# TRAVAUX

DU

# CERCLE LINGUISTIQUE DE COPENHAGUE

VOL. XX

## Typology and Genetics of Language

Proceedings of the Rask-Hjelmslev Symposium,
held at the University of Copenhagen
3rd – 5th September, 1979

*Edited by:*

Torben Thrane
Vibeke Winge
Lachlan Mackenzie
Una Canger
Niels Ege

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# Preface: The Rask-Hjelmslev Symposium

The University of Copenhagen celebrated its 500th anniversary on June 1st 1979. It seemed appropriate that the subjects in which academic activities were proposed to celebrate it should include linguistics. Not only is linguistics one of the fields, if not *the* field within the humanities, in which the University of Copenhagen has reached international repute, it is also a discipline that has had a continuous tradition in Denmark going back certainly as far as Martinus de Dacia, one of the key-figures in Modistic theory, in the 13th century. It also seemed appropriate that the approach to be taken in such an activity should try to encompass the topics and issues in linguistics which have had a central standing in the history of the subject as it has been explored by scholars associated with the University. Such reflections lay behind my suggestion that the University should hold an international symposium under the title

> *Typological and genetic relationships between languages: a linguistic symposium in memory of Rasmus Rask and Louis Hjelmslev, on the occasion of the 500th anniversary of the University of Copenhagen, June 1st 1979*

Apart from my own department, nine others seconded the proposal and a committee was set up by the organizing departments with a view to drawing up a programme. Members of the committee were Henning Andersen (Slavic), Birthe Arendrup (East Asiatic Languages) Una Canger (Linguistics), Niels Ege (Linguistics), Eli Fischer-Jørgensen (Phonetics), Ole Mørdrup (Romance), Th. Damsgaard Olsen (Nordic), Karl-G. Prasse (Egyptology), Peter Springborg (Arnamagnean Institute), Torben Thrane (English; convener), Vibeke Winge (German).

The symposium was planned to last three days, and for practical purposes the dates were set for September 3rd-5th, with the following programme:

1. Rasmus Rask's position in genetic and typological linguistics
2. Louis Hjelmslev's position in genetic and typological linguistics
3. Naturalness as a principle in genetic and typological linguistics
4. To what extent can genetic-comparative classifications be based on typological considerations?
5. Essential criteria for the establishment of linguistic typologies
6. Summarizing discussion

The following scholars were invited to introduce and discuss these six topics:

1. (Chairman: Eli Fischer-Jørgensen)
   Marie Bjerrum, University of Copenhagen (MB)

Hreinn Benediktsson, University of Reykjavik (HB)
R.H. Robins, University of London (RHR)
2. (Chairmann: Ebbe Spang-Hanssen)
Francis J. Whitfield, Berkeley (FJW)
Sidney M. Lamb (SML)
Jørgen Rischel, University of Copenhagen (JR)
3. (Chairmann: Niels Ege)
Wolfgang U. Dressler, University of Vienna (WUD)
Roger Lass, University of Edinburgh*
Wolfgang U. Wurzel, AdW der DDR, Berlin (WUW)
4. (Chairman: Una Canger)
Søren Egerod, University of Copenhagen (SE)
Eric P. Hamp, University of Chicago (EH)
Eugénie J.A.Henderson, University of Londen (EJAH)
5. (Chairman: Jørgen Rischel)
Eugenio Coseriu, University of Tübingen (EC)
John M. Anderson, University of Edinburgh (JMA)
Hansjakob Seiler, University of Cologne (HS)
6. (Chairman: Henning Spang-Hanssen)
Henning Andersen, University of Copenhagen

Each scholar invited to introduce a topic was asked to prepare a typescript of his contribution which was then sent to the scholars invited to discuss the same topic well in advance of the symposium, whereas the introducers were given no advance copy of the discussants' contributions. The main reason for adopting this procedure was a wish to ensure some degree of coherence around each topic without rendering further discussion superfluous.

In each session the three main contributions were followed by a panel discussion among all the invited scholars, and by an open discussion where members of the audience, consisting mainly of Danish professional linguists and students, were given the opportunity of intervention.

The organizers would like here to thank all the invited scholars for the seriousness and dedication they showed throughout the symposium. I shall refrain from protesting the high academic standard of their contributions. The present volume speaks for that much more eloquently then can be done here.

Likewise they wish to thank Dr. Lachlan Mackenzie, Amsterdam, who undertook the unenviable task of keeping the minutes of the panel- and open discussions.

Finally they wish to thank the Danish Research Council for the Humanities, the Tuborg Foundation, The British Council, and Deutsches Kulturinstitut for their generous financial support without which the symposium could not have been held.

Torben Thrane

* Unfortunately, Roger Lass was prevented by illness from coming to Copenhagen. His contribution, however, is included in the present volume, but it was not available during the symposium.

# Editors' introduction

After the symposium described in the Preface an editorial committee was set up, consisting of Una Canger, Niels Ege, Lachlan Mackenzie, Torben Thrane, and Vibeke Winge, with a view to publishing the proceedings. Matters of editorial policy were decided by the full committee, but the specific editorial tasks were assigned to Vibeke Winge (the contributions in German), Lachlan Mackenzie (panel- and open discussions), and Torben Thrane (contributions in English, overall coordination).

Two points of editorial policy deserve comment. Although the panel and open discussions were recorded on tape, so that a fuller and more detailed reproduction of them could in fact have been made, it was decided by the editors to present a strongly edited and condensed version, focusing on the main points raised, for two reasons. Firstly, much of the discussion consisted of restatements and alternative formulations of points treated in detail in the main contributions. This is the reason, also, why the discussion following Henning Andersen's introduction to the summarizing discussion is not included. Secondly, a high degree of editing was required in any event if a coherent text was to be produced, owing to the natural fact that different contributors would raise new points and reopen old ones in a not necessarily directly reproducible order. The most important consequence of this editorial decision is that the actual wording of the discussion-sections is the responsibility of the editors, except in one or two cases where direct quotation is indicated.

The second point concerns the decision to print individual bibliographies and lists of references at the end of each contribution. It was felt to be desirable to maintain the unity of such highly topic-orientated bibligraphies as those of Marie Bjerrum and Søren Egerod, despite the amount of bibliographical overlap between papers that this decision obviously gives rise to.

Finally the editors wish to thank the Linguistic Circle of Copenhagen for permission to publish these proceedings in *Travaux du cercle linguistique de Copenhague*, and to thank the Danish Research Council for the Humanities whose grant (j. nr. 515-20068) made the publication possible.

*Copenhagen, December 1979*

Una Canger
Niels Ege
Lachlan Mackenzie
Torben Thrane
Vibeke Winge

# 1 Rasmus Rask's position in genetic and typological linguistics

## *Marie Bjerrum:* Introduction

I wish to thank the organizers of this linguistic symposium for their invitation to give an introduction to the discussion of typological and genetic relationships between languages by speaking about Rasmus Rask's position in genetic and typological linguistics. After my doctoral dissertation (Bjerrum 1959) on Rasmus Rask's Essays on the Danish Language I did not for some years deal with the topic of Rasmus Rask because I had other things to do; but after Hjemslev's death in 1965 nobody was found able to finish the Commentary on the Rask letters, planned by Hjelmslev who had published the letters in two volumes (Rask 1941). As I had done preparatory work for Hjelmslev I took it upon myself to complete the Commentary as well as was practicable. A catalogue prepared by Hjelmslev of Rask's more than 200 unpublished manuscripts with a minute description of each, was printed as addenda to the Commentary and published in Rask (1968). It is to be regretted that Thomas Markey, who has lately written about Rask, did not mention the Commentary with the manuscript catalogue in his bibliography to the latest edition (in *Amsterdam Classics in Linguistics* 2 (1976)) of the English translation from 1843 of the revised Swedish edition from 1818 of Rask (1811). And now, after several years away from Rask studies, I have consented to give an introductory lecture on Rasmus Rask's comparative linguistics, despite the fact that I am a philologist and far from being a comparative linguist. Therefore: what I can do is to give an introduction to a discussion among the learned comparativists.

Rasmus Rask lived from 1787 to 1832; his major works date from the periods 1811–1818 and 1824–1832; in between he made his long Asiatic journey. He was educated in 18th century linguistics and philosophy, his successors were 19th century romanticists and geneticists, but Rask was the individual genius in between in linguistic thinking.

It seems rather difficult to demonstrate how Rask's linguistic theory is really to be understood. Learned men like Louis Hjelmslev and Paul Diderichsen tried to understand it, and their efforts have led to different results. Hjelmslev (1951) considered Rask a typologist, Diderichsen (1960) considered him a geneticist, and they never came to an agreement. Hjelmslev never responded to Diderichsen's attack on his typological interpretation of Rask's linguistics. I had the misfortune to be involved because Hjelmslev's point of view appealed to me, although at the same time I was not unable to concur with Diderichsen; as for Diderichsen, he was unable to realise that Rask might be studied in different ways, depending on the eyes which read, and depending on the fact that the distinction between typological and genetic linguistics, later on so sharply outlined, was not at Rask's time a reality.

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Here I must mention that it is an intricate problem to determine whether Rask held a clear theory of linguistic investigation or not. From the very beginning of his linguistic research he had no elaborate theory; he took over the methods of his predecessors in etymological research, but he tried to make them more stringent and to sort out inconsistencies. This may clearly be seen from the introduction to his prize essay on the origin of the Icelandic language (Rask 1818). If one considers any single work from Rask's hand, it may be difficult to grasp a specific theory behind it. In the prize essay for instance one may point out different views, also views opposed to each other. When I speak about Rask's linguistic theory I mean a not too restricted main view from which to understand his linguistic work, books and manuscripts, as a whole. I think that some of the discrepancies between Hjelmslev and Diderichsen in their concepts of Rask's linguistic work may be due to the fact that Hjelmslev regarded it as a whole, in its intirety, whereas Diderichsen saw it as individual parts, work by work.

In my opinion Rask was above all a typologist; his aim was to establish connections between languages based on typological affinity, whether or not they were genetically related. In his prize essay he treated genetically related as well as genetically unrelated European languages, and he was aware that some of these languages were older than others; what he tried to find, however, was not necessarily the oldest language, but the language that might be regarded as the typological basis for the Icelandic language with respect to morphemic and phonemic structure. He stopped with Latin and Greek, languages which he found were representatives of this basic linguistic system, and he eliminated languages displaying a typologically different structure, for instance Finnish, Hungarian, Greenlandic. He also eliminated Celtic because at that time he had not yet observed the fundamental correspondences between Celtic and other Indo-European languages. The prize essay may be interpreted as a genetic investigation, but to do so is in my opinion to consider it in only one of its aspects, not to grasp it as a whole. Rask's comparative linguistics was in its methods purely typological, but he had in the prize essay a genetic aim: he was in search of the mother tongue of Icelandic. Therefore the essay may be interpreted as well typologically as genetically, but that is to Rask two aspects of the same thing. The more he worked on especially non-Indo-European languages and tried to discover affinities between them, the more his descriptions came to be based on typological comparison alone. He gradually disengaged himself from the genetic aspect of his studies and endeavoured to formulate general principles and methods of comparative linguistics. This can be seen from a brief lecture on the philosophy of language which he wrote in 1831, and which forms only a rough outline of a theory and is in no way a fully elaborated philosophy.

Hjelmslev has laid down his concept of Rask in his 'Commentaire sur la vie et l'œvre de Rasmus Rask' (1951). In this paper Hjelmslev takes as his first starting-point Rask's own account of his linguistic theory as outlined in 'A Lecture on the Philosophy of Language' (1831). Hjelmslev came to the conclusion that Rask tried to expose the grammatical and phonemic structures or systems of language, a conclusion which is based not only on Rask's brief lecture, but also on Hjelmslev's profound knowledge of Rask's work. Hjelmslev maintains that comparative linguistics was the main subject of Rask's studies, and that to him the structures or systems of various languages must be amenable to description by the same methods in order to be comparable. The large collection of uncompleted and unpublished manuscripts from Rask's hand shows us that throughout his life he was preoccupied with synchronic descriptions of languages from all over the world. Hjelmslev points out that nobody has given a complete description of Rask's work because nobody has known the contents of all these manuscripts. That is his second starting-point for a new view on Rask's linguistics; he had acquainted himself with all Rask's works.

In the paper referred to above, Hjelmslev gives a statement of Rask's position in comparative linguistics, an evaluation which differs radically from the general conception. 19th century linguists, dominated by the genetic or historical point of view, only saw the genetic aspect of Rask's comparative work. Regarding Rask's life and works as a whole Hjelmslev, as a 20th century linguist, was able to conclude as follows: Rask was not the founder of historical linguistics, but of comparative linguistics. His thoughts on language are fundamentally different from those otherwise characterizing the 19th century. The history of languages did not interest him, only their systems and their structure. The aim of his comparison of languages was not to establish genetic relations, but to give typological classifications of languages. His classsification of the Indo-European languages, for instance, is a classification on typological criteria. Yet it has been possible to interpret it as a genetic one, and therefore Rask has been called the founder of 19th century historical linguistics. But a family of languages to Rask was not a group of languages which might be set out in a genealogical tree, but rather a system of systems. So far Hjelmslev. He expresses himself more categorically than reported here; I am convinced that he is right from his point of view. Rask was a typologist.

Diderichsen had not the same general view on Rask as Hjelmslev; in that respect he was a 19th century linguist. Let me go into a little more detail. Diderichsen wrote his book on Rasmus Rask and the grammatical tradition after having read my interpretation of Rask and Hjelmslev in my dissertation, and it was translated into German in 1976. In the chapter on Rask and the History of Language ('Rask und die Sprachgeschichte') we find his diverging opinions. It is of little use to repeat Diderichsens opposition here, for his passionate *engagement* makes it exaggerated and inaccurate. I prefer to mention a few central passages from Rask's works which have been differently interpreted by Hjelmslev and Diderichsen.

What does Rask mean when he claims that languages change (*se changent*, *sich ändern*)? Diderichsen interprets these words in the same way as Jacob Grimm did: a language changes in time, and we can follow the change, for instance, by observing a particular word at successive moments through the history of the language. Hjelmslev, and to my mind rightly, understands these words to mean that one system has replaced another system, but how and why is of no interest. You compare two different systems when comparing for instance Icelandic and Danish. Icelandic has not developed into Danish, but the system of the Danish language has replaced and may be deduced from the Icelandic language system; Icelandic has four cases of the nouns, Danish two, which in their manifestations correspond to certain Icelandic case manifestations.

What does Rask mean when he says that two languages are related? Diderichsen takes it for granted that the meaning is as follows: both languages have developed from a common parent language; that is to say, they have evolved in the Darwinistic sense of the word. What Rask really meant was that two related languages can be traced back to the same system; or, in Rask's words: they can be explained from the same source. The meaning is in fact that they can be logically deduced from the same common system, as in the case of French and Italian from Latin, for instance.

Speaking about Rask's comparative linguistics it is of importance to emphasize the stringency of his methods. Certain rules must be strictly observed in the process of derivation or deduction of language systems from each other. One rule is that a more simple system, for instance the Danish system, must be deduced from a more complex system, in this particular case the Icelandic system. Another rule is that mutual correspondences, grammatical and phonemic, must be observed. Rask does not speak about correspondences but of accordances and phonetic similarity. He demands regularity in these accordances, evidence of the strictness of his methods, and that is why his accordances can be transformed into correspondences in the sense of stages in grammatical or phonetic development, as Jacob Grimm and the later geneticists did. When Rask speaks of transitions or changes of speech sounds, this may of course be understood from a historical point of view, as Diderichsen did, but that would be to grasp only one aspect of it. Rask's sound correspondences were prior to Grimm and ought not to be judged by the standards of historical liguistics after Grimm. Rask's transitions of speech sounds are just correspondences: Danish *e* corresponds to Icelandic *ei*, Danish *ø* to Icelandic *au* and so on; how and why is of no primary interest. Rask may also turn the correspondences the opposite way: Icelandic *æ* corresponds to Danish *æ* and *ø* (*mæla, færa to mæle, føre*).

It must be considered quite impossible to reassume Rask's linguistics. His linguistics is history. What we can do is to read him with an unbiassed mind and admire his ability to catch sight of the system of a language even if he had only poor texts at his disposal. Rask based most of his work on written texts, and linguistic descriptions of such texts might be troublesome if the texts were orthographically bad, that is to say: if the written text did not represent the spoken language in question in a clear way. One-to-one-correspondence between written and spoken language, between grapheme and phoneme, was desirable, but of rare occurrence. Now and then Rask also worked with informants. That was the case, for instance, when he wrote his Lappish grammar (Rask 1832). He had a description of the Lappish language before him, Knud Leem's Lappish grammar (1748), in which the Lappish sounds were written in Norwegian in the sense that Leem had tried to render the Lappish sounds by means of orthographical rules applicable to Norwegian. Rask had difficulties in identifying sounds that Leem had written in different ways, and therefore, in order to acquaint himself better with spoken Lappish, he had a Lappish teacher sent to Copenhagen. His genius had not failed, however. In many cases his study of the orthographically defective texts had already led to results that were now confirmed by his informant. Incidentally, all Rask's orthographical work must be regarded as closely connected with his comparative linguistics.

It is to be regretted that Louis Hjelmslev did not live long enough to write his book on Rask's life and work; in that case we should have had *the* book on Rask. I also regret that what Diderichsen wrote about Rask's life and work in *Rasmus Rask und die grammatische Tradition* is not quite fair to Rask's theories and personality. Unfortunately Thomas Markey has got Diderichsen's book as his main source for what he wrote on Rask's life and work in his introduction to the edition of Rask's *A Grammar of the Icelandic or Old Norse Tongue* from 1976. Still, Markey's account of what for his purposes he calls Rask's three major works is, in my opinion, instructive and profitable. The works are the Icelandic grammar from 1811 (*Vejledning*), the prize essay, printed 1818, and the revised Swedish edition of the Icelandic grammar, printed in the same year; in the last of these Rask corrected some of his statements from 1811, for during his work on the prize essay he had arrived at other views of certain details. Markey also gives much information on Rask's scholarly position in relation to Jacob Grimm, Franz Bopp and other contemporaries, and he gives an account of works on comparative linguistics in which Rask is mentioned and his works evaluated. But I think the historical aspect has asserted itself too much in Markey's account. I would not, for instance, call Rask's Icelandic grammar (1811), the first historical grammar, but the first systematic one, a rationalist grammar like his later grammatical works. When in the preface to the book Rask states that many things in the Danish language could be better understood if one knew Icelandic, in grammar as well as in vocabulary, he first and foremost intended to emphasize the *merit* of his book, not its historical character; it was no easy matter at that time to publish an Icelandic grammar.

The idea that Danish grammar might be explained on the basis of Icelandic inspired Rask to a work of quite another type, namely the essay on Danish grammar (Rask 1820) where he suggests that the terminations in Danish should be explained from the Icelandic. This paper was prepared simultaneously with the Icelandic grammar and is discussed by me in my dissertation as Rask's first comparative work; it may be read historically, but it is in fact a typologically comparative work, in which he emphasizes his wish to deduce the Danish grammatical system from the Icelandic, not to describe how the Icelandic system has developed into the Danish system in the course of time.

Knowledge of the Danish language at the beginning of the 19th century is necessary for the proper understanding of Rask's books and manuscripts; they are not easy to read for foreigners, not even to a Dane of our days. I should be very pleased if a young competent Danish linguist would undertake the task to read Rask with fresh eyes and an open mind, not weighed down by too much knowledge of historical linguistics after Rask. That might lead to interesting results. But it would demand a first-hand knowledge of Rask's life and works, which can only be obtained by studying his books, manuscripts and letters.

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## *Hreinn Benediktsson:* Discussion*

The topic of this first session of the symposium is not an easy one. The reason is the equivocal status, at this early stage in the history of modern linguistics, of the general concepts involved. But it is true also, in part, because of the huge amount of material left by Rasmus Rask, both the published and, especially, the manuscript part of it. Rask's achievements during his short life of only forty-four years are indeed overwhelming and astonishing.

In view of this, I certainly agree with Dr. Bjerrum's remark that Rask may "be studied in different ways, depending on the eyes which read" (1980:10). This is true, in particular, if one embarks upon the task of comparing Rask's thoughts with subsequent, nineteenth- and twentieth-century, trends in linguistics. In the case of early linguistic work, ancient, medieval, or early modern, such a comparison is indeed tempting and as a rule rewarding, though great caution and circumspection is always in place. For, if one yields to the temptation of reading more into an early text than actually *is* there, the number of different conclusions is bound almost to equal the number of different readers.

As Dr. Bjerrum rightly remarked, what we can do, and what it is imperative we do, in order to come to a more precise understanding of Rask's position in genetic and typological linguistics, is to read his work "with an unbiassed mind" (1980:13), for there is indeed a lot to read!

As regards Rask's methodology, in general, I should like to emphasize that, in my opinion, it is all too easy to pay too much attention to his own pronouncements, such as in his brief 'Lecture on the Philosophy of Language' (Rask 1932–35:II 373–8). We must remember that this short essay dates from very late in his life, less than a year and a half before he died (*viz* from June, 1831); that the occasion was an anonymous and, according to Rask, no doubt invidious attack in a newspaper, dealing with 'the spirit which characterizes a certain dispute among our learned scholars' (see Skårup 1960:104); and that Rask's contribution is clearly polemical in some of its statements. Besides, in general, one must bear in mind that the person most capable of analyzing a written work and presenting the general lines of such a work is not necessarily the author himself.

Accordingly, to come to a more precise understanding of Rask's position, the only way is to study and analyze the whole body of his work. But, of course, not all of what we find in his works is of equal significance for promoting this understanding. For, one thing is what a scholar-author actually *does* in his own original, creative work; another thing is what, according to him, *could*, or perhaps even *should*, be done, especially when at the same time he declares that he is not going to do it! Also, only by studying Rask's work from beginning to end is it possible to obtain a

* *Editors' note.* In this paper single quotation marks around quotations indicate that the passage quoted has been translated into English by Professor Benediktsson, whereas double quotes indicate that the quotation is given in the language of the original.

clear view of the gradual development and possible change of his aims, thoughts, and methods; for, in spite of the obstinacy or stubbornness which is traditionally taken to have characterized Rask's later years, no scholar – no human being – is immutable.

In what follows I shall discuss mainly one problem, the question of the exact delimitation or circumscription, in Rask's work, of what we now call the Indo-European family of languages or what Rask called alternately the Caucasian, the Sarmatic, the Japhetic, or the European race or family of languages; this will inevitably involve also the examination of his principles of classification. The problem has to do with the classification of Armenian, Albanian, and the Celtic languages; it concerns also Etruscan and the Dravidian languages.

Armenian is perhaps the simplest case. There is never any doubt that it is Indo-European, according to Rask. In the prize essay (Rask 1932–35:I $325^{9-21}$) he treats it as a separate class within the family, on a par with Thracian (*ie* Greek and Latin), Lettish (*ie* Baltic), Slavonic, and Gothic (*ie* Germanic), and even presents two perfectly correct Armenian-Icelandic word correspondences ($65^{21}$). But traditionally (Pedersen 1932:xxix), and no doubt rightly, this is regarded simply as a stroke of luck, rather than as the outcome of systematic study. For, later, Rask repeatedly classifies Armenian as an Iranian (or Median) language (*viz* in two letters to Professor P. E. Müller, from Petersburg, dated June 11, 1818, and January 29, 1819; Rask 1941:I 313–8, 379–89), until in his treatise on the Zend language, the manuscript of which (NkS 4° 149 $c^{56}$) was finished in October, 1821 (Rask 1932–35:III 201), he appears to correct this error and revert to the view expressed in the prize essay: for, in one passage in this treatise, dealing with a list of Modern Persian "radical words ... evidenctly derived or corrupted from Zend and not Sanscrit", he appears to contrast Armenian, just like Sanskrit, Greek, etc., with Iranian, saying: "I am well aware that several of these words may be compared with Sanscrit, nay some of them appear even in Armenian, Greek, Sclavonian & Icelandic; but the Persians have evidently adopted them from the Zend" (Rask 1932–35:II $164^{8}$–$166^{14}$). This shift of view would appear to agree well with a remark in a letter from Tabriz, dated April 10, 1820, as well as in the diary, to the effect that 'I have also had better opportunity here than elsewhere of acquiring knowledge of Armenian', *viz* from a number of Russian-speaking Armenians he had met there (Rask 1941: II 13; 1968:187).

Yet, in a later manuscript (NkS 4° 149 $c^{120}$), dating probably from about 1825 and dealing, among other things, with the Iranian class of languages, Rask – 'strangely enough', as Hjelmslev says in his commentary – includes Armenian (Rask 1932–35:II $219^{11}$–$220^{9}$, III 223, 228), contrary to what he is supposed to have done four years earlier.

The fact, however, seems to be that even in the treatise on the Zend language Rask's position is by no means unequivocal. For, in another passage, pointing out that Zend has certain consonant letters (such as *f*) which are wanting in Sanskrit, he proceeds to remark that "the Armenian, which is known to be a very old and radical language on the boundaries of ancient Media, has also the *f*" (and the other consonants in question) and that "Fārsī also, the other immediate neighbouring tongue to the old Media, has all these letters in genuine Persian, not Arabic, words preserved even till this day"; he concludes: "This coincidence in sound with the other Iranian languages, and difference from the Indian ones, seems strongly to reclaim the Zend from India to the old place assigned to it" (Rask 1932–35:II $150^{8}$–$158^{1}$). In other words, here he seems to regard Armenian as one of the "other" Iranian languages, though he may of course have been in doubt.[1]

This case, though simple, is typical. Rask's argument is purely typological: it has to do with the structure of the set of letters, or the "system of sounds", as he also calls it, both in this passage and elsewhere (Dan. *Lydsystem*). In 1825, on the other hand, his criteria are more elaborate, *viz* (1) the absence of genders in nouns, (2) the 1st pers. verbal ending *-m*, and (3) a number of basic word correspondences.

Celtic and Albanian represent a much more complex case. The traditional view, as expounded for instance by Holger Pedersen (1932:xx–xxii, xxviii–xxix), is that, because of insufficient data, Rask made the serious error in the prize essay of excluding the Celtic languages from the Indo-European family (see, especially, Rask 1932–35:I $103^{1}$–$^{8}$), while his only remark about Albanian ($323^{22}$) seems to indicate that he regarded it as an Oriental (that is, Semitic) language; and further, that he corrected these errors only a few years later, *viz* as regards Celtic, in the letter of June 11, 1818, to Professor Müller, in which he unreservedly lists Celtic among the Caucasian Languages, and for Albanian, in the letter of January 29, 1819, where he considers Albanian as belonging to the Illyrian stock of the Thracian class of languages (Rask 1941:I 315, 384). Yet, as regards Albanian, we see, in Holger Pedersen's words (1932:xxix, note 1), 'a strange relapse back to the earlier view ... in a review from one of the last years of Rask's life', *viz* from 1829.

However, this does not exhaust the references. Thus, posterior to the two letters written in Petersburg to Professor Müller, in both of which Celtic is unreservedly listed as belonging to the Caucasian or Sarmatic race, there is the manuscript (NkS 4° 149 $c^{2}$) of the treatise on the affinity of the Icelandic language with the Asiatic tongues, written in Tiflis at the end of 1819 (Rask 1932–35:II 1–45), in which Rask is much less unequivocal. In this treatise there are several statements on Celtic, all indicating hesitation; thus: 'The similarities [of Albanian] with the Celtic class of peoples do not appear to be so significant as fully to justify ... the setting up of Albanian simply as a Celtic language. ... The Celtic class (...) might perhaps be grouped with the same large European family as the Gothic, Slavonic, Lettish, and Thracian classes; for its ever

1. The fact that, in this passage, Rask calls Armenian "a very old and radical language" ("et ældgammelt Grundsprog" in his Danish translation) does not imply that he regarded it as forming a class of its own, separate from the Iranian class. Thus, according to Rask, Icelandic, for example, was a 'radical language' in relation to Danish, and yet, together with other 'radical languages', it was a member of the Gothic class. In 1825 he calls Armenian 'a separate main language' ("et eget Hovedsprog") of the Iranian class.

remarkable agreements with the Scythian race of languages . . . do not stretch to the inner structure, but only to individual words' ($10^{1-16}$); and further: 'Yet it does not influence our present further investigation of the origin of the large European family of languages as much as one might suppose whether one considers Albanian as belonging to the Thracian or the Celtic class, and whether one groups the Celtic class with the European family or with the Scythian languages' ($11^{4-8}$); and still further: 'But the Celtic class is now so reduced and its basic affinity with the other European languages so unclear that it can by no means be compared with the Gothic, the Slavonic, the Lettish, or the Thracian class; yet, I did not want to pass over this, since in my prize essay I was more inclined to exclude the Celtic class from the European family' ($11^{11-17}$); and finally, Rask concludes in a note: 'This calls for a more detailed investigation, especially of the organization of the Welsh pronouns and verbs and of the Gaelic language structure in general. This much is certain that quite a large number of words agree with Icelandic, and such an old and remarkable class of languages must by no means be left out of a comparison of the European family with the Asiatic languages. But it must be understood that it should first be proved that the Celtic class belongs to the European family' ($11^{18-23}$).[2]

As regards Rask's position in the prize essay, it must be remembered that he was categorical only to the extent directly occasioned by the formulation of the prize subject, *viz* in saying 'that in any case it would be altogether absurd and contrary to all sound language analysis to derive the Gothic class of languages from the Celtic or conversely' (Rask 1932–35:I $103^{3-5}$) and that even though the Celtic languages are 'not without usefulness for word analysis in Latin, French, and the Gothic languages', yet they 'are in no way to be regarded as the source of any of these' ($109^{7-9}$); otherwise, though 'more inclined to exclude the Celtic class', he was less unequivocal, saying only that from the evidence adduced it will be 'easy to convince oneself that between the Celtic languages and the Gothic ones there is in essence no, or at any rate very little, connection' ($103^{1-3}$) and that 'the similarity which might be found in the Welsh vocabulary, as well as certain agreements in derivation and inflection, cannot well be attributed to anything but language mixture' ($103^{5-7}$), that is, borrowing.

A similar uncertainty is apparent in two reviews, almost ten years later. In a review published in 1828 Rask is more inclined to regard Celtic as Indo-European and to consider the agreements between the Celtic and

2. This agrees well with the following passage, dating probably from Rask's sojourn in Petersburg in 1818–19, in a manuscript (NkS 4° 149 $c^{12}$) containing notes on Armenian: 'Armenian appears to be connected with Welsh and thus to confirm the correctness of including Celtic in the large Sarmatic (Caucasian) family of languages (or race)' (Rask 1932–35:III 158–9, 1968:551). The inherent uncertainty becomes clear when it is borne in mind that this passage dates from the period when Rask firsk placed Armenian unreservedly among the Iranian languages; accordingly, as regards the place of Armenian, Hjelmslev is certainly right when he says in his commentary that Rask 'for quite a long time vacillated between different views', though this was true not only of Armenian.

the Northern Asiatic languages, which had been pointed out 'not without reason' (1828:18), as being due to language mixture. But in the review from 1829 referred to by Holger Pedersen, Rask inclines towards the opposite view. This review deals with Etruscan, which he had regarded as Indo-European ten years earlier (*viz* in his letter of January 29, 1819, and in the treatise on the affinity of the Icelandic language with the Asiatic tongues; Rask 1941:I 384, 1932–35:II 9); he then classified it, together with Latin, as a member of the Italic stock of the Thracian class.[3] Now, in 1829, on the other hand, he regards Etruscan as belonging, together with the Finnic languages, to the Scythian race; his main arguments are: (1) the absence of the lenes *b*, *d*, *g* from Etruscan, and (2) the place of the accent on the initial syllable in Etruscan: that is, his arguments are strictly typological. He counts Etruscan among the Illyrian languages, which, in other words, now belong to the Scythian family, not the Indo-European as earlier. Of Illyrian he says, 'there is still a remnant in Arnautic or Albanian ... That this language is connected with the Celtic languages and together with them shows remarkable agreements with the Finnic languages, has already been observed', he concludes (Rask 1834–38:II 420–1).

In other words, the apparent 'strange relapse back to the earlier view' spoken about by Holger Pedersen is not limited to Albanian, but applies to Celtic as well.

In reality, therefore, there appears to have been no 'strange relapse back to the earlier view' in Rask's later years, neither as regards Celtic and Albanian nor even Armenian, as traditionally assumed. On the contrary, the records strongly indicate that Rask in fact never came to a definite and definitive opinion about the classification of any of these languages.

However, what is important to note is that Rask's doubts or uncertainty about the precise classification of these languages persist even after his criteria of classification become relatively precise and definitive. In his letter of January 29, 1819, he says: 'When I wrote my [prize] essay, absolutely no definite system of classification had occurred to me' (Rask 1941:I 382). Neither does his letter of June 11, 1818, which is usually referred to in order to show his awareness of the Indo-European character of Celtic, contain any discussion of his classificational criteria; there is only a bare list of the language classes belonging to 'this (our) race' (the Caucasian). His classification is still tentative; his uncertainty appears clearly from what he says about the classification of human languages in general: 'The human races about which I think I have a clear idea from their languages are: (a) the Caucasian (ours), (b) the Scythian (Greenlandic), (c) the Malayan (Australian), (d) the Chinese (Seric)': that is, altogether four races; and further: 'To this may be added with relative certainty (e) the Negritic, (f) the American; but it is quite possible that there may be more' (315).

3. It must be remembered that at this time 'Etruscan' also included Umbrian and even Oscan and the minor dialects of Southern Italy.

It is not until his letter to Professor Müller of January 29, 1819, that a definite view about the principles of classification appears: 'Secondly, I believe I have not remembered to inform the Herr Professor about my new system for the grouping of all the languages on earth, which I regard as equally necessary in this branch of science (linguistics) as the Linnean in botany, if one is not to get completely lost in the infinite multitude of languages and dialects ... This scheme consists of the following six degrees: Race – Class – Stock – Branch – Language – Dialect' (382) (Dan. *Race*, *Klasse*, *Stamme*, *Gren*, *Sprog*, *Sprogart*).[4]

On the basis of this system of six degrees or levels Rask then proceeds, in the same letter, to divide the languages of the world, not into four or six races as earlier, but into the following eight: (1) the Sarmatic (Caucasian), (2) the Oriental (Semitic), (3) the Scythian (the Polar race), (4) the Seric (the monosyllabic languages), (5) the Meridional (Australian), (6) the Ethiopian (Negric), (7) the African (North African), and (8) the American race.

Rask's first criterion is the number of genders; this is followed by type of declension and conjugation, and these in turn by other features, if necessary. Thus, the definition of the Sarmatic race is that it has 'three genders, on which is based a declension by means of endings, of which each two and two belong together, one subjective, the other objective'; of the Oriental (Semitic) race, that it has 'two genders without declension, together with a strange and complex conjugation with gender inflection'; and so on (382–3). In other words, the criteria are strictly typological.

The types of criterion for the classification of languages appear perhaps nowhere as clearly as they do two and a half years later, in a postscript from Madras, dated September 27, 1821, to a letter to Professor Müller. In this postscript Rask presents a brief report on his studies of the Southern Indic languages (Telugu, Tamil, Canarese, Malayalam, Tulu); earlier (in his letters of June 11, 1818, and January 29, 1819; 315, 384) he had taken these languages to be Indo-European, constituting the Decan class of the Caucasian (Sarmatic) race. Now, on the other hand, he thinks that he has 'made a remarkable discovery, *viz* that these languages ... form a language class of their own basically different from Sanskrit and from our

4. It should be observed, however, that this same system of six levels appears in the manuscript essays (NkS 4° 149 f 7a) entitled 'A Survey of the Scythian Human and Language Race' which was discovered and published by Paul Diderichsen (1960:172, 197–202; see also Rask 1968:585). For orthographic reasons this manuscript must antedate 1819, according to Diderichsen, and he is inclined to date it as early as 1815–16 (1960:194). This early date, however, is most unlikely. Diderichsen's evidence for it is that, to judge from a note in the manuscript, the essay seems to have been intended to be delivered as a lecture in Copenhagen; therefore, according to Diderichsen, it must have been written during Rask's stay in Copenhagen in 1815–16. But since Rask had plans to return to Copenhagen from Petersburg (see Rask 1941:I 349, 353, 360), this evidence seems to be quite insufficient.

Rask's principle appears clearly in this essay; he says: 'In any classification it is a principal requirement that an appropriate gradation is observed, and that the subordinate is not confused with the coordinate' (Diderichsen 1960:198); he then proceeds to apply this basic hierarchical principle to linguistics by stating and defining his system of six levels.

entire human family (...) and showing, on the contrary, basic affinity with the languages of Central and Northern Asia', in particular the Tatar and Finnic languages, and that they thus belong to the Scythian family. As evidence he lists ten characteristics of Decan language structure, eight of which are purely typological, (1) that there are no genders except the natural ones, (2) that the nominal inflection is completely mechanic, (3) that there are post- rather than prepositions, (4) that there are (especially in Tamil) no word-initial consonant clusters, (5) that neuter words (for objects) never have the same form for the nominative and the accusative, (6) that all tenses are inflected by means of the same personal endings, (7) that the verbs have a separate negative inflection, and (8) that the word order is definite and strict and quite the same as in Tatar; the remaining two items are more concrete, *viz* (9) that the genetive ending is *-in* as in Finnish, and (10) that there is considerable word similarity (fifteen examples) (Rask 1941:II 45–7).

Even the last two points are perhaps typological in nature rather than genetic: only the superficial phonetic similarity of the endings and word forms is noted. No attempt is made to establish a regular phonological correspondence, or 'letter transitions' (*Bogstavovergange*), between them; the question is not raised.

It is important to note this, for it was no doubt clear to Rask that superficial phonetic similarity and regular phonological correspondence are two different things. In the introduction to his prize essay he says (forthcoming:57, 1932–35:I 54[13–15]): "What has been said here about difference in the meanings of related words is equally true with respect to the forms of related words, which may also present very great dissimilarities in spite of unquestionable kinship". This appears to show that, to Rask, a regular phonological correspondence was possible without superficial phonetic similarity, as Meillet, for instance, was to stress so emphatically later (see *eg* 1925:29–33). The converse, however, may not be true: Rask may have thought that phonetic similarity, if not due to 'language mixture', necessarily implied phonological correspondence, that is, that phonological correspondence was the wider concept. I have found no statement in Rask to decide the issue. But his later practice may perhaps be taken to point in this direction.

Hjelmslev, in his important Paris lecture, emphasized that the two points of view, the typological and the genetic, the distinction of which was later to become so important, are confused in Rask's work: "La distinction entre le point de vue *typologique* et le point de vue *génétique*, qui est des plus importantes dans la linguistique moderne, n'a pas été faite par Rask; chez lui, les deux points de vue se confondent" (1951:154). And Dr. Bjerrum has pointed out that the two at the time still 'constituted a complex unity' (1959:59; 1980:00).

Yet, in examining Rask's method of language comparison, it is necessary to separate the different aspects of the problem; 'point of view' as a unitary term is not very precise. In particular, it seems useful to distinguish at least two aspects: on the one hand, the aim or object of the classification, that is, what kind of further conclusions it is assumed to permit or what type of additional information it is supposed to imply; on the other hand, the set of principles or criteria employed in order to decide and establish the classification, as well as Rask's own assessment of the relative significance of the different criteria used. Only on this basis can the comparison of his procedure with those of modern genetic and typological classifications become fruitful, and only on this basis does it make sense to pass judgment on the correctness or incorrectness, in each case, of Rask's results in terms of our modern standard of evaluation.

As regards Rask's aim, the most notable feature is his repeated insistence upon the value of language and language classification as evidence bearing upon the early history or prehistory of the peoples concerned; thus, to give only one quotation, he says in a draft of a letter from 1819 (*cf* Diderichsen 1960:137, 142): 'My purpose is, simply and solely, to study the languages, in their varying structure and multifarious conditions, in order thereby to be able to judge of the migrations, relationship, and origin of the peoples concerned, especially in so far as they can be regarded as being related to, or having influence on, the Nordic ones' (Rask 1941:I 369).

At this early period, of course, one cannot possibly expect anything similar to our modern definition of the genetic relationship of two or more languages as having developed out of different dialects of a single language, ultimately resulting from a gradual and continuous dialect differentiation of the original language area; a language classification with implications such as those of the above and similar statements is as close to the modern concept of genetic classification as one may reasonably expect at this time.

As we have seen, Rask uses a number of criteria in order to determine his classification. Some are strictly typological, others are similar to those underlying a modern genetic classification. Rask's assessment of their relative value is also clear in many cases. Thus he says in the introduction to the prize essay (forthcoming:52–3, 1932–35:I $50^{14}$–$51^{7}$): "A language, however mixed, belongs to the same language class as another, when the most essential, most concrete, most indispensable and very first words, the foundations of language, are common to them both... When correspondences are found between two languages in such words, in fact so many of them that rules can be deduced for the shifts of letters from one to the other, a basic kinship is found between these languages; especially when they are matched by similarities in the structure and system of the two languages".

The last statement seems clearly to indicate that there are two different criteria, *viz* 'rules for the shifts of letters' and 'similarities in the structure and system', and that the two do not necessarily go together, but if they do, the assumption of basic kinship is much strengthened. In other words, regular phonological correspondences imply kinship, even when important structural dissimilarities are present, as would no doubt have been the case, according to Rask, for a modern Germanic language like Danish in relation to an old language such as Anglo-Saxon. In such cases

Rask gives clear priority to the regular phonological correspondences, that is, to the genetic criterion. Thus, though the number of genders is Rask's primary criterion in his classification in 1819, there is never any doubt that the Iranian (Median) class of languages belongs to the Sarmatic race, in spite of the absence of genders; he simply inserts a note about this, adding that the absence of genders may be due to language mixture. Neither is there ever any doubt about the Indo-European character of Baltic (Lettish) in spite of the fact that Baltic substantives have only two genders.[5]

But whether the converse is true, according to Rask, that is, whether significant structural similarity is possible without regular phonological correspondences, is uncertain. I have been unable to find any clear and unequivocal statement in Rask. But his practice, as we have seen, is to base kinship upon structural similarity, even alone, but sometimes also accompanied by superficial phonetic similarity of endings and words. This may have been intended to imply the presence of regular phonological correspondences, since, as we saw, phonetic similarity may have implied phonological correspondence, according to Rask. Paul Diderichsen may therefore be right when he says: 'The idea of a systematic structural similarity that was *not* proof of genealogical kinship was for Rask a patent contradiction in terms' (1960:197).

This is the principal hierarchy of the criteria in the prize essay. But somewhat later it may appear as if the priorities have been reversed, although the matter is far from being clear, both because of Rask's sketchy argument, especially in his letters and shorter essays, and because the above hierarchy is cut across by another, which appears at least as early as the prize essay and remains unaltered throughout; in the introduction to the prize essay Rask says (forthcoming:50–1, 1932–35:I $49^{5-17}$): "Now if we want to compare several languages, and if this comparison is to be complete and to enable us to judge of their kinship, age, and other circumstances, we must necessarily take both of these parts [*viz* the lexical and the grammatical parts] of the languages into consideration and, in particular, not forget the grammatical part; for experience has shown lexical agreement to be most uncertain ... Grammatical agreement is a much more certain sign of kinship or basic unity".

To take the ten above-mentioned points concerning the Dravidian languages, this hierarchy opposes points (1–9) to (10), whereas the first hierarchy separates points (9–10) from (1–8), provided that phonetic similarity is taken to imply phonological correspondence as suggested above.

5. It may be observed here that whereas Rask was inclined, in his prize essay (1932–35:I $173^{21}$), to consider the Baltic languages as being closer to Germanic than to Slavonic, Holger Pedersen is of course right when he points out (1932:xxvii) that Rask soon abandoned this view, in that in his letter of June 11, 1818, he groups 'Lettish' and Slavonic together into one class, for which he at that time uses the term 'Sarmatic'. Yet, in the letter of January 29, 1819, Lettish and Slavonic are regarded as two separate classes, parallel to Thracian, Gothic, etc.; still, there may be some doubt, for Rask is uncertain whether to call Lithuanian and Lettish proper two stocks or two branches of the Lettish class (Rask 1941:I 315, 384).

In order to show the difficulties in interpreting Rask's statements about his criteria which are due to their sketchiness, we may quote again part of his statement on the Celtic languages in the treatise on the affinity of the Icelandic language with the Asiatic tongues, written in Tiflis at the end of 1819: 'The Celtic class (...) might perhaps be grouped with the same large European family as the Gothic ... for its ever remarkable agreements with the Scythian race of languages ... do not stretch to the inner structure, but only to individual words' (Rask 1932–35:I $10^{10-16}$). That is, priority seems to be accorded to structural rather than word similarity. But since the question of 'letter transitions' (*Bogstavovergange*) is not mentioned, this may be simply a statement of the second hierarchy rather than of the first with a reversed sequence of priorities.

Somewhat earlier we find a similar situation in Rask's manuscript essay (NkS 4° 149 $c^{16}$; Rask 1968:540) on 'the Oldest Germanic Languages'. This is the essay from which Paul Diderichsen published two paragraphs (1960:143), leaving out, unfortunately, several significant sections; the paper appears to stem from 1816. In the part published by Diderichsen Rask says: 'The *grammatical structure* is the most unfailing sign by which properly to classify languages; for when this structure decays or changes, then the language disintegrates and a new one emerges'. A little later, in the section left out by Diderichsen, Rask says: 'The highly mixed languages [such as English] may also conveniently be arranged according to their *words for the first, simplest, and most tangible notions.* This is the second criterion by which we can classify languages, when the difference between them becomes large and the language structure, having quite disintegrated, fails us'. That is, the grammatical structure is the primary criterion. However, some uncertainty remains, since there is no question of 'letter transitions' and the concrete problem to which the primary criterion is applied has to do, not with a classification based on kinship, but with drawing the dividing line between Old Germanic and Modern German as two distinct languages; the Danish term involved, *inddele*, is ambiguous, either 'divide' or 'classify, group together'.

