centuries to think out.

At least a hundred Hurrian and Hittite words like these have come into Armenian literature of the fifth and the following centuries. Here is a linguistic problem of the development of the Proto-Indo-European, which promises to yield entirely unexpected results, if and when further research is undertaken independently of the prevailing Indo-European tradition.

A. SAFRASTIAN

The conclusions about Hitt. 3rd sg. indicative verbal forms in *-a* summarized on pages 173-4 above must be presented as provisional, since the evidence on which they were based cannot be given adequately in the space available here. It will be published as soon as possible, probably in the *Journal of Cuneiform Studies*. A few references to examples in texts available in transcription of the usages in question follow.

(a) *kitta* in pres. sequence in a ritual text: F. Sommer and H. Ehelolf, *Das heth. Ritual des Papanikri*, p. 6*, ll. 16 ff.; *kianta* ib. l. 19; J. Friedrich, *Heth. Elementarbuch* II, p. 55. text 22, ll. 27-31.

(b) *kitta* in pret. sequence: E. Forrer, *Die Boghazköi-Texte in Umschrift* II, text 12A, col. III, ll. 20-1.

¹ E. Forrer (*Mitteilungen der Deutschen Orient-Gesellschaft*, 61 (1921)) thought that the name of Eve, the first woman according to Genesis, was derived from the Hurrian mother-goddess Hebat.

(c) An *a*-form from an *sk*-verb in a prohibition, in which a pres. would be normal: F. Sommer and A. Falkenstein, *Die heth.-akk. Bilingue des Hattusili I*, p. 7, l. 36.

(d) Similar forms in pret. sequence: E. H. Sturtevant and G. Bechtel, *Hitt. Chrestomathy*, p. 186, § 21; E. Forrer, *loc. cit.*, ll. 15-7 (The general context in these passages is pret. The pres. active forms seem to express repeated action).

(e) A form in *-tta*, preceded by the "glossenkeil," in pret. sequence: A. Goetze, *Hattusilis: die Berichte über seine Thronbesteigung*, p. 10, ll. 34-5. (The *a*-form is apparently impersonal).

(f) A similar form from a well-attested Hitt. stem and without the "glossenkeil" in pret. sequence: J. Friedrich, *Staatsverträge des Hatti-Reiches I*, p. 128, l. D52. (Version A shows the normal Hitt. form *li-in-ik-ta*. The *a*-form in D54 has the "glossenkeil" in version B, III, 13.)

The apparent survival of primitive usage of the medio-passive forms in **-o* into Old Hittite should not be considered to imply that the general simplicity of the Hitt. verbal system, in comparison with those of classical Greek and Sanskrit, is primitive and not the result of processes of simplification.

R. A. CROSSLAND

There is no fundamental divergence of opinion between Professor Scherer and myself, with regard to the thematic vowel. His theory of an originally "uninflected" *e/o* form fits well into my view, that this may be an old case which has lost its function.

With regard to the remarks, made by Dr. Knobloch, I should like to point out that in the languages having an ergative case, we have very often to reckon with an opposition between a case which is an absolutive, sometimes called intransitive, i.e. a case which does not enter into a subordinate combination, and, on the other side, cases which do enter into such combinations. The absolutive may stand for the subject of the intransitive verb and for the object (which, in fact, is identical with the subject of the intransitive verb). But within the non-absolutive cases, we generally find an opposition between the case of subordination (which being at the same time the

expression of the subject of the transitive verb, is just what we call ergative) and a case of superordination. If the ergative is split up into a nominative and a genitive, we do not know what becomes of the absolutive; but the case of superordination must become functionless. This is my main point.

Perhaps I might add a warning. We who keep ourselves busy with Indo-European, do not always realize that it is by no means universal to have the same expression ("nominative") for the subject and the predicate; many languages have a special expression for the latter ("essive").

L. L. HAMMERICH

In the field of linguistics, conservatism seems to be more and more intensified. Scientists prefer to base their studies only on well-known standards, and avoid the less certain elements. This is of course the best attitude. But, is it the best way for the development of linguistic science? Accuracy depends not only on the accuracy of standards but also on the completion of premises. Our conclusions will be accurate only, when our premises are complete as well as accurate. It seems we remain stationary, because we take into consideration only a limited number of standards, and, taking an extremely negative a priori attitude towards any new element we do not make any effort to broaden our field and complete our premises. I think we should have the courage to take some risks, in order to be able to broaden our field. For example, Armenian is proved to be an IE language. Scholars, in general, use and abuse the "*Esquisse d'une Grammaire Comparée de l'Arménien Classique*" (A. Meillet), which is only a "commencement" (a good one, of course) in the studies of Armenian, but it is based on very limited "premises." Nobody makes a real effort to broaden this field. However, it seems to contain more than scholars realize. Taking ten pages from every fifth century Armenian classical work I have examined 6,000 roots, and find that 40.07% of Armenian vocabulary is related to Indo-European. The social roots are most frequently found in Iranian.

The remaining 59.93% of Armenian vocabulary is still of unknown origin. Whence did it come? Does it contain IE, or

Proto-IE or are there other elements? Only a deeper and more accurate study of unknown or little known languages will help us to complete our linguistic premises and to reach better conclusions. Among the "little known" languages, the study of Hurrian, Urartean, or the Asianic languages seems to be of primary importance in the development of Proto-IE or IE languages. From India to the Armenian Plateau or Asia Minor, the cradle of Greek Civilization, the Indo-European peoples were super-imposed on the ancient linguistic and geographical areas. The substrata remaining in Classical Armenian (as well as in Semitic) from those old languages, are eloquent. An inscription which relates the incursion of Sargon the Great (about 2,700 B.C.) into Armenia, mentions the names of plants and fruits imported from Armenia into Babylonia. Those terms are still preserved in Armenian.

The studies of Gr. Kapantzian trace numerous grammatical elements surviving in Hittite as well as in Classical Armenian from the oldest languages of Armenia or Asia-Minor. The non-IE elements in Greek are probably of a similar origin.

In short, it seems very important to study Hurrian, Urartean or the Asianic languages in order to draw a clear conclusion about Proto-IE and IE, as well as the Semitic languages which were superimposed on the oldest languages of the Near East. If there is no relationship at all between them, it will still be worth while to establish this, once and for all. Let us not forget that as linguists we should study them all, even if they do not represent any relationship with each other.

Classical Armenian and Hittite are two open doorways toward those languages and Proto-IE.

ARMEN JEREJIAN

Versuche zur Aufhellung der Vorgeschichte des Indogermanischen begegnen heute nicht mehr grundsätzlichem Misstrauen. Eine Gefahr liegt darin, dass mögliche Erklärungen (z.B. die des Kasussystems aus Erscheinungen bei Sprachen mit "ergativem" Typus) vorschnell als Erkenntnisse gewertet und als Grundlage zu weiteren Schlussfolgerungen benutzt werden. An ihrem richtigen Platze, als Arbeitshypothesen, können die "glottogonischen" Versuche nützlich sein, indem sie sich entweder durch immer

weiteres Tatsachenmaterial stützen lassen oder aber eine bessere Erklärung vorbereiten helfen.

Sehr verbreitet ist die Hypothese, dass das Indogermanische in nicht allzu ferner Vergangenheit nur endungslose Stämme als Wörter gebraucht habe. Ihre Beliebtheit gründet sich zum grössten Teil darauf, dass man sich nicht bewusst wird, dass die indogermanische Ursprache nicht am Anfang der Sprache überhaupt steht, sondern dass in ihrer Vorgeschichte bereits ein vieljahrtausendlanges Auf und Ab der Entwicklung anzunehmen ist. Es mag mehrmals ein Schwanken zwischen mehr analytischem und mehr synthetischem Sprachbau stattgefunden haben.