The clearest statement, probably, is to be found in another section of the treatise on the affinity of the Icelandic language with the Asiatic tongues, *viz* in the section on the Turanian (that is, Turco-Tatar) family of languages, where Rask says:

> 'This inordinately large number of derivatives [in Turanian], especially of the verbs, also agrees with the structure of the Finnic language family (...) but is basically different from the European. It is true that the endings themselves can rarely be compared with the Finnish and Lappish endings proper. But it is important to observe the mere fact that the entire spirit which reigns in both of these wide-ranging language families is the same; this in fact provides every reason to assume basic kinship between the two. If one considers the southern classes of the Finnic family, *eg* the Ugrian, the similarity in words and structure becomes so extensive that no observer can deny it. Some of this, it is true, can be attributed to language mixture. But

> when some day someone undertakes to investigate the letter transitions of these languages and, with their help, to trace many of the correspondences in the languages of the northern tribes, even all the way to Greenland, then it will not be so easy to deny a basic kinship between the Turanian and the Finnic family. All similarities of the Turanian with the European family, on the other hand, must be explained, as is clear from the above, by the mixing and mutual intercourse of the peoples, since their language structures are basically so entirely different' (Rask 1932–35:II $35^{13}$–$36^{9}$).

Here, in other words, the 'letter transitions', which are expressly mentioned, appear clearly to take second place: structural similarity alone 'provides every reason' to assume kinship, even though the endings themselves 'can rarely be compared', and conversely, structural dissimilarity prohibits this assumption; the 'letter transitions', if looked into, may be expected further to strengthen or confirm the assumption of kinship.[6]

To conclude, whether or not it is appropriate to speak of a true reversal of the hierarchy of genetic and typological criteria established in the prize essay, there is at any rate a distinct shift of emphasis from genetic to typological in the choice and ranking of the criteria in Rask's later works, as is witnessed by the relative neglect of the notion of 'letter transitions' posterior to the prize essay. As regards the aim of his classifications, on the other hand, no change is visible in their types of historical implication.

Accordingly, Rask's varying statements about the Celtic languages may simply witness only a vague realization, on his part, that phonetic similarity of individual words, though not due to borrowing, does not necessarily imply regular phonological correspondences and, especially, that he had never come to grips with the question of establishing such regular correspondences between Celtic and the unquestionably Indo-European languages. His position, as regards Celtic, was therefore in reality, as Holger Pedersen rightly observed (1932:xxii), the same as for Finnish, even though his conclusions were at times different. As in the case of Finnish, Rask had observed certain word similarities with the various Indo-European languages, on the one hand, but important structural differences from them, on the other. In his mind each of these two contradictory criteria alternately gained the upper hand, so that he hesitated whether to regard Celtic as Sarmatic or Scythian; the dilemma, as we saw, is solved by regarding, alternately, one characteristic as evidence of kinship, the other as due to language mixture. Though Franz Bopp was soon to provide the definitive proof of the Indo-European character of Celtic (1838), the situation is still, at present, basically the same as regards the question whether or not a fundamental kinship is to be posited between the Finno-Ugric and the Indo-European languages on

6. I am indebted to Professor Henning Andersen for this important reference.

the basis of certain remarkable phonetic similarities especially in some pronouns and verbal endings.

After all, in some respects we still have not advanced so very far beyond Rask.

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## *R. H. Robins:* Discussion

I am grateful for having been sent an advance copy of Dr. Bjerrum's paper on Rask, and I should like to stress its importance and that of the research on which it is based, together with the work of Louis Hjelmslev (1951). Rask is a famous figure in the history of linguistics, in Denmark and throughout the world; but it is always a danger that such figures will come to be known just from one or two items in their scholarly achievement and one or two passages in their total writing, and that the general reader thus gets both a distorted picture of the scholar himself and an inadequate and perhaps inaccurate presentation of the history of the scholarship involved. Sir William Jones has undoubtedly suffered in this way, and Rasmus Rask deserves a better tribute from his successors than that he first formulated Grimm's Law.

Having said this much, and, I hope, having confirmed my estimation of the importance of the theme of Dr. Bjerrum's paper, I would like to suggest that the question whether Rask was a typologist or a geneticist in his studies in comparative linguistics is not perhaps the most fruitful way of framing an investigation into his position in the history of linguistics (Bjerrum 1980:11). In the first place it suggests that a scholar's work is necessarily all of one piece, so that the demonstration of his achievement and intentions in one sphere automatically demonstrates his lack of central concern with another sphere. Cannot Diderichsen and Hjelmslev both be right in their main contentions on Rask, without one invalidating the conclusions of the other? Secondly, and more importantly, one runs the risk of anachronistically assuming that the scholarship of times past was organized and understood as organized in the manner to which we are accustomed today.

This is a sort of 'Whig history' applied to the history of ideas, in which adherents of a particular interpretation or development of theory and method look for support to the work of noteworthy thinkers of past centuries who laboured in the same field as themselves. In politics the English Civil War was not a contest between left and right in twentieth century terms, and in linguistics the controversies between rationalist and empiricist grammarians in seventeenth century Europe were not conducted on the same lines as those between generative and structuralist linguists today, though this is not to say that seventeenth and twentieth century debates do not throw light on each other's thinking.

Today we broadly recognize genetic classifications and typological classifications, based on different criteria and serving different purposes. A typological family and a genetic family may broadly coincide in membership, as is often said to be the case in the Bantu languages (accepting for the moment the separate existence of the Bantu family in historical linguistics). Though this is interesting and may be important, it is contingent, and recognized as contingent by linguists today. But the very unorganized and unsystematic state in which etymological studies, and indeed general historical linguistic studies, were in even as late as the beginning

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of the nineteenth century is shown by what Rask thought it necessary to include in the Introduction to his *Investigation*, notably the rejection of the literal historical interpretation of the Biblical, and of other, creation stories, and of what he calls 'the irrational patriotism' (Rask forthcoming: 43) of claims for this or that language and people to be 'the oldest', claims earlier stigmatized by Leibniz as 'Goropianism', but still evidently considered part of current thinking. One of Rask's motives in establishing systematic lexical correspondences (Rask forthcoming: 45) was to rescue etymology from the parlous state in which it had remained since antiquity.

Are we than justified in expecting from Rask's work, of the greatest historical significance though it is, the sort of clear-cut organization of the subject that we ought to find today? The translator of the Introduction, Niels Ege, writes of the imprecision of some vital terms in Rask's Danish and of the consequent difficulties of their translation. This is especially the case in words translated by 'relationship', 'relatedness', 'affinity', 'kinship', and the like, words used in ways that a modern reader might regard as equivocations on essential points of theory. Much of nineteenth century linguistics scholarship, culminating in the work of the Neogrammarians, was concerned just with the clarification of concepts and the refinement of terminology and, consequently, of method. We cannot expect to find it already achieved in Rask's writings.

The point is that Rask did contribute very greatly to this process. The use that Grimm made of Rask's formulation of what came to be known as *die erste Lautverschiebung* or Grimm's Law in redrafting the phonological part of the *Deutsche Grammatik* between the first and second editions is too well known to require comment. But what is relevant to our discussions at this time is that Rask definitely saw his work in the *Investigation* as a contribution to linguistic history. He stressed the importance of language as evidence for a people's early history (Rask forthcoming: 13, 42), and in making more precise the uses of the term *etymology* he emphasized its historical orientation (25, 31). The significance of regular correspondences ('resemblances'), especially in relation to basic vocabulary and grammatical inflections, is expressly said to be in relation to historical cognation; and whatever criticism we may now make of his view (shared, after all, in relation to Indo-European, by the later Schleicher) that more complex means older, Rask's historical interest in what he was asserting is manifest (51–2).

All this is unaffected by the primitive state of Rask's phonetics (*eg* 67), his bringing in of etymologies from Hebrew into Indo-European (60–1 a legacy from the past?), and his retention of the conception of antiquity that historically Latin was derived directly from Greek (53–4).

Hjelmslev in his 'Commentaire sur la vie et l'œuvre de Rasmus Rask' puts forward strongly his conclusion, based substantially on Rask's 'Lecon sur la philosophie du langage' (Hjelmslev 1951: 7) and the waning influence of romanticism on his thinking (14), that Rask's later interests lay in a version of what would now be called structural linguistics and in typological rather than genetic classification of languages. This certainly provides a convincing explanation of the course of his later years and especially of some apparently strange aspects of his extensive travels in Asia; and had his scholarly life been longer these interests might well have dominated his later research, teaching, and publication. But nothing in this need downgrade Rask's essential contribution to historical, genetic, linguistics, nor to his expressed interest in this branch of linguistics in his earlier years and while he was working on his *Investigation*.

Hjelmslev declares (1951: 12–13) that for Rask languages did not change, they only disappeared, to be replaced by other languages (*ie* by other linguistic systems), citing the disappearance of Latin and its replacement by the modern Romance languages in contrast to the continuance of Greek from antiquity to the present day. One may wonder whether, if we had continuous records of French earlier than the famous Strasbourg Oaths, one could speak so confidently about disappearance and replacement; but certainly Rask did in his *Investigation* (14) write more than once of languages changing to a greater or lesser extent.

Hjelmslev sums up his evaluation of Rask's position in the history of linguistics by his statement (1951: 10) 'La linguistique comparative de Rask n'est pas génétique, mais générale', a passage quoted with great approval by Allen (1953: 102) in his radical proposal at that time for a wholly non-historical theory of comparative linguistics.

However, what appears to be the position as far as Rask is concerned is that language change is not uniform, but that periods of relative stability are followed by periods, in his terminology, of 'fermentation and confusion', in which what one may call new languages emerge from the breakdown of older languages, though we can always relate the later to the earlier languages across such changes, in a way that, of course, one does not even try to relate just typologically similar languages, such as, for example, Sapir's Yana and Classical Greek (Percival 1974: 308–9). This is still, surely, historical linguistics, even though linguists would not look at historical linguistics in this way today.

I would suggest that Hjelmslev misrepresents Rask's position in the history of linguistics in his sharp distinction between comparative and historical linguistics. Rask holds a place of honour in both subdisciplines of linguistics, typological and historical, as we know them today, but for him there was only one, in Percival's words (1974: 311), 'a conception of historical linguistics which was not shared by some of his contemporaries and has not been shared by any generation of linguists since his time'. Such is the fate and the achievement of pioneers. Rask was a pioneer in both historical and typological linguistics, even though, as Hjelmslev justifiably says, his interests were moving in the direction of typological comparison in his later years.

Changes of interest are nothing strange or improper in scholarship, especially when a subject is developing before the eyes and under the influence of a particular scholar. Rask's life and his career were in some respects, as Hjelmslev shows, a disappointment to Rask himself. This was due to his health, to his personal relations with some contemporaries, and to a conflict between his developing interests and the requirements of his patrons. But if Rask anticipated and prepared the ground for the Danish structural linguists Jespersen and Hjelmslev, he also anticipated and prepared the ground for the Danish historical linguist, Karl Verner; and it is wholly right that we should honour his name on this five hundredth anniversary and pay our tributes to Rasmus Rask as a citizen of Denmark and as a distinguished member of the University of Copenhagen.

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## Panel and open discussion

Two main questions were proposed for discussion: 1) What did Rask mean by 'relationship'? 2) What did Rask mean by 'change'?

It was emphasized that Rask's achievement should be seen in the proper chronological perspective. It was not to be wondered at that Celtic, Albanian, and Armenian constituted problems of proper classification, as to some extent they still do, even today. Nor should it be surprising to us that Rask drew no clearer distinction between genetic/historical relationships on the one hand and typological ones on the other, for the distinction was not so cut and dry then as it supposedly is now. He was aware of the distinction – just as he was aware of the much later distinction between synchrony and diachrony – only he used it for different purposes from us. It could be argued, for example, that Rask used typological considerations in order to *establish* genetic relationships and not just – as is our wont – to take typological affinities as a possible indication that genetic relationships may in fact exist, so that actually genetic classification was his ultimate aim.

This is especially true since the criteria he used for establishing relationships differed from work to work. It is clear, for example, that his primary criteria in his later works are typological, as we would say. But in the prize essay the situation is slightly different. There we find that the purely genetic criterion of correspondences between letters takes first place; and there may be a relationship even if no structural similarity can be found, as would be the case with Danish and Anglo-Saxon. He also consistently makes the point that peoples speaking related languages either have lived near each other or have been otherwise in contact, so that he to some extent uses extralinguistic criteria. However, the prize essay may stand apart from his other works in this respect.

And yet it is clear that the possibility of tracing languages back was a real one to Rask, even if the languages to be compared in this respect had to be *extant* languages. The methods of reconstruction, and the possibility of incorporating reconstructed languages among the data, were unknown to him. This may account for the fact that no concept akin to the later *Stammbaum* theory can be found in Rask. His view of language relationships was rather an *areal* one.

It was asked with respect to Rask's view on change what strategies and what kind of arguments he used in order to relate such languages as Danish and Icelandic. Specifically, these arguments concerned the replacement of the four Icelandic case-morphemes by the two Danish ones. Generally, Rask's view of change was held to be two-fold: 1) change understood as the *replacement* of one language system by another; 2) as *language-internal* change, whereby the grammar of, for example, Danish changes over time.

Participants in the discussion were HS, MB, EC, WUW, EH, WUD, JR, JMA, HB, HA, RHR and Eli Fischer-Jørgensen (chairman).

### *Niels Ege:* **Rask and language relatedness**

It is an established fact that the principles and methods expounded by Rask and applied so originally and so consistently by him in comparing the languages of Europe are not themselves entirely original with him.

Scholars before him had observed the occurrence of regular sound correspondences between related languages (*eg* Wachter 1737, Ihre 1769); others had asserted the crucial role of basic vocabulary items in matters of proving or disproving language affinities (*eg* Gatterer 1771); and several had argued the overriding importance of grammatical evidence (*eg* Sajnovics 1770, Gyarmathi 1799).

It is also commonly accepted that others before Rask had already grasped the basic unity of many of the languages now known as Indo-European, had guessed the nature of their relationship, and had, in fact, come very close to sketching the outline of that great family of languages.

What is new in Rask in thus neither his methods nor his results, strange as it may seem.

What distinguishes Rask from his precursors is the relentless stringency and detail with which he presents his case, yielding positive and irrefutable proof.

In fact, Rask himself acknowledges that the end result of his research is rather commonplace. On the very last page of his *Investigation* he writes:

'Admittedly, the result which I have attained is nothing less than new' – according to more recent usage, of course, Rask's words would rather seem to express the exact opposite, but he immediately continues – 'but since any scholar ought to be guided, not by a desire for novelty, but rather by his quest for truth, I gladly renounce all claims to having made any new discovery' (Rask 1932–35: I.328).

In order to assess Rask's approach and the implications of his procedure it is important to realize, first of all, that there never was, and never can be, a clear-cut distinction between grammatical and lexical (or phonological) evidence in comparative linguistics.

This is so because comparing the grammars of various languages comprises two rather different, but interconnected, aspects.

On the one hand, it involves establishing which grammatical systems and subsystems, categories and subcategories are equivalent in the languages in question.

On the other, it means pairing off individual corresponding formatives (paradigms, case endings, etc.) as they occur in the languages under scrutiny. This latter operation is, in fact, nothing but a special case of the standard process of comparing vocabulary items and establishing their equivalence on the basis of recurring sound correspondences, and, as such, is not a matter of grammatical comparison in the strict sense.

There is no question in my mind that to Rask the most important type of evidence of language affinity was neither correspondences of vocabulary nor of grammar (in the strict sense), but correspondences in the formative (or 'lexical') items employed by the language to express grammatical notions.

However, prior to confronting formative X in language A with formative Y in language B, it must somehow be established that formatives X and Y are (or were) systemically equivalent to the extent that it makes sense to compare them at all – just as the comparison of two lexical items, in order to be meaningful, presupposes a common ground of (original) meaning equivalence.

Logically, then, the use of concrete inflectional evidence and the like to prove or disprove relatedness presupposes *a presentation in comparable terms* of the respective systems in which the evidence manifests itself.

In Rask's time there was, of course, as yet no viable system of general or synchronic linguistic description, let alone a universal grammar; in fact, the grammatical descriptions of the languages investigated appeared each in its individual, more or less haphazard, form.

What appears to some as subtle analyses in order to display a common underlying typological system to be used as evidence of original affinity may thus turn out to be rather more like practical steps required for Rask's main undertaking; *ie*, it may be the only way open to him of bringing the data on an equal footing for the purposes of comparison.

Placing ourselves in Rask's day and age, we thus see that his choice of approach to grammatical comparison is not a matter of recognizing typological rather than genetic criteria in questions of language affinity

(Hjelmslev), or of not making the distinction (Marie Bjerrum). Although more to the point, his approach does not even derive from the theoretical issue of diachrony always presupposing synchrony, but rather from the practical problem of deciding, as Rask puts it so well in an important place, 'which individual part in one language corresponds to each part of the other' (Rask 1932–35: I.204).

This is also borne out by the way in which Rask sets up the description of each of the languages he discusses in his *Investigation*. In each case, the description falls clearly into two parts: a section containing a more general, but frequently quite detailed characterization, followed by a more specifically comparative section. The purpose of the former section is stated in one place more generally as that of 'improving the grammar, as far as system and presentation is concerned' (1932–35: I.190), and in another even quite unequivocally as that of 'presenting all languages to the reader from one single point of view, something which is unavoidably necessary in order to realize and judge their similarity or dissimilarity' (1932–35: I.191), *ie*, making possible the actual comparisons of grammatical material, which then follow in the second section.

This is not to say that Rask is not interested in typological matters, or in evidence that bears on his assumption that the languages he discusses show a general tendency to develop from a relatively 'complex' system into a relatively 'simple' one.

But it seems to me quite clear that he is able to draw a sharp line between the two types of argument.

Rask's ability to distinguish between typological and historical affinities between languages appears most clearly from the passage in the *Investigation* in which he discusses the relationship between Germanic and Baltic: '*If* the Lettic languages were available to us from times as remote as the Gothic, *it might not be entirely unreasonable* to derive the latter from the former, and consider the Gothic tribe to have its origin in the Lettic' (1932–35: I.176). From the point of view of Rask's favorite typological thesis it would be more 'reasonable' to explain the 'Gothic' languages on the basis of the 'Lettic' languages, because the latter 'present a system which is so much more complex' (1932–35: I.173).

In other words, typology and history come up with conflicting evidence inasmuch as Lithuanian, although more complex, is so much younger than the oldest stages of Germanic that we know of.

Most importantly: it is quite evident from the way Rask expresses himself here that the historical evidence wins out. That is to say, to Rask deriving language A from language B presupposes that A is younger than B. 'Derivation' and 'origin' are thus to be interpreted historically.

In general, I have no doubt that Rask's comparativism is genuinely *historical*. In this respect I think I am in line with Diderichsen. The *Investigation* abounds in evidence explicitly based on real or alleged historical sequences of events and claims of actual linguistic change. Rask's thesis of the relation between 'simpler' languages and 'complex' languages, the former presupposing the latter, is itself hardly reconcilable with a static, or even neutral, view of language variation; these are differences that involve not only varying usage, but disparities in the very core of the system and are thus, in the end, necessarily due to *changes* in that system.

It seems to me much more doubtful whether Rask had a clear notion of what *genetic* relationship actually involves.

Holger Pedersen and others have maintained that by the term 'Old Thracian' Rask referred to what we today would call Proto-Indo-European. But this is almost certainly not so. It is true that Rask's Old Thracian denotes an *Ursprache*, older than *eg* Greek and not directly attested, but it is obvious from internal evidence in the *Investigation* that it does not correspond to our IE proto-language.

For one thing, Rask claims that 'all the Northern languages of this class [the Thracian] have been completely lost' (1932–35: I.180). If Old Thracian were to mean PIE, this would make sense only under the assumption that by 'Thracian' Rask refers to something like *Ancient* Indo-European, representing – besides Greek and Latin – Proto-Slavic, Proto-Baltic, Proto-Germanic, etc., which have, of course, been 'completely lost'.

But note that 'Thracian' is referred to here as a *class*, not as a family, *ie* on a par with the 'Gothic', 'Lettic', etc. classes.

Add to this that Rask talks repeatedly of Greek and Latin as 'the oldest *and only* remains' of Old Thracian (1932–35: I.178, 180, 323; II.5), and in the follow-up article to his *Investigation* summarizes his findings by stating that the Slavic, Lettic, and, in particular, the Gothic class were seen most reasonably to be derived *from the Thracian class*, 'that being the oldest and most original'. As the entire group of languages makes up one large family, further investigations of the origins of Old Icelandic will involve 'not just the Scandinavian branch ... nor just the Thracian language class, bot both of these *with all that lies in between*' (1932–35: II.8).

More than terminology is at issue here. Rask appears to have vacillated between viewing genetic relationship as a matter of coordinate *vs* subordinate configuration. In actual chronology, of course, Latin and Greek were earlier than the other languages investigated by him. This fact, and the general correlation which he observed between relative age and degree of structural complexity led him to set up a hierarchical model of IE, reflecting the historical development of the languages, not just in two levels – Ancient IE and Modern IE – but with a level for each separate class, its position in the hierarchy being determined, where external criteria of absolute or relative chronology failed him, by the relative 'complexity' of its system.

This is, of course, a fairly muddled vision of genetic relationship between languages as we now understand it, and certainly not amenable to representation in classical *Stammbaum* terms.

But at the same time it is clear how this view of his lends itself to an interpretation of his findings and methods that sees his 'derivations' as predominantly typological, or denies the distinction in his work between the typological and the genetic line of approach.

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# 2 Louis Hjelmslev's position in genetic and typological linguistics

## *Francis J. Whitfield:* Introduction

The honor that you have accorded me in inviting me to introduce this discussion is at least equaled by my trepidation as I set about the task of fulfilling my assignment. I am especially – and naturally – abashed by the knowledge that there are in this audience many students, colleagues, and collaborators of Louis Hjelmslev's – people who have more than once heard him speak in person on many of the questions that will arise in the course of this Symposium. At the same time, I am grateful for this occasion that you have offered me to test my perception and understanding of some of his views against the more intimate and nuanced impressions that you had the enviable opportunity to form of the great linguist at work in this University.

I hope that you will not find it impertinent or presumptuous of me to felicitate you on your concept of an appropriate contribution to the University's celebration of its five-hundredth anniversary. What area of scholarship could be more appropriate than the study of language – in all its aspects – to represent the sponsors of these meetings? And what names better calculated than those of Rasmus Rask and Louis Hjelmslev to recall the whole magnificent history – studded with so many other distinguished names – of the study of language at this University and in this country?

Finally, how appropriate that – in honoring the University – you have also chosen to honor, among others, Louis Hjelmslev, whose devotion to the University is known to us all, if only from his inaugural lecture on appointment to the Chair of Comparative Linguistics. Those who remember him from his student years will remember how he exhorted his fellow-students to take greater pride in their University. Later – but now over a quarter of a century ago – he was to write to a friend:

> I have long since obtained whatever I might wish for in life in the way of honors! For me it has been a question of two things: a good sound basis for scientific work, and a connexion with, and identification with, the institutions that are my native habitat, so to speak, and to which I feel a very strong personal attachment: the University of Copenhagen and the Academy of Sciences and Letters – along with, of course, the institution that I myself took part in creating and forming, the institution that is my fair-haired child: The Linguistic Circle.

Science, to be sure, transcends national boundaries – not least in the eyes of Hjelmslev, whose broad and solid acquaintance with a variety of linguistic schools and linguists from different countries is well attested.

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But it is his very cosmopolitanism that makes such expressions of attachment to his University espeially moving, just as it gives authority to his warm – though not uncritical – appreciations of the work of his predecessors.

Among those predecessors, one – as we shall shortly see – could be looked on by Hjelmslev as being, in a certain sense, a scientific contemporary, facing identical problems and hindered by comparable obstacles in the way towards their solution. It will have devolved on others, more competent than I, to explore in this Symposium Hjelmslev's interpretations of Rask and to try to determine their correctness. Is he right in thinking that our conception of Rask's 'mental set' and aims have been colored and distorted by what he calls 'transcendent' histories of linguistics – histories that make use of criteria and presuppositions foreign to Rask's thought? Have serious misunderstandings resulted from Grimm's displacement of a systematic-comparative view by an historical-comparative view?

These are fascinating questions, and not only for the linguist: they concern – or should concern – the historian of science and all workers in those humanistic disciplines that appear now to be clustering ever more closely around linguistics and to be seeking guidance from the experience of linguistics. Both the interpretations of Rask by Hjelmslev and those by another eminent figure in this University's history – Paul Diderichsen – show us what delicate problems are involved. They leave – as professors are fond of forever telling their students – beautiful opportunities for significant research and, I would add, exceptional opportunities for cooperative, interdisciplinary research.

Fortunately for me, I am not called upon to offer decisive answers to these problems. For whatever my opinion may be worth, I happen to think that Hjelmslev's *general view* finds powerful support in those lecture notes of Rask's that have been given the title of 'A Lecture on the Philosophy of Language'. But the important thing is that, in any case, the study of Hjelmslev's thought necessarily implies study of his appreciation of Rask. In bringing their names together in this Symposium, you have not performed an arbitrary act of piety, but have expressed a sense of underlying continuity that still remains to be explored and interpreted.

Already in 1928, in the *Principes de grammaire générale*, Hjelmslev would be writing about Rask's astonishing anticipation of the requisites for progress in the science of linguistics: an empirical foundation of data from real languages *and* a point of departure taken from the linguistic form itself and not from the ideas that find expression in the linguistic form. Let us put aside the persistent ambiguities connected with that word 'form'. Here, at least, Hjelmslev found inspiration and encouragement for some of his strongest instincts with respect to the study of language. But if Rask was a support, he also represented a challenge. It must be admitted that he is also correct, writes Hjelmslev, in the objections he raises against general grammar: 'It is true that it would require a knowledge of all the languages of the world and a good exposition of their system; and it is no less true that these *desiderata* are far from being realized. We are in the same situation today as in the times of Rask. More than a hundred years of indefatigable labor have changed nothing in this situation of linguistic science. But this is precisely the reason', continues Hjelmslev, 'why we think it necessary to undertake the task. To wait for a perfect and complete synchronic grammar would be to postpone our work to the Greek calends. A beginning has to be made sometime. And besides, we by no means believe in the impossibility of an incomplete induction... From our point of view, Rask was too pessimistic'.

Those who have had occasion to make any particular study of Hjelmslev's thought know well how long he was to wrestle with this question of induction and its implications, how he found himnself 'forced', as he puts it, 'in some degree to invade the domain of epistemology' as he hammered out definitions of such terms as 'induction' and 'deduction' that would fit into the whole glossematic definition-system – and, as he himself admitted, occasionally get himself into trouble with his philosopher-colleagues. I believe that these are not, however, matters that require detailed attention in an initial survey of our subject, and I do not intend to probe them here. Rather, as I have done elsewhere, I would begin more simply by alluding to one of Hjelmslev's favorite maxims, derived from another great predecessor, Vilhelm Thomsen, of whom he wrote: 'He wished to distinguish, and did distinguish everywhere as sharply as possible, between, as he says, "what can be known" and "what must remain only rough hypothesis": between what we know for certain and what we do not know for certain'.

This is, of course, a large order, and there is no reason to believe that Hjelmslev, with all his enthusiasm, ever naively thought it to be otherwise. But it is through his tireless pursuit of this distinction that we may perhaps most easily come to understand the positions he arrived at with respect to typological and genetic linguistics. 'Tireless', I have said – but far from humorless. Consider, for example, the following pontifical pronouncement: 'For your information, the Indo-European proto-language had 1 (one) vowel, no consonants, 2 fundamental accents, and 26 (twenty-six) converted accents. *Those* were glorious times!'

The letter to Hans Jørgen Uldall from which I have drawn this excerpt comes from the no less glorious time of their happy and exciting collaboration, being Hjelmslev's New Year's greetings for the year 1937, 'which', he writes, 'will be remembered as the year when glossematics made its published appearance'. Although it cannot be so remembered, 1937 *is* the publication date of Hjelmslev's essay dedicated to Holger Pedersen on the phonic system of Indo-European, where we find much to interest us. As its title indicates, the essay consists of 'some reflexions' on the subject: these should be seen as guideposts leading to future investigation, not anything to be taken as conclusive statement. But clear as clear can be is the dogged, persistent striving to distinguish between what we know for certain and what we do not know for certain. We know for certain that our reconstructions will – subject to not unimportant conditions – lead to a linguistic *system* (according to the terminology that

Hjelmslev was using at the time); we do *not* have means at our disposal to determine for certain the *norm* in which the system was manifested.

Later, in his little book *Language*, he will – with unhesitating consistency – go so far as to emphasize that we cannot even know for certain whether a linguistic system that we have reconstructed was ever manifested at all – that is to say, whether the reconstructed language was ever used by anybody or whether it was transformed from the very start in the directions implied by the languages on whose comparison the reconstruction rests. This is pretty heady stuff – especially when found in the context of what the reader expects to be a popular introduction to the study of language. (At this point, by the way, I cannot help recalling my own naive astonishment when Hjelmslev told me, during our review of the English translation of the *Prolegomena*, that the book had been conceived and written as an *ouvrage de vulgarisation*. I should add that he said this with a certain apologetic smile. I am reasonably certain that I now well understand what he meant, but I can also easily understand how paradoxical the statement rings on first hearing and how difficult it has been for some commentators to view the *Prolegomena* in this light).

Again, I shall not dwell on questions of terminology. The central message of the essay dedicated to Holger Pedersen is clear, and is made even clearer through references to the longer study, dated the same year, on 'Accent, Intonation, Quantity'. The conventionally styled *phonic* system of Indo-European is, in the end, a *formal* system – a system of 'sounds' only in the sense of being a system of *phonemes* (and, moreover, of Saussure's phonemes, as Hjelmslev understood Saussure to have used the term – not the phonemes of Baudouin de Courtenay, or of Trubetzkoy, or of the 'phonologists'). They are more appropriately called *cenemes* so as to avoid any suggestion that their purely formal definition depends on the phonetic material that may or may not have been used to 'manifest' them.

Holger Pedersen had said that it is not theoretical clarity that has produced the great advances in the concrete domain, but the other way round: it is concrete advances that have given birth to theoretical clarity. (I have sometimes wondered whether certain episodes in the history of mathematics were not perhaps in the back of his mind when he said this). Hjelmslev, in the very act of paying tribute to his teacher, boldly comments: 'we dare to add that it is theoretical clarity, born of acquired discoveries, which alone makes possible the renewal of method necessary – in its turn – for new concrete advances. To be good, the method must be conscious, that is to say, it must be drawn from theoretical considerations founded on empirical facts. It is by such a cyclical formula: empirical discoveries – theoretical discussion – renovated methods – new discoveries, that the evolution, past and future, of our science is defined'.

The statement is a bold one, not only in its friendly confrontation with the dictum of his teacher, but also in its courageous acceptance of what Hjelmslev certainly realized would lead him into philosophical inquiries not commonly regarded as the linguist's concern and to which he was not, I believe, by temperament attracted. At least – as he would sometimes remind his correspondents – he was not a devotee of theory for theory's sake, and theoretical questions for which he could not foresee practical application held little appeal for him.

You will have recognized this idea of the interplay between theory and practice as a constant in Hjelmslev's work. The theory may – and indeed does – change, but the search for firm theoretical foundations is there from the beginning – from the *Principes de grammaire générale* on. And the main line of development is always clear: 1) We set our aim, once for all, at distinguishing what we know for certain from what we do not know for certain; 2) If only as a result of recognizing past errors (such as inconsistencies resulting from a naive empiricism), we are led to an increased awareness of theory and, as we have seen, 'forced in some degree to invade the domain of epistemology'. In particular, we are led to understand how we must look to theory to validate our methods of description – to provide the guidelines for our analytical procedure and to make explicit to ourselves and others the premisses from which our rules of procedure are derived.

The word 'procedure' is still, I know, a red rag in the eyes of many a taurine linguist – at least in my country. I do not know to what extent the publication of Hjelmslev's *Résumé of a Theory of Language* has – as I hoped it might – scotched the notion that a so-called 'discovery procedure' is involved – 'discovery procedure' in the sense of a never-never machine that absorbs so-called 'raw data' and emits a finished analysis without any human intervention's being required. Anyway, it would be easy enough to demonstrate that Hjelmslev was under no such illusions about the glossematic procedure in its final form.

(Nor – I should probably interject here – is the procedure to be viewed as a mere sorting-machine, into which ready-labeled linguistic elements are thrown and out of which they come laundered and displayed in neat array. For where would the ready-made labels have come from in the first place?)

There is no mechanization of scientific work for Hjelmslev, any more than there was for Rask. But neither can scientific work be haphazard. If it is true – as Hjelmslev's 'invasions' into epistemology had convinced him – that there is, from a certain scientific point of view, no absolute *datum*, nothing absolutely 'given'; if it is true that we contribute part, at least, of the 'outside world', that there are no absolute boundaries in that world but that the world is so constituted as to permit its being apprehended by a principle of analyses; and, finally, if the several sciences are seen as so many different *linguistic* (or *semiotic*) nets casting their shadow-boundaries on such a world – then the procedure of *linguistic* science assumes a preeminent importance.

The linguist, whose object of study must be language in general, at which he aims through comparison of individual languages (initially, of course, through comparisons of linguistic processes or 'texts'), has to ensure that his analyses of those languages and texts will yield results that are scientifically comparable, or – to put it differently – he must have a uniform principle of analysis. He must not – at the risk of thwarting the whole purpose of any kind of comparative linguistics – unwittingly compare the apples of one language with the pears of another.

Once this has been clearly seen, it is not hard to understand how Hjelmslev is inevitably driven toward centering his efforts on the task of evolving an appropriate analytical procedure. From the beginning we find in both his published and unpublished papers the complaint, for example, that 'traditional linguistics' has failed to provide us with a clear line of division between semantemes and morphemes. It has even forgotten (what it once knew) how to distinguish consonants from vowels. Both on the side of the expression and on the side of the content, it has not only remained satisfied with older 'transcendent' principles of analysis, definition, and identification, but it has gone on to seek new ones.

Meanwhile, as Meillet was also to observe, an enormous task remained to be performed: that of ordering the facts of language from the point of view of language itself. And in 1934, when he was introducing his students at Aarhus to the theory of morpheme categories, Hjelmslev began by warning them that only a professional linguist could have any idea how little the field had been explored in anything like a scientific manner:

> For the solution of the problem we have almost nothing at our disposal beyond a number of descriptions of individual languages, descriptions of very different degrees of completeness, of very different degrees of clarity, and constructed according to very different methods. So it is clear that the problem cannot be solved in one move, indeed that in great part it simply cannot be solved but can only be set, and that we shall be able to get no further than propounding certain hypotheses and operating with certain probabilities.

As we read these words, we may well feel that Hjelmslev is moving backward – back to the skepticism that he had found unjustified in Rask. Skepticism there certainly is, but I venture to think of it as a skepticism looking *forward* to a refinement of the notion of 'incomplete induction', in which he had seemed to put his trust – a refinement that eventually makes the very word 'induction' seem inappropriate. Over and again, from very early, we find signposts in his notes and unpublished papers of what we can now appreciate, with hindsight, as the direction in which his 'incomplete induction' is leading him. Take, for example, the following, with its gloriously mixed metaphor:

> Even if it is correct that each linguistic state must be viewed by itself alone, still it is valid to search out a grammatical system of such nature that all the individual linguistic states can be fitted into it. Each individual grammatical category that is realized in a given linguistic state must be an instance of a general grammatical possibility that man bears within himself and that must necessarily be realized under certain given conditions. Provided we succeed in finding such a general, Archimedean point, where, like the spider in his web, we can gather all the threads together, where we are aware of all the grammatical

possibilities to be found in human language, and know under what conditions these possibilities become actuality, then every individual linguistic state and every individual linguistic development will be explainable by general laws.

To be sure, the Archimedean point was already mentioned in the *Principes de grammaire générale* (without the spider), but in company with 'an induction as complete as possible, an empirical method':

> The languages of the world seem not to be so different, despite everything, as to make it impossible to reduce their systems to a general formula. If, under different conditions, one linguistic possibility is realized here and another there, that does not excuse us from admitting that all the observed possibilities are still possibilities of language. In our view, then, it is the conditions, more or less contingent, that differ, not the nature of human language itself. A linguistic state is the product of the general possibilities of language and certain particular conditions, and it is the aim of linguistics to distinguish that which, at any moment, is due to general possibilities and that which is due to particular conditions.

And

> . . . if it is true that languages are not everywhere the same, that does not prevent there being elements that are everywhere identical. If this be granted, those elements can naturally be brought to light only by a comparative study, by an induction as complete as possible, by an empirical method.

I do not know whether you will agree with me in sensing a difference between the unpublished and the published passages that I have just quoted. It is enough, perhaps, to be aware that Hjelmslev was continually mulling over and reformulating the concept of the 'Archimedean point', the 'general formula'. And, at any rate, we can surely all agree in seeing how the incomplete induction of the *Principes* has been transformed by the time when Hjelmslev will observe, in the *Prolegomena*, that even if we could imagine as accomplished the humanly impossible work of describing *all* existing linguistic texts

> the labor would be futile since the theory must also cover texts as yet unrealized. Hence the linguistic theoretician, like any other theoretician, must take the precaution to foresee all conceivable possibilities – even such possibilities as he himself has not experienced or seen realized – and to admit them into his theory so that it will be applicable even to texts and languages that have perhaps never been realized, and some of which will probably never be realized. Only thus can he produce a linguistic theory of ensured applicability.

It would be quite outside the bounds of this general introduction to examine in detail how Hjelmslev believed the theoretician could be liberated from the servitude of incomplete induction – which had from the beginning been a kind of *pis aller*. Stated briefly – and therefore perhaps cryptically – the liberation comes through the realization of the full possibilities of *form*, of purely formal methods of description and definition such as Hjelmslev attempted to present most rigorously in his *Résumé of a Theory of Language*, but which are also well known to readers of other works by him. Actually, we have seen that form had always been standing by, waiting to take over a major role in Hjelmslev's theory. In the *Principes*, Hjelmslev had noted that an important legacy from Rask was the doctrine that the grammarian must start from the linguistic form and not from the thought embodied in the form. Saussure and Sapir also, of course, come immediately to mind in this connexion. For our purpose here, it is not so important to follow the notion of 'form' through all the vagaries and ambiguities of usage that the word has undergone, as to realize that form – derived from the 'functions' or special interrelationships between the parts and parts-of-parts of language – represents for Hjelmslev the solution to the problem raised, but not solved, by him in the *Principes* and there traced back by him to Rask, and that *the solution so reached is directly and immediately related to his position on the typology of language*.

To show this, let us return to the matter of Hjelmslev's glossematic 'procedure'. And I would now ask you for a moment to consider the procedure, not in any usual sense of the word, but as a kind of device for producing languages – an unlimited number of languages, all conceivable languages. The idea is not original with me, but can be derived from remarks in Hjelmslev's own correspondence. From this point of view, the linguist – who has sovereignly defined what a language is – may be thought of as sitting in his study and deriving from his definition as many different languages as he pleases. He may then – and probably will wish to – go out into the marketplace and see which of the languages he has invented are actually in use. And – always provided that his essentially arbitrary definition of 'language' is appropriate – he will find a place prepared in his general schema of possible languages for every language that he encounters. He has, in other words, overcome the difficulties with which he had previously wrestled and can be said to know, virtually, *all* conceivable languages and to know them in such a way that they can be scientifically compared. They can be scientifically compared because they are now described uniformly in accordance with what Hjelmslev dubbed the 'empirical principle' – the principle that requires the description to be self-consistent, exhaustive, and as simple as possible.

And in doing this, the linguist has at the same time arrived at his fundamental typology of language. In his 1939 'Note sur les oppositions supprimables' he had asserted unconditionally: 'Without the support of the function of form, any classification is possible, and no classification is necessary. From the point of view of the function of form, one classification alone is revealed as being at once possible and necessary'. Within the context of that essay, the pronouncement is applied to the distinction between linguistic 'oppositions' (in a terminology of those times) and mere differences which, from the linguistic point of view remain simply differences. But there can be no doubt that the dictum has far wider applications so far as Hjelmslev is concerned. And the glossematic procedure, which can be seen as a device for creating all conceivable languages, can in consequence also be viewed as a *flora* – not simply one possible *classification* of languages, but *the* scientifically necessary *typology* of languages. This is why Hjelmslev can relegate 'typological relationship' to a mere short note in the *Résumé*, saying: 'Actually, 'typological relationship' is a superfluous term in glossematics since typologically related semiotics ... are simply semiotics that enter into one and the same class of semiotics'.

This is the typology 'from more penetrating and immanent points of view than have been chosen till now', as Hjelmslev puts it in his popular book on *Language*, where he goes on to declare that

> An exhaustive linguistic typology is, in fact, the biggest and most important task facing linguistics... Its ultimate aim must be to show which linguistic structures are possible, in general, and why it is just those structures, and not others, that are possible. And here it will come closer than any other kind of linguistics to what might be called the problem of the essence of language... Only through typology does linguistics rise to quite general points of view and become a science.

Typological linguistics, then, is not exhausted by the typology of possible languages that is offered virtually by the glossematic procedure. The linguist, like any other scientist in his situation, will be called on to propose and test hypotheses and laws governing the actual manifestation of the structures provided by his calculus. And a part of this work will be concerned with genetic linguistics. Genetic linguistics is thus subordinated to typological linguistics – not as being less valuable (whatever that might mean), but as presupposing typological linguistics. We are now in a better position to understand what Hjelmslev meant in saying both that nineteenth-century classical linguistics achieved permanent results as regards the genetic relationship of languages and still that those results require reinterpretation according to the requirements of a newer point of view. This newer point of view will be one that seeks laws governing change of linguistic structures and the possibilities of change that are implied by a given linguistic type.

In the changes of language that are implied by the concept of genetic linguistics, Hjelmslev insists on distinguishing clearly the changes in linguistic *usage* (for example, changes in pronunciation, changes in meaning, changes in signs and their inventory, etc.) from the changes in 'pure form' – the linguistic structure considered independently of the substance in which it is manifested or the uses to which it is put. Since he believes that a typology of linguistic usage 'has crucial difficulties to contend with', it follows that scientific study of changes in linguistic usage – scientific study as contrasted to chronicling – also faces enormous difficulties. 'Man is a capricious and enigmatic creature, and here, man is at work'. With changes in the linguistic structure, on the other hand, he sees the situation as being quite different. Linguistic structure is defined independently of speakers and their tendencies. Here the linguist is justified in looking for *dispositions* towards changes within the system – dispositions that may be held in check by unpredictable external circumstances and then released by equally unpredictable circumstances. 'And here', writes Hjelmslev, 'it is linguistic typology, with what it has to say about categories that seek out and favor each other and categories that shun or avoid each other, which must identify the causes of linguistic change'.

Given the nascent state of the typology of linguistic structure and the enormous work required to lay the foundations of such a typology, it is not surprising that genetic linguistics – in this new conception of genetic linguistics as a science looking for laws of change within the linguistic schema – will be at first confined to the most tentative hypotheses, perhaps more properly termed surmises – about evidence that certain categories have favored or avoided each other, and to hypotheses about relative and absolute optima, towards which linguistic schemata may tend and which may be used to explain convergent developments. Notions of 'tension' between parts of the formal schema, as well as between form and manifestation, will also require refinement. Not least, we must bear in mind how 'language' has been replaced by 'linguistic structure' or 'linguistic schema' in our considerations. We have already seen the possibility admitted that a given reconstructed 'linguistic structure' – of the kind which might be expected to appear in the genetic linguistics proposed – might well never have been used at all – in other words, might well not have been manifested in anything that we would think of calling a language. Nor is this all: what we ordinarily *do* call a language – the Danish language, for example, or the Polish language – we have unconsciously defined on extra-linguistic, sociological bases. Consequently, there is no guarantee that analysis of a number of texts that we might identify as Danish texts would not yield more than one linguistic structure – indeed, it is almost sure to do so. Why should we expect linguistic structures to be dealt out with such parsimony as to allow only one to a nation or otherwise organized community? Finally, to mention only one more foreseeable complication, we must be prepared to encounter 'virtual systems' – systems, to put it roughly, that are almost, but not quite, manifested in the texts that we have taken for analysis. Since these are obvious candidates for full manifestation when the observed system or systems come to be replaced, it is clearly the task of the linguist to seek these out, as well, in the course of his investigations. A good part of his job is to predict, and he may expect to narrow the range of predicted possibilities by learning to recognize these virtual systems as such. All this, and much more, is included in that 'vision of a science still to be created' that constituted Hjelmslev's unifying view of typological and genetic linguistics.

## *Sydney M. Lamb:* Discussion[1]

It is a privilege for a linguist to be in Denmark, this small country which occupies such a large place in the history of linguistics. And it is a special pleasure to be able to participate in this celebration of a great university and of two giants in the long line of great Danish linguists. I would like to add my felicitations to those of Professor Whitfield.

And while Whitfield expresses some trepidation in undertaking his task in this symposium, even though he is a distinguished authority on Louis Hjelmslev, my trepidation is I think more justified since I am merely a dabbler in the works of this momentous scholar.