Eine Sprache aus blossen "Stämmen" müsste wohl entweder die (numerischen, örtlichen, zeitlichen, modalen) Bestimmungen durch Formwörter wie Prä- oder Postpositionen, Hilfsverba u.dgl. zum Ausdruck bringen oder, wo dies nicht geschieht, ihren Zustand einem fortgeschrittenen Formenverfall verdanken (wie etwa die altchinesische Schriftsprache). Aber es muss nicht notwendig an der Grenze zwischen einer zur Auflösung des Formensystems hinzielenden Entwicklungslinie und dem Neuaufbau eines anderen eine Periode der reinen Stämme liegen, denn der Neuaufbau kann beginnen, bevor die Auflösung zu Ende ist. So scheinen im Indogermanischen Reste eines älteren Sprachbaus erhalten zu sein, z.B. in den Heteroklitika und im verbalen *n*-Infix. Die ablautenden Ausgänge der für verschiedene Kasus verwendeten Stämme können auf verlorenen Endungen beruhen. Wo alte Endungslosigkeit in die Augen fällt, handelt es sich meist um Formen, die keiner Bestimmung bedürfen, wie der Imperativ, oder um mangelnde Andeutung einer blossen "Beziehung" (Subjektskasus; Kongruenzbeziehungen in den Stammkomposita).

Wenn in alter Zeit, wie ich annehme ("Wesen und Entstehung der idg. Kongruenz," in *"Gymnasium u. Wissenschaft," Festgabe des Maximiliansgymn. in München*, 1949, 140 ff.), die Kongruenz nicht ausgedrückt wurde, dann hatten damals die Adjektiva weder Deklination noch Motion (vgl. auch das "Neutrum" in *mutabile femina* und beim Infinitiv oder Nebensatz als Subjekt). Die *o*-Stämme, ursprünglich wohl adjektivische Nomina agentis, bekamen ihre Deklination erst nachträglich, teils von substantivischen Stammklassen (Nom.sg. -s, pl. -ōs < -o-es), teils vom Pronomen (Gen. -syo,

Nom. pl. in den meisten Sprachen *-oi*; die Scheidung von Gen. und Abl. sg., die den anderen Stammklassen fehlt, stammt überhaupt aus dem Pronomen), teils durch Umdeutung einer Art Adjektiva qualitatis auf *-i* zum Gen. sg. Deshalb gibt es zunächst auch keine Genetiv-, Dativ- und Ablativformen von *o*-Stämmen im ersten Glied der Komposita: *oi-philos*, aber *θιο-ίκελος*; *διώσ-δοτος*, aber *θειό-δοτος*.

Die Sonderstellung der thematischen Deklination und Konjugation gegenüber dem Ablaut könnte darauf beruhen, dass beiden Kategorien adjektivische, daher unveränderliche, Nomina agentis zugrundeliegen, die dem quantitativen Ablaut nur beschränkt ausgesetzt waren: **léikwe* "λείπων", (Auslautvokal vielleicht durch seine Funktion geschützt), **likwé* "λιπών". In prädikativer Verwendung konnten sie nachträglich die Personalendungen der (athematischen) Verba annehmen. Darum scheinen alle thematischen Präsensstamm-suffixe nominalen Ursprungs zu sein (*-e*-, *-ye*-, *-eye*-, *-we*-, *-ne*-, *-te*- sind auch adjektivbildend; zu *-ske*- vgl. die Abstrakta auf *-skā*), während die athematischen vielmehr *-ne*- Infix, Reduplikation oder die bloße Wurzel haben.

ANTON SCHERER

Le PROFESSEUR CARNOY, en une brève intervention, s'élève contre l'affirmation que les bases dissyllabiques indo-européennes sont des combinaisons de substantifs. Elles peuvent l'être parfois, mais il y a lieu de croire que les "déterminants" ont plutôt été primitivement des particules emphatiques ou déictiques.

En effet, celles-ci perdent rapidement leur force, ce qui rend compte du manque de signification de ces élargissements. En outre, plus les textes indo-européens sont anciens, plus ils fourmillent de particules sentimentales. Enfin, beaucoup de ces éléments adventices existent, en fait, à l'état indépendant comme particule ou pronom.

La voyelle thématique est apparemment le pronom *e/o* et a visiblement servi d'élément déictique ou de procédé de subordination (celui qui . . . , celui de . . .).

On a fait apparaître l'état encore incertain des terminaisons verbales en Hittite. Elles étaient encore emphatiques,

renforçantes. Nous ne devons pas oublier que l'indo-européen a connu une période où le radical nu était la forme normale (comme c'est encore le cas pour des impératifs, pour l'aoriste passif Sanscrit du type *adarsi* et pour les cas sans terminaisons).

Il faut donc nous attendre à trouver dans des langues plus ou moins parentes de l'indo-européen des verbes sans terminaisons. A propos de pareils idiomes, qu'on pourrait appeler "para-indoeuropéens" il y aurait lieu d'en recueillir le plus grand nombre possible. Au lycien et au lydien, il semble déjà qu'on puisse joindre le "pelasgique" et l'étrusque. Cherchons-en d'autres et décidons, aussi une bonne fois, s'il faut rejeter définitivement le rapprochement Aryen-Sémitique (pour lequel Møller et Cuny ont amassé des éléments plus ou moins probants) ou s'il faut, au contraire, admettre en principe l'existence d'une certaine parenté, car il y a là un élément d'importance, susceptible de modifier tout le paysage.

(A. CARNOY)

The information to be drawn about the development of proto-Indo-European from its later morphological history is strictly limited by its tendency to discard the old system and to replace it by one less elaborate. The morphology of the classical languages shows that the prehistoric dialects possessed the following features;

- (1) A full paradigm of cases (already contracting in Greek and Latin),
- (2) Three genders,
- (3) Three numbers of nouns,
- (4) Three personal forms of tenses,
- (5) Three voices,
- (6) A clear distinction between finite forms of verbs and participles.

These features are not retained uniformly by the modern languages and any single one may be entirely absent. The modern developments can be traced historically, but inferences which are open to question are the only means of ascertaining

the prehistorical processes, which produced the early morphology.

The records of the Dravidian languages, which date from the first or second centuries for Tamil, from the fifth century for Kannada and from the eighth or ninth centuries for Telugu and Malayalam, reveal a movement from a simpler to a more complex system of the early Indo-Aryan type involving a paradigm of cases, and in short, a morphology which, though not so elaborate as that of Sanskrit, outstrips that of most modern Indo-Aryan languages. We first encounter Dravidian not in an isolating stage, but in one of agglutination passing, though never completely, into synthesis. Postpositions are normal and there are the rudiments of cases, there are two genders (persons of superior status, commonly called rational and persons of inferior status, animals, things and abstractions or irrational), two tenses or aspects with the beginnings of inflection for person, one voice, the active, with a rare periphrastic passive, and a shadowy distinction between finite verb-forms and participles. Dravidian, therefore, when it first comes to notice, is moving in a direction opposite to Indo-Aryan. Grammatical works are among the earliest or even the earliest of literary as opposed to epigraphic monuments and are modelled on Sanskrit theory. Consequently they adopt a schematic morphology, which calls to mind the Latin scheme adopted in former times by English grammarians. A tradition of earlier grammars now lost supports the inference from internal evidence that the movement suggested by it was not recent.

Dravidian has moved from a postpositional system to a not quite complete case-system. Proto-Indo-European has few postpositions, traces of which still remain in the English *homeward, thereon*. Hittite combines a case-system with postpositions in spite of the Accadian example of a more limited case system (as limited as in proto-Dravidian) and prepositions. It is possible to attribute the Accadian and proto-Dravidian "cases" to an expressive origin, but those of proto-European are too elaborate and self-sufficient to account for except by postpositions. Hittite has two genders, personal and impersonal. Is this a reduction from a three gender system or an inheritance from pre-Indo-European? Apparently the latter. The Indo-Aryan languages with less

than three genders have amalgamated masculine and neuter, or have abandoned gender altogether. Genders, when distinguished, still mark sex, where sex exists, in all animals, not persons only. The Hittite and Dravidian distinction by person is more easily resolved into sex-distinction, whether followed or not by purely formal gender than conversely.