As Whitfield appropriately points out in his excellent introduction, a proper genetic linguistics, in Hjelmslev's view, must be based upon a proper linguistic typology, which in turn must be based upon, and must indeed be included within, a suitable theory of linguistic structure. At the risk of putting it too simply, we may say that it is hardly possible to specify the types of linguistic structure, or the ways in which languages can change, if we don't know what a linguistic structure is. This point is worth emphasizing because linguistics still has not arrived at a sufficient understanding of the nature of linguistic structure to provide the basis for the kind of typology which Hjelmslev envisaged.Linguistic structure is so complex, so hard to grasp, so elusive in its inner secrets, that despite generations of work, theoreticians remain mystified by it to this day.

Our position in this respect has not advanced very far since Hjelmslev. While progress has been made in understanding at the level of details in the areas of phonology, syntax, and semantics, the overall picture remains obscure. Since Hjelmslev's time dozens of theories and varieties of theories have been proposed and none has won widespread long-term acceptance. The reason is, I think, that none of them is acceptable.

In some respects, the theories proposed since Hjelmslev's time have even represented steps backward, rather than progress. And I want to devote most of this presentation to two important aspects of that point.[2]

But first let us pause for a moment on the question of how it is that linguistic theory was unable to simply appreciate and accept the work of Hjelmslev and just proceed from there. Two observations are appropriate. First, progress in theoretical studies is always along a crooked path.

1. I am assuming that the ambiguity in the title of this session is intentional, and I am choosing to interpret it as 'the position which Hjelmslev occupies in the history of genetic and typological linguistics'.

2. In order to devote attention to these two points, I omit several other important features of Hjelmslev's thought which likewise merit increased consideration by contemporary theorists, such as his notion of catalysis as applied to linguistic form (*cf* Lamb *in press*) and his view of the position of linguistics among the sciences (mentioned in Whitfield's introduction). I have previously (1966a) written a more general commentary on the *Prolegomena*, mentioning various valuable features which deserve increased attention while criticizing certain other features. In the present paper I take back the criticism directed at Hjelmslev's failure to distinguish separate layers of content (Lamb 1966a, 567-572).

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Second, Hjelmslev's theoretical writings are not easy to understand. I still remember my first attempt to read his *Prolegomena to a Theory of Language*, when I was a naive student in Professor Whitfield's course in Russian morphology at the University of California. With all the effort I was able to muster up I managed to get through the first few pages with some small degree of understanding. By the time I got to about page 5 I found myself forced to give up altogether. Later I discovered that those first few pages are the easy ones! Fortunately, with help from Professor Whitfield in class, I was able to get somewhat further on my second attempt, and further still on the third.

But it was not until more than a decade later, when I was writing a review article on the *Prolegomena* (Lamb 1966a) that I finally really understood and appreciated his assertion that a language is purely a system of relationships. Up to that time I had *thought* I appreciated what he meant, but I hadn't really, until one afternoon in the fall of 1964 when I had the wonderful experience of a light turning on in my head, as it were. By this time, I was developing, under the influence of Professor M. A. K. Halliday, a diagrammatic notation to aid in the sudy of structural relationships among linguistic elements. If you will permit me to describe the path by which I came to see the light, I may be able to convey something of what it felt like to finally understand the notion of linguistic structure as purely relational.

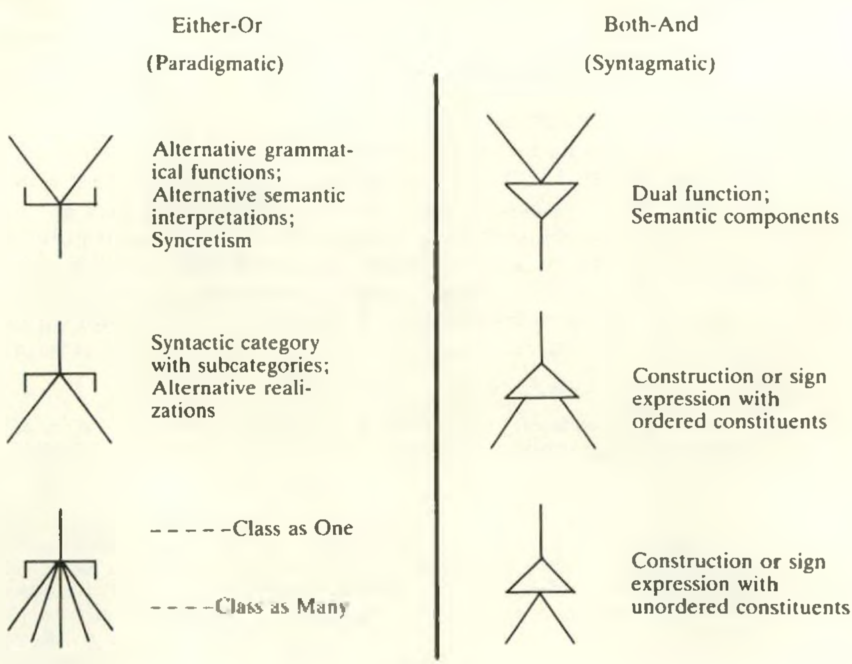


Figure 1.

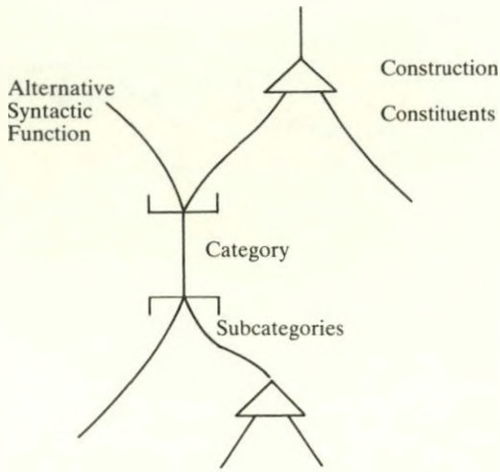


Figure 2.

By borrowing and adapting some notational devices from Halliday I was able to draw diagrams of structural relations. Thus Hjelmslev, like various other linguists including Halliday, drew a fundamental distinction between the *either-or* and the *both-and* type of relation, or in other words, between paradigmatic and syntagmatic relations (Figure 1). It was further necessary to distinguish two directions, which can be depicted as upward and downward in diagrams. Upward is toward meaning or function, downward towards expression or realization.

This is not the place to go into a detailed demonstration, but perhaps it is apparent or at least plausible that an entire syntactic pattern can be diagrammed as purely relational, since it is made up of network elements of the types shown, interconnected in various ways, as suggested by Figure 2. Realizational relations are also amenable to such treatment (Figure 3).

We can thus be led to an appreciation of a linguistic structure as a network of relations among various elements, these elements being things like phonemes or phonological components, lexical items, and the like. Consider for example the element *go* of English (Figure 3). The diagram shows how it is related to its three sets of properties, semantic, grammatical, and phonological, including the alternative realizations on the expression plane. Without taking the time to offer a demonstration, let me just assert here that the semantic relations can be diagrammed using the same notation, so that the connection to semantic properties is to a point in the semantic network of the linguistic structure under consideration. The line to 'grammatical properties' leads to the subclass of verbs to which *go* belongs, by virtue of the combinations in which it can occur.

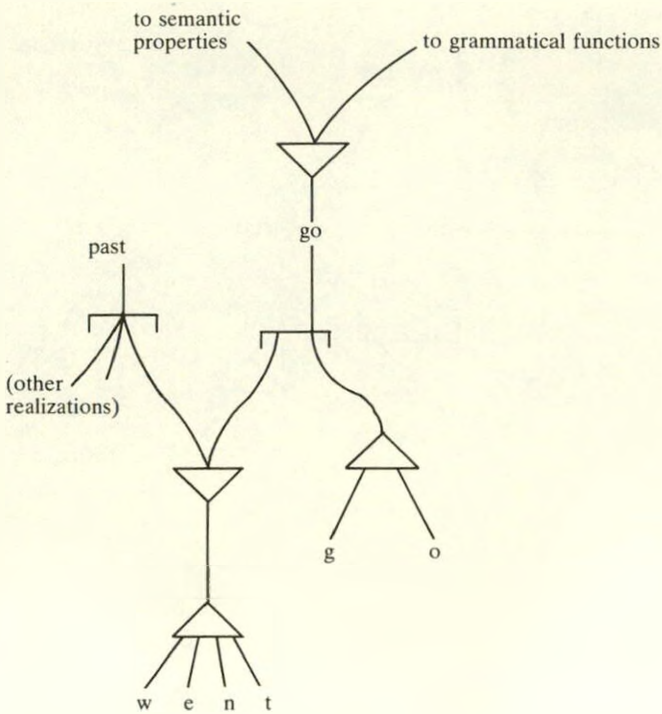


Figure 3.

Although I now view this diagram of *go* as imperfect, since the analysis into meaning components should be recognized as interwoven with the syntactic properties, the point I am attempting to illustrate is not affected by that issue.

Now when we have completely characterized an element in this manner, then we can conclude that the symbol by which we have been representing this element is completely vacuous – we can erase the symbol (*go* in Figure 3) with no loss of information! It adds no information to that which we already have in the diagram of the relationships. The 'element' is in fact not an object at all but merely a point of innerconnection in a network of relationships. And by extending the examination to other regions of the linguistic structure we come upon the same finding over and over. What about the phonological units, *g* for example? It is a point of interconnection in the phonological network (Figure 4). How then is *g* distinguished from other phonological units? By its connections – it has different phonological components and/or different combining potentials within syllable structure, different participation in lexical units in whose expression it appears.

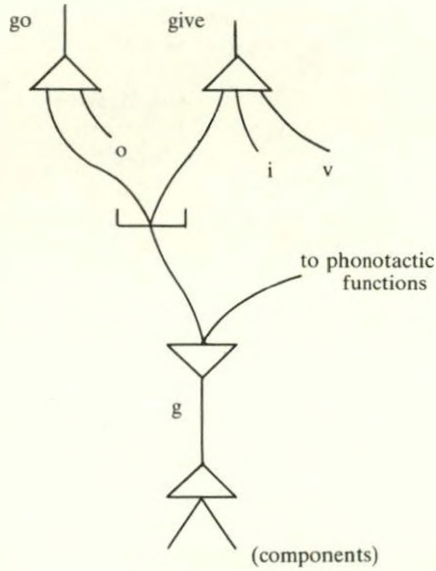


Figure 4.

What about the possibility of two phonological units, or perhaps two lexical items, which would have all the same connections in the network but would still be different? This is the crucial consideration – and just a little reflection shows that that situation is quite impossible. If they are two different items, let us say lexical items – then they must differ with respect to some property. Do they differ in expression? Then that will be shown in the connections to expression. Different in meaning perhaps – then that will be shown by the different semantic connections. If in a diagram there were two different points having all the same connections then this would just be a redundant depiction – therefore one which could be simplified with no loss of information. In other words, there could be no discernible linguistic evidence to justify keeping them separate since all linguistic features of the two (as indicated by their connections in the network) are identical.

And so, in general, whenever we find a symbol in a relational diagram of a linguistic structure, we can erase it provided its immediate relations have been fully specified; leaving only relations – not a system of elements and their relations, just a network of relations. The linguistic 'elements' have their existence not as objects but by virtue of connections in the relational network.[3]

Now I suggested earlier that Hjelmslev's work in theoretical linguistics

3. Admittedly, this view of language as relational is not exactly the same as Hjelmslev's, since it is built directly upon the *and-or* distinction rather than upon Hjelmslev's dependences as such. I nevertheless consider it to be an essentially Hjelmslevian view.

was more advanced in certain important ways than that of more recent theoreticians. I would like to point in particular to two aspects of his theory of linguistic structure.

First, his view of that structure as a network of relations is in my opinion more advanced than those of some other schools which still treat linguistic structure as a system made up of objects or of objects and relations or of objects and processes. We have for example the various 'item-and-arrangement' characterizations of language and the 'item-and-process' view, which was elaborated into transformational-generative grammar (TG), perhaps the dominant theory in the years since Hjelmslev's time. According to TG grammar, the linguistic structure consists of a series of mutation rules which perform mutational (or rewriting) operations upon symbols. Although Noam Chomsky, the most influential member of the school, has spoken of language as a network of relations, that view is certainly not readily apparent in his system of symbols and the various processes which shuffle them around. On the other hand, we may note that Chomsky and his colleagues did succeed in popularizing the notion of a formal, quasi-mathematical approach to linguistic structure, which is certainly a step in the direction of seeing that structure as purely relational. It came as something of a revelation to many both within and outside of the field of linguistics that linguistic structure was amenable to such formal treatment, and that notational devices from mathematics could insightfully be applied. Without deprecating Chomsky's intentions it should be pointed out that the particular type of notational system that had developed in mathematics and mathematical logic, which was applied in linguistics by Chomsky, following his teacher Zellig Harris, is not really well suited to linguistic structure. An alternative approach to mathematics, which may be more appropriate to Hjelmslevian thinking, has now been proposed in a brilliant book by G. Spencer Brown called *Laws of Form* (1969). Like Hjelmslev's *Prolegomena*, this book is small, difficult to read, and brilliant. Brown's mathematics is more purely relational than conventional mathematics in that it has no distinction between operators and operands. Like Hjelmslev's linguistic form, it is free from objects.

I would like to devote the rest of my discussion to one other major aspect of Hjelmslev's linguistic form, in its relation to more recent views.

Where Hjelmslev saw the two levels of content and expression, more recent theories have commonly seen three levels, or even more than three.

Despite their disagreement on various other points, several prominent schools of linguistic thought have as if by mutual agreement converged on the three-level view, and there have even been occasional proposals involving more than three levels, including those for which I myself must accept the blame. In general the various three-level schemes of recent times have one level corresponding to Hjelmslev's *expression*, while his *content* is divided into two parts.

In the Tagmemic theory of Pike and Longacre and their many followers, a linguistic structure is composed of hierarchies, phonological, grammatical, and lexical. The 'System-Structure' school of Halliday and his co-workers in the U. K. and elsewhere proposes a similar three-way division. In my work and those of others associated with me in what has been called the stratificational school, we have explored a variety of hypotheses concerning the number of fundamental components of linguistic structure, and among them a tripartite view has been quite popular.[4]

And the history of TG and its various daughter schools has been quite interesting in this connection. Chomsky's influential *Syntactic Structures* (1957) proposed a two-level scheme with a grammar (including a transformational component) and a phonology, somewhat comparable to Hjelmslev's content and expression. This scheme was criticized by various linguists, both within and without his circle of followers, who argued that the existence of the transformational component actually established a distinction of two different levels of grammar, which were being conflated by the formal structure of the 1957 model. In accordance with these critics Chomsky began proposing a tripartite view in the early sixties, which was crystalled in his *Aspects of the Theory of Syntax* (1965). In this scheme there were 'deep structures' and 'surface structures', mediated by the transformations, which could now be seen as constituting a device for describing quasi-realizational relations between two strata.

This direction was explored further by some of Chomsky's students who proposed the theory known as generative semantics, in which the deeper (or higher) level was merged with semantic structure.[5]

According to the generative semanticists, Chomsky's deep structure was not deep enough – their proposal had a semantic level which incorporated the 'deepest' grammar, while surface grammar could be viewed as intermediate between content and expression.

It can be regarded as healthy, even if it makes the work difficult to follow, that in both the transformational and the stratificational traditions a wide variety of hypotheses have been proposed and explored. It is also significant, I think, that none of them has stood up very well when subjected to close scrutiny. Indeed, that's why it is healthy that new alternatives continue to be proposed. It would be unfortunate if theory-builders stubbornly persisted in mistaken ways after the mistakes became apparent.

But in general, among the variety of hypotheses, we have continued to see models with more than just the two levels of expression and content.

Now let us look briefly at some of the evidence which has led to these multi-stratal views.

Consider clause structure, for example in Indo-European languages. According to a traditional type of analysis the standard clause consists of

4. One variant of this view (Lamb, 1966b) subdivided each of the three major divisions into two levels; that distinction is somewhat comparable to that drawn by Hjelmslev between idealized and actualized chains within each of the planes of expression and content.

5. If one adds a semantic level to Chomsky's 1965 scheme it can be seen as a 4-level view of linguistic structure, thus roughly comparable to the 4-stratum scheme of the stratificational school, in which the four strata are called phonemic, morphemic, lexemic, and sememic.

a subject and a predicate. In many such languages the subject is marked by nominative case, and if the predicate contains a direct object it is marked by accusative case. But this is only one of a number of apparently conflicting ways of looking at the structural relations of elements in a clause. According to another, we recognize various participant roles such as agent, patient and instrument, and those roles do not correspond with the categories of subject and object. Consider, as a simple example, the sentences:

(1) (a) Hjelmslev developed Glossematics
    (a) Glossematics was developed by Hjelmslev.

They differ with respect to what is the subject, but in both *Hjelmslev* is agent and *Glossematics* is the patient. We may likewise note that in (b), *Glossematics* functions as subject in what has been called 'surface grammer' and as patient in 'deep grammar'.

Now it appears to many – and certainly it did to me for a number of years – that these participant roles, agent, patient, instrument, beneficiary, etc. are closer to meaning than such entities as nominative and accusative. Thus it is tempting to view the case endings in a language like Latin or Russian as occupying a position of content in relation to phonological or written expression but as expression in relation to the participant roles (or 'deep cases') which they somehow represent, and which are more truly, it seems, at a level of content.

Going hand in hand with this argument is that involving the syntactic categories of lexical items. The types of syntactic categories needed to account for constructions of subjects and predicates, with direct objects and prepositional phrases and so forth, as they have been traditionally treated, are *noun, verb, preposition* etc.; and of course among verbs it is necessary to distinguish between transitive and intransitive, as well as to distinguish those, like *give* in English, which can have two objects. And among nouns certain grammatical subclasses must be distinguished. In Latin, for example, gender and declension classes. In English, count nouns, mass nouns, and abstract nouns have often been distinguished on the basis of properties of combinability with the definite and indefinite articles. It is such types of syntactic categories – those which we are inclined to think of as more grammatical than semantic – that seem to involve surface grammar. By contrast, the types of categories needed to describe co-occurrence possibilities involving participant roles like agent, patient, instrument, beneficiary, etc. seem to be more like semantic categories.

Analysis[6] in this complex region of linguistic structure shows that there is apparently in all languages a multi-level taxonomy of processes, another of participant roles, and another of the class of possible participants – that is, 'noun-like things' – these taxonomies being interrelated and

6. Rather, catalysis if we operate from a Hjelmslevian standpoint, so that we encatalyze the linguistic form to account for the substance.

indeed mutually defining in consideration of co-occurrence possibilities. And of course, co-occurrence potential means syntax. Thus it is necessary in many, perhaps even all languages, to distinguish concrete objects from abstractions on the basis that concrete objects but not abstractions can be patients of certain processes. Among concrete objects it is necessary (again for at least many languages) to distinguish animate from inanimate on the basis that the animate but not the inanimate can be agents of a certain class of processes. Similarly among the animate a subclass 'human' must be distinguished. And so on, and so on.

In short, we find in continuing this type of investigation that there is in any language an elaborate hierarchy of 'things' and another of 'processes' and that the categories and levels of the hierarchy of things are defined on the basis of their co-occurrence possibilities with the various categories of processes, and vice-versa. In other words, we are here dealing with syntax and with linguistic form in the Hjelmslevian sense, and not with semantics in the traditional sense – in Glossematic terminology, not with content substance.

The conclusion of this line of reasoning is that there appears to be a syntax which is closer to meaning than traditional syntax. It thus appears that we need to distinguish (at least) two separate strata – semological and grammatical, each with its own syntax, where Hjelmslev had just *content*.

Another argument in this connection involves homonymy. As an example, English has two quite distinct types of *table* in *the book on the table* and *the table in the book*. It is very tempting to say that in a three-structure scheme we have the lexical item *table* at the middle stratum, as a syncretized realization of the two different semological units at the higher stratum (Figure 5). Such a view fits well with the syntactic arguments I have briefly considered, since this syncretized lexical unit *table* belongs to the category of count nouns regardless of its meaning, while the two different types of table would have to occupy different (if related) positions in a fully elaborated taxonomy of objects.

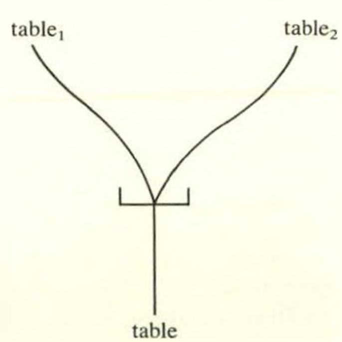


Figure 5.

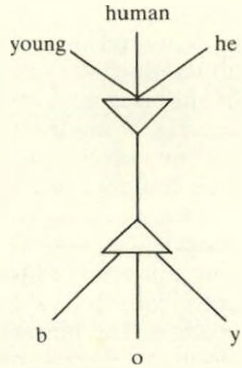


Figure 6.

The last argument I shall mention makes use of Hjelmslev's principal criterion for distinguishing his planes of expression and content. In his book *Language* (1970), he uses English *boy* as an illustration. Using relational network notation, we can diagram the illustration as in Figure 6. And as Hjelmslev observes there is no correlation between the elements into which the expression can be analyzed and those into which the content can be analyzed.

Now, we may be tempted to argue, might not the same type of situation be present at another level. Here I will repeat the argument as I wrote it at a time when I actually believed it (1966a:567–568):

> Consider, then, a sign-expression such as *undergo*. To the English speaker it is on the one hand immediately obvious that this sign-expression has the components *under* and *go*. But on the other hand, *undergo* does not mean 'to go under' or anything of the kind. In other words, its corresponding sign-content does not consist of two-components such that one corresponds to *under* while the other corresponds to *go*. Thus, applying Hjelmslev's line of reasoning, we are forced to conclude that *undergo* is a minimal sign-expression, that the native speaker's intuitive notion to the effect that it has two parts is illusory. The same conclusion holds for *go through with*, *go in for*, *go back on*. But if none of these allows partitioning other than into their constituent phonemes, then how do we account for the fact that their past tense forms are respectively *underwent*, *went through with*, *went back on?* Evidently the native speaker's intuition does have some basis after all, and at some level the component *go* must be recognized in each of these sign-expressions, or else the description will have to account for their past tense forms repeatedly instead of once. Consider also *understand* and *withstand*, whose corresponding sign-contents have nothing to do with standing, but whose past tense forms are *understood* and *withstood*, repsectively. Consider finally *leatherneck* (neither of

> leather nor a neck), *tightwad*, *skinflint*, *black-eyed-Susan* (a type of flower)...
>
> The ... solution is ... to recognize that there are really two sign-systems involved, not just one, and hence three rather than just two planes. The middle plane is 'content' relative to the lower one and 'expression' relative to the upper one...
>
> By virtue of this distinction the linguist may recognize *under* and *go*, *with* and *stand*, *leather* and *neck*, *black*, *eye*, *d*, and *Susan*, etc. as signs in the lower sign system, and *undergo*, *withstand*, *leatherneck*, *black-eyed Susan* as signs in the upper sign system.

All of the above arguments seem to point in the direction of a tripartite linguistic structure, with a middle stratum that is intermediate between expression and content.

But I would now like to propose an alternative interpretation which supports Hjelmslev's view.

Consider first the taxonomies of things, processes, and roles. As I have suggested, they have many layers, and as we go to finer and finer sub-categories we seem to get closer and closer to meaning. What then if we look in the opposite direction, toward the most inclusive categories? I would like to suggest that as we do so within the taxonomy of things we may come ultimately to the category *noun*. This hypothesis is tantamount to saying that the traditional high-school English teacher, so often maligned by structural linguists of the 'forties and 'fifties, was correct after all in her assertion that 'a noun is the name of a person, place, or thing'. Similarly the taxonomy of processes, if we follow it to the most inclusive class, leads us to the category *verb*. What I am suggesting is that, instead of treating the seemingly less meaning-oriented 'surface grammar' as involving an intermediate stratum we see it as involving the most general – hence the vaguest – portion of the syntax of content.

To support this viewpoint we may take a look at phonological syntax, where we find that the more inclusive, more general levels are necessarily less concerned with phonological detail than the finer levels. For many languages, we can apparently specify the phonological word as, roughly, a sequence of syllables, without regard to the finer details of phonological composition of those syllables. But to specify the structure of, say, the initial consonant cluster it may be necessary to bring the distinctive features into consideration. Here it might appear that we are closer to the phonetic substance than we are at the level of the more general constructions such as the phonological word, but it would surely be a mistake to conclude that there must therefore be two strata of phonological structure.

Now against this proposal various examples can be adduced which give evidence of conflicts between different categorizations – for example, in English the categories like *animate*, *human*, *concrete object*, and so forth fail to fit neatly with categories such as *mass noun* and *count noun*. *Water* and *sand* are concrete objects and mass nouns while *pond* and *stone* are concrete objects but count nouns. Such considerations, however, do not

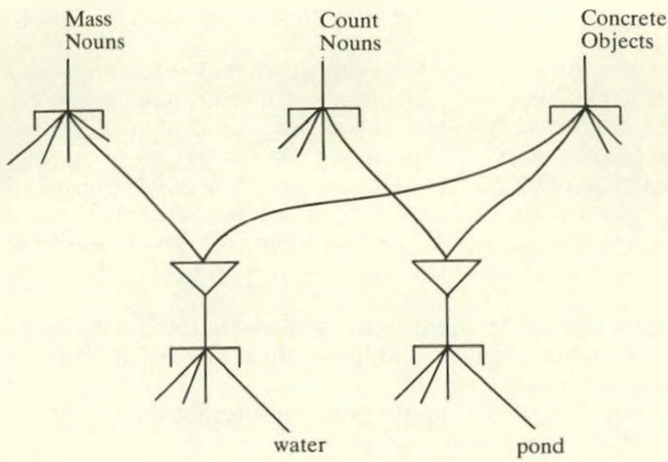


Figure 7. Note the use of the 'upward and' node for simultaneous membership in different syntactic categories. This is of course a highly simplified diagram, which omits various intermediate subcategories.

invalidate the proposal, since we already have need, within syntax, of devices for cross-cutting categorization within the syntax of a stratum (Figure 7).

The same type of consideration applies to the data involving conflicting syntactic constructions, for example subject-verb-object as opposed to agent-process-patient. The use of the 'upward-and' node in a syntactic pattern allows the category involved to simultaneously participate in more than one syntactic construction. Thus, in

(1) (b) Glossematics was developed by Hjelmslev

*Hjelmslev* can be analyzed as both agent and as the object of the preposition *by*, while *Glossematics* likewise has two simultaneous syntactic functions – it is both patient and subject. The use of this device in relational network description is related to the use of transformations in TG grammar. That is, situations which require the use of simultaneous participation in different syntactic constructions within a relational network model in general seem to require the use of transformations in a transformational model. This conclusion, if correct, would mean that Chomsky's essentially two-stratum view of 1957 was in this respect closer to the truth than the various three-stratum and four-stratum models which emerged in the subsequent evolution of transformational theory.

Now let us turn our attention to the traditionally recognized cases like nominative, accusative, dative, *vis-à-vis* the various participant roles like agent, patient, instrument, etc.

It is noteworthy here that the traditional grammarians treated the various participant roles under the heading of syntax, thus as part of grammar. That they saw no need for a stratal distinction between such roles and the cases themselves *could* just be a consequence of the fact that they were not constructing formal grammars, but it is at least clear that general awareness of the phenomena greatly antedated Glossematics and the various three-level schemes of linguistic structure. Thus in Bennett's *New Latin Grammar*, the first edition of which was published in 1895, we find under the heading 'Syntax' a description of the various syntactic functions of the cases covering in rather extensive detail the various participant roles utilized in Latin grammar. Under *Dative*, for example, we find the dative of reference (131), dative of agency, dative of possession, dative of purpose or tendency, dative with adjectives (*mihi inimicus*), and dative of direction.

As the treatments of Bennett and other traditional syntacticians suggest, the structures of participant roles *vis-à-vis* cases do not in fact appear to involve any relationship that is out of place within the syntax of a single stratum (Figure 8). Here we are dealing with alternative syntactic functions of the case elements (rather than simultaneous ones).

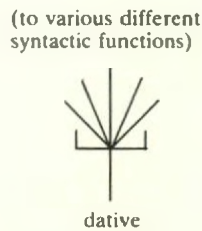


Figure 8.

Now what about homonymy, for example the case of the two types of *table* with their homonymous expression?

According to the three-stratum view, it was necessary to recognize *table* as a noun, alongside the two (or more) kinds of *table*, each belonging to its appropriate category in semological syntax. Thus we needed a surface element *table*, since *noun* was a category of surface grammar. But if surface grammar turns out not to involve a separate stratum, the whole situation is different. Either of the kinds of *table*, with their coinciding expressions, turn out to belong to the class of nouns through the most general level of the taxonomy of things, and there is thus no need for a separate direct connection from the surface element *table* to the category *noun*.

And, finally, the question of complex lexical items whose meanings are different from the combinations of the meanings of their parts, such as *undergo*, *go in for*, *leatherneck*. The argument which applies here is like that which applies to homonymy. The relationships involved are not different from those which we have to recognize anyway within a stratum (Figure 9). We can recognize multiple layers within a sign pattern without the need to set up a separate complete stratum for each layer.

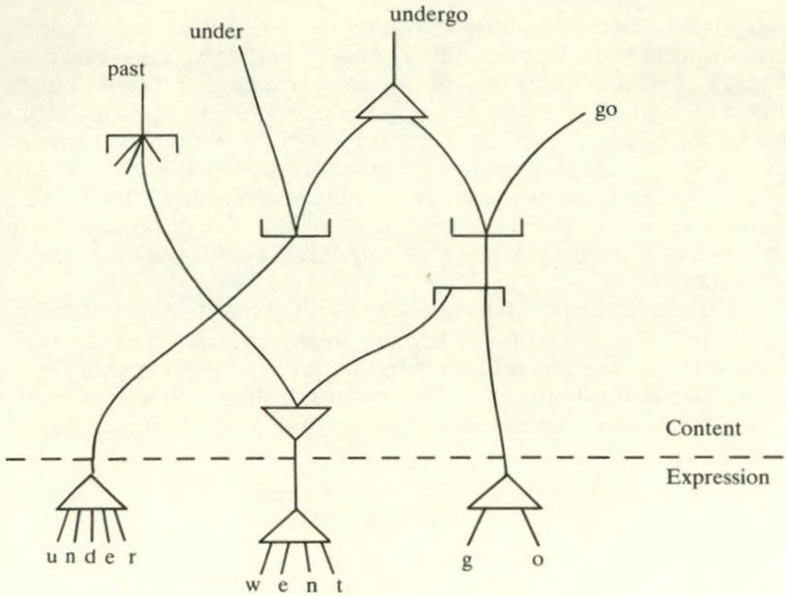


Figure 9.

Again the argument which seemed to make it attractive to recognize a common element *go* in surface grammar disappears when we combine the two syntactic levels into one, since the single *go* will connect (indirectly) to the general category *verb* while the various complex lexemes in which it participates are also connected to their appropriate syntactic categories within the same stratum.

I conclude that we must seriously reconsider Hjelmslev's view of linguistic structure. Perhaps, after all, there are not three levels, nor four or more, but just the two planes of expression and content.[7] Perhaps such reconsideration is especially timely now that there is such widespread dissatisfaction with more recent theories.

The position which Hjelmslev occupies in the history of linguistic theory, and thereby of typological and genetic linguistics, thus appears even more important now than it did, say, 15 years go.

7. On the other hand, there may indeed be another level in the area of what has sometimes been called hypersememic or gnostemic, but this level should probably be considered outside language. *Cf* the conclusion of Lamb 1971.

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## *Jørgen Rischel:* Discussion

Seen in relation to the long history of the University of Copenhagen, linguistics is a very young science, but we can mention several outstanding linguists of the last three centuries who served it one way or the other, and who deserve being commemorated on this occasion. As Hjelmslev pointed out in 1937 in his inaugural lecture (published in the *Essais* 1959) this line of scholars are 'a number of detached and independent personalities, almost unrelated to one another'. There are, however, obvious links between the two eminent scholars whose contributions to linguistics are in focus today. There is a relatedness in scholarly aims, which sets off these two scholars from many of their colleagues in the interval of a century separating their two careers.

In his introduction to this afternoon's meeting Professor Whitfield defined Hjelmslev's conception of genetic and typological linguistics very clearly and very carefully, on the basis of his singularly thorough knowledge of Hjelmslev's work. I do not feel I can add much to this excellent exposition, so I wish instead to make some comments on the *application* of Hjelmslev's theories.

Glossematics may be viewed as a calculus generating all possible linguistic systems. It makes possible a complete language classification provided that all languages can be matched against it and fitted into appropriate slots. In this sense it is a real *theory* of language, not just a technique for describing languages. Any such theory may be more or less appropriate. Here empirism comes in, and, indeed, it would seem that a huge amount of work is needed to establish the success of a theory. Now, glossematics came into shape, by and large, in the late thirties, and there has in fact been little revision of the theory since that time. This may suggest that empirical data from sufficiently many languages had been analysed by rigid glossematic methods by that time. But this was hardly the case, and it was certainly not what Hjelmslev himself would say. In the decades after the creation of the theory descriptive work was done, of course, and much of this work was profoundly influenced by Hjelmslev's ideas. However, it was not generally the case that Danish scholars followed the glossematic format rigidly in the description of languages; rather, they tended to take a pragmatic approach, using the deep insights they found in Hjelmslev's contributions to theory to direct them toward the goal of describing individual language as precisely as possible, and with the emphasis on language form which is a salient feature of glossematics. In this mass of work genetic and typological perspectives are not always very apparent.

As a student of Hjelmslev's one was fascinated by the theory and by its perspectives for the study of language, and one struggled hard to understand the theory and to understand how it could be applied to practical descriptive work. With growing apprehension came a rebellious desire to challenge details in the theory, or point out apparent or real inconsistencies between theory and authoritative applications. For decades there

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Travaux du cercle linguistique de Copenhague XX.
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was a lively debate dominated to a large extent by Hjelmslev's work and by his personality. The scholars engaged in this activity may perhaps be said to have constituted a 'school', but in fact glossematics in a strict sense never became *the* research paradigm for Danish linguistics. It was rather the case that various aspects of the theory came to exert an anormous influence on the linguistic thinking of whole generations of linguists, especially in Denmark, but gradually also abroad. Many of us are in fact rooted in the glossematic conception of language, with its dichotomies of content *vs* expression, form *vs* substance, etc., although we have taken issue with the emphasis of glossematic descriptions on specific formal aspects of language, and although we have preferred to attempt to work within the descriptive format of other linguistic schools – sometimes with ultimate frustration, I might add.

Hjelmslev's conception of comparative linguistics is well attested, but not quite easily understood in all its aspects. To me, one of the most difficult things to grasp is the way the linguistic *sign function* enters comparative research, according to Hjelmslev. Let us first look at the notion of genetic relationship, and how genetic relationship is attested.

The sign function is basic to language. Languages have sign inventories and are indeed languages by virtue of the interrelation between content and expression which is reflected in the signs. However, Hjelmslev emphasized that the categories of elements within the content form and the expression form are not defined with reference to signs but in terms of intrinsic, homostratal relations. To take a simple-minded example, *f* is an expression constituent in English not by virtue of its constituent role in WORDS or ROOTS such as *fish* and *foot*, but by virtue of its role as a constituent of SYLLABLES such as the ones I just mentioned. The entire hierarchies of expression form and content form are self-contained strata, and the structuring of the linguistic chain in terms of signs is a matter of interference between these two strata. The interference between content and expression enables us to use the commutation test, but the formal categories of each stratum are defined independently of it. This logically entails that genetic relationship between languages, considered as formal systems, must be established in terms of systematic correspondences between expression elements and between content elements, not between roots or affixes or words.

Now, if one establishes the element function between Latin *p* and English *f*, for example, it must be stated (and indeed discovered) with reference to signs which somehow count as identical in the two languages, for example *piscis* in Latin and *fish* in English. In this way the sign function crucially enters the analysis of genetic relationship, which of course does not change the fact that the element function can be generalized to obtain between the expression taxeme *p* in Latin and the expression taxeme *f* in English.

Off-hand this conception of 'genetic relationship' might seem to preclude exceptions to a correspondence rule, that is, irregularities due to loan, sporadic changes like dissimilation, and what else there may be. However, this is not the case. In *Language* Hjelmslev discusses exception types at length, and the difference between loan contact and genetic relationship is taken care of in the definitions of the *Resumé*. Still I think the glossematic conception of genetic relationship somehow presupposes that we are comparing states of languages for which the element functions work neatly. What about strongly mixed vocabularies? What about sound change in progress? What about lexical diffusion? Is this concept irreconcilable with the glossematic view, and is it justly so?

However, I wish to raise another point. There is a basic difficulty in the use of an operational definition of genetic relationship which refers to expression element functions only. I think it is hard to accept a statement to the effect that language A and language B are related in terms of element functions unless we somehow know that *the same signs* recur in the two languages. In what sense does English *f* contract an element function with Latin *p*, unless it is by virtue of some kind of pairwise identity between signs in which English *f* occurs and signs in which Latin *p* occurs? Although Hjelmslev speaks at length about genetic relationship in *Language* I find it difficult to interpret what exactly is implied by ELEMENT FUNCTION. If language form consists of two self-contained form hierarchies of content and expression, and if genetic relationship is defined with reference to language form, it is possible – by carrying this idea to its extreme – to argue that genetic relationship is a matter of systematic match between inventories of expression elements. As I see it this interpretation would lead to intolerable consequences. Now, Hjelmslev states a difference between TYPOLOGICAL RELATIONSHIP having to do with functions between categories, and GENETIC RELATIONSHIP having to do with functions between elements. I can only understand this difference if it implies that some specific identity is established between elements such as Latin *p* and English *f*, and this identity cannot have anything to do with the status of these elements in the expression systems of the two languages – what would support a claim to the effect that English *f* occupies the same place in the English consonant system as *p* does in Latin? Obviously, it is via the sign function that there is an identity. If we can somehow take Latin *piscis* and English *fish* to be the same sign, and similarly for other pairs of apparent cognates, we can define a kind of equivalence between *p* and *f* in the two languages (just as a synchronic analysis may show that free variants are functionally equivalent since they may occur in the same position in the same sign).

It is tempting to use common sense to tell us that a demonstration of genetic relationship between languages must involve signs which are obviously related in the opposite plane, such as *piscis* and *fish*. But I do not see anything suggesting that this is relevant to the glossematic conception of genetic relationship, even though such cognates *may* be used in the *discovery procedure*. I should like to mention an unpublished piece of evidence to the contrary, namely an exercise used by Hjelmslev in his course in elementary linguistics some time during the second half of the fifties. The text of the exercise says: 'Set up the element functions between languages *a* and *b* on the basis of the following stock of words', and then follows a list of 18 words in language *a* and a list of 18 words in language *b*. On inspection the two languages exhibit almost the same inventory of consonants and at least rather similar inventories of vowels. But this is deceptive. The items do not match in accordance with the most expected pairings of the vowels and consonants of the two languages. If, however, a trial and error procedure is used independently of the 'phonetic' expectations of the student, it soon turns out that the items can indeed be matched, and exhaustive element functions can be set up, according to which each item of language *a* has a precisely corresponding cognate in language *b*. The interesting thing is that Hjelmslev supplied no meanings or any other kind of information giving any hint as to the content or grammatical status of the words in the lists. This suggests to me, and indeed suggested to me when I did this exercise as a student of Hjelmslev's, that in his view relatedness in meaning is no important criterion. As I understand this, genetic relationship can in principle be established as an operation dealing with expression elements only, provided that the frame used in this operation is some kind of sign expression, not a pure expression unit such as the syllable.

This is certainly an extreme position. It may be attractive in that one avoids all the difficulties with lexical items changing meaning in linguistically unsystematic ways (Hjelmslev mentions the word *father* as an example). But is it not objectionable to disregard meaning in principle? If the sign function is arbitrary, should we not, then, foresee as a theoretical possibility that the inventories of minimal sign expressions in two languages may match in a way which is due to pure chance, that is, without the languages being related in terms of continuation (or 'historically' related, to use a less fortunate term) in the real world?

On the other hand, I do not see what would be a feasible alternative in a glossematic framework. It would be very nice to have a definition of the notion SAME SIGN, saying, for example, something to the effect that sign x in language A is the same as sign y in language B *if* the expression sides for the two languages can be related by element functions and *if* likewise the content sides of the signs can be related by element functions. In setting up element functions in one plane we would then be working on the assumption that there is this kind of equivalence in the other plane, which, of course, would mean that both planes must be analysed more or less at the same time.

Now, to the extent that one can establish something like element functions on the content side such a notion of SAME SIGN is indeed applicable. It makes sense to compare inflectional affixes, for example, and to see what the correspondences are with respect to the content elements of them, and in doing such comparison we may employ the knowledge of expression element functions which enable us to link inflectional material in different languages together. Unfortunately, of course, the content side of language is not easily accessible to analysis in its totality. This means that we do not have access to a sufficient repertory of content element functions in comparing possible cognates in the expression plane, and it also means that we cannot hope to be able to establish a total set of content element functions even if we have a complete set of expression element functions at our disposal. Furthermore, the meanings of words change idiosyncratically, and we simply do not expect generalized content element functions to obtain except possibly in very restricted sets like inflectional material.

As it is, Hjelmslev's operational definition of GENETIC RELATIONSHIP in *Language* is based on expression only, and this is in a sense quite understandable although there is something problematic about the whole notion, as I have tried to show. In the general definition in the *Résumé* Hjelmslev does not refer specifically to the expression plane at all. Here, the definition of GENETIC RELATIONSHIP is based on the notion of CONTINUATION, and CONTINUATION is a relation obtaining within a UNIT of VARIETIES of one and the same SEMIOTIC CLASS established by SELECTION. I will not go into detail with this set of definitions, but just stress the fact that here nothing is said that suggests any difference in the status of the two planes with regard to genetic relationship. If in actual practice there is a difference so basic that Hjelmslev based his operational definition on it, shouldn't this have repercussions on the whole theory? To me it just illustrates that the parallelism between the two planes does not go as far as it would appear from glossematic theory. This is true synchronically as well as diachronically.

Now I wish to turn to a different aspect, but still to something having to do with the *sign*.

According to glossematic theory, at least in the version of 'La stratification du language' (1954, reprinted in the *Essais* 1959), words belong to usage, not to the schema or system. Usage may vary even if the system does not change. This obviously means both that one should not expect the lexicon to remain unchanged, and that one should not expect complex signs to recur in related languages even if their constituent parts do. Hjelmslev made this very explicit.

Now, if sign inventories as such are relegated to the inferior status of usage, does this leave anything interesting to be said about sign-based inflectional paradigms, for example? I do not see how they would fit into a glossematic description of the schema. But would it be justified to neglect this aspect of language patterning in comparative work? Is inflectional material such as the endings of some conjugation class basically less interesting for comparative research than elements such as *p* and *f*, or the pure content elements 'imperfect' and 'perfect'? I think this question is legitimate in the context of today's topic.

If we turn now to typology, glossematics provides a typology in the sense of a classificatory system where each language schema has its slot, somewhat like the way in which chemical elements fit into slots in the periodic system. In this sense glossematics is a theory of language*s*. This whole calculus generating the possible systems refers to units, elements and relations within each plane, but it does not deal with categories of entities established by relations between content and expression. Hjelmslev explicitly criticized typology based on word structure, for example. In *Language* he speaks about word structure as something external and fortuitous in contradistinction to the categories on which he bases language typlogy. Clearly this is a point on which there is much disagreement. Isn't word formation, canonical forms, sign-based syntax including word order phenomena, etc., highly relevant to a typological characterization of languages? These phenomena add new dimensions, as it were, to the categorial display of languages according to a glossematic typology. I will not go further into this matter now, especially since the general topic of language typology will be taken up later by Professor Coseriu.

I should like finally to make a few comments on the question of form *vs* substance. It is generally understood that comparative work, according to Hjelmslev, has to do with form rather than substance. We may set up element functions and reconstruct the common antecedents of elements in attested languages as long as we refer to expression form only, and keep questions of manifestation aside. Now, if we look at Hjelmslev's glossematic papers on diachronic, or more correctly: METACHRONIC issues (I think in particular of 'Accent, intonation, quantité' from 1937, reprinted in the *Essais* vol. II), it turns out to be quite significant to keep track of a distinction between not just two but three levels of abstraction, namely the MANIFESTATION (phonic or graphic), the ACTUALIZED CHAIN, and the IDEAL CHAIN. (It may be mentioned in passing that Hjelmslev's reasoning about changes affecting only the manifestation or the actualization as against changes affecting the underlying or ideal representation, is highly reminiscent of ideas turning up in generative diachronic phonology more than a quarter of a century later. Both from a synchronic and from a diachronic point of view it seems very strange that Hjelmslev was never hailed as a forerunner of abstract generative phonology. It would not be fair to characterize glossematic expression analysis as a kind of generative phonology, certainly not. But some of Hjelmslev's insights do anticipate ideas in the phonological theory of the sixties, though generative phonology never acknowledged this fact).

Now, if there are actually three levels of linguistic specification as far as the expression side of language is concerned, what then about the content side? It is very hard to understand exactly what is going on here. To me it is a basic difficulty that it is hard to imagine a description of the linguistic content which does not somehow reflect the inventory of minimal and complex signs of the language under consideration. How independent are the syntagms of pure content form from those of a sign-based syntax? And how independent are the categories of content elements from the lexicon? What is content form really about? To take a much-cited example, the gamut of colours is divided up in different ways by different languages. But does this tell us something about content form? It obviously says something about the patterning of content substance, and it also says something about the lexicon, but does it tell us anything about the categories of abstract content elements? The only area in which we have some clear notion of what content form means, is the area of content exponents, or MORPHEMES in the glossematic sense. These are indeed relevant to language typology, as is obvious from such works as 'Essais d'une théorie des morphèmes'. But here we run into a problem making it more difficult to deal with these categories than with expression categories. The problem is: how do we delimit such a category? Take the category of case, for example. How many cases are there in a language with all kinds of fossilized forms expressing some kind of local or instrumental meaning? If this is not entirely well-defined, how interesting, then, is a typology based on such categories? I just wish to point to a difficulty here, and I think it is a real difficulty since Hjelmslev did *not* wish to delimit membership of morpheme categories strictly to elements which themselves contract relations defining them as fundamental morphemes.