In proto-Indo-European the genitive is often identical with the nominative and it has been suggested that they are both modifications of one form. In proto-Dravidian the place of the genitive is taken by a compound noun of the *taṭpuruṣa* type (the qualifying noun preceding) or by the addition of what appears to be an expressive vowel (later this is enlarged by a postpositional suffix). It is quite in accordance with the principle of this process that the pre-Indo-European roots which had been enlarged by an -s suffix should have assimilated the consonantal stems in Latin to those of the vowel stems by an expressive glide (*nuks*, *nucis*).

Dravidian compounds are very fruitful and even when forming new words are easily recognisable. The adjective is commonly denoted by a noun in *Karmadharāya* (e.g. *kalmane* "stone house") and there are a few words, generally monosyllables, which have no separate existence as nouns, such as *cen* "red," as in Tamil-Kannada *cembon* "red gold" = *cen* + *pon*, *cenduti* "red lip" = *cen* + *tuti*.

Monosyllables which are numerous in proto-Dravidian have disappeared except in Tamil and Malayālam. So *kal* "stone," becomes *kallu* in Kannada and Telugu and replaces *kal* in new formations. The enlargement of monosyllables and the contraction of polysyllables e.g. Tamil *per* from *peyar* Kannada *pesar* "name" is a continual process and reminds us that apparent roots are not necessarily original forms and that enlarged forms may often be older than co-existent biliteral or trilateral roots.

It has been suggested that Indo-European forms exemplified by the Hittite *wātar*, *weten*- result from the application of different suffixes to one root. Dravidian words have often several forms with no difference in meaning, e.g. Telugu *tinga*, *tīnge*, *tīngiya* "wire," and *cēraka*, *cēruṇu*, *cēruwa* "proximity." There is however no selection of stems for different cases as in Hittite.

A question raised in the discussions was the relative priority

of verbs and nouns. In proto-Dravidian there is little difference in morphology or form, although later the difference is marked. But there is a number of words such as *kal* "stone," *nīr* "water," which are not used as verbs, and there are verbs such as *koḍu* "give," which are not used as nouns. The evidence thus points to synchrony of origin.

ALFRED MASTER

The first of these is the fact that the average life expectancy at birth in the United States is 74 years. This means that the average person is expected to live 74 years. The second fact is that the average person is expected to live 74 years. The third fact is that the average person is expected to live 74 years.

The fourth fact is that the average person is expected to live 74 years. The fifth fact is that the average person is expected to live 74 years. The sixth fact is that the average person is expected to live 74 years. The seventh fact is that the average person is expected to live 74 years.

The eighth fact is that the average person is expected to live 74 years. The ninth fact is that the average person is expected to live 74 years. The tenth fact is that the average person is expected to live 74 years. The eleventh fact is that the average person is expected to live 74 years.

The twelfth fact is that the average person is expected to live 74 years. The thirteenth fact is that the average person is expected to live 74 years. The fourteenth fact is that the average person is expected to live 74 years. The fifteenth fact is that the average person is expected to live 74 years.

The sixteenth fact is that the average person is expected to live 74 years. The seventeenth fact is that the average person is expected to live 74 years. The eighteenth fact is that the average person is expected to live 74 years. The nineteenth fact is that the average person is expected to live 74 years.

The twentieth fact is that the average person is expected to live 74 years. The twenty-first fact is that the average person is expected to live 74 years. The twenty-second fact is that the average person is expected to live 74 years. The twenty-third fact is that the average person is expected to live 74 years.

The twenty-fourth fact is that the average person is expected to live 74 years. The twenty-fifth fact is that the average person is expected to live 74 years. The twenty-sixth fact is that the average person is expected to live 74 years. The twenty-seventh fact is that the average person is expected to live 74 years.

The twenty-eighth fact is that the average person is expected to live 74 years. The twenty-ninth fact is that the average person is expected to live 74 years. The thirtieth fact is that the average person is expected to live 74 years. The thirty-first fact is that the average person is expected to live 74 years.

SPEECH SYNTHESIS AS A RESEARCH
TECHNIQUE

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I. INTRODUCTION

There is at present a renewed interest in the contributions to linguistic science which can come from acoustic studies. This is due in part to new instruments, developed during and after the war, which enable us to deal with the acoustic aspects of speech with a new facility — so much so that this new area has been termed "Acoustic Phonetics" by Martin Joos in his pioneering monograph.¹ The more popular label of "Visible Speech" has been applied by the Bell Telephone Laboratories to the best-known of these newer approaches.² And indeed the instrument which they developed does produce a "picture" from the speech wave. By studying this picture — this sound spectrogram — it is possible to see, more clearly than before, the acoustic patterns that correspond to the various linguistic units and to determine some of the relations between articulatory movements and sound.

However, we shall not deal further in the present paper with this essentially descriptive approach; rather we should like to discuss a different and newer research method which uses these spectrographic pictures as a basis for modifying or even synthesizing speech sounds, and thus enables an investigator to determine experimentally which aspects of the speech-wave are important to the listener; in short, to study speech by creating a variety of sounds and choosing those that are heard as speech.

2. TECHNIQUE OF SYNTHESIS

The way in which these speech sounds are generated is just the reverse of the familiar decomposition of speech into its harmonic components. It is, if you will, a recomposition of those same harmonics to produce synthetic speech. Perhaps the easiest way to explain this procedure would be to recompose some typical syllables.

[At this point in the lecture, sound recordings and slides were presented to demonstrate how the syllables MI DO LA can be synthesized. See Appendix and diagrams in pocket.]

¹ M. Joos. Acoustic phonetics. *Language Monograph* No. 23, 1948 (Suppl. to *Language*, 24, No. 2).

² R. K. Potter, G. A. Kopp, and H. C. Green. *Visible Speech*. New York: D. Van Nostrand, 1947, 441 pp.

The sounds that were heard in the demonstration mentioned above were produced from spectrographic patterns by a machine constructed at the Haskins Laboratories in New York and called a pattern playback — it plays back the patterns of speech that are visible on spectrograms.

The principle of the playback is somewhat like that of a player piano. Spectrograms correspond to the perforated piano roll, and the individual sounds are pure tones rather than harmonic-rich notes from a piano. Briefly, the playback generates 50 harmonic tones 120 cycles apart, from 120 to 6,000 cps, in the form of beams of light modulated by a tone wheel. When a spectrogram, drawn with white paint, passes under the modulated light, each painted portion of the spectrogram reflects a portion of the light. This causes the corresponding harmonics to be heard after conversion of the light into sound by means of a phototube, amplifier, and loudspeaker. In the innermost circle of the tone wheel are four cycles of variation in film density, and from the center of the disk to the periphery, the number of cycles per revolution increases in steps of 4 cycles, reaching 200 cycles at the outermost, or 50th, of the concentric rings. The wheel rotates normally at 30 rps, and the light which passes through the wheel is therefore modulated at a fundamental of 120 cycles per second (corresponding to the innermost circle), and at each of the first 50 harmonics of that fundamental (corresponding to the 50 rings). It should be noted that the playback operates entirely on the basis of these 50 steady-state harmonics of the 120 cps fundamental, and does not resort to any generator of inharmonic sound — or noise — even for the production of highly fricative or plosive speech sounds; the noise-like and click-like sounds are produced, on the pattern playback, by brief tone-bursts scattered through restricted frequency-time ranges.

The very great advantage of the playback over other means of producing synthetic sounds is that it enables one to experiment with the dynamic aspect of speech — that is, with the rapid changes of formant frequencies in time — though it can also be used to deal with steady-state sounds. Thus, the acoustic counterparts of all linguistic units, from isolated phones to syllables and words, can be produced — and evaluated by ear — to determine the relation between acoustic stimulus and perception.

Our first experiments were to determine just how intelligibly the playback would talk. Spectrograms of sentences were played back, and the resulting sounds were presented to groups of college students. In spite of its monotone intonation, the playback speech was understood with rather few errors. Photographic spectrograms of human speech were used in one test; simplified, hand-painted copies of these spectrograms were used in the other. There was little difference in intelligibility.

[Some of these sentences, and the differences between playback speech from original spectrograms and speech from hand-painted simplifications, were demonstrated by recordings and slides.]

3. AN EXPERIMENT WITH VOWEL TRANSITIONS: THE VOICED STOPS

These experiments suggested a number of specific problems and a rather different approach to their solution. Instead of continuing with the simplification of the patterns found in spectrograms of connected speech, we have chosen to attempt the synthesis of individual speech sounds from the simplest possible patterns. Then, by varying systematically the components of these patterns and by presenting the resulting sound groups of listeners, we have attempted to determine which unitary configurations are essential constituents of speech patterns.

By proceeding in this way, we have been able to find a definite answer to the question of the importance of transitions in the perception of speech.¹ The fact that transitions exist in connected speech is immediately evident from spectrograms; that is, there are many instances in which the frequency positions of the vowel formants shift rapidly where vowels and consonants join. It seems clear that these transitions are the acoustic counterparts of the rapid articulatory shifts involved in passing from one speech sound to the next. The problem is one of interpretation: are these changes in the sound streams no more than the necessary transitions (as the name implies)

¹A more detailed discussion will be found in: F. S. Cooper, P. Delattre, A. M. Liberman, J. M. Borst, and L. J. Gerstman. Some Experiments on the perception of synthetic speech sounds. *J. Acoust. Soc. Amer.*, 1952, Vol. 24, pp. 597-606.

between steady-state sounds which serve to identify successive phonemes, or do they serve as important distinguishing characteristics of the phonemes? In other words, is the transition which one observes in a consonant-vowel combination merely a useless (or even confusing) residue, or may it be, in fact, a principal acoustic cue for the recognition of that combination?

Our exploratory work suggested the latter conclusion: we had, for example, found that the stop consonants in initial position were often characterized spectrographically by rapid transitions, and, conversely, that copying these transitions in the painted sentences seemed to convey the impression of a stop. However it seemed desirable to test the matter more systematically, and the stop consonants seemed to provide a useful starting point.

The design of the experiment is indicated in Fig. 1. Seven cardinal vowels (*i e ε a ɔ o u*) were used, each drawn with only two formants.¹ As can be seen in the figure, the extent of frequency shift (transition) of the second formant was varied in eleven steps (four transitions from a frequency above the steady-state position, one "straight", and six from below); the first formant had a constant "rising" transition in all cases. Part A of Fig. 1 shows these eleven transitions for one vowel, part B indicates the formant frequencies of the seven vowels, and part C shows one of the 77 "syllables" employed in the test.

The playback sounds derived from these patterns were assembled in random order and presented to groups of college undergraduates (not trained in phonetics) with instructions to identify the initial consonants as *b*, or *d*, or *g*. The listeners were in rather good agreement as to the particular transitions which corresponded to the individual stops, and it is therefore clear that transitions serve as cues for the stop consonants.

The contours of Fig. 2 indicate which transition-vowel combinations were most clearly heard as each of the three consonants. It should be noted that the best transitions for a particular consonant (especially *d* and *g*) vary from vowel

¹ See: P. Delattre, A. M. Liberman, and F. S. Cooper. *Voyelles synthétiques à deux formantes et voyelles cardinales. Le Maître Phonétique*, 1951, No. 96, 30-36; P. Delattre, A. M. Liberman, F. S. Cooper, and L. J. Gerstman. An experimental study of the acoustic determinants of vowel color; observations on one- and two-formant vowels synthesized from spectrographic patterns. *Word*, 1952, Vol. 8, pp. 195-210.

to vowel — although in a systematic manner — so that one cannot identify a particular transition with a particular stop in all cases.

4. BURSTS OF SOUND AS CUES FOR THE UNVOICED STOPS

Another cue which we have studied in a similar set of experiments is the burst of noise which regularly precedes a voiceless stop at the beginning of a syllable.¹ Spectrograms show that the energy of such a burst tends to be localized in frequency, but the spectrograms do not indicate an unambiguous correlation of this frequency region with the identity of the consonant. Our test syllables combined a brief burst of noise centered at one of 12 frequency positions (ranging from 360 to 4,320 cps) with one of the seven cardinal vowels mentioned above. The vowels were represented by two formants only and did not have transitions. The 84 syllables were presented to groups of students for identification of the consonants (as *p*, or *t*, or *k*) with the results shown in the dominance contours of Fig. 3.

Again, the cue given by a single variable — the frequency region of the burst of noise — provided distinctions on which nearly all the subjects were agreed. Also, for the bursts, as for the transitions, the identification of a particular consonant cue varied according to the vowel with which that cue was associated. For example, the burst at 1,440 cps was heard as *p* before *i*, as *k* before *a*, and as *p* again before *u*. (We have already seen that the identification of transitions depended on the vowel with which the transition occurred). In short, the perception of these stimuli, and perhaps also the perception of their spoken counterparts, requires the consonant-vowel combination as a minimal acoustic unit.

5. RATE OF TRANSITION AND THE DISTINCTION BETWEEN VOWELS AND CONSONANTS

In our discussion of transitions we have so far considered the effects on perception of variations in the direction and extent of the frequency shifts. Exploratory research with the

¹ A detailed account may be found in: A. M. Liberman, P. Delattre, and F. S. Cooper. The role of selected stimulus variables in the perception of the unvoiced stop consonants. *Amer. J. Psychol.*, 1952, Vol. 65, pp. 497-516.

playback has indicated that perceived distinctions among speech sounds can also be produced by varying the rate of transition, i.e., the rate at which the frequency of the formant changes.

Three instances of this are shown on Fig. 4. In the first (top row), all three spectrographic patterns start at the formant frequencies of *u* and terminate at the formant frequencies of *a*. The first pattern shows a change from *u* to *a* with the rate of change kept relatively slow for both formants. The auditory result (from the playback) is a vocalic sequence with a very gradual change of colour from *u* to *a*, which can be transcribed approximately by *ua*. The second pattern shows an increase in the rate of change which is sufficient to transform the initial *u* into the semi-vowel *w*, so that the auditory impression of the whole pattern is now *wa* instead of *ua*. The third pattern shows a further increase in rate of change, to the point where the auditory effect of the initial part reaches the consonantal state of *b*, so that the total syllable is perceived as *ba*. Relative durations of the transitions which yield *ua*, *wa*, *ba*, are to each other as 6:2:1. (It should be noted that the transitions of *wa* and *ba* are drawn in curved shape. This was done in order to conform with spectrograms of human speech; if the transitions were drawn as straight lines, the duration of the changes would be somewhat shorter for similar — but less life-like — sounds).

In the second illustration of the effects of rate of change (middle row), the three patterns start at about the frequency positions of *i* and terminate at those of *æ*. The first pattern, when played back, is heard as *iæ*, the second as *jæ*, the third as *gæ*. The relative durations of the three changes are roughly the same as before, that is, 6:2:1. Again, the successive increases in rate of change cause the auditory impression of an initial vowel (*i*) to become that of a semi-vowel (*j*) and finally that of a true consonant (*g*).

In these two series of illustrations, a difference in rate of change of the frequencies of formants 1 and 2 was sufficient to produce perceived differences ranging from vowel to consonant: a relatively slow rate of change from one frequency to another will be heard as a vowel or diphthong, while a somewhat faster change over the same frequency range will produce a clear impression of a consonant.

The third illustration (bottom row of Fig. 4) of the effects of rate of change is suggested by the results of some preliminary research on *l* and *m*. Comparing the first and second patterns in the third row of Fig. 4, we see that both start at approximately the frequency positions of *a* and terminate at those of *u*. They have the same first formants, but different second formants. The first pattern, with a gradual frequency change for the low formant, is heard as *æu*; the second pattern, with a sudden frequency change in the low formant, is heard as *æl*. The difference between a vowel, *u*, and a consonant, *l* is here due to a difference in rate of change of formant 1 only.