As with genetic comparison in particular, it seems true for typological comparison as well that glossematic expression form is somewhat easier to handle than content form. Languages can indeed be characterized in terms of their expression form categories, often with striking or even paradoxical results, as when French and Danish, for example, turn out to differ radically in that French has neither accents, nor syllables, nor vowels, nor consonants, while Danish has all of these categories represented. It is not difficult to see the point in defining categories quite rigidly and universally and facing the empirical consequences. But the question is whether such a typology will tell the whole story about structurally interesting properties of languages. Several languages have comparable units serving more or less the same purposes of structural organization, and one may feel that it is in fact more interesting to state that French has much the same kind of clustering of expression taxemes in recurrent units as Danish has. Hjelmslev spoke of 'pseudosyllables' in the case of French, and thus set up an entity corresponding in size to the Danish accent-syllable but defined quite differently. Thus he was, of course, fully equipped with a theoretical device enabling him, after all,to state the similarity in combinatorics between French and Danish. But such statements would refer to quite different types of categories in the two languages and thus hardly reveal any topological relationship. This is not just a question of terminology: it is a question of EMPHASIS on certain properties of language schemata.

Hjelmslev has given us new insights about language. His doctrine is in part difficult to apprehend, sometimes paradoxical or even shocking, but always insightful. He boldly set out to make linguistics a science by providing a comprehensive theory of language, not just a strategy for describing linguistic phenomena. This monumental work has much to tell us today even if we prefer to transcend the boundaries of linguistics in the glossematic sense and look for answers to questions which may be inappopriate in a glossematic framework.

Hjelmslev was one of the greatest thinkers of our science; his work has put its stamp on modern linguistics, and it will remain a great challenge and inspiration in the future.

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## Panel and open discussion

Two series of questions were proposed for discussion: 1) the historical one (what did Hjelmslev mean?); 2) the evaluative one (was he right?).

Discussion around the first set of questions took two directions: What did Hjelmslev mean by 'genetic relationship'; and what did he mean by 'reconstruction'? In clarification of the first of these Eli Fischer-Jørgensen quoted from Hjelmslev's (unpublished) introduction to the Helsinki conference in 1950:

> Evolutionary linguistics consists in establishing language families. These in their turn are classes of languages, and if we define language typology so as to mean any classification of languages, evolutionary linguistics is nothing but a special case of language typology. It is a special case which should not be confused with other possible classifications... A language family may be defined as a class of languages with a particular kind of highly specialized correspondences. Within a class of this kind, each possible variant in one language corresponds regularly to or, in other terms, is from this point of view identical with a definite positional variant in the expression in another language.

Although language-families may be established in this sense we cannot know from linguistic data alone what internal relationships exist among the members of the family. We cannot know, that is, which language is daughter and which is mother, for example. To determine that we have to draw on extra-linguistic knowledge.

It was further suggested that Hjelmslev continued the tradition originating with Hervas y Panduro and Humboldt, according to which language-change can be understood only as the replacement of one system by another (*cf* above, p. 33), and Coseriu's paper, pp. 157–170).

The other direction taken by this discussion emerged from Rischel's comments on the feasibility of reconstructing languages on the basis of expression-elements alone. It was argued that Hjelmslev theorized in favour of such a view, but that his practice was different, in that it relied on matters of content; and further that only expression-form was reconstructible, not content-form. Against this view it was held that the reconstruction of meaning has prospered since Hjelmslev's time (kinship-terminology, numeral systems), but even so it was conceded that no irreversible laws could be established for meaning-change, as they can for changes in expression. It was suggested that this difference should be seen in terms of a priority-list on Hjelmslev's part, and it was emphasized that Hjelmslev had no intention of throwing anything, least of all meaning, out of linguistics; but even if it were possible to establish general laws concerning change in content, reconstruction should nevertheless be concerned primarily with expression and only secondarily with content.

The second set of questions invited the suggestion that Hjelmslev was clearly right in regarding language-change as the replacement of system by system. Nevertheless, the distinction between system and usage was held to lead to a sterile view of language-change, primarily because it embodies an evaluation: for the theoretical linguist the system is the all-important thing, usage is uninteresting. A plea was made for serious investigation, by theoretically orientated linguists, into, for example, the intermediate steps between allophone and phoneme.

Contributors to the discussion were: FJW, HA, JR, SE, EC, WUD, WUW, EH, SML, Ebbe Spang-Hanssen (chairman) and Eli Fischer-Jørgensen.

# 3 Naturalness as a principle in genetic and typological linguistics

*Wolfgang U. Dressler:* **Introduction**

### 1. Preliminaries

Since I have the privilege of having my paper subjected to the discussion of two eminent linguists who have worked for a long time on problems of linguistic naturalness, it has little sense for me to discuss their views on the subject. Instead I have chosen to put forward my own personal views, because this is my only hope of being able to achieve a coherent presentation, although it will be necessarily subjective. My bias is that of Natural Phonology, (henceforth NatPhon), a school of thought (Stampe 1969) in which I have been actively interested since 1970 and some of whose principles I have since then extended to other fields in an approach which I call polycentristic (*eg* Dressler 1977a, b).

In a preceding 'Copenhagen paper' (Dressler, to appear a) I have argued that naturalness must not be equated with (typological) frequency, concreteness, simplicity, nor intuitive plausibility,[1] but must be a basic principle of a linguistic approach that relies heavily on external (or substantial or substantive) evidence.

Naturalness must be derived from considerations of the nature of man, who is not only a speaker-listener, but also a non-verbally communicating being conditioned by biological, psychological and social properties. Therefore any 'natural linguistics' must be based on such extralinguistic considerations as in the case of natural phonology, the study of articulation and perception, and their neurological bases, whereas natural grammar must have a basis in cognition. Yet one must not exaggerate the Physical Basis Constitution hypothesis, (Botha 1978:II, 16ff) and must avoid equating phonology with phonetics (and grammar with its cognitive/psychological bases). Thus I can agree with Ohala's (1979) dictum that 'Universal sound patterns are universal because physics and human anatomy and physiology are universal', but I have to add psychological and social reasons for universality.

However, typical 'naturalists' implicitly agree with a methodological separation of the psychological/biological and the social factors. The former have to be dealt with first, the latter later on. Unfortunately, most of us tend to omit or postpone indefinitely the treatment of social factors (with the exception of the most elementary sociopsychological bases of human communication). In other traditions internal and external causes

1. Naturalness as an intuitive feeling or common sensical guiding principle in the stages of concept formation and first evaluation of hypotheses is very wide-spread among linguists but also extremely vague.

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of language change are differentiated (*eg* Hagège & Haudricourt 1978). However, social factors are not external insofar as the purposes of communication are of a social nature.

As examples of strong exponents of such naturalness considerations I would like to cite the NatPhon of Stampe (1969) Donegan & Stampe (to appear a) and 'Natural Morphology' as founded *eg* by Mayerthaler (to appear a) on properties of access, perceptual complexity, pragmatic presuppositions; or *cf* Wurzel's (1977a:132f) statement that 'universelle perzeptive Grundordnungskategorien' are candidates for language specific categories. (For both fields *cf* Dressler 1977 a, b). I differ, however, from both authors cited above in that I lay more emphasis on the communicative or semiotic functions of language or the 'functional approach, the idea of purposiveness in language' as Seiler (1978b:14) puts it (see also Holenstein 1978 for a defense of functionalism). That is, the nature of man presents a finite number of possibilities for and obstacles to fulfilling his communicative needs; from these possibilities and obstacles (both to be subsumed as capacities) a finite number of universal techniques can be deduced; these are universal tendencies or, more precisely, operations (which are inherently universal; see van den Boom 1978).

'Strong naturalists' seem to agree with H. Seiler's approach, *ie* their orientation is 'towards a theory of language, not toward a theory of grammar' (Seiler 1978b:14). This demands an interdisciplinary approach, because external (substantial) evidence (Zwicky 1975; Skousen 1975; Dressler 1979) cannot be dealt with successfully by purely linguistic means. Moreover, the whole outlook is different, as can be most easily seen when comparing Stampean NatPhon and Natural Generative Phonology [= NGP (Hooper 1976; Vennemann 1972)]. Whereas external evidence (child language, diachrony, casual speech, slips of the tongue, language games) has been investigated by D. Stampe from the very beginning, their consideration has only recently come into NGP.[2] For NGP properties of naturalness tend to play a role of evaluative criteria which restrict the explanatory power of a theory and remove the danger of non-unique solutions, whereas naturalness tends to be a fundamental axiom of NatPhon.

NGP can be called a variant of generative phonology (henceforth GenPh) which has introduced some constraints of naturalness (here essentially in the sense of concreteness), whereas NatPhon is basically distinct from generative phonology, but uses many techniques and results of generative phonology for convenience. Thus, I would call only those linguists who share the axiom of naturalness 'strong naturalists' and, in order to make stronger claims and to arouse a more lively discussion, I will concentrate in the paper on such 'strong naturalism'.

One of the primary concerns of NGP, *ie* restricting the power of the theory, is of secondary, but still of great importance for 'strong naturalism'. For could one not say that all human behavior is natural? In this

2. *Eg* Hooper, to appear a, b. Of course, diachronic studies have been undertaken within NGP from the very beginning, but studies of casual speech phonology only for a few years.

sense the explanatory effect of naturalness and similar concepts is doubted by Lass (1975a; to appear). One answer is the formalization of naturalness by means of markedness and markedness calculus in Mayerthaler (to appear a) where markedness is not identical with the purely evaluative notion of markedness in generative phonology (and grammar),[3] although lately markedness in GenPh has acquired more substantial properties (Houlihan & Iverson 1977; Hooper 1979).[4]

A second answer is to develop naturalness categories for each component and subcomponent of languages and to explain 'unnatural' phenomena as the outcome of naturalness conflicts (Dressler 1977a, b; Mayerthaler, to appear a) and by means of other intervening variables (*eg* of a social character).

## 2. Naturalness in diachrony

In a similar way to what generative and structural linguists claim about markedness, simplicity, symmetry,[5] etc., a naturalist claims that more natural values of any dimension, *ceteris paribus*, are more stable (*eg* in language disorders) than less natural ones. Therefore natural values should be more resistant to (diachronic) change and should be preferred goals of such change. Works of 'strong naturalists' are full of these assumptions. For recent generative examples in phonology and morphology, see Herbert (1978) and Zwicky (1978) respectively. Let us call a change which fulfills these assumptions a natural change.

3. Cairns (1969) is a notable exception. He based markedness (influenced by N. S. Trubetzkoy) on universal phonological rules derived from physical origins. However, his contribution has remained rather isolated.

4. Markedness was already introduced (following the Prague School of phonology) in Chomsky & Halle (1968:402) to distinguish 'expected' and 'unexpected' or 'natural' and 'unnatural' cases of rules and symbol configurations. As to the positions on markedness in TG today, the papers read at GLOW 4 have shown quite clearly (with the notable exception of Basbøll, to appear) that markedness is still a very peripheral or at least secondary and interpretative concept based on rule complexity, violations of formal universals, or derivations from optimal organization of grammar as evidenced in rareness (of languages using a property) and stylistic oddness. Basbøll (to appear) characterized the ideal of the TG markedness conventions correctly as a 'price-book' (for the costliness of rules and grammars).

5. Martinet (1955) derived symmetry from the communicative purpose of language and the tendency towards economy of means to fulfil it, with the asymmetry of the articulatory organs as intervening variable. In this respect he (and other structuralists) could be called a 'strong naturalist'. However, he compared successive diachronic stages rather than dealing with diachrony intself, an attitude which has been amply discussed since Coseriu (1958) as vitiating the nature of change. Another property which distinguishes A. Martinet and other functionalists from 'naturalists' as described here is the naturalists' adherence to process models. A third difference is the much smaller role of external evidence in many schools of structural linguistics, although Jakobson (1941) was the most important pioneer of its use. In this connection one may cite Martinet (1949:6) 'it is much safer to study strictly linguistic data than the psychological reflexes of them'; 'strictly linguistic data' has too often been taken in a very narrow sense.

But at least for sound change it has been shown that a new phonological process soon abstracts away from its phonetic basis, a modification that Hyman (1977) calls phonologization and which corresponds to my concept (Dressler 1977a:32, 48, 58ff) of allophonic processes changing their outputs from intrinsic to extrinsic allophones. This implies already a small amount of denaturalization, and may be connected with considerations of allophonic space (*ie* distribution of allophones in articulatory/perceptual space), perceptibility, imitation by new users of the process (especially in the case of adaptation to another lect, but quite generally spread of innovations is often accompanied by exaggeration). For example, the backing and rounding of the nasalized variant of [a] to [ɔ̃] is a change which goes in the direction (but beyond) of automatic tongue retraction and perceptual damping due to nasalization (*cf* Bhat 1975).

An example of the greater stability of a more natural grammatical category would be case syncretism. Among oblique cases (at least in non-ergative languages) the case of the direct object is more basic than the other adverbal oblique cases, *ie* it is more natural to express the direct object with a pure case and not with a pre- or postposition than other oblique case functions.[6] As we expect, many Romance, Germanic, Iranian, Bulgarian, Albanian, Greek languages/dialects have given up or severely reduced all oblique cases besides the accusative. Winter (1969b) explains this with reference to frequency considerations.

Many word formation affixes (and other derivational processes such as compounding and ablaut) have the following types of polysemies: agentive and instrumental meaning (*eg* Latin *-b/-culum*), agentive, instrumental and locative meaning (*eg* Hung. *-ó/ő*, Russ. *-nik*). For reasons that I will discuss elsewhere, the agentive meaning seems to be the most basic, the locative the least basic meaning (*cf* below, § 3). However, change goes not in the direction of increasing, but of decreasing basicness. Often agent suffixes acquire instrumental meanings later on (Panagl 1977:13), and instrumental suffixes acquire locative meanings. Thus lat. *-tor* is only agentive, and Romance languages, *eg* Italien, have retained this suffix (It. *-tore*) but added an instrumental meaning to the agentive one, and the modern dialect of Rome has also a locative meaning (*eg pisciatore* 'urinal'). Thus we have a natural hierarchy, but we cannot yet predict the direction of change, although change is unidirectional; there seem to exist no true counterexamples, but *cf* Lat. *mendicabulum* 'begging machine' used for 'beggar' which is a poetic neologism of Plautus'.

A further complication is the interaction of natural processes or tendencies (Dressler 1977a, b) of different components or subcomponents, which may result in unnatural language change, if the respective tendencies/processes conflict. *Eg* iconism is a basic natural tendency of grammar (Anttila 1975; Dressler 1977a:17f; Mayerthaler, to appear a). One subtendency of it, in morphology and in the lexicon, is sound symbolism. Since smallness and affections are the usual meanings of diminutives and

6. See already Kuryłowicz (1949). For consequences for word order see Khrakovski (1970).

since they are best symbolized (or rather 'iconized') by the vowel *i* (Ultan 1978), it is a natural phenomenon that *i* (or the features [+ high] [− back]) should be the most natural vowel of diminutive suffixes.

However, normal sound-laws (all of them representing natural phonological processes) have not spared diminutives and have changed the more iconic suffixes Late Latin *-illus*, *-ittus* > Ital. *-ello*, *-etto*, Germanic *-ikīna*, *-ila*, *-līna* > G. *-chen*, *-el*, *-lein*, Slavic *-ĭcĭ* > Russ. *-ec*.

This is one instance of the well-known fact that a relatively high percentage of neologistic lexemes and morphemes are iconic, but only exceptionally resist diachronic modification into non-iconic signs due to otherwise natural phonological and grammatical changes. For there is a naturalness conflict between phonology and morphology; see Dressler (1977a), Mayerthaler (to appear a), Wurzel (to appear). In all these cases unnaturalness results from naturalness conflicts (*cf* the concept of 'crazy rule' in Bach & Harms 1972).

Before dealing with intervening variables of a sociological nature which may produce unnatural change (*cf* Hellberg 1978), I must discuss the distinction between origin and spread of change.

Although insisting on the biological substratum of natural change (see Chen 1974), especially of sound change, a strong naturalist must not forget the mental character of change, as has already been recognized and lucidly formulated by Bréal (1897:7; *cf* 10) 'C'est donc dans l'intelligence, dans le cerveau, qu'il faut chercher la cause première des changements phoniques'.

The question as to which age group is the originator of different types of linguistic change (*cf* Drachman 1978; Ferguson & Farwell 1975) is important for the naturalist, especially if he believes in the imperfect learning hypothesis (*ie* that change results from imperfect learning, where learning generally means acquiring the language-specific restrictions of naturalness – Stampe 1969). For different parts of phonology, grammar and the lexicon are learned at different stages of life,[7] and naturalness may not mean the same thing at all these stages, for reasons of both biological and social maturation.

Let us draw examples from analogy:[8] the general prediction is that the less 'natural' member of a class will undergo analogical influence from its more natural partner. For example, due to increasing cerebral lateralization during childhood the right side increasingly becomes more natural to right-handers than the left side. This explains why words for 'left' are often influenced by words for 'right', but apparently never vice-versa (Mayerthaler, to appear b), *eg* Latin *sinister* > *senexter* after *dexter*.

Often the phonological forms of adjacent number names influence

7. *Ie* stages of intensive learning progress or the completion of learning occurs at different ages.

8. See Anttila (1977) for a state-of-the-art report. Anttila bases analogy on semiotics and perception, and thus shares many properties with 'naturalism' as described here. However most 'naturalists' espouse process models to a much larger extent than Anttila does, and are therefore much more open to TG as, by far, the most elaborated process model.

each other: *eg* Anc. Gr. dial. *héks* (6), *heptá* (7), *oktō* > *hoktō*, *optō* (8); *ennéa* > *hennéa* (9). Lith. *devynì*, OCSI *devętĭ* (9) with initial *d* instead of *n* after Lith. *dešimt*, OCSI *desęti* (10), OIr nasalization after '8' in analogy with inherited nasalization of '7' and '9', etc. More examples in Winter (1969a).

Obviously we cannot claim that in general '7' is more 'natural' than '8' or that '9' is less natural then '10', but only that in a decimal system '10' is more basic than other numbers. But such reasoning could not explain why analogy is restricted to adjacent numbers.[9]

Children learn numbers by counting; anticipation and perseveration errors due to adjacent items are well-known psychological phenomena in the sense of performance errors. Thus the imperfect learning hypothesis explains such number name analogy in a natural way.[10]

In the first case of analogy, more 'natural' means semantically or functionally more basic. Analogical influence from a more natural member of a class might also mean the victory of more frequent forms (see Schindler 1974; Vennemann 1972b). However, psycholinguistic research has shown that frequency is more important for the conservation or resistance of an element than for the direction of substitutions. Frequency is only an epiphenomenon of analogy. Therefore the analogical change from Lat. *honōs*, Gen. *honōris* to *honor*, *honōris* should not be explained as analogical change of the more basic form (= nominative singular) due to the more frequent forms (all other case forms[11]) but as being due to the parallel paradigm *-ōr* (later *-ŏr*), *-ōris* (Schindler 1974:4), which fits well with Hooper's (to appear) recent hypothesis on paradigm-internal changes being motivated largely by semantic relations and hierarchies within the paradigm and on analogy of phonological shape as paradigm-external change. However, why did prehistoric **mulies*, Gen. *mulieris* 'wife' change to *mulier*, *mulieris*, although 1) there existed no feminine nouns in *-er*, *-eris* and 2) the theonym *Cerēs*, *Cereris* remained unchanged? Either we say that frequency counts (despite all linguistic critiques) or that **mulies* was changed after masculine *r*-stems in *-er*, *-eris*, which presents us with the problem of how to define a paradigm as an analogical model. If we argue that the gender difference plays no rôle, then the analogical reshaping of **mulies* to *mulier* falls into Hooper's second category, in this case: change of class membership (passing into the class of [otherwise masculine] *r*-stems). Notice also the problem that

9. Otto Back (Vienna), a specialist in number names, has informed me that he knows no good examples of analogous influences from non-adjacent numbers (but perhaps Span. *tercero* 'third' after *primero* 'first'? *Cf* similar exceptions with ordinals in Winter 1969a:38).

10. Winter (1969a) rejects this explanation and prefers the hypothesis of a patronymic analogy. However, with this hypothesis one would expect isolated analogies from *eg* 4 to 14 and for 40 (and not only analogies concerning whole series *eg* 11–19 after 1–9). Exceptions with ordinals to the 'neighborhood constraint' can be explained by the enumeration learning hypothesis, since children count much more cardinal than ordinal numbers.

11. It would be hazardous to claim that Roman children learned paradigms by way of declension and that they changed *honōs* to *honōr* by anticipating the *r* of the oblique cases.

many such analogies can be explained as back formations and then the analogy should take place within the same paradigm.

These examples show that naturalness considerations must be tied to a number of principles if they are to be of any help in the complicated area of analogy. Of course, we could recede to the position that in the case of naturalness conflicts one has the liberty to choose between several strategies; but then at least these strategies should be restricted in number and in the (*eg* probabilistic) conditions of their application.

Another concept is markedness reversal (Andersen 1972:45f; Anttila 1977:107; Mayerthaler, to appear a; Shapiro 1972). Normally the singular is more basic than the plural; but in one case of Latin rhotacism the plural *Lares* is more basic than the singular *Las* (which is then changed to *Lar*), since these Roman gods were normally thought of in the plural (collectively; Schindler 1974:3f), *cf* the markedness reversal from singular-plural to plural-singulative (Mayerthaler, to appear a). However, this concept is a dangerous one, since it is bound to give principles of naturalness far too great power.

What becomes clear from Anttila's (1977:80ff) presentation of H. Andersen's typology of language change is that diachronic analogy is a term covering several related techniques of solving different problems. If this is so, then we can hardly hope to predict unequivocally in which direction analogy goes.

As a reaction to TG formalism many historical linguists (generativists included) have become interested in establishing various scenarios for linguistic change which go beyond characteristics of the purely linguistic system itself and beyond the question of which age-group initiates change. In these developments, considerations of naturalness and of external evidence involving an interdisciplinary approach have played an important rôle. However, these considerations have remained too fragmented. Let us examine cursorily how naturalness enters into such scenarios.

First we must differentiate between origin and spread of change. As to origin of change, H. Andersen's classification of abductive and deductive changes (since Andersen 1973, 1974) is the most systematic study of the initiation of change. At least in phonology it is not difficult to integrate these types of change systematically within a deductive typology of natural processes (Donegan & Stampe, to appear a, Dressler & Drachman 1977).

Closely connected with this first stage of origin is a second stage on which recently Ohala & Greenlee (to appear) have insisted, the stabilization phase of change, as I want to call it, *ie* the question of how an innovator gets away with his innovation. Ohala & Greenlee's answer is that 'this happens potentially every time the pronunciation of a new word is learned and there are minimal sources available to the learner to verify the nature of the pronunciation intended'. These scenarios have to be elaborated and extended to other levels of language than phonology. Then it will be feasible to state and justify more precise constraints on possible natural changes.

Scenarios of the stabilization phase will vary greatly in cases of so-called external change, profiting from very detailed Russian descriptions. Thus Holden (1976) has lucidly discussed naturalness principles in the phonological integration of scholarly words and loan words.

If a new process is due to a substratum or language/dialect contact in general, then we would expect there to be a preference for more natural phenomena to be loaned. But the nature of the differences between the two (or more) linguistic systems involved, the degree of competence speakers have of the other language, and sociocultural factors of all sorts may interfere so heavily (Campbell, to appear; Ferguson 1978:426; Hagège & Haudricourt 1978:40ff) that unnatural changes may result.

In pidginization and language death, naturalists expect more natural categories to be more resistant, whereas in creolization or elaboration of pidgins, less natural categories should develop later than more natural ones, (*eg* Mayerthaler, to appear a). However, due to specific intervening variables these domains of language change also have their own typical stages of evolution (see *eg* Dressler & Wodak 1977; Muehlhausler 1978).

As to the spread of change, many important insights have been obtained by new methods and models. However, they lack the basis of a unifying sociological theory; one candidate might be sociological theories of the diffusion of innovations (reviewed by Cooper, to appear; Fainberg 1977).

For the purpose of this paper, I would like to dichotomize the spread of change into two phases: spread within the same sociolect and dialect (= in-group spread) and spread beyond. For in-group spread two models have been developed: lexical diffusion and change starting from allegro-processes.[12] Since both models are only elaborated in some detail for phonology, I can connect them with the naturalness principles only in phonology. As I have tried to show elsewhere with cases of lexical diffusion in several (ancient) Indo-European languages (Dressler, to appear c) and Breton dialects (Dressler & Hufgard, to appear), lexical diffusion often does not proceed as chaotically as its major proponents (see Wang 1977) seem to admit. Some diffusion processes respect word classes and semantic fields. Moreover, most sound changes spreading *via* lexical diffusion seem to belong to the class of clarification processes, whereas all

12. Unfortunately, the two types of spread are often confused. If only the phonological environment of a process is being generalized, then it is not yet a lexical diffusion process (*pace* Keel 1977), and it has every chance of belonging to the other type of diffusion (from allegro to lento styles), if the process is an obscuration process. Of course such a diffusion may sometimes also show aspects of lexical diffusion (see Dressler, to appear b, c).

13. In NatPhon this distinction goes back at least to the late 1960s when D. Stampe opposed context-free and context-sensitive processes; then P. Donegan Miller renamed them in an LSA talk paradigmatic and syntagmatic processes; now they (Donegan & Stampe, to appear a, b) use the terms fortition and lenition. I find these terms infelicitous, because all process types are (at least basically) context-sensitive and because some obscuration processes (*eg* final devoicing) look like fortitions. Of course, the basic idea is much older.

sound changes spreading from casual speech (allegro styles) to formal speech (lento styles) are obscuration processes. Clarification processes (*Verdeutlichungsprozesse*, see Dressler & Drachman 1977; Dressler, to appear c) (*sc* phonological processes of strengthening, polarization, dissimilation, insertion) are techniques/operations optimizing perception, obscuration processes (*Entdeutlichungsprozesse*) (processes of weakening, assimilation, fusion, deletion) are techniques/operations easing articulation (see Donegan & Stampe to appear a).[13] Both in lexical diffusion and generalization of innovations from casual to formal styles (see *eg* Dressler 1974), sound change may be modified. The study of casual speech is also important for phenomena of drift, on which I will not report, since R. Lass has himself published extensively on such phenomena (*cf* his notions of 'family universals' and 'orthogenesis' in *eg* Lass 1974).

As for 'translectal' spread (*ie* spread beyond boundaries of sociolects and dialects) I want to cite only Labov's (1975) differentiation of change from above and change from below (*cf* Ferguson 1978:436) and to mention that social change is one of the main sources of unnaturalness (Tuttle 1977; Wurzel 1977b) *eg* the inversion of the attribution of more natural processes to lower sociolects (Denison 1977).

In this way the evolution of obscuration processes to generalization in time *via* spread from casual speech (thus obtaining more natural forms also in formal speech) or the generalization of clarification processes from above *via* lexical diffusion may be halted or reversed. The same argumentation may hold for 'natural rule order'. Normally there is a linear order of rule 'blocks': all syntactic rules, then all morphological rules, then all morphophonemic rules, then all allophonic rules.[14] Natural change is to be notated as (1) final rule addition at the end of the syntactic and phonological rule components, and (2) in gradual (irreversible) morphologization of syntactic and allophonic rules.

Within, at least, syntactic and phonological rules the natural order is feeding order and this is also the natural goal of linguistic change (since Kiparsky 1968). If exceptions to these predictions were found, linguists looked either for new formal principles or for external disturbing variables of a sociocultural type.

### 3. Naturalness in typology

Language typology has to be based on language universals,[15] language universals in their turn on extralinguistic universals; see Antinucci

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14. See Koutsoudas (1977); Linell (1977:15 property K). NGP relegates morphophonemic alternations to the lexicon. As to other types of rules, adherents of extended standard theory make a very strict division between rules of different types (rule blocks) and rule blocks are supposed to be applied in a strict order according to the given flowchart of the model.

15. By this I mean universals of language (*langage*), not universals of languages (*langues*) or universals of linguistics, following Coseriu's (1974:§ 3.1) useful distinctions.

(1977), Kacnel'son (1972), Seiler (1978a), for a variety of schools with whom my own approach (see § 1) coincides in this respect.[16] According to a classification dating back to the Prague School, there are three types of typology; general typology (= typology proper), contrastive/confrontative linguistics, and characterological typology, *ie* characterization of a single language from a universalist and typological point of view.[17] On this background any theoretical linguistic research guided by naturalness principles in the way sketched above is automatically a typological study, and on this background the title of § 2 could be reworded as 'naturalness in diachronic typology'.

Since I think that typology means not only cross-linguistic typology, but also cross-domain (or cross-modal) typology, where different domains of external evidence are compared, naturalness becomes even more important. Typology, in the sense outlined here, is not an area of linguistics (as the appropriate section in the *Linguistic Bibliography* suggests), but an aspect of linguistic research, namely the aspect where universal and particular phenomena of language are brought together.

Typology and diachrony are interwoven in many ways, the most trivial one being the principle that 'no diachronic change gives rise to a synchronically non-existent type' (Greenberg 1966:510).

The most general typological claims about naturalness are identical with common claims about markedness. Unmarked categories are more general in the languages of the world than the respective marked ones, and the presence of a marked category within a language or a linguistic type implies the presence of the respective unmarked category (Baumann 1967; Greenberg 1963, 1966; Hjelmslev 1963:104, 109; Jakobson 1958; Keenan 1978:93ff).

However, due to historical accidents of a sociocultural nature (see § 2) exceptions to such typological implications may occur. Therefore it is better to say that linguistic elements do not imply each other, but favor each other's presence in a given linguistic type (Skalička 1966). This is an attenuation adequate for an inductive, probabilistic typology. However, deductive typology also relativizes the importance of typological implications. Just as Stampe (1973) explained Jakobson's (1958) phonological implications as reflexes of universal phonological processes, grammatical implications should be derived from universal operations and their functions (see § 2). Exceptions to implicational universals are then to be explained by very specific constellations of intervening variables or by naturalness conflicts (see § 2).[18]

16. Thus one could say that all universals are weak linguistic universals (*ie* dependent on extralinguistic facts), and that the existence of (strictly independent) strong linguistic universals (Mc Neill 1970:73f) is negative: also universals of linguistics must be connected with universals of other disciplines.

17. According to the theoretical stand of the linguist, he confronts the language under investigation with the deductive hypotheses derived from universals and specific intervening variables or he interprets hermeneutically phenomena of his language as manifestations of universals.

18. Therefore in Dressler (1977a, b) I spoke of universal tendencies rather than universals.

The interaction of natural processes of different components or subcomponents of language may lead to a conflict whose solution often inevitably diminishes the naturalness of one of the processes. Thus the fusion of vowel and nasal to a nasalized vowel is more natural before fricatives than before either obstruents (Mayerthaler 1975; Schourup 1973). Since *Ṽ* from *Vn* is the outcome of two obscuration processes (assimilation and 'monophthongization'), and since in casual speech obscuration processes (*Entdeutlichungsprozesse*) are generalized, there should be languages where /Vn/ gives [Ṽ(:)] in casual speech styles before all obstruents, in formal styles only before fricatives, but not *vice versa*. This is the case in Breton dialects (Dressler 1974). However Bailey (1978) has described an American dialect in which *talented* gives in lento [ˈthæləndId] and in allegro [ˈthælə̃dId] as we expect, whereas *demonstrate* gives [ˈdɛmə̃sˌthrɛˈt] in lento and [ˈdɛmənˌstrɛˈt] in allegro.

Justification of this paradox is twofold. (1) according to Bailey *Vn* → *Ṽ* is a tautosyllabic rule; (2) Bailey formulates the following principle of English syllabization: 'Provided that clusters which are not permitted wordinitially are excluded, more consonants are grouped with a following more-heavily accented nucleus than with a preceding less-heavily accented nucleus as the tempo increases and the pronunciation is less monitored'. (Bailey 1978, who intends this principle to be a natural one holding for languages with English-like prosodics). Thus we have a conflict between prosodic and segmental naturalness. If Donegan & Stampe (to appear a, b) are correct in giving priority to prosodic phonology over segmental phonology, then the contrast between allegro *Vn* and lento *Ṽ* is still an unnatural, but not an inexplicable idiosyncracy of English. (For other cases of conflicting principles, see Donegan & Stampe, to appear b § 7.4).

For other problems of phonological typology in a 'naturalist' framework I can refer to Dressler (to appear b). Here I want to show how current typological trends in word-order studies are based on naturalness principles similar to those in NatPhon and Natural Morphology and polycentristic phonology or morphology.

There seems to exist a psychological or psycholinguistic principle which makes the first [+ human] noun of a sentence likely to be an agent (Antinucci 1977:51ff; Bever 1974:1177). More specifically, in subject-object-languages, there is a stage in first language acquisition (Bever 1974:1186ff; Fava 1978) and there are syndromes in aphasia where all sequences of two nouns (which are suitable for expressing subjects or objects) are interpreted as subject-object and where only this order of subject and object is produced. This is the natural basis of the fact that with very rare exceptions (Derbyshire 1977) existing word-order types (of the elements O, S, V) are restricted to the three types SOV, SVO, VSO (Antinucci 1977:53ff; Greenberg 1963; Keenan 1978:113ff; Steele 1978).[19] The basic order OS, found in very few languages, must then be

19. Since one might claim that there is a sentence semantic level between the psychological and the syntactic levels, let me refer to the view (put forward, independently, by Antinucci 1977 and Keenan 1978) that the most natural syntactic order projects the underlying semantic order. Khrakovski (1970) even constructs a universal typological schema subject-object.

derived from very specific (and thus improbable) constellations. Secondary word orders OS are then explained by the priority of another natural principle of serialization based on the communicative purpose of giving new information only after mentioning old information which supplies the background (Antinucci 1977:115ff; Daneš & Firbas 1973; Dezső 1974; Li & Thompson 1975).

My final application of naturalness to typology lies in an area which is exciting but has often proved to be dangerous for linguists, poetic language and, more procisely, poetic license. Why is it the case that in English, German, Russian, and Polish poetry of our century much more (quantitatively) and more adventurous and deviant derivational neologisms[20] can be found than in comparable French and Italian poetry?[21] I suggest the following explanation: (1) Poetic language must be understood as the highest form of a language and its fullest realization (Coseriu 1971; Kloepfer 1975:8, 29ff); (2) poetic deviations from linguistic, stylistic, poetic and individual norms go in the directions of either the specific possibilities of the given language or of universal natural processes and tendencies (Dressler, to appear a); (3) in the common Roman types of languages word formation plays a much smaller rolle than in the German common types of Germanic and Slavic languages; (4) given these premises, we can understand that poets follow the nature of their language and are more creative in more productive components of their language(s).

Obviously, here, too, sociocultural factors are important, as we see if we compare poets with their competitors in word formation creativity, advertising specialists. For obvious sociocultural reasons, Italian advertisers create far more neologisms than Italian poets, whereas in the Soviet Union the reverse relationship seems to hold.

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20. At least according to my own humble reading of poets and secondary literature, and my – still provisional – analysis of neologistic word formation.

21. Eg *manunkind*, *wherewhen* (Cummings), *endlessnessness*, *he smellsipped* (Joyce), *Whom wouldst thou uncharnel?* (Byron), *der große Derdiedas* (Arp), *das Genicht*, (Celan), *ichig* (Musil), *šestvye zlyx černičej* (Belyi), *ljudogus'* (Majakovskij), *pesnijanka* (Kamenskij), *ltenie* (Hlebnikov), *brouillifiquement*, *melancholifié* (Balzac), *macchina distruggo-creativa* (Alesi).

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## *Roger Lass:* Discussion

### On some possible weaknesses of 'strong naturalism'

Since Professor Dressler has paid me the compliment of referring to me in his paper as 'eminent', it might seem a bit churlish to challenge his basic theoretical position. But I'm going to anyhow, because my own interpretation of the subject under review is rather different from his, and because I think the interests of conceptual clarity are always served by firm statements of opposing views. But, like Dressler, I too am putting forth a personal (though I hope argumentatively respectable) position; it will be up to others to decide which (if either) of us is right.

Dressler's reference to my own work on the problem of naturalness might suggest that we agree fundamentally on the importance and implications of the topic; but this is not really the case. Over the years, our concern with this general area seems to have moved us in opposite directions: Dressler toward an increasing commitment and interest, myself toward an increasing skepticism. For this reason, and because his paper seems to me to assume something of a pre-existing sympathy on the part of his audience (that he is in a sense 'preaching to the converted'), I shall deliberately assume the guise of one *in partibus infidelium*, and attempt something of a counter-sermon.

Perhaps the main reason for our divergence of views is a difference of approach to the subject matter: Dressler's interest has been focussed on the phenomenology of language change, while mine has increasingly focussed on the methodology of linguistic research, especially diachronic (*cf* Lass 1975, 1976, 1977, 1978a, b, 1980). It is no accident, I think, that this difference of focus has led to a diametrical difference of opinion on naturalness; and that is perhaps my main point here. Dressler's abstract and paper suggest (if I interpret him correctly) that his interest in naturalness, and his claims for its importance, are based on two cardinal assumptions:

(1) that the goal of diachronic (or any other) linguistics is the *explanation* of linguistic phenomena;
(2) that naturalness considerations are in some important sense explanatory, and that therefore a position of 'strong naturalness' conduces to the primary goal.

I will argue here that while the first assumption may seem to be laudable, it probably cannot serve as the basis of a viable research programme in linguistics; and that, be that as it may, the second assumption is probably untenable; at least within the bounds of a strong theory of explanatory goodness. Later on, I will make some suggestions about the minimum requirements for an explanatory theory of naturalness, and this should make it clear (a) that these requirements have not yet even approached

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being met, and (b) that they probably cannot in principle be met. What follows, then, will focus not on particular points in Dressler's presentation as such, but on the philosophical framework in which it is set.

Let me begin by agreeing with Dressler: on his rejection of what he calls a 'frequency' approach to problems of naturalness or markedness (I maintain that the two concepts are the same: *cf* Lass 1980: § 2.8). In fact I want to spend some time on the reasons for rejecting a frequency approach, because I hope to show that at least some of the same objections can be levelled against strong naturalism, *ie* naturalness theory based not on mere frequency but on so-called 'substantive' considerations.

Now the primary objection that can be brought against any theory which merely equates 'naturalness' with statistical frequency of occurrence (cross-linguistically, over time) is that it is simply an instance of the naming fallacy: a taxonomic generalization without content. In short, that such a theory (like the Chomsky & Halle 1968 theory of markedness, despite its talk about 'intrinsic content') is an uninterpreted calculus: the terms 'natural'/'unnatural', etc, have no empirical content, but are simply names for what might just as well be (for all we know) contingent facts about the distribution of properties across the set of naural languages. A frequency interpretation, that is, does not *motivate* the phenomena.

For instance: there is a marking convention in Chomsky & Halle (1968: 405, convention XI) which says in effect that it's 'natural' or 'unmarked' for front vowels to be unround, and for nonlow back vowels to be round. Now this is clearly a judgement based on frequency – and a reasonably accurate one. *Eg* the data gathered in Sedlak (1969) suggests that only about 15% of the world's languages have front round vowels, and 85% lack them. Thus front round vowels are 'highly marked', 'unnatural', etc. The usual tendency in generative marking theory is then simply to extrapolate these distributions into an evaluation measure (under the assumption that somehow they have 'content'), so that we can then 'reward' or 'punish' rules on the basis of their outputs, or languages on the basis of their inventories (for further discussion *cf* Lass 1975, 1980: ch. 2).

Thus Germanic *i*-umlaut is 'disvalued' because it violates the linking principle and creates front round vowels (even though, as an assimilation, it's a paradigmatically natural rule type: Schane 1972). But the unrounding of front round vowels in most forms of English or some Austrian dialects (Keller 1961: ch. VI) is 'good' because it reduces systemic markedness, whereas the restoration of front round vowels in these same Austrian dialects before [ɫ], is 'bad' because it increases markedness, etc. This much is simply symbol-manipulation and of no particular interest; unless you happen to like evaluation measures (and I suspect Professor Dressler would agree with me that they are essentially boring).

But what if it were the case that this particular (statistically correct) naturalness judgement could be shown in fact to be tied in in some way, or ideally derived from, some genuinely 'substantive' considerations – specifically, as Dressler says, 'considerations of the nature of man', in the case of phonology 'the study of articulation and perception'? Would this help any? Would this give the (former) frequency judgement a kind of intellectual respectability and explanatory force that it lacked before?

Now there has been at least one attempt to turn this particular frequency fact into a substantively natural fact, specifically by connecting it with perception. And the structure of this argument and its consequences are of particular interest. Chin-Wu Kim (1973) argues that the source of this distribution of vowel roundness is a perceptual principle that he calls 'the principle of optimum opposition'; which says that on perceptual grounds (1973:409).

> Language being a code of communication, the task of comprehension . . . is much easier if the two or more significant signals differ as much as possible . . . From the speaker's point of view, the optimum differentiation will give a margin of error, which would not be the case if all signals were clustered around the same area.

This works as follows here: the 'natural' distribution of vowel rounding creates a situation where back and front vowels at the same height are maximally distinct. *Ie* lip-rounding lowers F2 of vowels, and so does retraction; so a rounded front vowel is perceptually backer than an unround one. And since lip-spreading raises F2 on a rounded or a back vowel, a spread back vowel would sound fronter than a rounded one. Therefore the 'unmarked' distribution assures maximum distinctiveness.

But does this substantive interpretation really turn the marking convention into an explanation of the distribution? The answer is clearly no. In fact, I would claim that we have no more *strictly relevant* information than we had before. And the reason is that this empirical interpretation for what was formerly an uninterpreted frequency judgement fails to be explanatory *for any particular case*. For instance, if a language has no front round vowels, or loses them, we can attribute this to the 'natural' principle; but if a language creates front round vowels (especially context-free, as in Icelandic, Dutch, Scots), or retains them (as most Germanic languages have for at least a millennium and a half), what do we attribute *this* to? A 'competing tendency'? But this is empty unless we can define such a tendency in advance, and predict when interaction between two tendencies will cause one to be thwarted (and which one); and this I maintain we can't do to any significant degree. I will return to this shortly. For now, I merely observe that this is an exemplary instance of a (motivated) naturalistic explanation, at least from a methodological point of view.

The crucial point is that any explanation which relies on a 'natural' property which is not fully universal, and does so by allowing (undefined or loosely defined) 'intervening variables' or 'competing tendencies' is in form an argument that you can't lose; empirically speaking, no single occurrence can serve as a convincing counterexample. And this makes any claim to explanatory force, as we shall see, empty. That is, unless you can tell me precisely why German kept its front round vowels, but most forms of English and Yiddish lost theirs, no explanation on naturalness grounds will account for the English and Yiddish losses.

In fact, the only naturalness claims that have genuine empirical content are those whose range is the entire set of natural languages, without exception. Thus I can, on naturalness grounds, explain in a satisfying way why *no* language has apico-glottal consonants, but I can't explain why ejectives or clicks are rare. That is, universal occurrence or non-occurrence amounts to a series of individual predictions: each potentially occurring case is catered for. But statistical predictions hold only over aggregates, and cannot be falsified by any single failure; so, conversely, they cannot be explanatory for any single non-failure. Since ultimately we are concerned with the speakers of individual languages (for each particular case we are trying to explain), phenomena like 'rarity' have no explanatory force whatever, even if they are empirically interpreted. I would maintain that substantively based naturalness theories can furnish satisfactory explanations only of phenomena that involve the potential transgression of the boundary conditions defining natural languages.

All we can say at the moment, for any cases involving cross-language distributions of properties of other than 100% or zero, is that when languages behave 'naturally' we ought not to be surprised; but when they behave 'unnaturally', we ought to be surprised, to a degree corresponding to the improbability of the property in question. The problem is then what to do with languages that appear to flout the canons of optimalness. Is it the case that some speech communities just don't care about doing things the natural way, while others do? If this is true (as I suspect it is), then no 'natural' principle has a genuinely naturalistic basis: they are all 'contaminated' by culture. We are forced to admit speakers' intentions, or, broadly, free will, which takes us outside the realm of the naturalistically explicable (more on this later). Speakers are free to behave as they wish (within the limits set by the boundary conditions), and both cross-language distributions and naturalness considerations may be red herrings.

For instance: say some process can be described as following some natural inertial gradient in the vocal tract (like assimilations generally), or following some apparently natural perceptual or cognitive principles, etc. If it is still possible for a speaker *not* to utilize this process, then the inertial gradient or perceptual principle cannot be said to cause the process to be implemented for any speaker who does have it. The same is true of a 'natural' rule which is later 'denaturalized'.

What I am saying here is that if an event or property cannot be shown to be caused, it cannot be said to be explained. True explanation is causal, and 'explanations' which do not involve causality are not explanations in the proper sense, but something quite different. Even if some phenomenon is naturalistically *interpretable*, this doesn't mean that it's *explicable*.

The crucial test for an explanatory theory is its ability to account for *single spatiotemporal occurrences*: at least within the kind of theory of explanation that animates the (neo)positivist view of the natural sciences.