Now let us compare the second and third patterns (also in the bottom row of Fig. 4). They exhibit similar sudden changes in formant 1 — changes that apply to resonants in general — but they differ in the rate of change of formant 2: the second pattern, with a slow rate of change in formant 2, is heard as *æl*; the third pattern, with a more rapid rate of change of formant 2, is heard as *æm*. (It should be noted that if neither the quality of *l* nor that of *m* is very clear here, it is because the consonantal resonance of formant 1 (at the extreme right of each pattern) was set at an intermediate point between that of *l* and that of *m* — a resonance that can serve moderately well for both *l* and *m* but is not quite right for either. This intermediate resonance was chosen for the special purpose of demonstrating the effect of rate of change. With a slightly higher formant 1 frequency (for *l*), a better *l* would be heard; with a slightly lower formant 1 frequency (for *m*), a better *m* would be heard.)

[Sound recordings corresponding to the nine spectrographic patterns of Fig. 4 were played.]

It will be important to define precisely the articulatory movements which, in actual speech, produce the frequency shifts we have been discussing. Some of the relations between articulation and acoustic pattern are well-known,¹ and further possibly fruitful speculations might be attempted. For the present, however, it may be sufficient to note that in sound sequences, certain movements which change the volumes

¹ The present position, in regard to the relationships between articulation and sound spectrograms, is given by: M. Joos. *op. cit.*; H. K. Dunn. The calculations of vowel resonances, and an electrical vocal tract. *J. Acoust. Soc. Amer.*, 1950, 22, 740-753; P. Delattre. The physiological interpretation of sound spectrograms. *Publ. Modern Lang. Assoc.*, 1951, 66, 864-875; and P. Delattre. Un triangle acoustique des voyelles orales du français. *French Review*, May 1948, 21, 6.

and openings (hence, the resonant frequencies) of the mouth cavities (see Fig. 4) might correspond either to vowels or to consonants depending on whether they are made slowly or rapidly.

6. SOME SPECULATIONS

We should like to turn now, to some speculations about the ways in which the acoustic patterns may be related to the linguistic structure of the language.¹ We have described briefly two experiments on the characterization of the voiced and voiceless stops by means of transitions and bursts of noise. These results (for the single vowel *a*), together with those of a related experiment on second-formant transitions as cues for the nasal resonants, can be displayed in a 3 by 3 array based on articulatory features (Fig. 5).

When the acoustic patterns are thus arranged, one finds that the acoustic data seem to fall naturally into place, with the distinctions among columns (place of articulation) being given by the transitions of the second formant, and the distinctions among rows (manner of articulation) by three markers, namely, rising transitions of the first formant for the voiced stops, bursts of noise for the unvoiced stops, and a steady resonant portion of low intensity for the nasal resonants.

[The playback sounds corresponding to the nine syllables shown in Fig. 5 were played, first row by row, and then column by column.]

The details of such an array will differ for the different vowels, but acoustic similarities among linguistically related sounds seem to be the rule.

The data on bursts and transitions tempt one to further speculation about the perceptual process and its possible dependence on a set of binary choices. That is, Fig. 3 shows that a burst of noise preceding a vowel was always heard as *t* when the center frequency of the burst was high, but otherwise as *p* or *k*:

$$\text{Bursts: } \begin{cases} \text{High (+)} = t \\ \text{Low (-)} = p \text{ or } k \end{cases}$$

Also, as may be seen in Fig. 2, minus transitions of the second formant were always heard as *b* (or as *p* in another series of

¹ See reference at p. 548, note ², above.

tests with second formant transitions and "forced" choices among the voiceless stops), whereas plus transitions were heard as either $d(t)$ or $g(k)$, depending on the vowel. Thus, ignoring for the moment the distinction between voiced and voiceless stops which would, in any case, be given by other cues:

$$\text{Transitions: } \begin{cases} \text{Plus} = t \text{ or } k \\ \text{Minus} = p \end{cases}$$

This provides a possible basis for deciding among p , t , and k by only two binary choices when bursts and transitions are both available as cues:

$$\begin{aligned} p &= -- \text{ (low burst, minus transition)} \\ t &= ++ \text{ (high burst, plus transition)} \\ k &= -+ \text{ (low burst, plus transition)} \end{aligned}$$

Whether or not this means that satisfactory stop consonants can be synthesized simply on a "plus-minus" basis without attention to the exact placement of bursts, or the precise degree of transitions, must remain a rather speculative question until explicit tests of this point are made.¹

We have discussed elsewhere the significance which we attach to the apparently simple fact that spectrograms of many familiar sounds yield distinctive "pictures."² This seems to imply a parallelism between auditory and visual perception of patterned information sufficient to justify one's thinking about, and experimenting with, auditory patterns (after conversion into spectrograms) as if they really were visual patterns, and hence subject to all of the laws of visual patterning. This is, in fact, the rationale of our research procedure. In working with speech sounds, however, we are dealing with a special category of auditory patterns which have been correlated in the listener's experience with the motor gestures used in speaking, gestures which are not always related in a simple one-to-one fashion to the acoustic patterns they produce.

¹ We should, perhaps, point out that the kind of binary scheme being considered here differs in several respects from the system put forward by Jakobson *et al.* R. Jakobson, C. G. M. Fant, and M. Halle. *Preliminaries to Speech Analysis*. Technical Report No. 13, Acoustics Laboratory, Massachusetts Institute of Technology, May, 1952.

² F. S. Cooper, A. M. Liberman, and J. M. Borst. The interconversion of audible and visible patterns as a basis for research in the perception of speech. *Proceedings of the National Academy of Sciences*, 1951, 37, 318-325; and F. S. Cooper. Spectrum analysis. *J. Acoust. Soc. Amer.*, 1950, 22, 761-762.

Some of our data — in particular the results of the experiment on bursts as stimuli for *ptk* (Fig. 3) — suggest that the listener's perception is more closely related to the articulatory gestures than to the acoustic patterns.¹ This is to say, for example, that certain acoustic stimuli which differ but little, or not at all, may nevertheless be perceived quite differently if the patterns to which they belong happen to be produced by very different kinds of articulatory movements.

We should like to make the further general observation that we have come to doubt the wisdom of searching too eagerly for the acoustic invariants of speech, insofar as the term "invariants" may be taken to imply a one-to-one correspondence between successive acoustic features of the sound stream and the phonemes of the message.² For the perception of some speech sounds, as we have pointed out earlier in this paper, it seems that the acoustic unit (or pattern) must include at least a consonant-vowel syllable.

7. SYNTHESIS OF WORDS

In conclusion, we shall consider the synthesis of connected speech. It was natural that we should attempt, even at this early stage in our research, to synthesize sentences, not by copying spectrograms of human speech but rather by free-hand drawing, using principally the limited information that we had obtained about a few phonemes. Figure 6 shows the hand-drawn spectrogram of the sentence.

A PLAYBACK CAN TALK BACK. This spectrogram, which is one of many we have synthesized without having previously seen the corresponding spectrograms of human speech, produces highly intelligible speech when converted into sound by the pattern playback.

Little need be said about syllabic durations and loudnesses here; they are almost as unvarying as the pitch is monotone! Only the first syllable is markedly shorter than the others, and the third is less intense. As to the vowels, we may note that they use only two formants and that the frequencies of

¹ For further discussion on this point see: reference 5; and P. Delattre, F. S. Cooper and A. M. Liberman. Some suggestions for teaching methods arising from research on the acoustic analysis and synthesis of speech. Institute of Languages and Linguistics, Washington, D.C., Monograph Series Number 2, 1952, pp. 31-47.

² See reference 3 at p. 548, note ¹, above.

those two formants benefit from extrapolations of the cardinal vowels (the only ones on which we have worked systematically) to the vowels of American English.