We may question the appropriateness of this kind of explanation in linguistics – as I intend to do – but as long as we claim a physicalist (or even perceptual or cognitive) definition of naturalness, we at least appear to be arrogating to ourselves the kind of explanatory power that the natural sciences claim, and we therefore open ourselves to judgement by the standards applicable to them. By invoking 'law-like' statements in explanations, we claim implicitly to be operating in a law-governed domain; and this is the only kind of domain in which genuine theoretical explanation is possible.

The difficulty with strong naturalism is that (as I understand it) it claims to *derive* the properties of natural languages in stasis and change from the properties, physical or mental, of speakers: it is in essence reductionist. And there are good reasons, philosophical, empirical, and even moral, why this kind of reductionism is untenable (for two spirited philosophical attacks on reductionism see Polanyi & Prosch 1975 and Popper & Eccles 1977; see also the essays in Koestler & Smythies 1969, and the discussion in Lass 1980: ch. 4).

I ought now to clarify why this reductionism is necessary, why it is the case that only a fully reductionist theory can be truly explanatory. And to justify my rejection of such theories as appropriate for linguistics. I will first suggest what the structure of a genuinely explanatory theory must be like, by considering the epistemic properties of strong explanations; I will then suggest why explanations like this are inappropriate in *Geisteswissenschaften* like linguistics; and finally, I will suggest that strong naturalism may misconceive some fundamental properties of language ontology: in a way that can be brought into relief by considering the views of one of the men in whose honour this symposium is being held.

There are two possible interpretations, in standard metascience, of what an explanation is: one strong and one weak (*cf* Hempel 1966, Lass 1980: ch. 1 and reffs.):

(1) STRONG. A conjunction of statements (specifically initial conditions or state-descriptions) and laws that predicts (or retrodicts) some specific phenomenon.

(2) WEAK. A conjunction of statements (initial conditions or state-descriptions) and statistical laws that makes some specific phenomenon highly likely.

The strong type is the familiar 'deductive-nomological' or 'covering-law' explanation (Hempel & Oppenheim 1948); the weak type is Hempel's 'probabilistic explanation'. We might add to this a third, even weaker type, which has been extensively discussed, among others, by Michael Scriven (1959, 1963) and Ernst Mayr (1968):

(3) A conjunction of statements (none of them necessarily lawlike) that makes some specific phenomenon highly PLAUSIBLE, without making it either necessary (as in type 1) or even highly likely (as in type 2).

For any phenomenon p, then, we have the following relation between it and the three explanation types:

(1) makes p necessarily the case
(2) makes p highly likely to be the case
(3) makes p plausible, or makes it possible that p

Thus the modalities of explanation statements in these three frameworks are different, and therefore their epistemic force is different as well. Since all explanations of type (1) can be reconstructed as valid inferences (by *modus ponens*) from the conjunction of initial conditions and laws, they are in fact logically true. This is not the case for any of the others.

Therefore type (1) is epistemically so different from the others that if we want to call one of them 'explanation', the others ought to have a different name. And there is a clear hierarchical ranking, in terms of the quality of knowledge we get as output: (1) is the only type where, as the result of an explanation, we can be said to *know why* p happened or is the case. In the others, the act of explanation (or 'explanation') produces very different effects:

(2) Lack of surprise (in a statistical/actuarial sense) that p.
(3) Intuitive feeling that nothing in nature makes it necessary that not-p; but that p is 'conduced to', though with no actuarial intuitions attached, and thus no particular significance attached to not-p.

So in order to have a fully convincing causal explanation, you must have laws; and this means that explanation (in the strong sense) is possible only over fully deterministic domains. And the two other types do not seem to me to carry much epistemic weight (*cf* Lass 1980: §§ 2.3–8, 4.5). If we can accept this much, then it is fairly clear what directions a strong naturalist theory would have to move in to become an explanatory theory, and not a descriptive taxonomy, which it seems mainly to be at the moment. (Not that this is bad: in what I think may be a truly Hjelmslevian spirit I think there's a good deal to be said for it, but more on this shortly).

The basic problem then is this: in order for any event in language history, or any synchronic property, to be truly explained, it must have been possible to *predict* it: in the strict sense of deducing a sentence representing it from a conjunction of state-descriptions and laws. I think it is clear that this strong deducibility requirement is, at the moment, met only for relatively trivial cases, like the lack of apico-glottal consonants and other 'boring universals' (which are in effect merely epiphenomenal on speaker-defining boundary conditions).

It is further the case that a theoretical framework that predicts 'with high likelihood', or merely posits plausibility conditions, does not produce satisfactory explanations, since the events or properties in question are stille perfectly free not to occur. Further still, neither of these frameworks has empirical content (in the Popperian sense: Popper 1968, etc.), since they are so constructed that it is not possible to specify the potential falsifiers for any claim.

So if we want to have a genuinely explanatory theory of naturalness, it will have to be based on a precise formulation of all 'intervening variables' and the like; and it will have not only to predict all 'natural' phenomena, but to specify in advance the precise situations under which 'unnatural' ones will occur, and which ones. In principle, from the point of view of what we might call 'the progress of knowledge', this is surely a desirable goal to aim at, at least in a heuristic sense: for a programme of deductive subsumption of all phenomena under covering-laws will purge from out descriptions all the pseudo-indeterminisms that we were formerly unable to identify as such, and enable us to recognize the residue (of whatever size) of genuinely indeterministic instances. But I think the likelihood of achieving this is vanishingly small.

There are two main reasons for my pessimism, one concerning the nature of human beings (and by implication of the human sciences so-called), and the other concerning what I take to be some curious but significant properties of language itself. The human reasons first: these concern the necessity of having full determinism in order to have explanation. If explanation implies prediction, this implies law-governedness: so only a fully deterministic view of man will allow theoretical explanation of his artefacts and behaviour. And this kind of determinism has to be rejected on two sets of grounds, one empirical/argumentative, and the other moral. The first set of grounds is fairly clear, and probably has been since Chomsky's review of Skinner's *Verbal Behavior* (1959). Most of us, I think, would feel confident in rejecting, say, Bloomfield's behaviourist fable of Jack and Jill as the basis for a speech act theory (Bloomfield 1933:22f; *cf* the discussion in Wunderlich 1979:258ff), as we would reject any theory reducing linguistic behaviour to strict S – R terms, or in fact any theory of biological determinism that disallowed the mediation of an independent mind. And, to take up the moral issue, I think that even if it could be shown that such a theory were in fact defensible, we ought still to reject it, on the grounds that if we're really *bêtes-machines* like this, it would be better not to know about it, and dehumanizing and immoral to behave as if we believed it. But I doubt if any other framework could support strong naturalism.

The other problem for naturalism, at least insofar as I have not misrepresented it by claiming it to be fundamentally deterministic, lies in the fact that it may possibly misrepresent language ontology in a damaging way. I will approach this by way of some remarks of Hjelmslev's about the goals of linguistic theory. I think that the difficulty is that naturalistic theories essentially fall into what, following a suggestive metaphor of Hjelmslev's, I will call the Fallacy of Projection.

If I read him correctly, Hjelmslev suggests in his *Prolegomena* (1963) that the primary goal of linguistics is to be exhaustively taxonomic, not naturalistically explanatory. He warns against taking some putative 'locus' or ontological projection of language as the *Ding an sich*: language is to be studied first of all *in itself*, in terms of its immanent structure, and then only secondarily, if at all, 'projected'. Thus he writes (1963:4) that 'the physical, physiological, physchological and logical phenomena *per se* are not language itself, but only disconnected external facets of it, selected as objects of study'; these things are rather 'precipitations of language' (5).

What linguistic theory ought to do (8) is to

> seek a *constancy* [emphasis Hjelmslev's] which is not anchored in some 'reality' outside language – a constancy that makes a language a language ... When this constancy has been found and described, it may then be projected on the 'reality' outside language, of whatever sort that 'reality' may be ... so that, even in the consideration of that 'reality', language as the central point of reference remains the chief object – and not as a conglomerate, but as an organized totality with linguistic structure as the dominating principle.

Hjelmslev's claim can basically be interpreted as positing language as something *self-existent*: though one hesitates to attribute opinions to those no longer with us, it seems to me likely that he would not have been unsympathetic to the notion of language as essentially what Popper would call a 'World 3' object (Popper 1973, Popper & Eccles 1977; *cf* Lass 1980: ch. 4). That is, language as primarily a self-existent entity with which human beings interact; but one which, because of its historicity (*ie* its status as an object handed down by cultural tradition), its mode of acquisition, and its ontological independence is in no way determined by the specific properties of the systems that interact with it. Except of course in the sense of being responsive (of necessity) to the boundary conditions defining those systems, and (again of necessity) of being appropriate for its uses. *Ie* no language will – as a matter of definition – be unlearnable, unspeakable, non-communicative or inexpressive. But beyond this, to quote Hjelmslev again, there is 'an arbitrary relation between form and substance' (97): since substance is simply a culturally chosen – if biologically limited – way of giving form its necessary manifestation. And since (110) 'language is independent of any particular purpose' – except, broadly, to be 'usable' – the number of variant shapes it can take is immense, and probably only trivially affected by the grosser physical and mental properties of its users.

I present these admittedly speculative and polemical remarks primarily with the purpose of polarizing debate; since it seems to me that a position like Hjelmslev's is reasonable – certainly not one that ought to be dismissed out of hand. And considering the monistic innateness-ism and psychologism that seem to dominate 'mainstream' linguistics at the moment (I refer here not to Dressler's temperate naturalism, but to the kind of neo-Chomskyan excesses that characterize works like Smith & Wilson 1979), it seems worthwhile considering the opposite point of view. The 'truth' (if any) may lie on one side or the other, or in between: but the discipline is not well served by refusing to consider the less fashionable views, or to argue their merits seriously and see where the argument leads.

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## *Wolfgang U. Wurzel:* Discussion

## Some remarks on the relations between naturalness and typology*

It is always difficult to comment on a paper when one agrees with the basic principles underlying it. This is precisely the situation in which I find myself here. I believe that Wolfgang Dressler has mentioned most of the essential aspects of the relationship between naturalness in grammar and language typology and has given an appropriate outline of these concepts. Therefore I do not want to spend much time on Dressler's paper, but I would like to take the opportunity to address in more detail some problems that seem to me particularly important for a treatment of the question of naturalness and typology.

The most important theoretical point of Dressler's exposé is the notion of *naturalness* itself. This concept considers as *natural* those grammatical properties which arise in the course of language development without the benefit of language mixing, loans, or normative measures. Another way to define the notion of naturalness is to concentrate on those properties of natural languages which, conceptually, perceptively, and/or in articulation are clearly simpler than their counterparts. The occurence of non-natural or less natural properties, structures, and rules always implies the occurrece of natural or more natural ones, but not the other way around.

When one considers what the character of naturalness in grammar is, *ie* how natural elements arise in a language, *biological* and *social* factors can certainly be held responsible for such phenomena, as Dressler has already pointed out. In particular, those properties that all natural languages have in commen, the so-called universals of grammar, are not only based on innate, species-specific, human traits, as pointed out by Chomsky (1965: 27 ff). They are also determined by social factors, *ie* by the productive activity of humans in society and the consequent needs of communication. Man as opposed to all other animals is primarily a social animal. Therefore it would be highly unlikely that the common properties of natural languages are purely biologically determined. It seems to be beyond doubt that besides biologically based universals, we can assume universals that are socially determined, and universals in which both biological and social determinants interact (*cf* Neumann, Motsch & Wurzel 1979). I want particularly to stress this point, although Dressler has already mentioned it, in order to prevent any misinterpretation of naturalness in a biological sense, as an opposite to socialness.

We have said that naturalness in the sense of Natural Grammar is based on the biological and social determinants of language. I believe that it is no exaggeration to state that the concept of naturalness and its counterpart, *markedness*, are basic concepts for any understanding of the essence of natural language.

* I thank Henriette F. Schatz, University of Amsterdam, who translated this paper into English.

The evidence for naturalness can be obtained from language change, language acquisition, language disorders, error analysis ('*Fehlerlinguistik*'), speech perception and other such phenomena. When we consider these aspects of language with regard to the occurrence of grammatical structures, we see clearly that they are:

not influenced and produced to the same degree by language change;
not mastered at the same time in child language acquisition;
not equally simple in speech perception;
not affected by language disorders (aphasia etc) to the same extent;
not affected equally by speech and language errors;
not found with the same degree of frequency in the various langages of the world.

(Of course, this last point is of particular importance for our discussion of language typology.)

A grammatical structure or process can thus be considered *natural*, when it is extensively present in the various languages of the world, when it is relatively resistant to linguistic change, when it is acquired at an early age, and when it is relatively resistant to language disorders.

It follows from what we have discussed so far that naturalness is a concept that applies to all languages, and therefore can only be defined in terms of universal properties. Individual language-traits are taken to be representations of general properties of language. Consequently, the concept of naturalness is closely related to the notion of universals. Likewise, questions of typology are only meaningful when we presuppose that natural languages do have common traits and that on this basis specific properties of individual languages and language groups may be found. Naturalness and typology are interconnected, since they are both based on the concept of universality, so we are forced to posit the tripartite connection of naturalness, universality, and typology.

Given this relationship, it is entirely possible to posit typological classifications of language phenomena based on the notion of naturalness. Questions such as the following are of crucial importance in this context:

(I) What are the *natural foundations* of a linguistic phenomenon?
(II) *To what degree* is naturalness constrained in this linguistic phenomenon?
(III) *In what way* is naturalness constrained in this linguistic phenomenon?

The rules in Natural Grammar are not formulated on the criteria of formally understood simplicity or elegance, but on the criterion of naturalness. Any inquiry into and formulation of rules for an individual language must therefore always be a detailed response to these three questions. Grammar-writing in the sense of Natural Grammar always implicitly includes typology. This is shown in the following two examples, one phonological, the other morphological.

In German, as in many other languages, there is a phonological rule that assimilates a dental nasal to the place of articulation of a following velar or labial obstruent:[1]

(R 1)

$$\begin{bmatrix} +\text{nasal} \\ +\text{coronal} \end{bmatrix} \rightarrow \begin{bmatrix} \alpha\ \text{anterior} \\ \beta\ \text{coronal} \\ \gamma\ \text{high} \\ \delta\ \text{front} \end{bmatrix} / \_\_\_ \begin{bmatrix} +\text{obstruent} \\ \alpha\ \text{anterior} \\ \beta\ \text{coronal} \\ \gamma\ \text{high} \\ \delta\ \text{front} \end{bmatrix}$$

The application of this rule is constrained by a hierarchy of conditions, depending on style and speech-tempo. The rule applies:

in style (a) only within a morpheme: *Dank*, *Ding*; the syllable is not the decisive factor here, which is shown by *danken*, *Dinge* etc;
in style (b) as in (a) and also in non-native words of the type *konkret*, *Kongreß*;
in style (c) as in style (b) and also in words of the type *ungefähr*;
in style (d) as in style (c) and also in words of the type *ungenau*, *unpräzise*;
in style (e) as in style (d) and also in compounds of the type *Klein-garten*, *Klein-bahn*;
in style (f) as in style (e) and also in phrases of the type *an Gustav*, *an Bärbel*.

When the questions we formulated above are viewed with reference to this rule, we find the following:

(1) This phonological rule is a language-specific realization of the articulatorily conditioned phonological process of nasal assimilation in the sense of Stampe (1969).
(2) The naturalness of the rule is relatively weakly constrained; the phonetic motivation of the rule is intact.
(3) The constraints in the various styles have a clearly morphological (semiotic) character: the unique phonetic form of morphemes and words tends to be preserved, independently of the phonological context. The more closely two morphemes are related syntactically and morphologically, the more likely that assimilation takes place across morpheme and word boundaries.

Note, for example, in the case of *ungefähr* with [ŋ] vs. *ungenau* with [n] in style (c) that there is no such word as **gefähr*, but the word *genau* does exist.[2]

1. For a more detailed treatment of nasal assimilation in German and Italian in the framework of Natural Phonology *cf* Herok & Tonelli (1977).

2. A historical parallel of words like *ungefähr* is the case of *Him-beere* 'raspberry' from Old High German *hint-beri*, where the association with *hinte/hinde* 'hind' and in many German dialects also the word *hinte/hinde* itself got lost and the nasal became assimilated.

Here we see, by the way, the typical contradiction between phonological and morphological naturalness: phonological naturalness is constrained in favour of its morphological counterpart, especially in styles (a) and (b) which represent the norm of the standard language.

It is interesting to compare under what conditions nasal assimilation functions in other languages. In English the constraints on rule application are also hierarchized. Compare eg

| | | | | | |
|---|---|---|---|---|---|
| style (a) | *concrete* | 'aus Beton' | ['kaŋkri:t] | 'konkret' | [kən'kri:t] |
| style (b) | *concrete* | | ['kaŋkri:t] | | [kəŋ'kri:t] |
| But never: | *concrete* | | ['kankri:t] | | [kəŋ'kri:t] |

If we refer again to our three questions, the first two can be answered in the same way as for German, but the third (namely: In what way is naturalness constrained?) requires a different answer:

(3) The constraint on the rule in question is phonologically conditioned, under the influence of a hierarchy-principle for regressive consonant assimilations. It can be formulated as follows: the assimilation will not take place across a syllable boundary if it cannot also take place within the syllable.[3] In the case of '*concrete* the [k] is partially incorporated in the accented first syllable so that the syllable boundary lies within the [k]. Consequently the cluster 'nasal plus obstruent' becomes *tautosyllabic*. On the other hand, when the second syllable is accented, the [k] remains part of it, so the cluster 'nasal plus obstruent' is *heterosyllabic*. In style (a) the naturalness is constrained by non-application of the assimilation-rule, to the cluster /nk/ of *con'crete* [nk], but the constraint is purely phonologically conditioned: assimilation of a nasal to on obstruent is more natural than its non-assimilation, while assimilation that does not cross a syllable boundary is again more natural than one that does.

In Italian, clusters of the type 'nasal plus obstruent' behave in a different way, since the naturalness of nasal assimilation is neither morphologically nor phonologically constrained. Compare forms like *banco* 'bank', *incontrare* 'to meet', *congresso* 'congress', *in casa* 'in case', all pronounced with [ŋk], as well as *compassione* 'pity' and *con passione* 'with passion', both pronounced with [mp] in normal speech.

I believe that the example of nasal assimilation shows without further explanation how closely naturalness and typology are related in the framework of Natural Phonology.

The morphological example is taken from Russian noun-inflection, where we compare the paradigms of the most common masculine and feminine inflection classes, *stol* 'table' and *kniga* 'book':

| | | | | |
|---|---|---|---|---|
| nom. | sg. *stol* | pl. *stol–y* | sg *knig–a* | pl. *knig–i* |
| gen. | *stol–a* | *stol–ov* | *knig–i* | *knig–Ø* |
| dat. | *stol–u* | *stol–am* | *knig–e* | *knig–am* |
| acc. | *stol* | *stol–y* | *knig–u* | *knig–i* |
| instr. | *stol–om* | *stol–ami* | *knig–oj* | *knig–ami* |
| prep. | *stol–e* | *stol–ax* | *knig–e* | *knig–ax* |

3. I owe the formulation of the principle to Theo Vennemann.

In both inflection classes the case-number-forms are created by adding inflectional suffixes (apart from the plural feminine genitive), *ie* the rules that operate here are *additive inflection rules*. However, there is a difference between the masculine and feminine paradigms. In the masculine words *the whole word* is inflected, and the lexical representations are of the type $//\mathrm{stol}/_{\mathrm{St}}/_{\mathrm{N}}$ (stem and word are identical). In feminine words only *part of the word*, namely the *stem*, is inflected, resulting in lexical representations of the type $//\mathrm{knig}/_{\mathrm{St}}\mathrm{a}/_{\mathrm{N}}$. Compare for example the following inflection rules ('IC' means 'inflectional class'):

$$\text{(R 2)} \quad \begin{array}{l} +\text{Genitive} \\ -\text{Plural} \end{array} \rightarrow \mathrm{a}\ /\ \frac{\phantom{xx}}{+\mathrm{IC}_i}\ /\mathrm{N}\ \rule{1cm}{0.4pt}$$

$$\text{(R 3)} \quad \begin{array}{l} +\text{Genitive} \\ -\text{Plural} \end{array} \rightarrow \mathrm{i}\ /\ \frac{\phantom{xx}}{+\mathrm{IC}_j}\ /\mathrm{St}\ \rule{1cm}{0.4pt}$$

The genitive plural of *kniga* is formed in a different way, without attaching an inflectional affix to the word, by zero-inflection. This results in the shorter form *knig* compared with the nominative singular *kniga*:

$$\text{(R 4)} \quad \begin{array}{l} +\text{Genitive} \\ +\text{Plural} \end{array} \rightarrow \emptyset\ /\ \frac{\phantom{xx}}{+\mathrm{IC}_j}\ /\mathrm{ST}\ \rule{1cm}{0.4pt}$$

Although the rule does not really change anything it must appear in the grammar, because in all cases without explicit inflection, a form similar to the nominative singular is used, compare $//\mathrm{mest}/_{\mathrm{St}}\mathrm{o}/_{\mathrm{N}}$ 'place' which has an accusative singular like the nominative singular *mesto*.

Russian noun-inflection is governed by three different types of rules:

additive word-inflection rules;
additive stem-inflection rules;
stem-inflection rules with zero-inflection.

Let us now consider how these three types of rules are related to morphological naturalness and what universal criteria for naturalness exist.

There is a universal morphological principle that Mayerthaler (1979: 18 ff) has appropiately called the *principle of constructional iconicity* (*Prinzip des konstruktionellen Ikonismus*)[4]: Perceptively *marked* (*markierte*) categories are more naturally represented as *feature-bearing* (*merkmalhaft*) than as non-feature-bearing. This entails for inflecting languages that

oblique cases should be more feature-bearing than the nominative case;
plural should be more feature-bearing than singular.

4. For the concept of iconicity in Language *cf* also Jakobson (1965) and Anttila (1972:12 ff).

The strong realization of this principle involves a representation by means of *additional phonological substance*, the weak realization involves a representation by means of *changes in phonological substance*. The cited Russian inflection forms have different degrees of iconicity, *ie* naturalness:

> Genitive singular *stol-a vs* nominative singular *stol* is strongy iconic;
> genitive singular *knig-i vs* nominative singular *knig-a* is weakly iconic;
> genitive plural *knig-Ø vs* nominative singular *knig-a* is counter-iconic.

(English plural *sheep vs* singular *sheep* would be non-iconic, *ie* between weakly and counter-iconic.)

With reference to the three Russian inflection rules we can again go back to our three questions. To what degree do they realize the natural morphological principle of constructional iconicity? We start with rule (R 2):

> (1) The additive character of the rule and the inflection of the whole word realize optimally the principle of constructional iconicity. In this respect the rule is a natural morphological rule (an 'optimal' morphological rule) – in a general sense the rule is not optimal, since it is only applied to a subset of Russian nouns, namely one inflectional class.
> (2)/(3) The naturalness of the rule is not constrained as far as the constructional iconicity of its output is concerned. The putput is *strongly iconic*.

Concerning rule (R 3) we can state:

> (1) The additive character of the rule realizes the principle of constructional iconicity.
> (2) The rule's iconicity is no longer 'optimal', since it is constrained to some extent.
> (3) The rule is constrained in such a way that the inflection is not attached to the word but to the word-stem. Its output is therefore only *weakly iconic*.

To rule (R4) we get one answer only:

> The rule is not natural in terms of its constructional iconicity because it produces *counter-iconic* forms. Discussing the constraints on naturalness of this rule seems somewhat pointless since it is completely unnatural.

Our evaluation of the degree of naturalness of the three rule types as far as the constructional iconicity of their output is concerned, is confirmed by considering their frequency of occurrence. Rules of the type of (R 2) apparently occur in all languages that show grammatical categories by means of inflection. In strictly agglutinative languages rules of this type are the only ones to occur. Rules of the type of (R 3) are also widely spread (*cf* Ancient Greek, Latin, Old Germanic languages, Modern Icelandic), but apparently only in languages that also have rules of the type of (R 2) (*cf* the Old Germanic 'root-nouns'). Rules of the type of (R 4) are quite exotic. They are a kind of 'historical accident' and always arise only as a result of phonological influences (*cf knig* from Old Slavonic *knig-ŭ*). Over time these rules tend to disappear completely, as in certain Russian dialect forms like *knig-ov* analogous to *stol-ov*.[5]

This Russian morphological example also clearly shows that Natural Grammar automatically leads to questions concerning typology.

In Dressler's paper we have seen a large number of phonological and morphological phenomena that belong within the framework of naturalness and are important with respect to typological questions. All these phenomena are characterized by their *context-free naturalness, ie* the degree of naturalness of individual rules, forms or pairs of forms (*cf knig-i vs knig-a*) does not depend on other properties of the language in question. Compare the following examples.

For every language diminutive forms with front high vowels are more natural representations (independently of other properties of the language in question) than forms with low back vowels (although such forms do occur, *cf* Old High German *chindil-în* 'child' (dim.) and Swiss German *kxind-li vs* East Franconian *kind-la*).

For every language a feature-bearing encoding of the perceptionally marked plural category is more natural than a non-feature-bearing one (*eg* German *Hund* 'dog' *vs Hund-e – Dackel* 'badger dog' *vs Dackel-Ø*).

Apart from context-free principles there are other principles that bear on the specific character of a language system. Whatever is natural for a language is also determined by the general structure of a language system, *ie* by its *system-defining structural properties*. Any language system shows a universal tendency towards *increasing unification and systematization*. Each individual language has its own dominant grammatical structures and processes, which are relatively resistant to language change and continously reappear in the process of language change. This dominance is a typical expression of naturalness in language, as we have already shown above.[6]

Naturalness in this sense should be characterized as *context-sensitive naturalness* or *system-appropriateness* (*Systemangemessenheit*). Context-free and context-sensitive naturalness in morphology can be differentiated as follows:

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5. Note that for most masculine nouns also the genitive-plural inflection *-ov* is an analogical innovation, replacing the regular Old Russian Ø-ending in this category (Isacenko (1962:100)).

6. For this point *cf* many of C.-J. N. Bailey's papers, recently Bailey (1979a) and (1979b). In Bailey (1979a) for instance he states: 'Natural changes are those which native speakers impose on language – changes not due to inter-system contact' (Footnote 5).

Context-free naturalness favours the retention and formation of perceptively and conceptually appropriate morphological systems that operate according to *iconic* principles.
Context-sensitive naturalness favours the retention and formation of morphological systems that are based on *uniform* principles and operate in a regular manner.

Whereas it can be determined, independently of language, whether a grammatical category is strongly, weakly, non-, or counter-iconic in its encoding, the regularity of a particular form can only be captured in relationship to the entire system. Regularity can be achieved in different languages by totally different methods: what is regular in one language can be completely irregular in another.

A good example of this phenomenon is the status of the plural umlaut in English and German. In English plural umlaut has almost disappeared. It only remains in non-systematic relicts (*mice*, *men*, *geese*, *teeth*). In German plural umlaut is still expanding. Paul (1917: 11–2), for example, gives only plurals without umlaut for words such as *Mops* 'pug', *Rumpf* 'trunk', and *Zwang* 'coersion', while the umlauted plurals *Möpse*, *Rümpfe*, and *Zwänge* are currently the only ones in use.

The fact that context-sensitive naturalness does exist becomes clear when it collides with and overrides context-free naturalness, as is shown clearly in the following two examples.

The majority of Old High German neuter nouns (long syllabic *a*-, *ja*-, *wa*-nouns) no longer have the Pre-Old High German inflection of the plural nominative and accusative cases. Compare Pre-Old High German *wort* 'word' – plural *wort-u*, which '*lautgesetzlich*' become Old High German *wort – wort-Ø*. The plural encoding changes from strongly iconic to non-iconic and its context-free morphological naturalness is reduced under the influence of phonological factors. The other neuter nouns (short syllabic *a*- and *n*-stems) kept their plural affixes as in *faʒ* 'vat' – *faʒ-u* and *herz-a* 'heart' – *herz-un*. Although the Old High German neuter nouns with different forms for nominative/accusative singular and nominative/accusative plural are more iconic than the ones that do not show this distinction, they are less system-appropriate. As a result, an analogical morphological change takes place in Early Old High German: *faʒ – faʒ-u* and *herz-a – herz-un* become *faʒ – faʒ* and *herz-a – herz-a*. The plural marker disappears altogether, and context-sensitive naturalness overrides its context-free counterpart.

In Early New High German an interesting levelling takes place in the paradigm of verbs like *geben* 'to give', changing the first person singular of the present indicative from *ich gibe* to *ich gebe* under the influence of verb-paradigms like *schlagen* 'to beat':

| | | | | |
|---|---|---|---|---|
| *ich gibe* | > | *gebe* | like | *ich schlage* |
| *du gibst* | | | | *du schlägst* |
| *er gibt* | | | | *er schlägt* |
| *wir geben* | | | | *wir schlagen* etc. |

The vowel alternation in the present indicative is generalized from the type *schlagen* to the type *geben*, eliminating the uniform vowel for the singular, *ie* the uniform singular encoding. This violates a basic morphological principle, namely that of 'one meaning – one form', which holds for all languages, independent of context. However, in German the morphological change *gibe* to *gebe* is not unnatural, since German verbs with vowel alternation in the present indicative are more often of the umlaut type (infinitive/ 1 ps. sg./ pl.: $V_1$ *vs* 2/3 ps. sg.: $V_2$) than of the *e/i*-alternation type (infinitive/ pl.: $V_1$ *vs* sg.: $V_2$), and the umlaut also affects more different segments (*cf stoßen* 'to push' – *er stößt*, *saufen* 'to drink exessively' – *er säuft*). In other words, as far as system-appropriateness is concerned, umlaut is more natural than *e/i*-alternation and the type can become *productive* for verbs with vowel alternation in the present tense. This situation is also confirmed by the corresponding facts from Bavarian-Austrian, where umlaut does not occur in the present indicative of the strong verbs (*cf er schlagt*, *haltet* 'holds', *stoßt*, *sauft*). Here, of course, no umlaut class could become productive, and the old forms like *ich gib(e)* are preserved.

This type of naturalness, system-appropriateness, is evidently also of considerable importance for language typology, as it determines the relative grammatical coherence of a language and makes it easier to learn and to handle.

General morphological and syntactic typology of natural languages (such as defining isolating, agglutinative, inflecting and incorporating language types) as well as attempts at typology on various levels within a language are both based on idealizations. We are therefore, for instance, justified in formulating some of the system-defining structural properties of modern New High German as follows:

number and case are encoded separately (agglutinative tendency);
number distinctions are encoded morphologically (by inflections and umlaut) on the word itself;
case distinctions are encoded through inflection of the article (and the adjective).

On the basis of these criteria modern New High German is typologically distinct from Old High German, since Old High German had one uniform inflection only for both number and case, which was encoded by means of affixes, *cf tag* 'day', nominative plural *tag-a*, genitive plural *tag-o* etc. However, New High German does not uniformly show these system-defining structural properties in its inflectional system, since different inflectional classes have a varied degree of system-appropriateness. Most system-appropriate are nouns of the types *Mutti* 'mum' and *Tante* 'aunt'. They have plural affixes, but no case affixes at all:

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| nom. | sg. | *die Mutti* | pl. | *die Mutti–s* | sg. | *die Tante* | pl. | *die Tante–n* |
| gen. | | *der Mutti* | | *der Mutti–s* | | *der Tante* | | *der Tante–n* |
| dat. | | *der Mutti* | | *den Mutti–s* | | *der Tante* | | *den Tante–n* |
| acc. | | *die Mutti* | | *die Mutti–s* | | *die Tante* | | *die Tante–n* |

Least system-appropriate are nouns of the type *Dackel* that still show two case affixes, but no plural affix:

| | | | | |
|---|---|---|---|---|
| nom. | sg. | *der Dackel* | pl. | *die Dackel* |
| gen. | | *des Dackel–s* | | *der Dackel* |
| dat. | | *dem Dackel* | | *den Dackel–n* |
| acc. | | *den Dackel* | | *die Dackel* |

Note however recent non-standard plurals like South German *Dackel-n* and North German *Dackel-s*, where an affix is added in the plural and the distinctive dative affix is consequently lost.

Natural languages do not only differ in their system-defining structural properties, but also in the degree to which these properties are represented in the system, as is shown in the following two cases.

Old High German and Russian are languages that take on markers for both number and case. However, Russian is more consistent, since Old High German has one inflectional class with distinct case and number markers, *cf hrind* 'cow' – nominative plural *hrind-ir* – genitive plural *hrind-ir-o*, while Russian has no such class at all.

One of the defining characteristics of agglutinative languages is lack of inflectional classes. Both Turkish and Hungarian may be properly classified as agglutinative, but Hungarian shows clear signs of inflectional classes despite its agglutinative character, while Turkish, at least in the Istanbul vernacular, has morphologically uniform inflections and only shows phonologically conditioned vowel harmony in them. The Turkish plural marker is *-ler* in words with front vocalism and *-lar* in words with back vocalism: *türk* 'Turk' – *türk-ler* and *yηl* 'year' – *yηl-lar*, but compare Hungarian words with front vocalism like *szív* 'heart' – plural *szív*-**ek** and *híd* 'hridge' – plural *hid*-**ak** and with back vocalism like *ház* 'house' – plural *ház*-**ak** and *rab* 'prisoner' – plural *rab*-**ok**.

For a language typology that goes beyond the old stereotype classifications such as agglutinative *vs* inflecting, these distinctions are of great importance.

The notions of context-sensitive and context-free naturalness are both equally important links between theory of grammar and language typology. Context-sensitive naturalness, despite its universal basis (*cf* the tendency towards increasingly uniform language systems) explicitly addresses typological questions in a particular language and is therefore strictly language specific in its outcome. Perhaps the concept of naturalness in the sense of Natural Grammar can contribute to solve the problem of an effective and exhaustive language typology that Hjelmslev (1970: 96) has called 'the biggest and most important task facing linguistics'.

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## Panel and open discussion

The discussion concluding the present section took two main directions: 1) How can naturalness be objectivized? 2) To what extent is naturalness a different principle from other general principles?

It was stated clearly by several panelists that the main problem for natural linguistics is that of establishing objective criteria by which a tendency might be described as natural. In this connection naturalness was seen as a principle of long historical standing. The Neogrammarian conflict between *Lautgesetzmässigkeit* and analogy, for example, was a naturalist issue. It was suggested that naturalness was more appropriate as a principle in phonology and morphology than in syntax, in particular since few unidirectional tendencies could properly be characterized as natural in syntax, or between syntax and morphology. The historical development of the tense-system in the Germanic languages was mentioned as a case in point, as was the generalization of nominative and accusative in nouns in different areas of Indo-European.

A suggestion was made that several 'naturalness domains' should be recognized, and further that conflict between such would account for otherwise incongruous facts. For example, those languages which at some stage in their historical development have had front rounded vowels follow one of two general patterns: either they keep the front round vowels, or they lose them only to reacquire them at a later stage. In view of the scarcity of languages with front rounded vowels, and in view of the apparently valid universal that front rounded vowels imply front unrounded vowels, this area seems to contain such naturalness domains. As a possible further example was mentioned the case of Southern and Western Chinese, where final particles are becoming more and more polysemous, moving away from the naturalist optimum of one-to-one correspondence between expression and content.

With respect to the second direction, naturalness is not the same as frequency, but frequency should be taken into consideration. Here it is important to distinguish between 'text-frequency' (*ie* the actual frequency of occurrence of specific forms in fixed collocations or for specific purposes, in terms of which the nominative/vocative form of the names of deities, *eg Ceres,* is far more frequent than oblique forms) and 'paradigm-frequency' (*ie* the ratio of forms with one stem to those with another within the same paradigm, in terms of which *honor-* is far more frequent that *honos*).

It was suggested that naturalness is just another word for explanation, especially if no naturalist account could be given for the fact that South-West Polish has assimilated /nk/ and /ng/ to /ŋk/ and /ŋg/, as opposed to North-East Polish which has retained the non-velar nasal. This suggestion was countered (by Dressler) on the grounds that, whereas an explanatory principle (*eg* the A-over-A constraint in syntax) may be entirely *ad hoc*, a natural principle cannot be.

Within syntax it was suggested that naturalness is associated with the notion of constraint in generative theory, and it was asked to what extent naturalness considerations could be applied to non-surface syntactic structure.

Finally, a plea was made for reserving naturalness as a term for universal, or general, tendencies and not to make the notion of naturalness language-specific.

Contributors to the discussion were EC, WUD, HS, EH, WUW, EJAH, JMA, SE, HA, JR, Niels Ege (chairman), Steen Schousboe, and Eli Fischer-Jørgensen.

# 4 To what extent can genetic-comparative classifications be based on typological considerations?

## *Søren Egerod:* Introduction

The typological and the genetic classifications of languages in a well defined form date back to about the same epoch, just before 1820 (von Schlegel 1816; Grimm 1819), they were preceded by a century of speculations in several sciences on structural and genetic problems. The thinking of Rasmus Rask was born out of this same intellectual climate (note especially Adelung's *Mithridates* 1806–1817), but does not lead to a separation of typological and genetic comparison, even though it contributes significantly to both.

It is well known that the classical typological classification took as its point of departure the structure of words in a given language. Most often four types are adduced, the isolating which has no inflection, the agglutinating which has regular inflection, the inflecting which has irregular inflection (with a variety of conjugations and declinations, with syncretisms of grammatical content elements, and with unclear boundaries between root and affix), and the polysynthetic in which word and sentence often coincide. The system is impracticable for the simple reason that there are few (or no) pure examples of each type; therefore many improvements have been suggested (*eg* Sapir 1921 who operates with 4 fundamental types, 4 techniques, and 3 kinds of synthesis; Greenberg 1960 who introduces the idea of ranking languages according to indices of different typological features).

Rask had shown the way to other possible points of departure for typological classifications (grammatical gender; systems of verb conjugation), but died before he had given more than suggestions of the vast horizons opened by this approach (Hjelmslev 1951:15).

In contrast to the Sapir approach Bloomfieldian structural linguistics was not particularly interested in problems of comparative typology. In Europe the Saussurian inspiration led to typological endeavors especially in phonology (Trubetzkoy 1939). Hjelmslev's *Principes de grammaire générale* (1928) is an enthusiastic and innovating call for a linguistic theory, without which languages cannot be compared at all. In his subsequent works Hjelmslev laid the foundation for a careful separation of typology and genetics at the same time stressing the importance of the former in its most comprehensive sense (the sense which leads Hjelmslev to see Rask as the early forerunner of general typological linguistics, Hjelmslev 1951:10).

A serious search for linguistic universals began at about the same time that the generative-transformational approach took form. A happy meet-

Typology and Genetics of Language.
Travaux du cercle linguistique de Copenhague XX.
Ed. by Torben Thrane, Vibeke Winge, Lachlan Mackenzie, Una Canger, and Niels Ege.

ing of the two trends produced such important and seminal contributions as Fillmore 1968, whose claim to fame rests on more solid ground than just an eye-catching title (but *cf* Martinet 1972 which takes a negative view of its originality, if not of its message). The concept of linguistic universals has been applied to all sides and levels of language structure in a variety of languages, a symptom of the final abandoning of the ethnocentricity (or glossocentricity) which characterized the first decade of post-Bloomfieldian American linguistics.

A single and yet far-reaching innovation[1] has been the classification of languages according to universal syntactic types rather than morphological types, especially the order of the main sentence constituents (the sentence profile), setting up SOV, SVO, VOS etc. languages (the profile can be further elaborated by the indication of the arrangement of Time, Place, Beneficiary etc.). Placement of modifiers in relation to their heads will add another dimension, and especially useful and meaningful is the near universal interdependency of the word order in sentences and within noun and verb phrases (Greenberg 1963).

It is evident that unconditional universals (true for all languages) are of no use for typological comparisons. Only universals which depend on other features for their presence which are not themselves universal, will constitute a useful basis for typology.

Hjelmslev's (operational) definition of genetic relationship (Hjelmslev 1970:30) says that genetic relationship is a function between languages consisting in the fact that each expression element in each of the languages has function to an expression element in each of the other languages (and each particular element-function is conditioned by the other expression elements forming the environment of the expression element concerned and by the place that it occupies in the word).

This is indeed a tall order. To the linguist who has worked with possible relationships between languages far removed from the Indo-European or Uralic scenes in space (and time?) and structure, it may appear crushing. It presumes for instance complete clarity as to which sections of the vocabulary are loanwords, since they have element-functions to another language in the same way as the non-loan words have to a genetically related language.[2] We always have to exclude at least one section of the vocabulary for the comparison, how do we know which one to exclude?[3]

1. Actually not that new. This classification was used by Lacouperie 1887 under the designation 'ideology'.

2. Trubetzkoy (1939b:82): Um die Gesetzmässigkeit der Lautentsprechungen zu erklären, braucht man ... ...die Vermutung der gemeinsamen Abstammung nicht, da eine solche Gesetzmässigkeit auch beim Lehnverkehr zwischen benachbarten unverwandten Sprachen entsteht (die sogenannten 'Fremdlautgesetze').

3. In Hjelmslev 1975:124–5 another definition of genetic relationship is set forth and the term 'typological relationship' is said to be superfluous in glossematics. A 'loan contact' is defined as a contact that is not a genetic relationship. These definitions do not solve the problem of establishing to which layers of the language to apply them.

One answer to this old problem has been recourse to grammatical structure (which according to Hjelmslev above, would be a typological relationship if it does not entail element-function, *ie* related expression for related grammatical content). R. A. Hall (1964:370) says:

> In some instances, it has been difficult even to establish the genetic affiliation of certain languages, because of their having a very heavy overlay of borrowed elements. Such a language is Albanian, which has a large number of vocabulary items which were taken over from the popular Latin speech of the Balkans, such as /émtə/ 'aunt', /kusbrí/ 'cousin', /kál/ 'horse'; only on the deeper levels of grammatical structure is it evident that Albanian is not a Romance language, but is a separate member of the Indo-European stock. Such elements as the numerals are thought, in general, to be the most tenacious and resistant to replacement by borrowed forms; but Chamorro (the Malayo-Polynesian language spoken on Guam and in the Marianas), in addition to having borrowed even terms of close relationship like /pariéntes/ 'relative' from Spanish, has taken over the entire Spanish numeral system from /ún/ 'one' on upwards. Yet Chamorro is unquestionably Malayo-Polynesian, not Romance, as is shown by its grammatical structure, especially the widespread use of grammatical processes like infixation and reduplication.

The use of grammatical structure is without doubt a recourse to typology (what is called for here is evidently not just a search for element-function between the expressions of certain grammatical contents). Infixation may very well fall within Hjelmslev's definition of genetic relationship, namely if the same infixes (or their continuations in time) are found in both (or all) languages, but the criterion is typological if only the process as such is considered. Reduplication is a difficult border-line case. Even if the same consonants or sections (or their diachronic continuations) are submitted to reduplication in two languages, it is hard to tell whether this is an inherited feature or not, since reduplication of necessity hits whatever is available in both languages (a notable absence of reduplication with some phoneme or phonemes might to some linguists constitute a good, though negative, argument for relationship – but can we exclude borrowing of negative features?). Vowel alternation is a similar border-line case – if umlaut or ablaut hits the same vowels with the same change of content we would think that the process speaks for genetic relationship (but *cf* Pulleyblank 1965a, b). Both reduplication and vowel alternation fall within Sapir's symbolic (typological) features.

Another approach introduced to solve the problem of borrowing *vs* genetic relationship (and more specifically the problem of different element-function in different layers of a language) is that of establishing a basic, fundamental, or core vocabulary the members of which are supposed to be less subject to borrowing (conversely the term 'cultural vocabulary' is used of items likely to be borrowed). Small numerals, body parts, close relationship terms, natural phenomena, some plants and animals, are usually considered core vocabulary. There are however many known instances of such vocabulary being borrowed, so it can in any case only be a matter of tendencies (Hoijer 1956). Attempts to bring these matters under statistical control are known as lexico-statistics. Glotto-chronology further adds the notion of time-depth, which is supposed to be statistically measurable (Greenberg 1953, 1960, Swadesh 1955, Gudschinky 1956, Hymes 1956). All considerations involving core vocabulary take their departure from elements of content which are checked for phonetic resemblance on the expression side, that is ideally for element-functions, whereas classical comparative linguistics will start with related elements of expression and check them for element-function on the content side (which led Hjelmslev (1958) to rejet glotto-chronology).

Still another check on relevant layers is the arbitrary one of demanding a minimum number of, say 300–400 words which must fulfill the criteria for genetic relationship (*eg* Doerfer 1974:128). But what happens if we have 299 good cognates? 280? 275? (see also Miller 1976a:372–373, Lamb 1959). And what happens if two layers independently reach 300–400? And what happens if known historical facts tell us that the layer with most items is indeed a later addition to the language?

We may assume, perhaps, that functions which are not indicators of genetic relationships are typological. Hjelmslev does not, however, ascribe equal importance to all possible comparisons, but defines (operationally) (Hjelmslev 1970:95) typological relationship as a function between languages consisting in the fact that categories in each language have function to categories in each of the others. Hjelmslev deliberately excludes for instance comparison of word structure ('the most superficial of all [linguistic typologies]' Hjelmslev 1970:94). Superficial and accidental they may be, but by the kind of accident that deeply affects the nature of a language and significantly illuminates our knowledge of its history.