The most interesting features are those that furnish the cues to the perception of the consonants. These cues seem surprisingly simple, especially when we recall the apparent complexity of spectrograms of human speech:

- p* The burst of noise corresponding to the explosion of *p* has a low frequency when in sequence with a "clear *l*" (one whose second formant is relatively high).
- l* The length of the *l* in clusters such as *pl*, *kl*, etc., is less than half the length of an initial or intervocal *l*. The first formant transition between a resonant (*l*) and a vowel (*eI*) is so sharp that it can be omitted in the painting. A better example of this abrupt transition can be seen in the fourth syllable *kæn* between the first formants of *æ* and the resonant *n*.
- leI* The beginning of formant 2 in *eI* is bent toward formant 2 of *l*. This would apply to any vowel in sequence with an *l*: the formant 2 vowel transition is always "in continuity" with the formant 2 resonance of the *l*.
- b* Formant 1 of the vowels preceding and following *b* have minus transitions. (Minus means that the transition falls to, or rises from a lower frequency than that of the steady state of the vowel.) This minus transition applies to all voiced stops (*b d g*) and seems to make an important contribution to voicing. Formant 2 of the vowels preceding and following *b* also have minus transitions. This cue distinguishes labial stops (*p b m*) from dental or velar stops (*t d n, k g ŋ*). It should be noted that the minus transition of the vowel that precedes the intervocalic *b* is not indispensable — only that of the vowel which follows *b* is essential — but it helps in perceiving the word as an English word, since consonant anticipation is a characteristic feature of English.

- æk* Formant 1 of vowel *æ* in *bæk* ends in a very slight minus transition; this cue contributes to the voicelessness of the stops (*p t k*). However, the unvoicing effect is mainly produced, as in all the voiceless stops, by the burst of sound. Formant 2 of the vowel ends in a marked plus transition. This cue, with the vowel *æ*, distinguishes velar stops (*g k ŋ*) from dental or labial stops (*t d n, p b m*). The frequency of the main burst for *k* is located just above the frequency of formant 2 of the contiguous vowel, whatever the vowel may be. This cue aids in distinguishing from the other voiceless stops (*p, t*), but it probably contributes even more to the voicelessness of *k*.
- kæ* All that applies to *æk* on formant 1 transition, formant 2 transition and burst frequency, also applies to *kæ*, but in reversed sequence. The distance from the *k* burst to the vowel in *kæ* is sufficient to produce an effect of aspiration. In *æk*, the burst was placed close to the vowel in order to obtain the very implosive effect of the first element in a geminate; in absolute final, as in the last word of the sentence (*bæk*), the *k* burst is well separated from the preceding consonant.
- æn* Formant 2 of *æ* ends in a small plus transition. This cue, with *æ*, distinguishes dental stops (*n d t*) from velar and labial stops (*k g ŋ, p b m*).
- n* Formant 1 of *n* is lower than formant 1 of *l* (*plel*). Formant 2 of *n* is "in discontinuity" with the formant 2 transition of the contiguous vowel. Both these cues distinguish nasal stops (*m n ŋ*) from oral stops (*p b, k g, t d*).
- ɬk* As was the case for the *k* sounds of *bæk* and *kæn*, the voiceless effect in *t* and *k* of *ɬk* are due principally to the bursts and in lesser measure to the reduced transitions of formant 1.
- t* The *t* burst is always high, but with the vowel *ɔ*, this burst does very little to distinguish *t* from the other voiceless stops (*k p*). More important, with *ɔ*, is the large plus transition at the beginning of formant 2. This cue serves, in fact, to distinguish the dental stops from the velar or labial stops with the vowel *ɔ*.

k Exactly the opposite occurs in the case of *k* with *ɔ*. Here, the small plus transition at the end of formant 2 — a minor cue with *ɔ* — does little to distinguish *k* from the other voiceless stops (*p t*); for *k* with *ɔ*, this distinction is due mostly to the frequency position of the burst. The frequency of the burst, as for *k* with other vowels, is centered just above formant 2. The space between the the vowel *ɔ* and the *k* burst is shorter than in an absolute final (e.g., the *k* of the last word, *bæk*), but longer than in a geminate implosive such as that of the third syllable, *bæk*. The *k* of *tɔk* is also implosive, but is not in a geminate.

bæk The last word *bæk* is similar to the third word except for the longer space between the vowel and the *k* burst. The increased space gives the effect of a "release."

It may be useful to return to the *k* of *bæk* to compare it with the *t* and *k* of *tɔk*. Two cues are used in each case, but with important differences in their relative contributions as distinguishing factors: For *t* with *ɔ*, the main cue is the transition; the burst is minor. For *k* with *ɔ*, the main cue is the burst position; the transition is minor. For *k* with *æ*, both the transition and the burst are good cues. This explains why the *k* in *kæ* is more distinct than the *t* in *tɔ* or the *k* in *kɔ*. To add distinctness to the *t* and *k* of *tɔk*, a transition in the third formant can be used.

[Recordings of this sentence, and of other words and sentences, were played].

The synthesis of connected speech, such as that we have just presented, was not a goal in itself; rather, its purpose was to test the experimental results obtained with the phonemes we have studied thus far, and, in general, to assess whatever progress we may have made toward an understanding of the acoustic bases for the perception of speech.

8. APPENDIX¹

SYNTHESIS OF THE SYLLABLES OF MI DO LA

(Text of the London Demonstration)

The sounds will be displayed visually on a linear scale of harmonics [see any row Figs. 7, 8, 9], with the lowest frequencies at the bottom, and including the first 30 harmonics of a fundamental tone of 120 cps. Time is represented as moving from left to right. Intensity is shown by the width of each harmonic.

The first 30 harmonics to be used in our speech reconstruction will be played, as they are displayed visually in the three dimensions of frequency, time, and intensity [Row A].

Naturally, selected tones from the ones just heard can be combined to produce pleasant chords [B1].

But this type of chord does not occur in speech. Chords found in speech are mostly of a dissonant type [B2].

The three syllables MI DO LA, to be synthesized in small steps, will first be played fully reconstructed [B3].

The recomposition of these three syllables will start from the simplest elements in their vowels. (The two horizontal bars that are visible for *i*, *o*, *a*, correspond to formant 1 (the lower band) and formant 2 (the higher band) of these vowels).

Listen to three pure tones at the center frequencies of the second formant for *i*, *o*, *a* [C1].

Listen to three pure tones at the center frequencies of the first formants for *i*, *o*, *a* [C2].

Let us play together both notes of *i* [C3], both notes of *o* [C4], and both notes of *a* [C5].

The intelligibility is low, but the vowels can already be recognized.

Vowel formants for a male voice at 120 cps usually are composed of about three harmonics, and not just one as above. We shall add below and above the original notes the closest harmonics to each one. That will give us six notes per vowel, three for each formant.

Six notes that yield an *i* sound will be played successively [D1, D2]; then simultaneously [D3, D4]. (D4 should have

¹ A disk or tape recording of this, and the other sound demonstrations described in the paper, may be obtained, at cost, by writing to the Haskins Laboratories, 305 East 43rd Street, New York, U.S.A.

nearly the same sound as D3. The difference is that with the D3 type of drawing the distribution of intensity among the harmonics can be drawn with more precision, while with the D4 type, the drawing can be done more rapidly.)

Six notes that yield an *o* sound will be played successively. [E1, E2]; then simultaneously [E3, E4].

Six notes that yield an *a* sound will be played successively [F1, F2]; then simultaneously [F3, F4].

Compare the degrees of intelligibility for *i*, *o*, *a*, when composed of two notes [G1], and when composed of six notes for each vowel [G2].

Let us now reconstruct the syllable MI. Listen successively to the notes for a formant 1 and for a formant 2 that compose a nasal resonance such as that of a nasal vowel said with closed mouth [H1].

The same notes will be played simultaneously [H2].

The nasal resonance is not very intelligible by itself; but note that the corresponding resonance, produced in isolation by mouth, would not be very intelligible either.

Now, let us play in sequence, the nasal resonance and the vowel *i* [H3].

The result is intelligible, but poor, because the transitional part between the two sounds is not there. If we connect the nasal sound and the vowel *i* by a rapidly rising frequency for the second formant, the intelligibility is greatly improved [H4].

Some additional nasal resonance at a place where it is "in discontinuity" with the transition of the second formant will further increase the intelligibility [H5]. (This acoustic discontinuity seems to correspond in articulatory terms to the raising, or lowering, of the velum, which makes a sudden change in the acoustic pattern. "Continuity," on the other hand, would be required in passing from an oral consonant to an oral vowel, as in passing from *l* to *a* in the syllable LA [see K4].