Let us have a look at the kinds of comparisons which can be said to be typological in the widest possible sense of the word. Comparisons among languages of utilized articulatory zones (*eg* front, central, and back vowels) or actual phonetic features (*eg* rounding; preglottalization), of zones of meaning (*eg* tense, aspect) or actual features of meaning (*eg* past, present, future tense) are typological, as are comparisons of bundles of features, whether phonological (*eg* [voice] + [spirant] + [alveolar] = z; [velar] + [labial] + [stop] = $k^w$ ; or combinations of Jacobsonian binary features) or semantic (*eg* [male] + [horse] = *stallion* ; [male] + [pronominal reference] = *he*). Comparisons of phonological systems (*eg* 9 vowel systems; 4 tone systems) or semantic systems (*eg* number and arrangement of prepositions) are typological. And finally the comparisons of the structure of syllables (*eg* CV, CVC; tone, register), of words (*eg* monosyllabic, polysyllabic; accent, vowel harmony, tonal sandhi; tone, register; monosememic, polysememic; incorporation of basic or characterizing elements), of phrases (*eg* one word, several words; noun subordination, verb subordination; accent, vowel harmony, tonal sandhi; tone, register), and of sentences (*eg* word order, noun characterization, verb characterization, sentence characterization; sentence subordination; particles, intonation) are all typological. Only when phonetic features (*eg* umlaut of specific vowels to indicate plural), phonemes (*eg* Gothic -þ- *cf* Sanskrit -t-/after Sanskrit stressed syllable) or groups of phonemes (*eg* laryngeal + vowel, yielding other correspondences than vowel alone) in one language in a systematic way under well defined conditions relate to similar entities with related content in another language do we see the kind of relationship which may (or may not) indicate genetic affiliation. The element-functions are the necessary, but not the sufficient condition for establishing genetic relationship. The question is now, can typological arguments help us decide on this issue, as was assumed by so many comparativists – or do we need lexico-statistics or other kinds of proof of a quantitative nature? Or is there any way of deciding at all?

**The case of Tai**

East and Southeast Asia abound in languages of disputed relationship. Tai has been considered to be Sino-Tibetan or Austronesian; Vietnamese to be Austroasiatic or Tai (whatever Tai is), Miao-Yao to be Sino-Tibetan, Austroasiatic or Tai, Japanese to be Altaic or Austronesian (or both!), Ainu to be Indo-European or Austronesian, etc.

Terrien de Lacouperie (1887:68–9) described Tai as the result of a mixture of languages, especially Mon and Chinese, in historical times in China. His arguments are loose and impressionistic, but it is worthwhile to notice that Terrien de Lacouperie points out that one third of the Tai vocabulary is Chinese, and says that the Tai tones developed as compensation for phonetic losses (*cf* Egerod 1976:51, 59). August Conrady (1896) declared that Tai forms a linguistic unity together with Tibetan, Burmese and Chinese – in other words Tai was Sino-Tibetan (Indo-Chinese). Conrady's suggested Tai-Chinese cognates are however mostly unacceptable, and Conrady himself admitted that he had been unable to establish sound laws. In 1901 Gustav Schlegel assures us that Tai is a Malay tongue (that is to say Austronesian) on the basis of Malay words in Siamese (which are however mostly loans) and common linguistic structure (which is a typological argument). Henri Maspero (1934) points to extensive borrowing (by Tai from Chinese) as the most likely explanation of the common vocabulary.

Kurt Wulff (1934) was the first scholar who did not stop at vague and impressionistic generalisations, but got down to the arduous task of evaluating all the possible cognates he could find and establishing sound laws to prove the relationship. The clearest and most important sound correspondences established by Wulff are those of the tones. In both Chinese and Tai it is possible to reconstruct an original 4 tone system (in Chinese this system is also known from the Suei-Tang dynasty dictionaries, and in Tai from the conservative Siamese orthography) and the reconstructed systems have function to each other in such a way that to Chinese tone One (in the old dictionary order) corresponds Tai tone Zero (no marker in the writing system), to Chinese Two corresponds Tai Two (marked with a superscript figure 2 in the writing), to Chinese Three corresponds Tai One (marked with a superscript figure 1), and to Chinese Four (comprising all syllables ending in -p, -t or -k) correspond Tai 'Dead Syllables' (unmarked in writing, comprising all syllables ending in -p, -t or -k).

Also for consonants and vowels certain element-functions are established by Wulff but the picture is less transparent than is the case with the tones – Wulff was one of the first comparativists, but not the last, to struggle with what Karlgren has named 'word families' in Chinese (Karlgren's *Word Families* (1933) appeared the year before Wulff's *Chinesisch und Tai*), groups of obviously related words which differ in sound and meaning but only within certain phonetic and semantic limits. It is not always easy to establish the phonemic (morpho-phonemic?) correspondences within any one such family, and correspondingly difficult to know which member of a family (dubbed 'allofam' by Matisoff 1978) to compare with Tai (or other languages; Karlgren 1931:11).

In any case Wulff was sufficiently sure of his results to assume a genetic relationship to have been proved, and to embark on his even more ambitious plan (already attempted by Conrady 1916) of combining Sino-Tibetan (including Tai) with Austric (*ie* Austronesian + Austroasiatic) in one family (a task in which he according to Hjelmslev (1970:79) succeeded: 'the genetic relationship between Austric and Sino-Tibetan was finally demonstrated in a posthumous work of Wulff's'). It will be evident from Wulff's own book that he has shown important connections among a number of large language groups, but that he has not in any sense acceptable to present-day scholarship proved the existence of a Sino-Austric family (certainly not according to Hjelmslev's definition; see also Egerod 1976). Wulff states:

> Sind also M[alayo-] P[olynesisch] und Tai-chin. zwei glieder eines und desselben sprachstammes, so folgt daraus, dass auch die mit ihnen sicher verwandten sprachen, einerseits die austroasiatischen [Mon-Khmer], anderseits die tibeto-barmanischen, andere glieder derselben sprachenfamilie sind...
>
> So halte ich es für gerechtfertigt einen grossen ostasiatisch-ozeanischen sprachstamm festzustellen, innerhalb dessen die drei glieder Tai-chinesisch, austroasiatisch und austronesisch [= MP] in engerer beziehung zueinander stehen, während das tibeto-barmanische mit seinem ganzen anhang weiter abseits zu stehen scheint; ob Tai-chinesisch, austroasiatisch und austronesich drei einander gleichgestellte glieder sind, oder ob austroasiatisch und austronesisch gegenüber dem Tai-chin. widerum eine engere einheit bilden, kann ich nicht entscheiden. (Wulff 1942:40)

It is of course not correct that language A is related to language C if both share vocabulary items with B, unless it is a matter of largely the same vocabulary items and the same element-functions. And this is not the case with Austroasiatic and 'Tai-Chinese', which may both have a layer but certainly not the same layer in common with Austronesian.

Furthermore it is not the case with Chinese and Austronesian, which have a layer but not the same one in common with Tai. The fact of three languages A, B and C being related in the way described here is indeed one of the most important tests for proving or disproving the applicability of cognates for establishing large language families.

It is strange to compare Wulff's cautionary remarks in *Chinesisch und Tai* with the sweeping claims of Wulff 1942. In 1934:3–4 fn he warns:

> Das verhältnis des Tai zum chinesischen is noch sehr dunkel; wir wissen nichts darüber wie weit die trennung der beiden sprach-gruppen von einander zurückzuverlegen ist, und so lässt sich nicht von vornherein bestreiten, dass die Tai-sprachen einen teil der chinesischen sprachentwicklung seit der Shih-king-periode mitgemacht haben können; und diese annahme scheint vorläufig notwendig zu sein, sofern die rekonstruktion Karlgrens in allen wesentlichen punkten richtig ist.

To say that Chinese and Tai have shared a long period of their history is the same as saying that they have exerted a deep influence on each other. If this is the case the question arises whether the close correspondence of tonal systems, and to a large extent of consonantal and vowel systems, has to do with this contact layer in the two languages rather than a genetic one.

What Wulff (1942) has demonstrated is that a special relationship exists between Tai and Chinese (already proved by him in 1934), and between Tai and Austronesian (but not between Tai and Sino-Tibetan, nor between Tai and Austric, least of all between Sino-Tibetan and Austric).

The lasting value of Wulff (1942) is then to call attention to a Tai-Austronesian relationship. The book was unfinished and published posthumously, but a number of good sound correspondences have been established. It was a pity that this important achievement should be drowned among his more ambitious and less successful endeavors. Also in 1942 appeared the epoch-making article by Paul Benedict, which laid the foundation for Tai-Austronesian comparison ('Thai, Kadai and Indonesian'; *cf* also his later publications, especially Benedict 1975). These two first attempts had surprisingly few comparisons in common (*cf* Egerod 1959b, 1976). Wulff of course never became acquainted with Benedict's material, but Benedict has accepted most of Wulff's comparisons. Important Tai-Austronesian correspondences can be established, which significantly corroborated the results of internal proto-Tai reconstructions ('moon' proto-Austronesian **bulan*, proto-Tai **ˀblïan*, Siamese *dyan*; 'head, top' proto-Austronesian **hulu*, proto-Tai **thrua*, Siamese *hua*; see also Dahl [1]1976, [2]1977:109–116). The reconstructed proto-Austronesian, like the overwhelming majority of modern Austronesian languages has no tones. A significant majority of Tai-Austronesian cognates have tone Zero (*cf* Chinese tone One) or are 'Dead Syllables' (*cf* Chinese tone Four), *ie* the tones which must be reconstructed as having their origin in syllables with no extra syllabic modifications ('phonation type' *cf* Egerod 1971b) such as voiced laryngealization (Siamese and Chinese tone Two) or voiceless laryngealization (Siamese tone One, Chinese tone Three). In other words the tones have element-functions to Chinese, but not to Austronesian (*cf* also Sakamoto 1976).

Considering some of these facts Haudricourt (1948:235–236) concludes:

> On voit, d'après cette reconstruction des phonèmes de la langue commune [thai], combien le thai est éloigné du chinois. Seul coïncide le système des consonnes finales et des tons; les voyelles et les initiales sont profondément differentes. Les mots de la langue commune [thai] incontestablement proches de mots chinois sont les noms de nombres, des techniques militaires (cheval, selle, élephant, jouguet) et des techniques artisanales (métier à tisser, ouvrier, papier), bref un vocabulaire de civilisation susceptible d'emprunt. Au contraire le nom des parties de corps et le vocabulaire agricole ont peu d'affinité avec le vocabulaire chinois correspondent.

Contrariwise Nishida Tatsuo 1975, who operates with suffixes to facilitate certain Tai-Chinese comparisons and whose Tai reconstructions do not always belong to the same sources as adhered to above (*cf* Li Fang-kuei 1954, 1977; Benedict 1975) concludes p 10:

> In my opinion the problem of whether the Tai languages belong to the Sino-Tibetan family allows room for further discussion by additional compilation of words of this kind. I do not think that Tai belongs to a different language family from that of Chinese, nor can I determine that Tai words structurally similar to Chinese are all borrowings from it, though a considerable number of words are in fact borrowed from Chinese. As to whether the two languages are remotely related or not, questions still exist and will not be easily resolved.

This conclusion is not very helpful.

So we are now left with two groups of Tai words, one with relations to Chinese, another with relations to Austronesian. Important phonetic correspondences have been established for both groups. Is either one genetic? If so which one? Or both? The answer is impossible without corroborating evicence.

A naïve look at the number of correspondences in the two groups will give a lead to the one related to Chinese. Benedict's first Tai-Austronesian list contained only 30 words. With the addition of the tenable ones from Wulff and quite a lot of others (400 according to Benedict! but far from it all of the same quality) the list has been considerably expanded, but the Tai-Chinese list still runs ahead. If we add the notion of core *vs* cultural vocabulary the story is different. Already Benedict (1942) had discarded most Tai-Chinese contact words as cultural loans. Greenberg (1953), using the 110 words list, arrived at 25 Tai-Austronesian versus 15

Tai-Chinese contact words. Now Austronesian seemed to be ahead (if we accept the methodology). Benedict (1975:123 *et al.*) reaches the important conclusion that a large number of Chinese-Tai cultural contact words do not belong to the Sino-Tibetan layer of Chinese and therefore must have been borrowed by Chinese from Tai and not the other way round. Other words, like numerals and kin numeratives (see also Egerod 1959a) were loans in the opposite direction, since they are widespread in Sino-Tibetan, but not in Austronesian, and at the same time manifest other phonetic correspondences than the bulk of Tai-Chinese contact words. All of this will not remove every single such item from the list of possible cognates, but it will provide us with an impression akin to certainty, that if arguments outside of the sets of correspondences themselves are admitted, facts point strongly to the Tai-Austronesian relationship as a more fundamental one (*ie* probably genetic) and the Tai-Chinese relationship as a cultural one (*ie* one or more layers of loanwords in both languages).

The question is now, can typological arguments help us decide? It will be remembered that Schlegel (1901) had posited a Siamese-Malay genetic relationship on the basis of both common vocabulary and like grammars. What he actually says is this:

> the quantity of Malay words in Siamese is very considerable, and its grammar is absolutely like the Malay grammar: the subject standing before the predicate, the object of a verb following the verb, the adjective and genitive following the substantives and the adverb following the verb.

We would say, both are SOV and Head-Modifier languages.

What Schlegel says here is true of Siamese and most other Tai languages, and of Malay, but it is not true of most Austronesian languages and not of proto AN.

Already in Malay we meet the typological complication that, in contradistinction to Tai, verbs can be active or passive (*Si Ali membunuhnya* 'Ali killed him', *Si Ali dibunuhnya* 'Ali was killed by him') a pronominal third person actor or goal can be expressed by the possessive (... *dibunuhnya* 'killed by him'; ... *membunuhnya* 'killed him', *cf rumahnya* 'his house'). In Thai, passive can be expressed without change in the form of the verb, *sỳa kin wua* 'the tiger ate the cow', *wua thùug sỳa kin lέεw* 'the cow was eaten by the tiger' with marking of noun rather than of verb. In Malay predicativization of subject (*cf* 'cleft sentence') can be expressed by word order (*aku membunuhnya* 'I killed him', *membununuhnya aku* 'It was I, who ..., the one who killed him was I'), whereas in Tai the order VS is not possible. For predicativization Tai like Chinese must use a subordinated clause ('one who' Tai *thîi* ..., Cantonese ... *kɛ'*, Peking ... *de*). If we move East to languages of the Philippines and Taiwan (the center or closer to the center of AN, Dyen 1965) we find even more discrepancy. Instead of the two verbal genera of Malay (where furthermore the passive markings are not those typical of Austronesian) we find four (active, direct passive, indirect passive, instrumental passive) and instead of the almost total absence of case marking in Malay (as in Tai) we find extensive case markings in Filippine and Formosan languages. Many of these features have to be admitted for proto-Austronesian (see Dahl [1]1976, [2]1977:117–122).

The further in space and time we remove ourselves from Malay within AN the more different from Tai the languages become. This is certainly the opposite of what we would wish to find, if typology were to substantiate our previous findings. In matters of verbal genera, topicalization and predicativization Tai is closer to Chinese.

Let us look at some other typological features. The classical types were based on word structure. The Malay word is typically bisyllabic without tone or accent, whereas the Tai word is monosyllabic and tonal. Again Tai sides with Chinese. As for the phonemic make-up, Peking has a complicated consonant-vowel relationship where 22 initial consonants can be phonemically reduced to 15 if 8 vowels are maintained, and the 8 vowels reduced to two, if all initial consonants and two or three semivowels are maintained. Cantonese has 20 initials which are not easily further reduced, and 11 vowels which enter into various complementary distribution patterns depending on the final consonants (of which Canton has 6 plus semivowels, Peking two plus semivowels) (Egerod 1977). The non-reduced Cantonese vowel pattern is almost identical with the Tai one, and the consonantal pattern is very reminiscent of Tai. As regards the consonantal and vocalic systems Tai is close to Southern Chinese and more different from Northern Chinese, Malay (5–6 vowels), and even more from proto-Sino-Tibetan (6 vowels?) and proto AN (4 vowels?).

Where does all this lead us? A study of related vocabulary and phonetic correspondences leads us to the setting up of two layers (and a third one related to neither ST nor AN which we do not consider here) in Tai, one pointing to a possible genetic relationship with Chinese, the other with Austronesian. Secondary considerations of the distribution of vocabulary items over ST and AN, the number of core vocabulary items involved, and general ideas about the spread of civilization in East Asia, leads us to surmise that the AN layer is the genetic one. Consideration of typology does not help us decide. Overall features of syntactic word order point towards AN, but word structure and morphology favor Chinese, and phonology more specifically Southern Chinese.

**The case of Japanese**

The Japanese language just as Tai includes large numbers of words which are obviously related to Chinese (and the proportion is much more overwhelming than in Tai, because the modern technical and scientific terminology, which in Siamese and Lao is made up of Indic elements, in Japanese is built on Chinese). This has led Roy A. Miller (1967:88) to the conclusion that:

> If we were to approach the problem in total ignorance of the history of the two languages [Japanese and Chinese] and without any informa-

> tion about their long period of close contact with one another – and such blind investigations of languages are more often than not necessary in comparative linguistics, since we rarely have the rich documentation that is available for China and the Far East – it would be almost impossible to avoid the conclusion, on the basis of the assumptions of comparative grammar and its usual methodology, that the one language in all the world most closely related to Japanese is Chinese ... Fortunately, our knowledge of the history of the Far East and in particular our knowledge of the many centuries of cultural contact between China and Japan save us from this otherwise fatal error.

This is true. We would however at the same time have to explain why words behave so differently inside and outside compounds related to Chinese, and why there is a full and un-Chinese range of morphological possibilities with verbs not related to Chinese, and why the Japanese syntax moves along such different lines from those of Chinese. These typological considerations would not prove that Japanese is not related to Chinese but they would at least send us looking elsewhere. So in this hypothetical case and in this restricted sense, we can say that typology would give us a hint in our search for true cognates.

Of course the fact that most Chinese words are loans does not exclude the possibility of some being older cognates. But of this we have no evidence. The sound laws which govern the Japanese loans from Chinese are complicated by the fact that the borrowing has taken place over such a long period (from the 8th century on), but everything is explainable in terms of processes within this period.

It is necessary to look elsewhere for languages related to Japanese. Von Siebold (1832) proposed a genetic relationship to what we now call Altaic languages and the idea had many followers (*eg* Boller 1857). From the beginning of this century the predominant choice among Japanese linguists (for those who looked at all – many Japanese scholars have felt the task to be impossible or undesirable) had also been Altaic (or Uralic-Altaic). Fujioka (1908) is the first major Japanese statement in favor of this relationship – built on structural criteria (of which some concerned absence of certain Indo-European features from both Uralic-Altaic and Japanese) rather than phonetic laws. In the West the great names in the field are G. J. Ramstedt and Nicholas Poppe.

A special problem is the relationship between Japanese and Korean, which was considered related to Japanese even earlier than Altaic (Hervás 1800, Aston 1879), and soon was assumed to be related also to Altaic, that is a member of a large family stretching from Turkish (or even Finnish and Hungarian, Castrén 1857) to Japanese through Mongolian, Tungus and Korean. Aston (1879) bases his Korean-Japanese comparisons on sound laws in lexical items, as well as on morphological and syntactical structure.

The status of Korean as an Altaic language has been confirmed by the works of such eminent linguists as Ramstedt (*eg* 1952, 1957) and Poppe (1960), and (most recently) Samuel Martin (1966).

The most important recent works on the Japanese-Altaic hypothesis are those by Roy A. Miller (1967; 1971 with the provocative title *Japanese and the other Altaic languages*; *cf* the more cautious Street and Miller 1975). Miller (1976a:355) states characteristically: 'The similarities and correspondences that we can observe in phonology, morphology, and lexical resources, together with the similarities in syntactic structure, between Japanese and the Altaic family of languages (Turkic, Mongol, and Tungus) are far too impressive to be ignored or to have resulted from chance'. (See also Menges 1974 and 1975).

So Miller, who is a strict observer of the comparative method[4] establishes element-functions in lexicon and morphology, and only then brings in typological arguments from sentence structure. His arguments carry much weight, especially important are his comparisons of causatives, formed in some cases by the same elements in Japanese and Old Turkish (which forms appear as 'irregular' in both languages, *eg* Jap *tar-u* 'sufficient' – Jap *tas–u* 'make sufficient, fill out', *cf* Old Turkish *tol-* 'full' – Old Turkish *toš-* 'fill up, make full'). An evaluation of the shared morphological elements enables Miller to place Japanese – as far as the Altaic connection goes – as a member of the Tungusian subfamily (with Korean), which again shows closer ties with Turkic than with Mongolian; but these classifications have not been adopted by all scholars in the field.[5]

Since the 1920's there have also been attempts to relate Japanese with Austronesian (Malayo-Polynesian) *eg* Matsumoto 1928. In the Soviet Union E. D. Polivanov worked along the same lines, but unfortunately his most important (unpublished) work on the genetic position of Japanese was lost after his death in 1938, only the fragment Polivanov 1960 survives. His ideas were taken up by Murayama Shichirō who had already done important work which favored the Altaic connection (see

4. *pace* Doerfer 1974:141 *re* 'Miller's Fehler in der Methodik ... Lautgesetze werden aufgestellt aber nicht beachtet; junggrammatische Strenge fehlt ... Es ist nach der Methode 'Man nehme zwei Wörterbücher und vergleiche drauf los' gearbeitet worden'. This criticism is wide of the mark.

5. Famous is Miller's dictum in 1971:8: 'But because of curious psychological factors, we still have our critics of proto-Altaic, even given the irrefutable evidence of Poppe's *Lautlehre*. They continue to argue, despite the detailed sets of regular sound correspondences that have been demonstrated among many different Turkish, Mongol, and Tungus languages, that these correlations in matters of precise detail are either the result of chance or borrowing, or of other circumstances not generally too well defined ... No matter how well-worked-out the scholarly solution for a longstanding scientific problem may be, apparently there are always certain elements (generally, to be sure, on the fringes of academic circles) for whom the very fact that a solution has been attained, proves, in some peculiar psychological way, to be a deep disappointment'. To this, Gerhard Doerfer (1974) retaliated: 'Das heisst also: Wer an der altaischen Sprachverwandschaft zweifelt, ist ein am Rande akademischer Existenz lebender Psychopath. Ich glaube aber nicht, dass man Forscher wie Bang, Grønbech, Benzing und viele andere so abtun kann. Das Verfahren 'psychologischen Faktoren' von Kontrahenten nachzuspüren, ist nicht gentlemanlike: In der wissenschaftlichen Debatte ist es wohl erlaubt, den Gegner aufs Kreuz zu legen, nicht jedoch auf die berühmte Couch'.

for instance 1957). Murayama concludes (1973, 1976) that Japanese contains both Altaic and Malayo-Polynesian elements, distributed in such a way that he prefers to call Japanese a 'mixed language' (*Mischsprache*). His reason for choosing this term rather than a superstratum/substratum theory is the fact that, according to him

> those elements in the Japanese language that we are able ... to identify clearly as being Altaic in origin, are restricted to a few special features of syntax, and to a portion of the morphology, as well as to an extremely limited selection of the vocabulary ... [whereas] we find that a very large portion of the basic vocabulary of the language is of Malayo-Polynesian origin ... as well as certain morphological and phonological phenomena that are of great importance to the language, notably the important phonological phenomenon of intervocalic consonantal voicing (*rendaku*), together with structural similarities that may be identified between certain types of Japanese verbs and the verb in Malayo-Polynesian ... Rather than substratum, the Malayo-Polynesian elements in the Japanese language constitute a vital and powerful structural component of the language.

Examples will show that Murayama's basic comparisons concern items of vocabulary and morphology for which element-functions can be established. He recognized *eg* the existence in Japanese of the very widespread Austronesian verbal prefix **maN-* as in Old Japanese *mŭdak-u, mŭdakaF-aru* < **məN-dak(ap)* 'embrace', *cf* Proto AN **dakəp* 'embrace' (which should regularly have rendered Jap **tak(ap)* if the **maN-* had not interfered with the initial consonant), and OJ *mŭgasi* < **məN-kat'ih* 'suit the heart, happy', *cf* Proto AN **kat'ih* 'affection' (which should have had *-kasi* instead of *-gasi*).

Murayama is particularly careful not to let typological considerations enter the game too early (1976:428):

> Typological similarities ... will serve us in establishing the genetic affiliations ... only when such similarities are further supported by actual correspondences in matters of detail ... establishing phonological 'laws' and also at the same time exhibiting such laws of basic vocabulary, as well as correspondences in word-formation, together with real, precise correspondences in morphological details.

Murayama recognizes traces of the same Austronesian elements in Korean too. He attempts to show that many of Martin's (1966) Japanese-Korean correspondences are of AN rather than Altaic origin. Miller (1976:382) admits the importance of some of these contact words and concludes:

> If Murayama is correct in his arguments, we now know why 'box' in Japanese is *hako; hako* is *hako* because at some time in the very remote past at a time and place that we cannot today recover, Malayo-Polynesian **bakul* 'basket' ... was borrowed into the original common language that underlies both Korean and Japanese ('Proto-Korean-Japanese'), to become Middle Korean *paku(l)lŏi* 'basket', and eventually also Japanese hako ... We can, in effect, in this way reconstruct an event in prehistory, an event for which we have no other evidence, apart from the evidence of the linguistic forms themselves.

We are now faced with the situation that (having removed Chinese from consideration) we are left with one set of correspondences (element-functions) connecting Japanese with Altaic (or at least with Korean[6]) and another set of correspondences (element-functions) connecting Japanese with Austronesian. Which set is the 'real' genetic level? Or must we conclude with Murayama that Japanese is one truly 'mixed' language, in which we cannot give priority to either constituent?

Murayama bases his idea of Japanese as a *Mischsprache* on the fact that considerations of vocabulary point toward AN more than toward Altaic, but some important items are Altaic, whereas considerations of typology point toward Altaic more than toward AN, but some important items are still AN. He cannot reconcile these facts with a superstratum/substratum explanation. In other words he cannot say which layer would be older and therefore he prefers the rather neutral concept of *Mischsprache*. Even though Murayama perhaps underestimates the number of Altaic cognates, and overestimates the number of AN cognates, we have to admit that the situation is extremely unclear. It is difficult enough to clarify the history of a language where vocabulary and morphology pull in one direction, typology in another, but faced with almost equal pull of all components in both directions we may have to give up. But this perhaps tells more about our methodology and about the quality of the material on which we base our conclusions, than it tells about the history of the Japanese language.

Doerfer (1974) tries to introduce quantitative arguments to evaluate the Japanese-Altaic hypothesis. I shall not go into details, but just report that the computations call for a minimum of 300 cognates as '*Wahrscheinlichkeitsgrenze*' for a family the size and build-up of Altaic. Compare also the comments above (p 118) to this point. According to Doerfer, Miller only comes up with 109 cognates and therefore falls 'weit unter der Wahrscheinlichkeitsgrenze'. The argumentation is rather loose and not very convincing. Miller (1976a:340) does away with it in a footnote:

6. In spite of Miller's hard words, some scholars doubt not only the Japanese-Altaic connection, but also the existence of an Altaic family of languages altogether (Doerfer 1966, 1974). Krueger (1973) in a well-balanced study concludes with the following picture of the Altaic situation (p 578) : 'A strong patterning in the syntactic (typological) arrangement, a noticeable amount of identical morpheme behavior, if not of morphemes identical through derivation; a niggardly amount of shared lexical items found system-wide; and a respectable amount of phonological correspondence'.

'[Doerfer's] approach confuses the nature of what we are studying with the qualitative evaluation of what we have found out from our studies to date'.

More important are Doerfer's five conditions for genetic relationships in general:

1) 'Die strukturellen Ähnlichkeiten (in Syntax and Phonologie) müssen wachsen je weiter man zeitlich zurückgeht'. Doerfer finds the opposite to be true among Altaic languages – the older systems are less alike than the modern ones.

2) 'Die Morphologie muss sich zum erheblichen Teil auf gemeinsame ältere Vorbilder zurückführen lassen; insbesonders sind hier charakteristische Unregelmässigkeiten relevant'. In this respect Doerfer finds only negative evidence. He does not mention Miller's important discovery regarding Japanese and Old Turkish causative building. It should at least be taken into account.

3) 'Es muss ein genügend hohes lexikalisches Vergleichsgut da sein, d.h. gleiche Wurzeln für Grundbegriffe (Zahlwörter, Körperteilbezeichnungen u.ä.) sollten (und zwar umso mehr, je weiter man zurückgeht) in erheblicher Menge vorhanden sein ... (allerdings sollte die Gesamtzahl verwandter Wörter doch nicht unter 3–400 liegen)'. By adding the qualification in the parentheses Doerfer manages to turn the most positive feature into a negative one. Miller's argument that proof of affinity is a matter of quality not quantity (*cf* Poppe's Foreword to Miller 1971 'There is no rule concerning the obligatory minimum number of common stems that can be regarded as proof of linguistic affinity') is well taken. Anyhow Miller shows that the Japanese ratio of 109 cognates out of Poppe's 570 Proto-Altaic roots compares favorably with *eg* Hawaiian's 170 out of Dempwolff's 2080 Proto-Malayo-Polynesian roots.

4) 'Die Sprachzweige müssen kompatible phonologische Ursysteme aufweisen ... Das heisst, es sollten sich keine (relevanten) Lücken in den Lautentsprechungen ergeben, die Korrespondenzreihe sollte ein geschlossenes System dastellen'. This is close to Hjelmslev's definition of genetic relationship. It is the ideal requirement, but it presupposes, as we have seen above, knowledge about the status of different historical layers in the language. It is rather surprising how far in the direction of this ideal it has been possible to proceed in Japanese in regard to the Altaic and the AN strata taken separately.

5) 'Bei Sprachfamilien von mehr als zwei Sprachzweigen sollte ein geschlossenes System von Verbindungen herrschen and damit eine grosse Zahl von 'Ballungen' belegt sein'. Already Abel Rémusat (1820) stated that there were many Turkish-Mongolian cognates, and also many Mongolian-Tungus, but few if any Turkish-Tungus.

Doerfer takes up this argument. We found a similar picture with the Tai relationship to Sino-Tibetan which weakened with the distance from Tai. The conclusion must be that as long as this picture seems true we must look for 'Altaic'-Japanese connections closer to home. But Miller's work has begun to change the picture (and *cf* also Street-Miller 1975). Note especially Miller's arguments for a Tungusian-Turkic relationship on the level of shared morphological elements.

Doerfer concludes that the Altaic languages once were on their way to becoming truly incorporated, but that they 'sind auf der Schwelle zur Verwandschaft stehengeblieben, sie sind höchstens 'quasiverwandt'. This conclusion throws doubt on the validity of the very concept of genetic relationship. The conscientious linguist will take it ad notam and store it – hoping he will never need it – in the same confines of his mind where he has put away Trubetzkoy's 'Gedanken über das Indogermanenproblem' (1939b).

One structural argument which was once used against the Altaic-Japanese hypothesis, the consideration of vowel harmony which was said to be absent from Japanese (Shinmura 1911) was later turned into an argument in favor of such a relationship (Ōno 1973, *cf* Miller 1976a:361–362), because traces of vowel harmony turned out to be present in Old Japanese. The argument is typological and the phenomenon fits into a pattern of interconnected features, which have nothing to do with genetic relationship. All through the Northern and Central Eurasian continent stretches a zone characterized by the use of suprasegmental concord or suprasegmental government, which is manifested in the Altaic languages as vowel harmony. There are signs that Korean and Japanese may have at one time possessed this feature, but in historical time exchanged it for a similar system based on register (pitch patterns). South of Tungus and Mongolian the same phenomenon is manifested as tonal sandhi (and in Northern Chinese the beginnings of a stress accent system). Vowel harmony covers also Uralic languages to the west of Altaic, and further west suprasegmental government was at a certain period manifested as the Germanic umlaut. Umlaut gave way to stem accent, in addition to which the Scandinavian languages developed their tonal and/or glottalization system as manifestation of suprasegmental concord. Going south from Germanic, French breaks the chain as Cantonese does in the East (*cf* Egerod 1977).

So vowel harmony does not tell us much about genetic relationship. It does indicate possible contacts in the direction of Altaic. Austronesian is not in general characterized by suprasegmental concord, although modern Malay and Indonesian do exhibit government of the vowel of the second syllable in a word by the vowel of the first syllable (vowel harmony). Atayal in Taiwan shows traces of a strong stem accent system, but it seems too recent to be connected with Proto-Japanese phenomena.

Japanese is a typical Object-Verb language just like Korean and Altaic (and languages south of them like Tibeto-Burman and to a large extent Indo-European). Of this feature Miller (1976a:382) says that '[Japanese is] essentially an 'object-verb' (rather than like English and Chinese a 'verb-object') language ... because 'object-verb' is the inherited Altaic syntactic pattern, a pattern that goes with its genetic relationship to the other Altaic languages'. In other words, if we 'know' that Japanese is Altaic the word order fits. If we should happen to 'know' it is not, the word order would not fit.[7]

The Japanese language has a double origin, one manifested in an affinity with Korean, maybe also to Tungus, and perhaps even to Turkish and Mongolian, the other one relating to Austronesian. The picture is further complicated by the possibility of very old Austronesian loans in both Japanese and Korean. The overall linguistic structure points to a strong influence from Altaic, but some typological criteria point towards AN. We have a dilemma as far as genetic relationship is concerned – and typological considerations cannot solve the dilemma. The only exception would be the negative one mentioned by Doerfer: If it is true that internal Altaic reconstructions combined with written evidence arrive at a linguistic structure which is more different from Japanese than the modern Altaic languages, this could be taken to disfavor the genetic affinity of Japanese to Altaic.

Is there something wrong with our concepts of genetic and typological relationship? Does it make sense to maintain the distinction when we are faced with time depths as enormous as those involved in the Japanese case? Let us in any case not throw out the methodology with the dilemma. Perhaps one day we shall know more.[8]

Reconstructing the history of a language we must work towards a complete picture of all the evidence available. For languages spoken in historical times we have perhaps written records not to be neglected. For prehistoric stages archeology may provide crucial clues (for Japanese *cf* Ledyard 1975 and Pearson 1976). The establishment of element-functions is a most important first step and typological comparison an important second step, but they will never tell us the full story of a language, they cannot by themselves date the sets of correspondences, whether element-functions or category-functions.

**The case of Chamic**

The Chamic languages (Cham, Jarai, Radé, *cf* Egerod 1978b) are spoken by a few hundred thousand people in Vietnam and Cambodia. Himly

7. In itself it shows precious little. (As long as we believe that Chinese is Sino-Tibetan, we simply have to state that its syntactical word order contradicts this hypothesis – Chinese is V-O, Tibeto-Burman is O-V – but if we should one day conclude that Chinese 'is' Austric, the word order would fit the hypothesis nicely – but not prove it. *Cf* Benedict 1972:197, and Egerod 1973:505).

8. Miller (1978) discusses and refutes Nishida (1976) which had suggested a genetic relationship between Japanese and Tibeto-Burman. What Nishida may have shown is some kind of ancient contact (loan words and typological loans) between Altaic and Tibeto-Burman.

(1890:325) characterizes Cham as 'das Beispiel einer ächten Mischsprache'. He compares it to Mon-Khmer ('mon-annamische Sprachen') because of its tendency towards prefixation and infixation (also used by Malay) and its lack of suffixation (characteristic of Malay, but not Mon-Khmer). He admits however that Cham (as also Radé etc.) contains such a richness of words of Malay origin that one might be tempted (or rather seduced 'dass man sich leicht könnte verleiten lassen') to add it to 'the great Malay family of languages'. However, Himly says, also the sentence structure is Mon-Khmer, and furthermore one is able to construct a full sentence in Cham without a single Malay word *eg zöp hagêk bloh hü lô yau nan* 'why do you cry so much?' [*hü* and *nan* do however have 'Malay' cognates] – but also with Malay only *eg akan mötai* 'the fish is dead', Malay *ikan mati*! And final particles are found to be Mon rather than Malay in origin. Finally the phonology looks Mon-Khmer rather than Malay, for instance in that Cham (and Chamic) has initial aspirated stops which are unknown in any Malay tongue, but common in 'mon-annamisch'. In a footnote (p 327) Himly discloses that he knows the origin of these aspirated stops, they arise in some cases where an AN bisyllacic word has been contracted to a monosyllabic (Himly's own examples: Cham *thun* – Malay *tahun*, Cham *dhan* – Malay *dahan*), but 'dieser Vorgang ist den malayischen Lautgesetzen zuwider und findet in der Neigung des Tscham zur Einsilbigkeit seine Erklärung'. So even when the structure is known to be secondary it counts more than shared vocabulary!

Himly concludes that given the considerable number of monosyllabic words of Mon-Khmer origin and the low number of monosyllables in Malay languages, it seems justified to assume that the majority of the mixed Cham race was non-Malay ('dass die Mehrzahl des Mischvolkes der Tscham nicht malaiisch war') and that the many resemblances of Chamic languages to Mon-Khmer must carry more weight than the Malay words in assessing their position among languages.

Aymonier's Cham grammar (1889) does not take a stand on the origin of Cham, but in describing the function of morphemes and particles Aymonier all the time equates them with corresponding elements in Khmer or Annamese, sometimes finding even a phonetic resemblance (as for instance the causative prefix Cham *pa* – Khmer *ba*), which to some is an argument in favor of establishing an Austric superstock taking in both Austroasiatic and Austronesian (Schmidt 1906), an idea for the time being convincingly discarded by Benedict (1975). For our purpose the AA-AN dichotomy is the relevant level of investigation, since the morphological and syntactical structure of Chamic is so overwhelmingly Mon-Khmer-like whereas the strongest vocabulary component (with clear phonetic laws, albeit involving drastic reductions), including the 'basic' items, is AN.

Subsequent research (beginning with Cabaton 1901 and Aymonier & Cabaton 1906; see most recently Pittman 1959, D. W. Blood 1962, Thomas 1963, D. L. Blood 1967) have generally not hesitated to call Cham Malayo-Polynesian, recognizing the double nature of the evidence. Blood (1967) states that 'Cham is a Malayo-Polynesian language, as far as vocabulary is concerned, although both the phonology and grammar are very similar to Mon-Khmer languages'.

We have here a very clear-cut case: Cham is genetically AN (both according to classical comparative methods and according to lexico-statistics), typologically AA. The two independent approaches give different results, and neither can be used to support the other.

In one important respect the Chamic languages differ typologically from each other: Cham has pitch accent (two registers) just like Khmer, whereas Jarai and Radé do not. This means that Cham is even further integrated with Mon-Khmer typology than the other Chamic languages. All of these share the tendency towards monosyllabism (like Vitetnamese) or at least the special kind of bisyllabism where the first (minor) syllable has a more limited choice of phonemes than the second (major) syllable (like Cambodian) – as for instance Proto AN **bəγai* 'give' > Radé *brεi*, Proto AN **dalan* 'road' > Radé *ela`n* (only vowels *e* and *a* occur in minor syllables). Cham has followed Khmer in the (recent) reinterpretation of the voiced-voiceless contrast in initial stops to low and high register respectively.

We discussed the fact that Tai and Malay/Indonesian are structurally closer to each other than to either Proto AN or Proto ST. Vietnamese (whose basic vocabulary shows a preponderance of Mon-Khmer roots, but also quite a few of Tai affinity) is an Austroasiatic language (Haudricour 1955) with a typology extremely akin to Tai. There exists in other words a powerful typological center in Southeast Asia which has drawn into it languages of AN origin (Tai), AA origin (Viet), and ST origin (Cantonese) and which is in the process of incorporating Chamic (nearly completed) and Malay/Indonesian (partial) from AN, and Khmer from AA. This typological process hits languages of any origin which come near it, and knowledge of this process – which only becomes clear when genetic relationships have been determined – will discourage us from basing genetic assumptions on typological grounds. By Hjelmslevian definition genetic relationship must be kept separate from typological relationship, and actual examples prove that they may often yield different results.

Like the archeologist, the linguist should leave no stone untouched. But also like the archeologist he should endeavor to establish which stone was there first, and which stone has always been there and which one was brought in. The pattern of inheritance and the pattern of external influence can never tell us anything about each other, except that A is not B, and B is not A.

## Conclusions

In his 1951 lecture on Rask, Hjelmslev makes the important point that Rask's typological classifications could be used by geneticists because of non-predictable, non-universal features of Indo-European (pp 13–14):

> Un hasard étrange a voulu que les principaux classements établis par Rask, notamment le classement des langues indo-européennes, bien que fondés sur des critériums de structure, sur des critériums typologiques, ont pu être adoptés par la linguistique moderne et être réinterprétés par elle d'un point de vue théoriquement différent ... On a pu maintenir les classes de langues en les réinterprétant d'un point de vue historique. Il n'a fallu qu'une légère modification pour les adopter telles quelles. L'explication réside dans le fait que les langues indoeuropéennes reconnues comme telles par Rask présentent entre elles, outre les correspondences régulières entre phonèmes qui sont à la base des lois phonétiques, certaines ressemblances de structure qui pour Rask ont été décisives et qui lui ont permis de les classer ensemble.

There has in modern times been a growing awareness that lexical correspondences are valid for genetic research only if accompanied by clear and general sound laws, and that comparisons of sentence structure will always be a matter of typology pure and simple. The above-mentioned historical accident, which made Indo-European the first family of languages to be subjected to scholarly scrutiny, carried with it the curious consequence that in morphological matters no clear-cut distinction between genetic and typological comparison was observed – within Indo-European languages, like morphological structure usually entailed a large degree of phonetic similarity of the inflexional morphemes (so that within this family it is possible not only to trace a widespread gender system but also a widespread use of the same phonetically related case systems characterizing them, and not only widespread use of grammatical person in verbal conjugation, but also widespread specific phonetic elements used to indicate them, etc). But it is precisely within morphology that a crucial line must be drawn which separates one kind of comparison from the other – if Turkish and Armenian noun declensions ressemble each other it is a typological fact, for there are no sound laws involved, but the suffixes employed by the two languages can be compared with those of structurally dissimilar languages in order to establish the sound correspondences and the possible genetic relationship. If Cantonese employs a system of noun articles which strongly remind us of Indo-European (*kɔ˙ jàn* 'the man', *ce˙k kău* 'the dog', *tí˙ jĕ˙* 'the things') a look at the phonetic forms will convince us that the resemblance is typological, but not genetic (the Cantonese articles are genetically identical with the classifiers of other Chinese dialects). If Akha (a Lolo language within Tibeto-Burman) exhibits a system of sentence particles (or predicate particles) which in many important details resembles the Indo-European verbal conjugation (syncretisms of grammatical person, tense and mood; syncretism and semantic connection between verbal genus and tense; see Egerod 1978a and 1979) this is typological (and probably historically significant), but entails no phonetic correspondences beyond chance.

Chamic has shown us that a rich morphological system can be lost or partially adapted to that of another language family (from AN to AA), Tai that it can be lost altogether (unless Tai represents AN before it went east!), Japanese that one such system can be partially preserved (Altaic?) while another one is partially introduced (AN?).

Hjelmslev's definitions of genetic and typological relationships can help us avoid the pitfall of Rask (or of those who followed him without seeing what he had proved and not proved) – the confusion of genetically and structurally related morphologies. To this caveat must be added the pre-Benedict pitfall in South East Asian 'alignments' – the confusion of borrowed and inherited strata in languages. The mistake of assigning genetic affinity on the basis of sentence structure is less apt to occur nowadays, but one may hear the argument used that Chinese is not Sino-Tibetan because of its word order – or that it is, because the oldest known period has a word order more reminiscent of Tibeto-Burman than is the case of later Archaic Chinese. Actually, it is only on the basis of shared vocabulary that we can posit ST at all, and the structural phenomena become pertinent facts in the history of the language once a relationship is posited. To the extent that structure can help distinguishing two strata, competing for genetic status, structure might be said to have helped us in genetic matters (*cf* Doerfer's axiom that genetically related languages must be more alike structurally the further we go back), but such arguments only work where the strata involved (like Early and Later Archaic Chinese; Old and Modern Turkic and Japanese) have already been chronologically placed in relation to each other on historical evidence, such as dating of inscriptions and other written matter, so the argument is at least partially non-linguistic.

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## *Eric P. Hamp:* Discussion

Our topic is genetic comparison and the contribution of typology to that enterprise. This is a complex and vexing topic, and our time is limited and short. I will therefore confine myself to a few salient points.

We have heard Hjelmslev's writing characterized as difficult. True, his content typically was abstract, his striving for explicit precision intense, and the distilled richness of his discourse dense. But I shall never forget the day as a young student that I discovered for myself the first Hjelmslev article that I had ever read, an exposition of the thought of Saussure – a model of lucid, incisive prose. For me, Hjelmslev's writing has always been the embodiment of graceful and urbane clarity – dense and difficult concepts which he cleansed and purified with a disciplined literary skill.

But I must get on with genetic – evolutionary – cognacy and history. And here we must call to mind another luminous Copenhagen name. Just as I was electrified on reading, in Harvard's Widener Library, my first Hjelmslev article, so I was transfixed with admiration and engulfed with satisfaction as I took the bus home after a long and attentive day at the Bibliothèque Nationale with Holger Pedersen's brilliant and far too neglected article on 'Die Gutturale im Albanesischen' (1900). Not everybody's subject, I concede. Not written in a flowing style, nor organized on Aristotelian principles. But a gem nonetheless, cutting its way through a jungle of ill-documented and half explored material, tracking and weighing each fugitive etymology, inspecting every possible phonetic segment for relevant equivalences and *Lautgesetze*, ordering all changes in an assumed relative chronology, seeking phonetic naturalness in all assumed changes, placing all segments in systematic oppositions in successive chronological states. Of course we would hope to transcend (*ie* revise) it today; but dismiss it, never. Brugmann gave this article a fleeting and diffident mention in the revision of his monumental *Grundriss*. But Meillet and Kuryłowicz – all the world except for chronic Albanophiles such as Jokl and Tagliavini – have behaved as if this masterpiece of comparative artistry never existed. If they disapprove, let them refute it, and on solid knowledge.

Yet that is only one of Holger Pedersen's triumphs – an unsung victory. His vast comparative Celtic grammar (1909–13) is justly famous and enduring. True, Thurneysen's 1946 revision overtakes the Irish side; but the *VKG* remains a spectacular achievement on terrain that is recognized as demanding the most exacting performance merely to establish correct equations and formulations and to reach the basic indications of cognacy and evolutionary classifications. In these same years Pedersen tackled the riddle of Slavic initial *χ*-, which seems to lack a motivating context for its departure from *s*-; and he persevered with the Armenian demonstratives. Late in his life he laid the foundations for our correct perception of the difficult newcomer, Tocharian. Finally, after having mastered what could then be known of Hittite and having synthesized his grasp of its place in Indo-European for all to inspect, Pedersen returned incisively to the old

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love of his revered teacher, Vilhelm Thomsen, to place Lycian firmly in the newly burgeoning Anatolian sub-branch.

Now where does typology fit in all this tiresome detail and relentless tracing of minutiae, you may well ask? Not very prominently, you will say; and in great measure you will be correct, and that must be our response.

Yet there is indeed scope for typological considerations in our present concerns – in a sort of negative sense, if you will. Roman Jakobson dealt with this proposition in a lecture, much cited of late, delivered at the Oslo congress in 1957. In such a context we invoke typology as a constraint on reconstruction, and hence as a barrier to the allegation of indefensible classifications, *ie* erroneous claims of cognacy. Pedersen used such a criterion indirectly in his last monograph, dealing with the possible (distinctive) phonetic features of the Indo-European obstruent system. In simple terms, this principle says: If you cannot match the configuration in question in some well described natural language, be wary of reconstructing it in an alleged parent language.

This negative proviso aside, however, what of all the rest of the activity which we have alluded to? On what did the success of Holger Pedersen rest, apart from sheer stubborn observation and tireless accuracy? What was the *nature* of these classes of languages that form the special case of typology that Eli Fischer-Jørgensen has cited from Hjelmslev's 1950 Helsinki lecture (*cf* above, p. 73)? That is to say, what is the nature of the criteria which can be invoked as a diacritic for the genetic relation? I say that these criteria are essentially and quintessentially individual, particular, idiosyncratic. What we seek are linguistic signs which are so singular in configuration as to be remotely unlikely to find replicas through accident or the general properties of human beings. Meillet liked to claim that Indo-European could have been demonstrated on the basis of the verb 'to be' alone; his principle of laying fundamental weight on synchronically surviving irregularities is in effect an invocation of the text-critical dictum of the *lectio difficilior.* Likewise we prize remnants of IE **so sā tod* (the pronoun and anaphora), OIr *do·cer* (the only preterite 3 sg. in the attested language which was *not* palatalized in its final) = Skt *aśarīt* < **kerə-t,* the syllabic accents and accent displacements of Baltic and Slavic whose patterned vagaries have fascinated Leskien and Saussure through Hjelmslev and Kuryłowicz and now Dybo and Kortlandt. Afro-Asiatic essentially stands on the strangely distributed pronominal affixes. Wiyot and Algonquian share the arbitrary *t*-insertion rule for pronominal prefixes with stems in vowel-initial; their typologies are really somewhat distant. Yurok joins these two with a fossil unproductive labial (**m-*) indefinite/unspecified possessor prefix. It is more important in the context of genetic argument that Sandawe and Bushman-Hottentot share a feminine gender suffix and certain deictics than that they both display 3–4 click articulations.

In all these genetic arguments – and hundreds of others – what counts is the particular, the small, fine-grained, the odd, the quirky. It is the non-general; what the relentless eye of a Pedersen or a Sapir fastened on;

what the typologist will cast aside as a sport, as non-typifying. Note that Murayama in his own consideration of Japanese-Austronesian correspondences, whether genetic or very old-layer loan, relies on just such detail. One of the salient facts that unite Greek and Armenian is the oddity, the shared oddity, that the word for 'lamb' is declined like no other *n*-stem; or that the words for 'foot' and 'knee' have specialized the o-grade; or that nasal presents copy the nasal as a suffix. I challenge any typologist to seize quickly and efficiently on just these facts, and not on hoards of genetically uninteresting ones.

Let me be outrageous: It is not very interesting for our purposes that French *feu* = Romanian *foc*. One of the best proofs of Romance unity that I know is the complex, indirect and arbitrary sign-equivalence that holds for pronouns (proclitic) and definite markers, French (proclitic) *le la les lui leur* and Romanian *-l (u) -a -le -lui -lor* (suffixed, or enclitic).

After our paean to the particular, let me try to recover some respectability with my colleagues who feel we really should be scientists and busy ourselves with the discovery of general laws and broad regularities – yet let me hasten parenthetically to insist that, paradoxically, Karl Verner's eternal triumph of dazzling lawfulness for our entire discipline rests on his stupendous perception of a thoroughly idiosyncratic sprinkling of partly non-rule-governed accents through verbs and nouns.

What is the rôle of typology in cases of extreme diffusion that we have come to call *Sprachbünde* and to segregate carefully from what we have just now dwelt on under the rubric of cognacy? My time is presently running out, and I have in any case recently made the distinction that I consider essential and crucial between the non-historical (*ie* typology) and the historical (*ie* genetic, as well as *Sprachbund*), or if you will, between the typological and *Sprachbund* diffusional (*ie* the non-cognate) and the genetic (or cognate). For a *Sprachbund*, though we single out features that may seem typological (*eg* postposed definite markers that any Scandinavian speaker finds familiar), we impute to them a geographic and historical interpretation, thereby assigning them *eg* to the post-Roman Balkans, and divorcing them from their Scandinavian imposters. Or perhaps we should assign a similar account to the exact mapping of Tai tones on those of Chinese, which Professor Egerod has pointed to (above, p. 121).

Our recognition and refinement, on a theoretical level, of *Sprachbund* phenomena is fairly recent – in some senses a development of the last 20–40 years. I should insist here that the correct genetic assignment of all languages concerned is prerequisite to any *Sprachbund* determination. The classic case is the Balkan area, though in the past couple of decades a good half-dozen others have become clearly recognized, while dozens of other attenuated instances dot the map. Once again Copenhagen's chest may fill with pride. For the recognized initial codifier of the model specimen, *Balkanphilologie* or *linguistique balkanique*, was her own Kristian Sandfeld.

If the topics which I feel there to need emphasis have led me to names other than those of Rask and Hjelmslev, these topics were by no means strange to those giants of our field and they give Copenhagen's brilliant university only greater glory.

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## *Eugénie J. A. Henderson:* Discussion

Danish linguists past and present have excited the admiration of their colleagues in other countries for the depth and range of their scholarship and for the skill with which they have managed to balance penetrating insights into theoretical matters with a sound working knowledge of a variety of other languages and, in recent years, of the experimental and laboratory techniques required for their accurate analysis.

Professor Egerod's masterly introduction to the theme of this afternoon's discussion demonstrates a characteristic Danish command of a wide-ranging and varied language field. He has given us a very clear and a very fair survey of the tangled relations between genetic and typological comparison. In particular, he has reminded us of the dangers of basing genetic classifications upon typological evidence only – no matter how striking this may be. And he has drawn our attention to cases like Japanese, where vocabulary comparison as such, without regard to what we know of the history of the language, or to the kind of vocabulary, might lead us towards a link with Chinese, whilst typological considerations could pull us either towards Altaic or towards Malayo-Polynesian, depending upon what sort of criterion we decide to apply. He has also cited the interesting case of Cham, where vocabulary points to Austronesian relationship, while grammar and phonology both point towards Mon-Khmer.

The problem about using typological evidence for classification of any kind other than purely typological is that it is essential that we should be able to distinguish which characteristics are universal to human language as such, or are at least so widely diffused that they can hardly be used as evidence of anything at all except that a language *is* a language. An example referred to by Professor Egerod is the use of *rendaku*, that is, 'intervocalic consonantal voicing', to demonstrate or at least to support the alignment of Japanese with Malayo-Polynesian. Such a natural and widespread phenomenon as the voice-assimilation of intervocalic consonants does not carry conviction as an indicator of genetic relationship.[1]

It thus seems to me that it is in the current interest in and increasing knowledge of so-called 'language universals' that typology may be expected to contribute to genetic studies in future.

Greenberg has maintained that 'typological classification finds its sought-for justification in the investigation of universals'. Typological studies and linguistic universals are necessarily and closely interrelated. One implies the other. They are, says Saporta, merely different sides of the same coin. So what can they tell us that is relevant to the matter of genetic classification?

1. In the course of discussion Professor Egerod pointed out, in fairness to Murayama, that he did not claim that intervocalic consonantal voicing as such was an indicator of genetic relationship. His arguments were based on specific cases in which there is a striking similarity between both morphological and phonological elements in Japanese and Malayo-Polynesian.

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Ferguson has made the modest claim that 'the statement of universals has the advantage of making unspoken assumptions explicit so that they may be checked'.

To take one example from a field familiar to me, there seems to have been such an unspoken assumption behind the realignment of Vietnamese with the Tai languages in the 1952 edition of *Les langues du monde*. The great French scholar Maspero rejected the decision of the editors of the first 1924 edition to class Vietnamese with the Mon-Khmer languages, a classification which today would be accepted by all specialists in the field. Maspero's rejection of the Mon-Khmer connection was based on the very striking phonological similarities between the Tai languages and Vietnamese – the monosyllabism, the treatment of initial consonants, and their relation to the tones, and above all, the tonal system itself, appeared to him to outweigh the similarities between the core vocabularies of Vietnamese and the Mon-Khmer languages which he relegated to a 'substratum' – that useful device for dealing with awkward evidence! It seemed inconceivable to him that such striking and far-reaching phonological similarities could be a matter of mere borrowing, and in particular he appears to have found it difficult to accept that an originally disyllabic non-tonal language, like Khmer or Old Mon, could ever have developed into a monosyllabic highly tonal language such as Vietnamese.

Work on other languages since that time in South East Asia and in other parts of the world has tested the assumptions underlying these views and has shown them to be false. We now know of languages which are in the process of changing from non-tonal to tonal – languages like Riang in Burma, and some dialects of Khmu, where an earlier voice-quality distinction is being re-structured into a pitch-distinction. This change may well be speeded on its way by the influence of the surrounding non-related tonal languages – but this is not the whole story. There are internal forces at work within the phonologies of the languages themselves that also work towards this end. Linguists have only relatively recently begun to realise the phonological importance for some language families of certain distinctions in voice quality or phonation type, which were previously thought to have only emotional or affective significance. Voice quality differences variously referred to rather unhappily as 'creaky voice', 'breathy voice', 'murmur' and so on, occur regularly in many languages of South East Asia and Africa (and probably also elsewhere). They are often associated with some other feature such as tone, consonant type, or vowel quality, but sometimes have full phonemic status as the only feature distinguishing different lexical or grammatical items. Being buried in the larynx, the mechanisms involved in phenomena of this kind are difficult to examine or observe, and research into this field is only in its infancy. It is already clear however that the various phonation types are frequently correlated with changes in the segmental constituents of syllables, and with their pitch – such changes being observable in languages which are neither genetically related nor geographically close. Tongue root retraction or advancement is one factor that has been supposed to play a rôle here, but there is still much to learn. There is no doubt, however, that features of this kind can be expected to have far-reaching effects on phonological systems, in the matter of vowel quality changes, consonant voicing or devoicing, and in the development of tones. It is perhaps not too fanciful to suggest that it might be illuminating one day to review IE laryngeal theory in the light of this new phonological dimension.

To return to the subject of universals, they have sometimes been subclassified into *synchronic* and *diachronic*; synchronic universals being defined as those regularities discovered by observing the characteristics of given language states; while diachronic universals are defined as probabilistic tendencies which imply 'states of language' (*états de langue*) with a historical connection between them, as for example, a universal diachronic tendency for intervocalic unvoiced consonants to become voiced. At first sight it might seem that we should expect to find diachronic universals most relevant to our present purpose. But I venture to suggest that the distinction is somewhat artificial. No less a scholar than Roman Jakobson has spoken of 'the fictitious chasm between the study of constancy and changes'. Ideally, universals should be *panchronic*. It would make no sense, for example, to posit *diachronic* processes that would lead to synchronic states which violate *synchronic* universals. The universals linguists should be seeking must be valid for 'toute langue á toute époque' – to quote André Haudricourt – taking due account, of course, of the effects of borrowing and of areal dissemination of specific features in a given language area.

It seems to me that there are two areas of current research, both basically typological, that may be expected to affect our notions of genetic classification and language families in general.

First, there is the work on phonological hierarchies by scholars like Matthew Chen and James Foley,[2] who are seeking universal panchronic laws of linguistic change – *not* sound laws which can be stated as if they affected a whole class of consonants at one fell swoop, so to speak – *ie* as if all Proto-Germanic voiced stops simultaneously became unvoiced stops. Almost certainly no one ever believed that things happened in quite that way, but traditionally sound laws are framed in general terms which leave out of account the possibility that not all members of a class were affected at the same time or at the same rate. Foley, Chen and others are concerned with sound change as an on-going dynamic process. Their interest lies in trying to discover whether the course of these processes can be predicted, not just in one language or language family, but in any language. And to discover the hierarchical rules, supposedly universal, governing the *order* in which sounds of a given class will undergo a specific sound change.

Take for example, the so-called 'spirantisation' or 'weakening' of intervocalic voiced stops. (There is of course no new discovery as such

2. I am grateful to Professor Henning Andersen for drawing my attention to the important earlier work on phonological hierarchies by Ludwig Zabrocki.

involved here, as can be seen from the section headed 'Indolence' in Dr. Henning Andersen's translation of Bredsdorff's (1821) paper on *Linguistic Change*. What is new perhaps is the emphasis laid by Foley, Chen and others upon the process as such.) This 'weakening' process is said to have gone one stage further in Danish than in German, since we have

| | | | |
|---|---|---|---|
| *Danish* | kage | [kæγə] | 'cake' |
| | bide | [biðə] | 'bite' |

but *købe* 'buy' with no spirantisation, suggesting that this type of 'weakening' starts with the back consonants, the velars, and works towards the front. In some kinds of German a similar process is taking place, but has so far affected only the velar stop, *eg*:

| | | | |
|---|---|---|---|
| | sagen | [zaγən] | 'say' |
| but | baden | [badən] | 'bathe' |
| | beben | [bebən] | 'shake' |

A comparison with Latin indicates that Spanish has taken the process several stages further. Beside Latin *lego, credo, habere* we have Spanish *leo* and *creo*, where the velar and dental have weakened to the point of disappearance, even in the spelling, but the process has not yet got so far with the labials, where we still have *haber* [aβer], with a fricative intervocalically.

In my notes written before this meeting, I suggested at this point that 'one might predict that the next step for Danish will be the weakening of the *b* in *købe* to a fricative, and perhaps the disappearance altogether of the velar fricative in *kage*.' Since I have been here, I have learned that the latter appears in fact to have gone already, so that we have something like [kæ:ə] or at most [kæʲə], with a weak approximant! The speed of this particular sound change in Denmark appears to be pretty rapid and has already left Foley – from whom I took the example – well behind!

In North German the next stage should be weakening of *baden* to [baðən], and in Spanish, the further weakening of the intervocalic bilabial fricative, perhaps to some sort of approximant or semi-vowel, followed by its disappearance altogether.

Matthew Chen has demonstrated a precisely similar process that has taken place in the history of the Chinese dialects, and claims to show that while the order of weakening is always the same, the *timing* of the process may vary from one dialect to another, so that it is possible to find live illustrations of the workings of this phonological process at various stages of its course. Chen has also shown that in syllable final position in Chinese the hierarchy works the other way. Labials and dentals are lost first, being merged with velars which survive longest, but are in some dialects also reduced to the glottal stop, and in others even this goes, so that all syllables formerly ending in stops now end in vowels.

There are of course also strengthening processes, whereby initial stops may be 'strengthened' by, for example, so-called 'preglottalisation', as in Vietnamese. Here the hierarchy of change starts with the labials and works back towards the velars. Many languages have only one preglottalised or implosive initial consonant, and it is [ɓ] – others have two, [ɓ] and [ɗ]. It is much rarer to find languages where the process has gone still further, to incorporate an implosive palatal or velar. It is quite common in South East Asia, however, to find plosive systems with a full set of voiceless aspirated and unaspirated plosives:

| | | |
|---|---|---|
| p | t | k |
| ph | th | kh |

but only 2 voiced ones, always [b] and [d]. Sometimes the [b] and [d] are glottalized, sometimes not. This seeming asymmetry in the plosive system has puzzled linguists, especially since it is found in languages of different genetic affiliations – *eg* Tibeto-Burman, Tai and Mon-Khmer. Since such languages are however close to each other geographically, phonological diffusion seemed a plausible solution until it was discovered that certain quite unrelated languages in New Guinea have the same system too. Such striking phonological similarity might once have tempted linguists to take the view – as Maspero did about Vietnamese – that it could *not* be coincidence. But if the underlying process can be established as 'universal' in some sense, the problem is perhaps not solved, but it does at least cease to be a *genetic* problem.

If phonological hierarchies of this kind can be shown to be universal, or even quasi-universal,[3] it will not of course help us to date with precision any particular sound change, but it may help to some sort of relative dating of the stages reached by different languages, and it should help to account for some apparent 'exceptions' or 'breaches' of general sound laws in individual language. It will certainly make us hesitate to use features like intervocalic voicing as genetic indicators of any kind.

The other area of current research I would like to mention briefly is centered on the notion of linguistic change as occurring in cycles. Once again, this is not a new idea in itself, since it can be dated back at least as far as Schlegel, but if fully sustained and attested, it will cast grave doubts upon many of our hitherto 'unspoken assumptions' about typological features.

Recently, the work of James Matisoff and others on tonogenesis has pointed to a phonological cycle in the languages of Far and South East Asia, whereby over vast stretches of time a non-tonal language may become tonal, and a tonal language may become non-tonal. The conditions for such changes include processes of attrition under certain conditions of stress, which may lead to the dropping of unstressed syllables, with a consequent rise in the number of homophones. At the same time, there are other processes at work which have to do with the physiological mechanisms of speech production. For example, the voicing of an initial consonant entails lower frequency of the following vowel. This may be

3. Henning Andersen, in discussion, raised pertinent objections to the use of the term 'universal' in this context, suggesting that the terms 'generalisation' or 'general process' would be more appropriate. He referred to a recent (1978) paper by Greenberg, which I have not seen, in which Greenberg also now inclines to this view.

imperceptible at first, but in the course of time may become striking enough to be distinctive, thus leaving syllables redundantly distinguished both by the voicing of the initial and the pitch of the following vowel. In such circumstances, the pitch variation may take over as the phonologically relevant feature, and the voicing of the consonant may be dropped. The classic examples of the above process are to be found in Chinese and neighbouring languages.

Where the working of some phonological process leads to an inconvenient increase in the number of homophones, some of the latter may be 'glossed', so to speak, by the addition of an explanatory lexeme, just as those American dialects which make no phonetic distinction between the words *pin* and *pen* are said to distinguish the meanings by calling one a *stick-pin* and the other a *writin'pin*. A similar process appears to be going on now in Mandarin Chinese, which means that the tones of individual lexemes are becoming less important to comprehension, and the language may be on the way to becoming polysyllabic rather than monosyllabic, and perhaps also to becoming non-tonal or at most a pitch accent language.

But 'cycles' are not confined to phonology. Recent research by F. K. Lehman, LaRaw Maran and others points to what Matisoff has called a 'morphology/syntax' cycle. The simplest view of this is that (1) phonological change wears away morphological inflexions, (2) the loss of morphological distinctions is made good by periphrastic constructions, (3) the grammaticalized periphrases lose their original independent meaning, and are restructured as purely grammatical morphemes, so that the language reverts to a morphological type once again. Dr. Wurzel (above, p. 109) referred to similar changes in the phonological system of Old Germanic, when inflections were dropped and replaced by the use of articles etc., or by lexical means. (Here again Bredsdorff, in 1821, had something relevant to say in that same section on 'Indolence', where he discusses how the Danish postposed article and passive ending have arisen from earlier independent words, and says 'perhaps all inflexions go back to independent words, even though this is not evident in every case'. Whether the particular examples he gives are correct or not, does not affect the argument here.)

Vennemann has said: 'Languages develop cyclically ... with sound change being the causal factor throughout.' There are indications that this may be an over-simplification. Maran has pointed out that periphrastic causatives, for example, may arise while causative affixes are still in use, but once they have arisen they tend to take over from the earlier affixed forms. It seems therefore that some explanation other than phonological change *per se* must be sought here.

Looked at from the cyclical and hierarchical points of view, the classic typological distinctions – isolating, agglutinative, inflecting and polysynthetic begin to take on a new temporal dimension. The work on phonological hierarchies certainly casts some doubt on the validity of the theoretical requirement that we should compare systems only. We may also have to revise our notions about 'natural classes'. Perhaps, as Søren

Egerod has suggested, the time has indeed come or will shortly come when we shall have to rethink our old concepts about genetic and typological relationships – and even to query the usefulness of the distinction.

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## Panel and open discussion

Two major subjects were discussed: the descriptive procedures characterizing genetic and typological linguistics, and the question of the nature of the relationships between languages.

The difference between typological classification and genetic classification is quite clear. They have radically different starting-points and radically different goals. It would therefore be surprising if one could be used to solve the problems of the other. Typological classification presupposes knowledge of what is universally available in language, whereas genetic classification is based on highly idiosyncratic features of particular languages. Yet the two types of classification seem to meet in the role of the word. Genetic classification is usually based on comparisons of individual words or word-like structures in different languages, typological classifications on the comparison of word-structure and -order.

A plea was made for establishing a methodology of diachronic typology which, among other things, would set up a general typology of permissible sound-shifts, along the lines of Henderson (above, p. 000).

The situation in modern German was discussed in relation to Henderson's hierarchy, and it was shown to be more complex, involving possibly two hierarchies of opposite tendencies, one going from /g/→/b/, yielding High German /zaγŋ/ but not (yet?) */baðn/ or */bevn/, and one going from /b/→/g/ prenasally, yielding South German/Austrian /be:m/, /za:ŋ/ and possibly /ba:n/ – though this latter may be blocked due to homophony with *bahnen* – with *loss* of the plosive. It was also pointed out that features like syllable structure, accent, etc. make it difficult to establish universally valid hierarchies.

A suggestion was made that the linguistically significant distinction for genetic classification was that between 'morphology proper' (*ie* 'the system of grammatical signs') and 'morphophonemics' (*ie* 'the system of variants assigned to the expression elements of morphological signs, grammatical signs, or of lexical signs') (HA). With respect to this distinction it was suggested that genetic classification was based on morphophonemics, and further that Rask's successful classifications were genetic in precisely this sense. It was finally asked whether morphophonemics in this sense is indeed part of the language system or rather belongs to the norms of usage.

As for the question of the nature of the relationships between languages a strong case was put (by RHR) for regarding genetic relationships as essentially sociological, or sociolinguistic, phenomena. What 'genetic relationship' means is that there has been an uninterrupted *conscience linguistique* between speakers of the same or succeeding generations. A genetic link is either there or not, and it is there whether we can find it or not. The question whether a genetic relationship holds between two languages can only be answered by 'yes' or 'no'. On the other hand it is entirely legitimate to say, from a *typological* point of view, that a language is a *Mischsprache*. The question whether a language is typologically akin to another language admits of graduated answers.

This point of view was challenged on general grounds by Alvar Ellegård, who argued that the notion of *genetic* language-relation was inappropriate since there continued to be interactions between languages even after a 'split'. The *Stammbaum* view should be given up because it shows a language as contracting 'family relations' with only mother and daughter, whereas it should be properly shown in a network of relations, also to sisters. Where the notion of genetic relation is perhaps justified is in connection with the comparison of individual lexical items.

It was also challenged on specific grounds for such cultures where linguistic traffic has been very heavy and where the cultural strands are difficult to unravel, as in Hongkong and Japan.

Contributors were SE, EJAH, RHR, HB, JR, WUW, WUD, EC, HA, HS, SML, EH, Niels Ege, Alvar Ellegård, Una Canger (chairman).

***Niels Ege:* On the absence of ˀg in consonant systems of SE Asian languages**

In discussing the asymmetry observed in many languages of South East Asia of the type

| | | |
|---|---|---|
| p | t | k |
| ˀb | ˀd | |

*ie* the frequent occurrence of stop patterns lacking the preglottalized velar, Ms. Henderson suggests that an original *ˀg* was eventually lost because of 'universal' (= physiological) difficulties of combining velar stop articulation with glottalization (implosion).

I would like to point to another likely source of this asymmetry.

In Lawa, an Austro-Asiatic language of Northwestern Thailand, two distinct consonant inventories are observed. In one group of dialects, while there is a full series of glottalized nasals, including the velar nasal, the stop series exhibits the asymmetry in question, lacking the preglottalized or implosive velar:

| | | | |
|---|---|---|---|
| p | t | č | k |
| ˀb | ˀd | ˀɟ | |
| ⋮ | | | |
| m | n | ɲ | ŋ |
| m̉ | n̉ | ɲ̉ | ŋ̉ |
| ⋮ | | | |
| w | | j | |
| | l | | |
| | r | | |

In another group of dialects there are no glottalized stops at all, but here we find a non-glottalized/glottalized contrast not just in nasals, but in semivowels and liquids as well:

| | | | |
|---|---|---|---|
| p | t | č | k |
| ⋮ | | | |
| m | n | ɲ | ŋ |
| m̉ | n̉ | ɲ̉ | ŋ̉ |
| ⋮ | | | |
| w | | j | |
| w̉ | | j̉ | |
| | l | | |
| | l̉ | | |
| | r | | |
| | r̉ | | |

Words with preglottalized stops in the first dialect group invariably have cognates in the second group with $\mathring{w}$ (corresponding to $^{ʔ}b$), $\mathring{l}$ (corresponding to $^{ʔ}d$ in some words), $\mathring{r}$ (corresponding to $^{ʔ}d$ in other words), and $\mathring{j}$ (corresponding to $^{ʔ}j$); and vice versa.

Clearly, the second inventory represents the more original state of affairs.

Practically all Lawa dialects additionally have full series of voiceless nasals, semivowels, and liquids (not entered in the charts above). Evidently, then, in proto-Lawa – and in some present-day dialects – any non-obstruent consonant occurred in any one of three distinct phonation types: voiced, voiceless, 'glottal'.

The absence of the preglottalized velar stop in Lawa is thus due to (a) all preglottalized oral stops being secondary developments from 'glottal' liquids and semivowels; and (b) the absence, universally, of velar liquids and semivowels.

I submit that this explanation may account equally for the corresponding asymmetry in a number of other languages in this region, in particular those of the Austro-Asiatic family.

*Ie,* rather than being an illustration of how pattern incongruity may arise from the (natural) *loss* of a segment, this may serve to show how segments can *develop* (naturally) into – and despite the resultant – pattern incongruity.

*Editors' note:* In reply to Niels Ege, Søren Egerod warned that there are three phenomena in South East Asian languages that can be assessed in terms of 'preglottalization': a 'loose' type (in Formosan languages) which neither phonetically nor phonemically can be described as preglottalization but which reduces to underlying consonant clusters, preglottalization proper, and implosion. One should be careful not to confuse them and treat them all alike, and it might be that some of these distinctions were pertinent to Niels Ege's data.

# 5 Essential criteria for the establishment of linguistic typologies

## *E. Coseriu:* Introduction

### Der Sinn der Sprachtypologie

**0.1.** Die heutzutage in verschiedenen Kreisen und in verschiedenen Ländern übliche Sprachtypologie ist u.E. weitgehend durch zwei stillschweigend und oft auch ausdrücklich angenommene Gleichsetzungen vorbelastet: einerseits durch die Gleichsetzung von Typologie und Charakterisierung von Einzelsprachen bzw. von Sprachgruppen, andererseits durch die Gleichsetzung von Typologie und Klassifizierung der Sprachen. Oft wird nämlich jede mögliche "Sprachcharakteristik" als Typologie bzw. als Ersatz dafür aufgefaßt. Und in vielen Beiträgen zum Thema wird die Sprachtypologie unter dem Namen "Klassifikation der Sprachen" behandelt: die Hauptaufgabe der Sprachtypologie wäre demnach eben, "Klassen" von Sprachen aufzustellen, die Mannigfaltigkeit der Einzelsprachen auf einige wenige Klassen zu reduzieren, wenn auch auf andere "Klassen" als die genealogischen und die rein geographisch begründeten.

**0.2.** In diesem Referat stellen wir uns als erste Aufgabe, die Sprachtypologie im eigentlichen Sinne sowohl von der Sprachcharakteristik als auch von der Klassifikation der Sprachen zu trennen, um zugleich die Beziehungen der Typologie zur Sprachcharakteristik und zur Klassifikation der Sprachen zu klären. Unter "Sprachtypologie" im eigentlichen Sinne verstehen wir eine Disziplin, die im Rahmen der Sprachwissenschaft ihren eigenen und autonomen Gegenstand hat, d.h. die weder mit anderen Disziplinen letzten Endes zusammenfällt noch bloße Anwendung anderer sprachwissenschaftlicher Disziplinen ist. Damit glauben wir auch einer in der traditionellen Typologie enthaltenen Intuition gerecht zu werden.

**0.3.** Beide o.a. Gleichsetzungen sind zwar schon in der traditionellen Typologie, so wie sie von Adam Smith und von Friedrich und August Wilhelm Schlegel gegründet wurde (*cf* Coseriu 1968a, 1972: 113–17) stillschweigend oder auch ausdrücklich gegeben. Adam Smith, Friedrich und August Wilhelm Schlegel wollten sicherlich auch Einzelsprachen im Hinblick auf die von ihnen identifizierten Arten der Sprachgestaltung "charakterisieren". Auch spricht Friedrich Schlegel ausdrücklich von zwei "Hauptgattungen", und August Wilhelm Schlegel von drei "Klassen" von Sprachen.[1] Dabei handelte es sich jedoch um eine Charakterisierung auf der eigentlichen Strukturebene des Sprachtypus. Und was die Klassifikation betrifft, handelte es sich um eine Verwechslung zwischen der

1. Der zuerst gelegentlich für 'Sprachgestaltung' bzw. 'Sprachstruktur' ('Sprachbau') von Humboldt verwendete Terminus 'Sprachtypus' wurde bekanntlich erst von H. Steinthal für die 'Klassen' oder 'Gattungen' früherer Autoren eingeführt.

Typology and Genetics of Language.
Travaux du cercle linguistique de Copenhague XX.
Ed. by Torben Thrane, Vibeke Winge, Lachlan Mackenzie, Una Canger, and Niels Ege.

Identifizierung des Sprachtypus und seiner klassifikatorischen Anwendung und zugleich um eine Abweichung von der ursprünglichen, die Sprachstruktur als solche betreffenden Intuition, die diesen ersten Versuchen zugrunde lag und die erst später, nämlich von Humboldt, explizitiert wurde.

**1.1.** Die Sprachtypologie ist sicherlich auch Charakterisierung der sprachtypologisch untersuchten Sprachen; jedoch ist nicht jede Charakterisierung von Sprachen an sich schon typologisch. Sprachen kann man auch anders als typologisch charakterisieren, und zwar auf vielerlei Weise. Eine Charakterisierung kann zuerst eine rein "äußere" sein. So z.B. kann eine Sprache in bezug auf ihre "Architektur", d.h. in bezug auf die diatopische, diastratische und diaphasische Variation, die sie aufweist, (bzw. nicht aufweist), charakterisiert werden: es gibt bekanntlich in dieser Hinsicht weitgehend einheitliche und ausgesprochen stark differenzierte Sprachen. Ebenso kann man Sprachen als verhältnismäßig "rein" und verhältnismäßig "gemischt" (bi- oder polysystematisch) charakterisieren; und bei den in einer in diesem Sinne "gemischten" Sprache koexistierenden Systemen kann es sich um verschiedene Phasen derselben historischen Sprache handeln (wie im Falle der romanischen Sprachen gegenüber dem Lateinischen) oder auch um verschiedene historische Sprachen (wie im Falle des Neupersischen, des Maltesischen oder der Kreolsprachen). Solche Charakterisierungen wären aber natürlich keineswegs sprachtypologisch. Und eine "innere" Charakterisierung kann rein formal sein und den Sprachtypus überhaupt nicht (oder wenn, dann doch nur indirekt) betreffen. So kann man Sprachen je nach den Grenzen, die in ihnen der systematischen Sprachschöpfung gesetzt sind, als "normgebunden" (traditionell) oder als "frei" charakterisieren (vgl. z.B. das Französische gegenüber dem Ungarischen oder dem Türkischen); und je nach dem jeweiligen Geltungsbereich ihrer Regeln als "regelmäßig" (z.B. Turksprachen), "unregelmäßig" (kaukasische Sprachen) oder als in dieser Hinsicht "gemischt" (Indogermanisch). Ja, nicht einmal eine innere "stoffliche" Charakterisierung ist an sich schon typologisch. Jedes Merkmal, das eine Sprache aufweist, charakterisiert sie gegenüber anderen Sprachen, die es nicht aufweisen; aber nicht jedes Merkmal ist sprachtypologisch. So ist z.B. der Artikel für die Sprachen, die ihn besitzen, sicherlich "charakteristisch" und charakterisierend gegenüber den Sprachen, die den Artikel nicht kennen; aber der Artikel hat an und für sich keinen bestimmten typologischen Sinn: dadurch allein ist noch kein typologischer Zusammenhang z.B. zwischen den romanischen Sprachen, dem Bulgarischen, dem Ungarischen, dem Baskischen und dem Samoanischen gegeben. Eine innere "stoffliche" Charakterisierung ist nur dann sprachtypologisch, wenn sie die Strukturebene des Sprachtypus als solche betrifft; und in diesem Fall fällt sie tatsächlich mit der Sprachtypologie zusammen.

**1.2.1.** Das Klassifizieren von Sprachen wurde bezüglich seines wissenschaftlichen und theoretischen Wertes schon von Humboldt mit heute noch gültigen Argumenten und gerade in einem sprachtypologischen

Kontext kritisiert und als wissenschaftliche Operation abgelehnt: Erstens seien die Sprachen nicht als Gattungen, sondern als Individuen verschieden und seien daher nicht klassifizierbar; zweitens beziehe sich eine Klassifikation auf das teilweise Ähnliche und das teilweise Verschiedene, es seien aber nicht diese Einzelheiten, die den "Charakter" einer Sprache ausmachen, sondern nur ihre jeweilige Verbinding miteinander.[2] Deshalb läßt Humboldt Klassifizierungen von Sprachen nur zu praktischen Zwecken und als Hilfsmittel zu:

> Nur also zum Behuf der Betrachtung oder der Darstellung, nicht um über ihre wahre Natur zu entscheiden, lassen sich Classificationen der Sprachen versuchen, nur in Hinsicht auf einzelne ihrer Beschaffenheiten. Auf diese Weise aber sind sie nothwendig und unschädlich, wenn man nur dabei die jeder wahren und constitutiven Classification widerstrebende Natur der Sprache im Auge behält. (Humboldt 1827–9: 190).

**1.2.2.** Trotz dieser Kritik wurde der klassifikatorische Ansatz der ursprünglichen Typologie in der späteren typologischen Forschung leider beibehalten und sogar verstärkt, und selbst Anhänger Humboldts, wie Steinthal und Finck, haben ihre Sprachtypologie als "Klassifikation der Sprachen" aufgefaßt und als solche dargestellt. Und heute noch erscheinen Werke, in denen ungeachtet der Humboldtschen Kritik unter dem Namen "Sprachtypologie" ausdrücklich eine Klassifikation der Sprachen nach dem Muster der naturwissenschaftlichen Klassifikationen vertreten wird.[3]

**1.2.3.** Unser "leider" bedeutet allerdings nicht, daß die Sprachtypologie überhaupt nichts mit der Klassifikation der Sprachen zu tun hat. Aber e r s t e n s ist der Begriff "Klassifikation" ein viel umfassenderer als der Begriff "Typologie" und auch als der Begriff einer eventuellen typologischen Klassifikation. Man kann nämlich Sprachen je nach dem praktischen Ziel, das man sich setzt, nach vielen äußeren und inneren Zügen bzw. "Kriterien" klassifizieren: nach äußeren wie denjenigen, die wir bezüglich der möglichen Charakterisierungen von Sprachen erwähnt haben, und nach inneren, im Grunde beliebigen. So kann man z.B. Sprachen mit Nominalgenus und Sprachen ohne Genus, Sprachen mit Artikeln und Sprachen ohne Artikel, Sprachen mit geschlossenen Silben und

2. *Cf* Humboldt (1827–9:189–90), insb. zum zweiten (sprachtypologischen) Argument: 'Es ist nur ein mehr und ein weniger, ein theilweis ähnlich und verschieden seyn, was die einzelnen [Sprachen] unterscheidet, und es sind nicht diese Eigenschaften, einzeln herausgehoben, sondern ihre Masse, ihre Verbindung, die Art dieser, worin ihr Charakter besteht, und zwar alle diese Dinge nur auf die individuelle Weise, die sich vollständig gar nicht in Begriffe fassen lässt. Denn bei allem Individuellen ist dies nur mit einem Verluste möglich, welcher gerade das Entscheidende hinwegnimmt.' (S. 190)

3. *Cf* z.B. Altmann & Lehfeldt (1973), ein nicht nur in dieser Hinsicht irreführendes Büchlein.

Sprachen ohne geschlossene Silben usw. unterscheiden. In diesem Sinne ist einerseits die Klassifikation eine empirisch unendliche, nur durch den jeweiligen praktischen Zweck bestimmte Aufgabe; und andererseits würde dabei jede Sprache – wenn man solche "Klassen" als "Sprachtypen" ansieht – zu sehr vielen und je nach den Klassifikationskriterien unterschiedlichen Sprachtypen gehören. Zweitens ist das Klassifizieren eine rein empirische Operation der Anwendung des schon anders Festgestellten, durch die man nicht mehr erfährt, als man durch die Sprachbeschreibung oder die Sprachgeschichte schon weiß; und in diesem Sinne wäre die Sprachtypologie, wenn sie mit der Klassifikation zusammenfiele, keine autonome sprachwissenschaftliche Disziplin, sondern nur Anwendung anderer Disziplinen. Drittens würde auch eine Klassifikation nach tatsächlich typologischen Einzelzügen (z.B. Flexion oder Agglutination) nur das Überwiegen dieser Züge in der Gestaltung der auf diese Weise klassifizierten Sprachen feststellen und nichts über die Zusammenhänge solcher Züge mit anderen Züge derselben Sprachen aussagen, die in Wirklichkeit völlig verschiedenen Sprachtypen entsprechen könnten. Denn auch ein tatsächlich typologischer Zug ist in sprachtypologischer Hinsicht nur durch sein Zusammenhängen mit anderen Zügen signifikant, und nicht, wenn isoliert betrachtet. So z.B. wurden alle romanischen Sprachen einschließlich des Französischen wegen des Überwiegens der periphrastischen Verfahren als "analytisch" klassifiziert. In Wirklichkeit aber ist der französische Sprachtypus ein völlig anderer als derjenige der übrigen romanischen Sprachen.

**1.2.4.** Vom Gesichtspunkt der Sprachtypologie im eigentlichen Sinne aus kann das Klassifizieren nur eine (eventuelle) nachträgliche Operation sein. Nachdem man einen Sprachtypus bei einer bestimmten Sprache identifiziert hat, kann man sich fragen, ob der gleiche oder ein ähnlicher Sprachtypus auch für andere Sprachen gilt, und in dieser Hinsicht typologische Klassen aufstellen. So ist es nicht sinnlos, von einem "romanischen" Sprachtypus zu sprechen, zumal die meisten romanischen Sprachen – mit Ausnahme des Französischen und in geringerem Maße des Okzitanischen – tatsächlich (fast) den gleichen Typus aufweisen. Ebenso kann man eine bemerkenswerte sprachtypologische Ähnlichkeit zwischen dem Deutschen und dem Altgriechischen feststellen; vgl. w.u.

In diesem Sinne aber ist die Klassifikation nicht Aufgabe, sondern nur eventuelle Anwendung der Sprachtypologie. Die Aufgabe der Sprachtypologie besteht darin, Sprachtypen zu identifizieren und zu beschreiben, und zwar zunächst und im Grunde den Sprachtypus einer jeden Sprache: Ob sich der gleiche oder zumindest ein ähnlicher Sprachtypus auch in anderen Sprachen feststellen läßt, kann nicht im voraus gesagt werden, und für die eigentliche Sprachtypologie ist es auch belanglos.

**2.1.** Die o.a. schon in der ursprünglichen Sprachtypologie enthaltene wichtige Intuition – eine Intuition, die allerdings gleich durch die Berücksichtigung mehrerer oder sogar "aller" Sprachen verdunkelt wurde und zur Typologie als Klassifikation führte – war zumindest in bezug auf einige sprachliche Verfahren diejenige einer strukturellen Einheit einer jeden Sprache, die über die Strukturierungsebene der Sprachsysteme hinausgeht. Denn es handelte sich von Anfang an nicht eigentlich um Einzelverfahren auf der Ebene des Sprachsystems, sondern um höhere Einheiten: um Verfahrenstypen. So umfaßt schon bei Adam Smith die periphrastische Verfahrensweise (die er *composition* nennt) den Gebrauch der Präpositionen für Kasusfunktionen und denjenigen der Hilfsverben, d.h. zwei auf der Ebene des Sprachsystems völlig verschiedene Verfahren; und A.W. Schlegel fügt den Artikel, den Gebrauch der Personalpronomina in der Konjugation und die periphrastische Steigerung der Adjektive hinzu. Ebenso umfaßt die "Flexion" von Anfang an die Konjugation, die Deklination und die "synthetische" Steigerung der Adjektive, d.h. wiederum ziemlich verschiedene einzelverfahren.

**2.2.1.** Erst von Humboldt wurde jedoch diese Intuition explizit gemacht und zugleich auf die Gesamtgestaltung einer jeden Sprache übertragen. Es handelt sich nämlich um eine der drei Humboldtschen Anwendungen des Begriffs "Form", und zwar um die Form als einheitliches *Gestaltungsprinzip* (bzw. als einheitliches Gefüge von Gestaltungsprinzipien) einer Sprache. Diese Idee wird von Humboldt vor allem in der berühmten Einleitung zum *Kawi-Werk* (d.h. Humboldt 1827–9) in verschiedener Weise formuliert und immer wieder betont. So schreibt er u.a.:

> Die charakteristische Form der Sprachen hängt an jedem einzelnen ihrer kleinsten Elemente; jedes wird durch sie, wie unmerklich es im Einzelnen sey, auf irgend eine Weise bestimmt. Dagegen ist es kaum möglich, Punkte aufzufinden, von denen sich behaupten liesse, dass sie an ihnen, einzel genommen, entscheidend haftete. Wenn man daher irgend eine gegebene Sprache durchgeht, so findet man Vieles, das man sich, dem Wesen ihrer Form unbeschadet, auch wohl anders denken könnte, und wird, um diese rein geschieden zu erblicken, zu dem Gesamteindruck zurückgewiesen. (S. 420).
>
> Es versteht sich indess von selbst, dass in den Begriff der Form der Sprachen keine Einzelheiten [sic] als isolirte Thatsache, sondern immer nur insofern aufgenommen werden darf, als sich eine Methode der Sprachbildung an ihr entdecken lässt. (S. 423).

**2.2.2.** Die Sprachform in diesem Sinne – die man heute wohl "Sprachtypus" nennen darf – ist also die ideelle Einheit einer Sprache; es geht dabei nicht etwa um ihre Einzelerscheinungen, sondern um das prinzipielle Zusammenhängen dieser Erscheinungen:

> Denn in jeder Sprache liegt eine solche... zusammenfassende Einheit... Dieselbe Einheit muss sich also in der Darstellung wiederfinden; und nur wenn man von den zerstreuten Elementen bis zu dieser Einheit hinaufsteigt, erhält man wahrhaft einen Begriff von der Sprache selbst, da man, ohne ein solches Verfahren, offenbar Gefahr läuft, nicht einmal jene Elemente in ihrem realen Zusammenhange zu verstehen. (ibid.)

**2.2.3.** Dieses Verhältnis der Einzelheiten zur Einheit, zum Prinzip bzw. zu den Prinzipien einer jeden Sprache gilt für Humboldt sowohl in synchronischer als auch in diachronischer Hinsicht. Das heißt, eine Sprache entwickle sich im Grunde nach den in ihr schon gegebenen Prinzipien: Neue Elemente werden von den Sprechern gemäß der in ihrer Sprache schon gegebenen "Form" geschaffen bzw. dieser Form angepaßt. Erst wenn die Prinzipien selbst der Sprachgestaltung anders werden, hat man es mit einer neuen Sprache zu tun (SS. 679, 548–549, 644).