Let us now reconstruct the syllable DO.

In order to perceive a *d* before *o*, we could imitate an explosion, composed of a brief burst of sound at a high frequency, including in the burst 5 or 6 contiguous harmonics. Listen to three such bursts in quick succession [J1].

Let us play one such burst before the vowel *o* [J2]. The

resulting consonant sounds voiceless. We shall add voicing in the form of a fundamental tone at 120 cps. Listen to such a fundamental tone [J3].

Let us play the burst with voicing before the vowel [J4].

The result is still quite poor. Let us try formant transitions. From *d* to *o*, the first formant has to rise [J5].

The second formant has to fall sharply in frequency [J6].

Let us play both transitions together [J7].

The results are superior to those obtained with a voiced burst of sound [J4], indicating that the perception of *d* before *o* depends perhaps more on the transitional effects than on the explosive effects.

We could add the burst to the transitions, but it will make little difference: compare DO with and without the burst [J8, J9].

Even the voicing seems redundant. Its perception may come largely from the rise of the first formant: compare DO with and without voicing [J9, J7].

Let us now reconstruct the syllable LA.

For the *l*, we have a resonant sound of the same type as for the *m* in MI. Only the mouth is more open, so the first formant will be higher for *l* than it was for *m*. Listen to the notes of *l* in succession [K1].

Listen to them played together [K2].

Heard this way in isolation, it would be hard to recognize the resonance of *l*. But so would it if the sound were made in isolation by human voice.

Let us sound the *l* resonance and the *a* in succession [K3].

The result is already satisfactory. But it can be improved if some transition is added between the steady state of *l* and the steady state of the vowel. This transition, in contrast with that of *m*, must be "in continuity" with the resonance of the second formant of the *l* [K4].

Let us sound the three syllables in succession: MI DO LA [L1].

From the point of view of linguistic perception, we cannot improve very much on this. We could however add a third formant to DO and LA, in order to equalize the "voice" of the three vowels (*i* having more high notes than *o* or *a* in its two essential formants) [L2]. (Proper transitions in the third formant will often improve the intelligibility of the

consonants and, indeed, the third-formant transitions appear in some cases to make truly important contributions to the perception of these sounds).

We could further improve the realism, the naturalness, of the three syllables by adding voice formants in the high-frequency region where, it seems, one aspect of voice differentiation is located [L₃].

Note, however, that the use of only two formants gives a satisfactory degree of intelligibility. Compare the three syllables, played in reverse order [L₃, L₂, L₁].

The final visual patterns of MI DO LA that you have seen and heard [L₁], are hand-painted schematic spectrograms that show only the principal acoustic cues necessary for the perception of these syllables. Spectrograms made from human pronunciation of MI DO LA would, of course, be considerably more complex, and the important acoustic cues would in most cases be more difficult to see and specify.

CORRIGENDA

It was not possible in most cases to supply authors with proofs. They are therefore not responsible for any further errors which may not have been spotted by either the General Editor, or the Assistant Editor, or one or other of the gentlemen who have so kindly helped, and whose names are listed in the Preface on p. lxxii.

- p. 7, 6 lines from bottom: *for* defininition *read* definition
- p. 10, line 2: *for* Fréi *read* Frei
- p. 14, line 9: *for* und- *read* und
- p. 14, line 10: *for* entwicklung *read* Entwicklung
- p. 38, 6 lines from bottom: *for* dh. *read* d.h.
- p. 40, 8 lines from bottom: *for* formé *read* formées
- p. 50, 8 lines from bottom: *full stop at end of line*
- p. 50, last line: *full stop before* 'Order'
- p. 58, line 19: *for* (and *read* and
- p. 65, line 5: *for* e.g. Finnish *read* , e.g. Finnish,
- p. 81, line 9: *for* be, *read* be
- p. 81, line 10: *for* perform, *read* perform
- p. 81, line 16: *for* constructions, *read* constructions
- p. 81, line 19: *delete the whole line*
- p. 81, line 20: *delete* (Buyssens), *at the beginning of line* — *for* phenonena, *read* phenomena
- p. 88, 7 lines from bottom: *for* ist, *read* ist
- p. 96, line 12: *for* acquitions *read* acquisitions
- p. 98, line 20: *for* Finn, *read* Finn.
- p. 99, line 3: *for* puô *read* può
- p. 106, 7 lines from bottom: *for* effect *read* affect
- p. 107, line 16: *for* effect *read* effet
- p. 109, line 16: *for* chiaro-scuri *read* chiaroscuro
- p. 109, 7 lines from bottom: *for* linguistic *read* linguist
- p. 109, 12 lines from bottom: *for* Vendryes *read* Vendryès
- p. 122, 16 lines from bottom: *for* forms. *read* forms
- p. 125, 12 lines from bottom: *for* târsil aramiş *read* târsil, aramiş
- p. 125, 11 lines from bottom: *for* imis *read* imiş
- p. 125, 10 lines from bottom: *for* it *read* 'it
- p. 127, line 17: *for* Malayapolynésienne *read* Malayopoly-nésienne

- p. 128, line 24: *for* soviel *read* so viel
 p. 129, 10 lines from bottom: *for* a *read* à
 p. 130, line 1: *for* subjectif *read* objectif
 p. 134, line 12: *for* Romanian *read* Roumanian
 p. 139, line 21: *for* ē *read* ē
 p. 140, line 2: *for* Gothic *read* , Gothic
 p. 142, 2 lines from bottom: *for* proto-Anatolian, *read* proto-Anatolian
 p. 143, line 14: *for* vāti *read* vāti
 p. 143, line 18: *for* *dm²Htós *read* *dāmHtós
 p. 143, line 19: *for* *d²mHtós *read* *dāmHtós — *for* *gr²Htós *read* *grāHtós — *for* *g²rHtós *read* *gārHtós
 p. 149, last line: *for* l'interpretation *read* l'interprétation
 p. 152, line 21: *for* perché *read* perchè
 p. 158, last line: *for* Pederson *read* Pedersen
 p. 163, line 27: *for* 'Erz,' *read* "Erz,"
 p. 168, line 7: *for* perché *read* perchè
 p. 168, line 22: *for* Vendryes *read* Vendryès
 p. 174, 4 lines from bottom: *for* ou *read* où
 p. 177, line 3: *for* (als *read* als
 p. 178, 4 lines from bottom: *for* Some control of lexical meaning is thus part of the frame in which to test the formal signals of structural meanings. *read* Some control of lexical meaning is thus part of the frame in which to test phoneme contrasts or lexical form. Lexical meaning as such, however, forms no part of the frame in which to test the formal signals of structural meanings.
 p. 187, line 2: *for* â *read* à
 p. 188, line 12: *for* par *read* pas
 p. 192, last line: *for* selbsterleben *read* selbst erleben
 p. 198, line 6: *for* of their *read* and their
 p. 205, 12 lines from bottom: *for* francais *read* français
 p. 208, last line: *for* criterium *read* critérium
 p. 210, line 15: *for* unheathily *read* unhealthily
 p. 220, line 13: *for* sohr *read* sehr
 p. 225, 14 lines from bottom: *for* gewissen *read* gewisse
 p. 225, 9 lines from bottom: *for* Die *read* die
 p. 226, line 6: *for* als *read* , als
 p. 239, 15 lines from bottom: *for* "Primitiv" sprachen *read* "Primitiv"-Sprachen
 p. 241, line 15: *for* apriori *read* a priori

- p. 245, line 17: *for* rapporte *read* rapporto
 p. 245, line 24: *for* razione *read* ragione
 p. 245, line 26: *for* razione *read* ragione
 p. 245, 6 lines from bottom: *for* è *read* , è
 p. 251, last line of text: *for* definine *read* define
 p. 257, line 17: *for* Thescholastics *read* The scholastics
 p. 257, 10 lines from bottom: *for* Spaniard *read* Spaniard
 Sanctius
 p. 259, line 10: *for* Folg. *read* Idg.
 p. 267, line 24: *for* Innere *read* innere
 p. 270, 13 lines from bottom: *for* qualifier *read* qualifiers
 p. 274, line 11: *for* Is *read* If
 p. 274, line 12: *for* word " ? " *read* word".
 p. 277, line 7: *for* Stricke *read* Stücke
 p. 278, line 9: *for* Band *read* Bau
 p. 278, line 21: *for* (Man hofft) *read* (Man hoffte)
 p. 279, line 18: *for* englischen. *read* englischen
 p. 280, 9 lines from bottom: *for* und *read* auf
 p. 280, last line: *for* Wissenschaft *read* Wissenschaften
 p. 287, 12 lines from bottom: *for* classications *read* classifications
 p. 290, line 18: *for* fil *read* fi'l — *for* harf *read* harf
 p. 291, signature: *for* M. M. GOTTSTEIN *read* M. H. GOTTSTEIN
 p. 292, line 7: *for* designates *read* designs
 p. 292, 3 lines from bottom: *for* iamberla *read* iambér-la
 p. 293, line 4: *for* classifications *read* classification
 p. 293, line 19: *for* iamberla *read* iambér-la
 p. 294, 17 lines from bottom: *for* distinguishing *read* distinguishing
 p. 295, signature: *for* CHARLES W. FRIES *read* CHARLES C. FRIES
 p. 296, last line: *delete*) *at end of line*
 p. 297, line 9: *for* must *read* must —
 pp. 302, 304, 306: *for* GENERAL LINGUISTICS *read* GENERAL LINGUISTICS (A2)
 p. 306, line 13: *for* hist *read* hist.
 p. 307, line 2: *for* selbstständigen *read* selbständigen
 p. 343, line 11: *for* Widerspiegelung *read* Widerspiegelung
 p. 349, line 19: *for* philosphischen *read* philosophischen
 p. 350, line 20: *for* Durschnittsindividuum *read* Durchschnittsindividuum
 p. 352, line 12: *for* Steinzel *read* Stenzel

- p. 354, line 8: *for to duration read to serial extent or duration*
 p. 354, line 11: *for That which has read That has*
 p. 354, line 12: *for over read for*
 p. 357, line 12: *for metallurgistes read métallurgistes*
 p. 358, line 1: *for êtres tirée read être tirées*
 p. 359, heading: *for H. H. GOTTSTEIN read M. H. GOTTSTEIN*
 p. 360, signature: *for H. H. GOTTSTEIN read M. H. GOTTSTEIN*
 p. 367, 13 lines from bottom: *for sūmo read sūmo*
 p. 368, line 1: *for farstanden read farstandan*
 p. 377, line 3: *for Sgan read Sgau*
 p. 377, 14 lines from bottom: *for Klot read klot*
 p. 378, line 10: *for The special read The principal special*
 p. 378, signature: *for E. R. HAMP read E. P. HAMP*
 p. 388, line 21: *for mutuations read mutations*
 p. 388, line 22: *for makes read make*
 p. 391, line 23: *for behaviour read behaviour*
 p. 394, line 4: *for versuche read versuchte*
 p. 394, line 15: *for Sicherheit read Sicherheit*
 p. 397, 11 lines from bottom: *for colleagues read colleagues.*
 p. 405, line 7: *for français read français et en alsacien*
 p. 405, line 7: *for phonemes read phénomènes*
 p. 405, line 20: *for phonème read phénomène*
 p. 405, line 26: *for phonème read phénomène*
 p. 406, 5 lines from bottom: *for e read e*
 p. 406, 5 lines from bottom: *for d à a read a à a*
 p. 413, last line: *for l'idée read " l'idée*
 p. 414, line 13: *for d'education read d'éducation*
 p. 414, line 21: *for Iavhé read Iahvé*
 p. 422, line 19: *for Bdeluttomai read bdeluttomai*
 p. 433, line 19: *for The read . The*
 p. 435, signature: *for M. BROWN read MARGARET A. BRYAN*
 p. 450, line 20: *delete colon*
 p. 450, line 22: *for Elementarverwandtschaft read " Elementar-verwandtschaft*
 p. 478, 3 lines from bottom: *for confronté read confrontés*
 p. 478, 7 lines from bottom: *for Jour read jour*
 p. 482, 4 lines from bottom: *for Grm. read Germ.*
 p. 482, 2 lines from bottom: *for aus/wes- read aus-/wes-*
 p. 500, line 23: *for wih read with*
 p. 503, last line: *for ἡδ-ιστος read ἡδ-ιστος*
 p. 504, line 9: *for lit read lit.*
 p. 505, 5 lines from bottom: *for Krahé read Krahe*

- p. 506, line 14: for "Herrscher read "Herrscher"
p. 506, line 18: for Κόρς-αγος read Κόρρ-αγος
p. 510, line 26: for 739 read 749
p. 510, line 29: for isis read i sis — for his read hic
p. 510, line 33: for Pol read pol
p. 511, line 16: for Virodonicem read Viridovicem
p. 511, line 17: for consilio read concilio — for iis read his
p. 512, line 13: for Agents read Agens
p. 512, 6 lines from bottom: for Mem. read Mém.
p. 516, line 4: for Κύριος read κύριος
p. 528, line 27: for kennt. read kennt?
p. 531, footnote 2: for Mesopotmaniens read Mesopotamiens
p. 545, line 4: for way read war
p. 554, second footnote: for ¹ read ²

1. 250. The 25th of May "Friday" 1880.
1. 251. The 26th of May "Saturday" 1880.
1. 252. The 27th of May "Sunday" 1880.
1. 253. The 28th of May "Monday" 1880.
1. 254. The 29th of May "Tuesday" 1880.
1. 255. The 30th of May "Wednesday" 1880.
1. 256. The 31st of May "Thursday" 1880.
1. 257. The 1st of June "Friday" 1880.
1. 258. The 2nd of June "Saturday" 1880.
1. 259. The 3rd of June "Sunday" 1880.
1. 260. The 4th of June "Monday" 1880.
1. 261. The 5th of June "Tuesday" 1880.
1. 262. The 6th of June "Wednesday" 1880.
1. 263. The 7th of June "Thursday" 1880.
1. 264. The 8th of June "Friday" 1880.
1. 265. The 9th of June "Saturday" 1880.
1. 266. The 10th of June "Sunday" 1880.
1. 267. The 11th of June "Monday" 1880.
1. 268. The 12th of June "Tuesday" 1880.
1. 269. The 13th of June "Wednesday" 1880.
1. 270. The 14th of June "Thursday" 1880.
1. 271. The 15th of June "Friday" 1880.
1. 272. The 16th of June "Saturday" 1880.
1. 273. The 17th of June "Sunday" 1880.
1. 274. The 18th of June "Monday" 1880.
1. 275. The 19th of June "Tuesday" 1880.
1. 276. The 20th of June "Wednesday" 1880.
1. 277. The 21st of June "Thursday" 1880.
1. 278. The 22nd of June "Friday" 1880.
1. 279. The 23rd of June "Saturday" 1880.
1. 280. The 24th of June "Sunday" 1880.
1. 281. The 25th of June "Monday" 1880.
1. 282. The 26th of June "Tuesday" 1880.
1. 283. The 27th of June "Wednesday" 1880.
1. 284. The 28th of June "Thursday" 1880.
1. 285. The 29th of June "Friday" 1880.
1. 286. The 30th of June "Saturday" 1880.
1. 287. The 1st of July "Sunday" 1880.
1. 288. The 2nd of July "Monday" 1880.
1. 289. The 3rd of July "Tuesday" 1880.
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1. 309. The 23rd of July "Monday" 1880.
1. 310. The 24th of July "Tuesday" 1880.
1. 311. The 25th of July "Wednesday" 1880.
1. 312. The 26th of July "Thursday" 1880.
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1. 314. The 28th of July "Saturday" 1880.
1. 315. The 29th of July "Sunday" 1880.
1. 316. The 30th of July "Monday" 1880.
1. 317. The 31st of July "Tuesday" 1880.

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