**2.3.** Viel später taucht im Grunde dieselbe Auffassung von der einheitlichen Gestaltung einer jeden Sprache bei Georg von der Gabelentz wieder auf, und zwar ohne Bezug auf Humboldt, jedoch diesmal mit ausdrücklichem Hinweis auf die Sprachtypologie:

> Es scheint aber auch, als wären in der Sprachphysiognomie gewisse Züge entscheidender als andere. Diese Züge gälte es zu ermitteln; und dann müsste untersucht werden, welche andere Eigenthümlichkeiten regelmässig mit ihnen zusammentreffen. Ich denke an Eigenthümlichkeiten des Wort- und Satzbaues, an die Bevorzugung oder Verwahrlosung gewisser grammatischer Kategorien. Ich kann, ich muss mir aber auch denken, dass alles dies zugleich mit dem Lautwesen irgendwie in Wechselwirkung stehe. Die Induction, die ich hier verlange, dürfte ungeheuer schwierig sein; und wenn und soweit sie gelingen sollte, wird es scharfen philosophischen Nachdenkens bedürfen, um hinter der Gesetzlichkeit die Gesetze, die wirkenden Mächte zu erkennen. Aber welcher Gewinn wäre es auch, wenn wir einer Sprache auf den Kopf zusagen dürften: Du hast das und das Einzelmerkmal, folglich hast du die und die weiteren Eigenschaften, und den und den Gesammtcharakter! – wenn wir, wie es kühne Botaniker wohl versucht haben, aus dem Lindenblatte den Lindenbaum construiren könnten. Dürfte man ein ungeborenes Kind taufen, ich würde den Namen Typologie wählen. Hier sehe ich der allgemeinen Sprachwissenschaft eine Aufgabe gestellt, an deren Lösung sie sich schon mit ihren heutigen Mitteln wagen darf. Hier würde sie Früchte zeitigen, die jenen der sprachgeschichtlichen Forschung an Reife nicht nachstehen, an Erkenntnisswerthe sie wohl übertreffen sollten. (Gabelentz 1901:481).

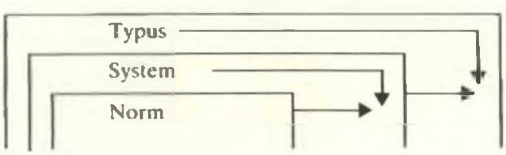
Auch bei Gabelentz handelt es sich offensichtlich um eine Einheit, die höher als diejenige der einzelnen Funktionen und Verfahren eines Sprachsystems liegt: um das durch "Gesetzlichkeit", d.h. durch Gestaltungsprinzipien motivierte Zusammenhängen der verschiedenen Bereiche der Sprachsysteme.

**3.1.** Wie ist dies alles in concreto zu verstehen? Humboldt formuliert die oben besprochene Idee der Sprachform immer wieder nur apodiktisch – es müsse eine solche Einheit einer jeden Sprache geben, es sei unmöglich, daß es sie nicht gibt usw. – ohne daß er sie jedoch auf historisch konkrete Fälle anwendet und faktisch wenigstens exemplarisch verdeutlicht. Und bei Gabelentz handelt es sich ausdrücklich um ein Desideratum.

Wir glauben der Humboldtschen Formidee und dem typologischen Desideratum von Gabelentz gerecht zu werden, wenn wir den Sprachtypus als eine Ebene der Sprachgestaltung, und zwar als die höchste Strukturebene einer Sprache aufstellen.

**3.2.1.** Eine Sprache ist eine historische Technik des Sprechens, ein Gefüge von inhaltlichen Funktionen und entsprechenden Ausdrucksverfahren. In dieser Technik kann man nun drei Strukturebenen unterscheiden, nämlich die Ebenen der *Sprachnorm*, des *Sprachsystems* und des *Sprachtypus*. Die Norm umfaßt die in der Sprache einer bestimmten Gemeinschaft historisch realisierte Technik, d.h. alles, was gemäß einer Technik des Sprechens tatsächlich geschaffen worden ist, was also als schon "Gemachtes" und Wiederholbares "existiert", und zwar unabhängig von dem Grade, in dem es auch funktionell ist: sie ist die Gesamtheit der in einer Sprache traditionellen Realisierungen. Zur Norm gehören deshalb alle "obligatorischen" oder auch nur üblichen Varianten, sowohl im Inhalt als auch im Ausdruck. Das System hingegen umfaßt alles, was in einer Sprachtechnik funktionell ("distinktiv") ist: die funktionellen Oppositionen und Verfahren, die eine Sprachstruktur im eigentlichen Sinne ausmachen; und es stellt daher die funktionellen Grenzen der Variabilität der betreffenden Sprache dar. Das System entspricht somit der Gesamtheit der in einer Sprache möglichen Realisierungen: es umfaßt auch das, was konkret noch nicht realisiert worden ist, jedoch virtuell in der Sprache schon existiert ("möglich" ist), d.h. was nach schon gegebenen funktionellen Regeln dieser Sprache gebildet werden kann. Der Sprachtypus schließlich umfaßt die Kategorien von inhaltlichen und materiellen Oppositionen, die Typen von Funktionen und Verfahren eines Sprachsystems, die funktionellen Prinzipien einer Sprachtechnik, und stellt somit die zwischen den einzelnen Teilen eines Sprachsystems feststellbare funktionelle Kohärenz dar. Der Typus enthält deshalb als Möglichkeit (als auf dieser Ebene virtuell existierend) auch Funktionen und Verfahren, die im Sprachsystem zwar noch nicht als solche gegeben sind, jedoch gemäß den gleichen funktionellen Prinzipien geschaffen werden können.

**3.2.2.** Was die sprachlichen "Fakten" betrifft, geht also das Sprachsystem über die Sprachnorm und der Typus über das System hinaus. Und in diesem Sinne ist jede Sprache eine offene ("dynamische"), d.h. teilweise realisierte und teilweise noch zu realisierende bzw. realisierbare Technik: das System ist System von Möglichkeiten hinsichtlich der Norm, der Typus ist es im Hinblick auf das System. Wenn wir das jeweils schon tatsächlich Existierende und das als Möglichkeit Gegebene zugleich berücksichtigen, so ist das Verhältnis zwischen der Norm, dem System und dem Typus einer Sprache folgendes:



Dies bedeutet, daß eine Weiterentwicklung der Norm einfach einer Anwendung des Systems entsprechen kann, und ebenso eine Weiterentwicklung des Systems einer Anwendung des Sprachtypus. Anders gesagt: Diachronie ("Wandel") der Norm bei Synchronie ("Funktionieren") des Systems, und Diachronie des Systems bei Synchronie des Sprachtypus.

**3.3.** So verstanden ist der Sprachtypus nicht etwa eine "Klasse" von Sprachen, sondern eine objektiv vorhandene sprachliche Struktur, eine f u n k t i o n e l l e Ebene einer Sprachtechnik, und er kann folglich in jeder Sprache, und zwar grundsätzlich jeweils in einer einzigen festgestellt werden. Und die Typologie ist demnach zunächst eine autonome Sektion der beschreibenden einzelsprachlichen Linguistik: sie ist die Untersuchung der Ebene des Sprachtypus einer jeden Sprache. Soweit es gerade um die Erfassung dieser Ebene geht, ist es deshalb ein Irrtum, wenn die Typologie als Feststellung von "Gemeinsamkeiten und Unterschieden" der Sprachsysteme angesehen wird. Dabei handelt es sich im Grunde um eine nicht zulässige Gleichsetzung oder Verwechslung der Sprachtypologie mit der konfrontativen Grammatik, die übrigens nicht selten zugleich auch mit der Universalienforschung gleichgesetzt bzw. verwechselt wird. Eine allgemeine konfrontative Grammatik ergibt in der Tat einerseits allgemeine (allen Sprachen gemeinsame) Züge, die deshalb als empirisch "universell" angesehen werden dürfen, und andererseits nur gewissen Sprachen gemeinsame und gegenüber anderen Sprachen differentielle Züge, die typologisch bedingt sein könnten. Diese Ergebnisse sind jedoch für die Typologie wie für die Universalienforschung nur Rohmaterialien, die im Rahmen dieser Disziplinen in völlig anderer Hinsicht als in der konfrontativen Grammatik als solcher interpretiert werden müssen. Es stimmt zwar, daß einem einzigen Sprachsystem als System von Möglichkeiten grundsätzlich verschiedene Normen und einem einzigen Sprachtypus verschiedene Systeme entsprechen können, so daß ein und derselbe Sprachtypus tatsächlich für mehrere Sprachen gelten kann. Man darf jedoch nicht die Ebene der Norm mit derjenigen des Systems noch die Ebene des Systems mit derjenigen des Typus gleichsetzen. Denn Fakten, die auf der Ebene der Norm ähnlich sind, können auf der Ebene des Systems verschieden sein; und auf der Ebene des Systems ähnliche Fakten können auf der Ebene des Typus einen völlig anderen Sinn haben. So z.B. kann die normale Aussprache von /f/ im Französischen und im Spanischen gleich sein, doch funktionieren frz. /f/ und span. /f/ in den entsprechenden Systemen in jeweils anderen Oppositionen. Ebenso war der typologische Status der periphrastischen Verfahren im Lateinischen anders als im Romanischen, und im Französischen ist der typologische Status derselben Verfahren anders als in den übrigen romanischen Sprachen.

**3.4.0.** Als Beispiele für das, was mit den zur Ebene des Sprachtypus gehörenden "Prinzipien" gemeint ist, kann man diejenigen anführen, die wir selbst in den romanischen Sprachen (Coseriu 1968b) und im Deutschen feststellen konnten.

**3.4.1.** In den romanischen Sprachen, mit Ausnahme des Französischen (und z.T. des Okzitanischen) gilt als allgemeines Kohärenzprinzip für völlig verschiedene Bereiche der entsprechenden Sprachsysteme (von der "Morphologie" bis zur Wort- und Satzbildung) der kategorielle Unterschied zwischen "inneren" (nicht relationellen) und "äußeren" (relationellen) Funktionen, und zwar in folgender Form: 'Innere (paradigmatische) materielle Bestimmungen für innere Funktionen, äußere (syntagmatische) materielle Bestimmungen – d.h. periphrastische Ausdrücke – für äußere Funktionen'. Funktionen wie Numerus, Genus oder diejenigen der einfachen Tempora des Verbs gehören zur ersten Gruppe, Funktionen wie Kasus, Steigerung oder Passiv hingegen zur zweiten. Das Lateinische kannte nämlich den kategoriellen Unterschied zwischen inneren und äußeren Funktionen nicht; so standen auch im Lateinischen die syntagmatischen Bestimmungen (wie im Falle von *magis idoneus, in schola, amatus sum*) nicht etwa den paradigmatischen gegenüber, sondern sie waren lediglich ein Zusatz und eine Ergänzung zu diesen (im Grunde für die Fälle, in denen der paradigmatische Ausdruck fehlte). Und im Französischen wurde der gleiche kategorielle Unterschied seit dem Mittelfranzösischen wieder aufgegeben, wenn auch, in materieller Hinsicht – beim Übergang zu einem anderen Sprachtypus – zugunsten der periphrastischen Verfahren. Dies wird auch durch die Geschichte dieser Sprachen bestätigt, d.h., daß sich der Sprachtypus in der Entwicklung der entsprechenden Sprachsysteme manifestiert. So haben die romanischen Sprachen, und zwar weitgehend unabhängig voneinander, den paradigmatischen Ausdruck von Numerus, Genus und Tempus beibehalten bzw. systematisch wiederhergestellt und ausgebaut, den paradigmatischen Ausdruck des Kasus und der Steigerung dagegen ebenso systematisch aufgegeben und abgebaut, was z.T. noch unter unseren Augen weitergeht (z.B. im Falle der Kasusformen der Personalpronomina). In ähnlicher Weise entspricht das romanische Passiv in funktioneller Hinsicht nicht dem lateinischen. Im Lateinischen drückte das sog. "Passiv" sowohl das eigentliche Passiv als auch das Unpersönliche (z.B. *dicitur*) und das Medium (z.B. *nominor*, 'ich heiße') aus. Das romanische materielle Passiv, das im ganzen periphrastisch ist, ist in typologischer Hinsicht nur für den Ausdruck einer relationellen Funktion, d.h. in diesem Fall des eigentlichen Passivs geeignet; und in der Tat drückt es auch nur dieses Passiv aus, wohingegen für die unpersönliche und die mediale Funktion andere Ausdrücke eingeführt wurden (*cf* z.B. it. *si dice, mi chiamo*). Die romanischen Sprachen sind nicht nur wegen ihrer gemeinsamen Grundlage und ihrer gegenseitigen Beeinflussung einander so ähnlich, sondern auch – und in gewisser Hinsicht sogar vor allem – deshalb, weil sie (mit Ausnahme des Französischen) weitgehend nach analogen technischen Prinzipien gestaltet wurden.

**3.4.2.** Für das Deutsche gilt u.a. als typologisches Prinzip die kontextuell-situationelle Bedingtheit, d.h. der Bezug des Gesagten auf den jeweiligen Kontext und die jeweilige Situation. In dieser Hinsicht hängen eben im Deutschen die sog. Partikeln ("Satzadverbien"), die Nominalkomposita und die präfigierten Verben kategoriell miteinander zusammen. Und ein ähnlicher kategorieller Zusammenhang galt auch im Altgriechischen.

**3.5.1.** Ob das für die romanischen Sprachen formulierte Prinzip das einzige typologische Prinzip dieser Sprachen ist, kann nicht im voraus und ohne weitere typologische Forschung gesagt werden. Es erklärt zwar sehr viele Fakten, jedoch freilich nicht alle Aspekte dieser Sprachsysteme. Und im Falle des Deutschen ist das Prinzip der kontextuell-situationellen Bedingtheit *ein* Prinzip, aber sicherlich nicht das einzige, und vielleicht nicht einmal das wesentlichste. Der Sprachtypus ist ein Forschungsfeld und nicht etwa ein einfaches Faktum, das auf einmal als solches wahrgenommen werden könnte. Auch wäre die Bemerkung, daß unsere Prinzipien synchronisch nicht in allen ähnlich gelagerten Fällen Anwendung finden, kein stichhaltiger Einwand gegen die hier vertretene Auffassung vom Sprachtypus und von der Sprachtypologie. Die Prinzipien sind nicht weniger solche, wenn sie nicht bzw. noch nicht in allen Fällen angewandt worden sind. Im Gegenteil: es gehört zu ihrem Wesen, daß sie in der Geschichte der Sprachen allmählich angewandt werden.

**3.5.2.** Ob andererseits für jede Sprache jeweils und zu jeder Zeit nur ein einheitliches Gefüge von funktionellen Prinzipien, d.h. nur ein einziger Sprachtypus gilt, muß ebenso dahingestellt bleiben. Es ist jedoch wahrscheinlich, daß man oft, wenn auch vielleicht nicht in allen Fällen, mit der Koexistenz von Sprachtypen in ein und derselben Sprache rechnen muß.

**3.5.3.** Ebensowenig kann man vom Gesichtspunkt unserer Auffassung vom Sprachtypus im voraus sagen, welches die Sprachtypen sein dürften oder ob sie bei den vielen Sprachen der Welt sehr zahlreich oder nur wenige sind, denn dies gehört eben zur Aufgabe der hier vorgeschlagenen Typologie. Aus der Tatsache, daß einem System verschiedene Normen und einem Sprachtypus verschiedene Sprachsysteme entsprechen können, kann man nur folgern, daß die Sprachtypen weniger zahlreich als die Sprachsysteme sein müssen, so wie die Sprachsysteme weniger zahlreich als die Sprachnormen sind. Es gibt aber Sprachsysteme, die in einer einzigen Norm realisiert werden, und so dürfte es auch Sprachtypen geben, die jeweils nur für eine einzige Sprache gelten.

**4.0.** Wie verhält es sich nun mit anderen Sprachtypologien gegenüber der Typologie als Untersuchung der funktionellen Ebene des Sprachtypus?

**4.1.1.** Bei Humboldt findet man außer dem schon gesehenen theoretischen Entwurf einer "eigentlichen" Sprachtypologie Ansätze zu zwei anderen Typologien, nämlich zu einer Typologie der "abstrakten" Ausdrucksverfahren und zu einer "partiellen" (nur auf wenigen Merkmalen bzw. nur auf einem einzigen für symptomatisch gehaltenen Merkmal fußenden) Typologie (Coseriu 1972:122–3,133). Die Typologie der Ausdrucksverfahren ist für Humboldt keine Typologie der Sprachen, sondern eben nur eine Typologie der Verfahren als solcher, und ergibt auch keine Klassifikation der Sprachen. Er bemerkt ausdrücklich, daß die Verfahrenstypen (Isolierung, Agglutinierung, Flexion und Einverleibung) grundsätzlich, wenn auch in verschiedenem Ausmaß, auch nebeneinander in ein und derselben Sprache vorkommen können: nur könne der eine oder der andere Verfahrenstyp in einigen oder in vielen Sprachen überwiegen. Auf die Möglichkeit einer partiellen Typologie ("Charakterisierung") bezieht sich dann Humboldt anläßlich einer kurzen Besprechung der "Partikel-Sprachen" (1827–9: 680–681), und nur diese Typologie stellt bei ihm auch einen Ansatz für eine Klassifikation der Sprachen dar, in dem Maße, in dem eine solche für ihn überhaupt zulässig ist.

**4.1.2.** Zu unserer Zeit, nach mehr als einem Jahrhundert typologischer Forschung, hat V. Skalička (1958) in einem grundlegenden und klärenden Beitrag fünf Arten von "Typologie" unterschieden:

1) die klassische "klassifikatorische" Typologie;
2) die "charakterisierende" Typologie, die man vielleicht besser "differentiell" nennen könnte (z.B. E. Lewy);
3) die Typologie der Einzelerscheinungen oder der einzelnen Bereiche der Sprache (Phonetik, Morphologie, Syntax, Wortbildung), d.h. in jedem Fall "partielle" Typologie (A. Isačenko, T. Milewski, V. Mathesius u.a.);
4) die "graduelle" Typologie, d.h. eine Kombination von verschiedenen partiellen Typologien, und zwar: a) nicht quantifizierend (E. Sapir); b) quantifizierend (J. H. Greenberg);
5) die Typologie der "bevorzugten Zusammenhänge" (d.h. seine eigene).

Letztere – sicherlich die bisher interessanteste und am besten fundierte Form der Sprachtypologie – ist im Grunde eine Erweiterung der Typologie der "abstrakten" Verfahren von W. v. Humboldt (obwohl Skalička sich nicht auf Humboldt bezieht und diesen sogar zur "klassifikatorischen" Typologie rechnet). Wie für Humboldt ist auch für Skalička diese Typologie keine Klassifikation der Sprachen, zumal in ein und derselben Sprache verschiedene Typen erscheinen können (und zu erscheinen pflegen). Außerdem nimmt Skalička einen Humboldtschen Ansatz auch darin wieder auf, daß er "bevorzugte" (und z.T. intern motivierte) Zusammenhänge der verschiedenen Verfahrenstypen aufstellt, wodurch er eben zu seinen fünf Idealtypen der äußeren Sprachgestaltung kommt: flexivisch, introflexivisch, agglutinierend, isolierend, polysynthetisch.

**4.2.0.** Der von Skalička vertretenen Einteilung der "Sprachtypologien" kann man im ganzen zustimmen, und man kann sie als Grundlage für die von uns beabsichtigte Gegenüberstellung annehmen.

**4.2.1.** Die klassifikatorische Typologie ist im Grunde keine Typologie im eigentlichen Sinne, sondern eben nur Klassifikation der Sprachen: sie stellt keine Sprachtypen als solche auf, sondern sie klassifiziert Sprachen, wenn auch freilich oft aufgrund typologischer Merkmale. Die partielle und die "differentielle" Sprachtypologie sind im Grunde Formen oder Anwendungen der konfrontativen Grammatik; d.h., sie können für die eigentliche Sprachtypologie allenfalls brauchbare Materialien liefern. Außerdem hat die differentielle Typologie auch als Charakterisierung der Sprachen nur einen relativen Wert, denn ein bestimmtes Merkmal – wenn nicht alle Sprachen der Welt berücksichtigt werden –

kann auch lediglich gegenüber bestimmten anderen Sprachen (z.B. innerhalb einer geographischen Sprachgruppe) "differentiell" sein. Auch die graduelle Typologie ist letzten Endes eine Form der konfrontativen Grammatik, wenn auch sicherlich eine viel komplexere. Auch sie stellt in Wirklichkeit keine Sprachtypen auf, sondern sie charakterisiert Sprachsysteme aufgrund von verschiedenen typologischen und nicht-typologischen Merkmalen. Als Charakterisierung von Sprachsystemen nähert sie sich jedoch der Typologie im eigentlichen Sinne, indem sie gewisse Zusammenhänge zwischen diesen Merkmalen zumindest empirisch (als faktisch gegebene positive oder negative "Implikationen") feststellt. Die Typologie Skaličkas schließlich steht sicherlich einer eigentlichen Sprachtypologie am nächsten, zumal sie auf dem wichtigen Kriterium der nicht nur empirisch festgestellten, sondern auch motivierten Zusammenhänge gründet. Sie ist aber keine Typologie der Sprachen als solcher, sondern ausdrücklich Typologie der "abstrakten" Verfahren, der exemplarischen Arten der (äußeren) Sprachgestaltung. Für die Untersuchung der funktionellen Ebene des Sprachtypus in verschiedenen Sprachen kann sie deshalb nur Ansätze, wenn auch manchmal sehr wichtige Ansätze, bieten.

**5.0.** Wir kommen zum Schluß zur Frage der Kriterien für die Aufstellung von Sprachtypologien. Diese Frage stellt sich nicht in gleichem Maße und in dem gleichen Sinn für die verschiedenen heute existierenden Typologien und für die hier vorgeschlagene.

**5.1.** Für alle bisher unternommenen Typologien sind die Kriterien "stofflich" oder "faktisch": es sind jeweils diese oder jene sprachlichen "Fakten". Diese Fakten stehen übrigens für die verschiedenen klassifikatorischen Typologien sowie für die graduelle Typologie und für die Typologie Skaličkas schon fest, und sie können nur im Rahmen dieser Typologien diskutiert und im Hinblick auf ihre jeweilige Zielsetzung angenommen oder abgelehnt werden. Für die partielle Sprachtypologie sind die Kriterien jeweils durch den dafür gewählten Sprachbereich bedingt; und für die differentielle Typologie durch die jeweils berücksichtigten Sprachen. Vom Gesichtspunkt der eigentlichen Sprachtypologie aus kann man sich deshalb nur wünschen, daß dabei nicht nur Merkmale auf der Ebene des Sprachsystems, sondern auch tatsächlich typologische Merkmale (d.h. Typen von Verfahren und insb. Typen von Funktionen) in Betracht gezogen werden, was übrigens bisweilen auch schon geschieht. Das gleiche gilt für die graduelle Typologie, soweit sie brauchbare Materialien für die Untersuchung der Sprachtypen liefern soll.

**5.2.1.** Hingegen kann man für die Typologie im eigentlichen Sinne keine "stofflichen" Kriterien, sondern nur ein formales angeben: es müssen funktionelle Prinzipien, d.h. kategorielle Zusammenhänge von Funktionen, von Verfahren oder, besser, von Funktionen *und* Verfahren gesucht werden. Man weiß also, *was für* Prinzipien man suchen muß, nicht aber, *welche* Prinzipien, denn diese müssen eben jeweils entdeckt werden, und für verschiedene Sprachen könnten auch völlig andere Prinzipien gelten.

**5.2.2.** Dazu kommen noch zwei negative Kriterien, ebenfalls formaler Art. Erstens ist das bloße gleichzeitige Vorhandensein (oder Nichtvorhandensein) dieser oder jener Merkmale an und für sich nicht typologisch relevant. Denn die typologische Relevanz ist nicht durch die bloße Kopräsenz, sondern durch den funktionellen Status der Merkmale und durch die Art, wie sie miteinander zusammenhängen, gegeben. Zweitens gilt in typologischer Hinsicht nicht die Bedingung, daß es sich um Merkmale handeln muß, die in analoger Weise in mehreren Sprachen vorkommen, denn diese Bedingung selbst beruht auf der Annahme, die Sprachtypen seien "Klassen" von Sprachen. Das gleichzeitige Vorhandensein oder Nichtvorhandensein (die empirischen positiven oder negativen Implikationen) bilden für die Typologie nur eine empirische Grundlage: diese Implikationen *könnten* typologisch motiviert sein. Ähnliches gilt für die Feststellung analoger empirischer Zusammenhänge in verschiedenen Sprachen, die für die Typologie im eigentlichen Sinne ebenfalls nur heuristischen Wert haben. Aufgrund einer solchen Feststellung kann man nämlich nur vermuten, daß diese Zusammenhänge typologisch bedingt sein könnten, eventuell auch, daß die betreffenden Sprachen typologisch zusammenhängen, d.h., daß sie entweder demselben Typus entsprechen oder wenigstens gewisse typologische Prinzipien (vielleicht in völlig anderen Zusammenhängen) gemeinsam haben. Ob dies stimmt oder nicht, muß jedoch durch die jeweilige einzelsprachliche Untersuchung nachgewiesen werden.

**6.** Wir haben uns erlaubt, unsere eigene Sprachtypologie, die Humboldtsche Ansätze sinnvoll weiterzuentwickeln bestrebt ist, als "Typologie im eigentlichen Sinne" darzustellen. Dies bedeutet natürlich nicht, daß man andere "Typologien" mit anderer Zielsetzung nicht betreiben sollte noch daß man sie nicht "Typologie" nennen darf. Doch müßte dann für die Untersuchung der funktionellen Ebene des Sprachtypus ein anderer Name gefunden werden. Dies, freilich, wenn man sie überhaupt betreiben will; wenn nicht, wird dieses für das Verständnis der sprachlichen Strukturierung sowie des Wesens der Sprachen als solcher äußerst wichtige Forschungsfeld wohl weiterhin wie bisher brachliegen müssen.

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## *Hansjakob Seiler:* Discussion

### Introduction

There are three orders or problems in which every linguist is constantly involved, no matter where he stands and what his particular topic is: The problems of language universals research [henceforth LUR], the problems of language typology [henceforth LTYP], and the problems of writing a grammar for an individual language [henceforth GRW]. A clear understanding of the problems and aims of LTYP can only be reached through a clear insight into the interrelation between these three kinds of linguistic activities. Professor Coseriu has warned us against confounding LTYP with contrastive grammar and/or with LUR. I agree with him. And I shold add that LTYP is not, as is still often maintained, a mere heuristic for LUR; nor does it consist of statements that simply fall short of being universally valid. A language is not an aggregate of properties where you can sort out those that are universal from those characterizing a particular group of languages and from those that are strictly idiosyncratic. LUR constitutes the basis for a typological comparison between languages; and GRW in turn is based on LTYP, for a grammar of an individual language is complete to the extent that it presents a language as a type.

My following contribution is mainly by way of illustrations, *per ostensionem,* so to speak. In Cologne, we have a Project on Language Universals and Typology. It will surely not be possible here to sum up what we have been doing so far. But I can indicate, by means of diagrams and comments, the direction in which we are going. In a first part I shall illustrate our views on LUR; in a second part I shall briefly indicate what we plan to do in LTYP. And I shall supplement the illustrations by a few general theses about the essential criteria in LTYP.

### An illustration of LUR: Individuation

Fig. 1 below is a graphic representation of a universally valid operational plan for fulfilling a particular function, *viz* that of capturing or representing an object – of thought or a physical object – through language in order to be able to predicate something about that object. This function emerged as the final product of our inquiries; we did not take it as a given; we gave this function the name of individuation. The contrast between such a conception and traditional views on language universals is salient: According to those still widespread views a universal of language is a structural property found in all languages. In contradistinction, the Cologne Project on Language Universals and Language Typology has proposed and substantiated a view according to which the properties which we find in various languages, the so-called 'data' or 'facts', are the end product of a mental process or plan that takes its primary motivation from certain well circumscribed problems or purposes, called functions,

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which must be solved in language communication. These functions are not immediately given to us, we cannot a priori decide what they should be. They must result from a research strategy that leads us from the 'given', from the sounds and morphemes and words, from the more thing-like over a series of intermediate steps – each being more operational and less thing-like than the preceding one – to inductively arriving at the functions. In going this way of induction we have already experienced the truth of G. von der Gabelentz' prediction as quoted in Professor Coseriu's paper (above, p. 000): 'Die Induction, die ich hier verlange, dürfte ungeheuer schwierig sein.' It is natural, however, that induction here must be duly supplemented by deduction. Our claim now is that what is truly invariant for all languages are the connections and relationships within such an operational plan as schematized in Fig. 1.

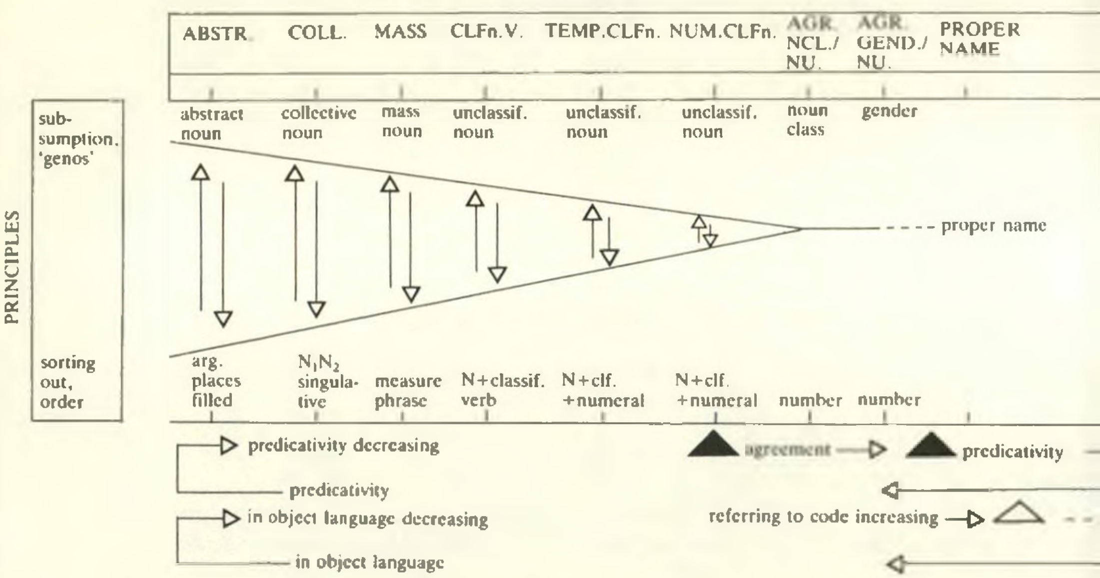


Fig. 1
*The Universal Dimension of Individuation*

On this plan, one will find categories and properties as they figure in the grammar of individual languages. They appear on the two horizontal lines connected by the vertical arrows: Abstract nouns, collectives, mass nouns; classificatory verbs as found, among others, in Athapascan languages, numeral classifiers of various sorts – some temporary, *ie* with semantic value and substitutable, some obligatory, *ie* desemanticized and not substitutable – as found in a vast area of languages around the Pacific; noun classes-number, as found chiefly in African languages; gender-number, as found in Afro-Asiatic and in Indo-European languages; and finally proper nouns. Some of them, like proper nouns, may be found in all languages, some others not. But this is not the question we ask. Rather, we note that these categories interact syntactically with other categories. Thus, the mass nouns interact with measure phrases, *eg milk – one glass of milk.* Abstract nouns being nominalized predicates on the basis of main verbs show interaction between an absolute use, *eg destruction is an activity,* and other uses where all or some argument positions are filled, *eg the destruction of Carthago by the Romans in the year 146 B. C.* The difference is one of genericity, the absolute use being highly generic, the relational use highly specific. And there are intermediate stages, too. Such interacting is symbolized by the converse vertical arrows. Now, we have on the one side the traditional categories like mass noun, etc – and on the other we systematize the view that they are only the elements used in an operation, which we have earlier called an interaction. This interaction is an instance of what we mean by the term technique. The interaction is syntactic, and it has predicative value or semanticity – *ie* it adds meaning to the assertion in which it occurs. Syntactic freedom and semanticity are maximal in the technique ABSTRACT and they decrease gradually, which is symbolized by the decreasing length of the arrows. This gradual decrease, for which we have empirical evidence, gives the program its directionality, and the techniques can thus be ordered correspondingly. Their names are given in the top frame in capital letters: ABSTRACTION, COLLECTION, MASS, CLASSIFICATION BY VERBS, TEMPORARY CLASSIFICATION, NUMERAL CLASSIFICATION, AGREEMENT OF NOUN CLASS AND NUMBER, AGREEMENT OF GENDER AND NUMBER, PROPER NAME.

The arrows on the bottom of the scheme suggest that the program can be run through according to two criteria; one has to do with predicativity (semanticity), the other with the question whether the predication is in the object language or refers to the code itself (meta-language). The arrows also suggest that the program should actually be in a circle and that it shows reversibility. This would mean, among other things, that ABSTRACT is the technique most closely akin to PROPER NAME. I cannot delve into the proplems of justifying such a claim at this point. The symbolism in the bottom part finally suggests that there are several turning points in this program, and hence, that gradience or continuum does not exclude, but rather presupposes, discreteness. If we leave the very complex problems connected with PROPER NAME aside for a moment, we have a turning point right at the placement of NUMERAL CLASSIFICATION. With this technique, semanticity is practically nil, because the numeral classifier does not predicate anything. Syntactic freedom is also nil, because in languages like Thai, a given noun is assigned one and only one classifier. And in the two following techniques we find, instead of syntax, a monomorphematic amalgam – and, as the decisive factor of these two techniques, agreement.

But even in these monomorphematic amalgams we find interaction between two aspects, one: the gender or noun class, the other: number. There is thus interaction in all the techniques of this program, and a common denominator can be found in the form of two converse principles, mentioned in the left-hand frame: class or 'genos' formation by subsumption, and ordering relation by sorting out individuals.

This then, as we suggest, is the universally valid plan – we call it a dimension – according to which the speakers of various languages solve the problem of capturing an object in order to say something about the object. It is a maximal program in the sense that every language, as far as we can see, shows some of the techniques, but no language shows all of them. In part they are even mutually exclusive. But whatever techniques a particular language shows, they will always, so the claim goes, combine the two basic principles, and they will always, relative to one another, show an ordering which conforms to the program.

Many points, of course, would need clarification – which I cannot give here for lack of time. Some points still remain controversial, and thus the proposal is to be considered preliminary. In its ultimate form, the dimension will enable us to explain numerous as yet poorly understood phenomena, including historical developments.

Among the phenomena for which we think we have an explanation on the basis of the dimensional approach are the following: 1. The degree of semanticity is an explanatory principle not only for the interrelation between the different techniques but also for a full understanding of the variations within one and the same technique. Example: The classificatory verbs. For one and the same verbal meaning, *eg* 'to lie', different verb stems are chosen according to the class to which a particular noun as subject (or object) of the verb belongs. Some languages showing this technique have a compulsory relation between a given noun and a verb stem. Other languages show greater freedom of substituting different verb stems in relation to a noun, each stem contributing something to the total meaning of the expression and thus showing greater semanticity. In an analogous way, we find different kinds of numeral classifiers: some that are rigidly tied up with a given noun and a numeral, others that are substitutable and show greater semanticity. Diachronically, we observe that languages tend to evolve in the direction from left to right, *ie* from greater to lesser semanticity – and also from syntagmatic to paradigmatic expressions. Thus, *eg* noun class systems of African languages may evolve into gender systems, while gender systems never turn into noun class systems. 2. The reversibility relationship between the two principles of 'genos'-formation and ordering relationship is another explanatory principle. It accounts among many other things for the fact that in the technique MASS we have 'genos'-formation by means of ordering relation: Any noun may appear as a 'mass' if I use it in a measure phrase, which, *per se*, has an ordering or separating or individualizing effect, *cf how much* (measure phrase) *automobile* (N appears as mass) *do we get*? Within the technique NUMERAL CLASSIFIERS we have an exactly reversed relationship: Here a 'genos'-forming device, *ie* the classifier, is used in order to make the noun countable, *ie* individualized, *eg* Thai

| | | | |
|---|---|---|---|
| wǽ æ n | nʏŋ | woŋ | 'one ring' |
| ring | 1 | circle | |
| N | Q | Cl | |

**An illustration of LTYP: Individuation**

I needed to give this whatever sketchy illustration of our project's work in LUR in order now to be able to illustrate our views on the essential criteria in LTYP. The project has only recently turned its efforts to LTYP, and even the illustrations I am now about to give have a symbolic value and make no claims of presenting any definitive solutions.

It is in keeping with our approach to LUR that we consider LTYP to be a linguistic activity that tries to reconstruct a circumscribed program which in successive steps leads from the functions as reconstructed in LUR on to the properties of particular language groups and individual languages. With such a typology program we want to describe something that has reality in the activities of language communication.

In our concrete example the problem may be formulated in the following terms: Given the circumscribed function of individuation, and given the ordered spectrum of techniques serving throughout the languages of the world to fulfill this function – how is the choice among these techniques made in a particular language? And are there groups of languages that show the same choice, *ie* that solve the problem by the same techniques? The formulation of the problem should still be made more precise: If choices are made, there must be conditions that determine the choice. These would then be the criteria for typology. One might use the construct of a decision tree to represent such a typology program. A somewhat similar construct has been proposed by E. Keenan, although for a different purpose, *viz* LUR.

Fig. 2 shows a proposed decision tree assigned to the function of individuation. One major choice is between syntagmatic expressions showing a certain freedom of combination and paradigmatic expressions being strictly obligatory. The distinction reminds us of Professor Coseriu's distinction between *'äußere'* and *'innere Bestimmungen'*. Since languages differ primarily by what they must and not by what they can express, the paradigmatic expressions seem to be more relevant for typology. This is in keeping with a wide-spread intuition according to which we speek of numeral classifier languages, of noun class and gender languages, but hardly of collective or mass languages. Among the obligatory expressions we have a choice between a full word in the case of numeral classifiers and a bound element in the case of noun class and gender languages. What would be the condition or criterion for such a choice? I can only answer this in vague terms: If the language is otherwise an isolating language, it has numeral classifiers as full words. But what does 'otherwise isolating' mean? Where else in the total language structure does

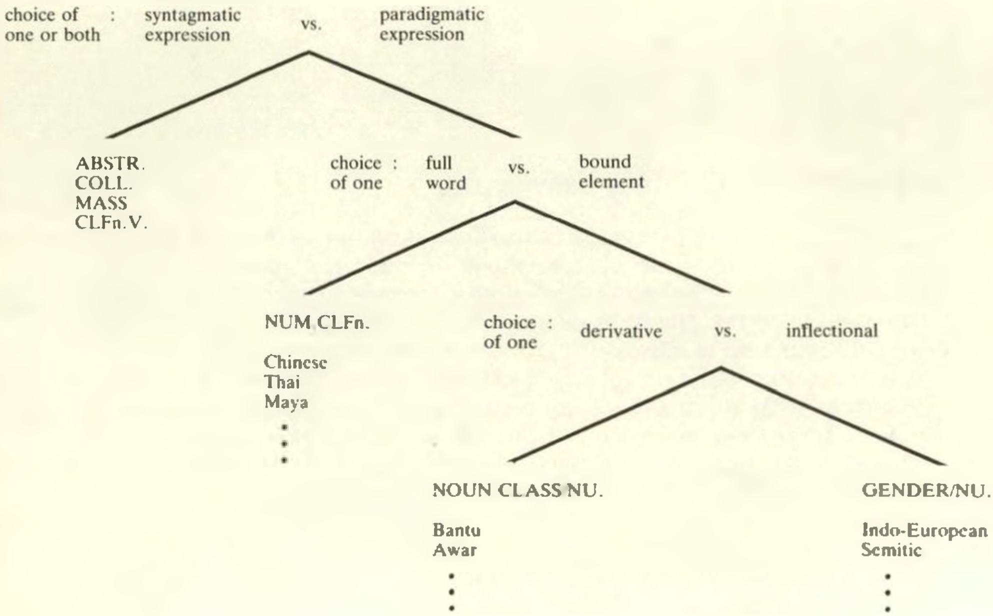


Fig. 2
*A Typology Subprogram for the Function of Individuation*

isolation occur – assuming for the moment that we do know how to determine isolation without falling into a vicious circle? And if isolation does occur elsewhere, how exactly is it connected with the phenomenon of numeral classifiers? The quest for the essential criteria of typology must base itself on the preferred connections between phenomena in groups of languages. Returning to our scheme we find a further choice for bound elements between derivation and inflection. Again we are faced with the problem of determining the criteria for the choices. And again we find the difficulty of exactly pinpointing the connections responsible for the conditioning. It would not suffice to say that inflective techniques are used because the language is heavily inflectional. We still lack the necessary empirical ground-work for giving more precise answers.

Let me come to a close by enumerating a few matters of principle centering around the problem of criteria in typology:

1. From among the total number of properties found in the languages of the world an indefinite number of typologies may be constructed, especially in the form of implicational statements such as: 'If a language has subject-verb inversion, it has oral vowels.' The statement is true, although not very many linguists will find it very enlightening. What we want to avoid, then, is the infinity of such, partly unreasonable, statements and resulting typologies; what we are looking for are precisely the

'essential criteria'. In a first approximation we observe the 'typological clusterings', as Greenberg has called them, the 'preferred connections' of V. Skalička's typological model, as highlighted by Professor Coseriu. In a second step, they must be correctly interpreted from a functional point of view.

2. An important heuristic for discovering and interpreting these connections consists precisely in the implicational statements proposed by Greenberg. However, I think it is misleading to call them universals. And, as we have seen above, implicational statements *per se* will not tell us the whole story. They must be supplemented by an insight into the functions of the respective phenomena. Take universal No. 41: If a language has SOV as the dominant order, it almost always has a case system. We do not know what this implication means for the total structure of a language unless we see the functional connection between the two sides of the implication. It will thus not do to consider just any correlations of phenomena. Even a quantitative mathematical approach will not improve the results.

3. LTYP is not coextensive with a classification of languages. But the class aspect is nevertheless important for LTYP. It shows us that, in a program for constructing a language, the choices are available, the decisions are made and the conditioning factors are followed, not just for a single language but for groups of languages. One important aspect of these programs – once we know them in more detail – will surely be that they are hierarchically structured according to a hierarchy of functions. The number of classes will differ according to the hierarchical level considered: Decision steps pertaining to superordinated functions will be fewer, and the classes of languages that follow the same decisions will be fewer in number and richer in membership.

4. The ultimate claim of typology would be that it uncovers operations that have reality in the communication process. This would then account for translatability and learnability of foreign languages. It is a strong claim, but I hope it will motivate us in a kind of research for which we still have long ways to go.

## *John M. Anderson:* Discussion

What follows is in part a plea for taxonomy.* For it seems to me that it is precisely the role of typology, as I see it, to establish a classification: only secondarily of language systems and sub-systems;[1] primarily of language properties. Typology provides a categorisation of those linguistic properties that are necessary, preferred or merely possible, together with a characterisation of the relations between them. In saying this I don't think I'm being merely perverse. The viability of such a field is in principle clear, such that it makes independent predictions concerning the status of different properties, and it remains distinct both from genetic, areal and social classifications and from contrastive studies. The former associate the shared and distinguishing properties of different languages with extra-linguistic dimensions; the latter are concerned with highlighting the differences, whatever their typological status, between particular pairs, triples etc. of languages. And it is just such a classificatory view which informs both the wide and fruitful range of typological studies now in progress – as evidenced *eg* in the recent Greenberg (1978), Li (1978) and Lehmann (1978) volumes – and to some extent the suggestions in this area made by Hjelmslev, though that is perhaps more controversial.

On the other hand, I remain somewhat agnostic as to the necessity or even the possibility of establishing for each language a distinct typological level. We need some very strong evidence for us to contemplate such a multiplication of entities. At the very least various crucial basic questions need to be satisfactorily answered. How, precisely, are the levels of system and type articulated distinctly, and how related to each other? Do properties of type and properties of system differ in some systematic way? What specific predictions are offered by the positing of a level of type? Does, for example, type limit evolution in any demonstrable way? What evidence is there that change of type is different in kind from

* In terms of anything but an absurdly primitive ethical system, nobody is responsible for the contents of this paper; but I wish to explicitly absolve Henning Andersen, Torben Andersen, Eugenio Coseriu, Niels Ege, Mike Fortescue, Sidney Lamb, and Lachlan Mackenzie, to whom I am nevertheless grateful for their comments made during the Rask-Hjelmslev Symposium; also Mauricio Brito de Carvalho, who helped me revise this version, and Geoff Pullum, whose ideas, published and unpublished, I have blithely appropriated. None of them will agree, I'm sure, with everything, or perhaps anything, said here; moreover, in this version it has not been possible to take account of all the comments received. But I trust that they (and others) will continue to tell me where I've got it wrong (and maybe occasionally right): our primitive ethical system has that benefit.

1. To the extent that it is possible to classify linguistic properties in terms of a taxonomy like that outlined here, a classification of language systems also emerges, the more distinctly if clusters of implications between properties can be established. Further, it may be possible to further hierarchise properties with respect to their intrinsic significance for classification: this will partly depend on the range of other properties with which they enter into implicational relationships, but also perhaps on *eg* the inherent scope of the property itself. Having an inflexional morphology or not, and having articles or not (to take a couple of examples discussed below) are probably more far-reaching both in consequences for the system and in their own scope than, say, possession or lack of dual number.

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change in system? If, for instance, we associate those mysterious global diachronic tendencies that are often labelled 'drift' with the properties of a level of type, then these properties and their connexion with the language system still need to be spelled out explicitly, and their existence and appropriateness given independent motivation; otherwise, we are simply labelling in like manner disparate aspects of the system and its evolution.

Finally, how are we to counter the scepticism that might suggest that a level of type is necessary only to theory which fails to offer an adequate framework for the characterisation of the system? For instance, the presence in a language of inflexional modification with verbs, nouns and adjectives (rather than total reliance on 'analytic' devices) is characteristic of a language whose grammar contains a particular kind of morphological component. The characterisation of this does not seem to require the positing of a distinct typological level of description for each language. Saying that a language has an inflexional morphology is simply a more general statement than one attributing, say, articles to it. Vietnamese lacks a particular subcomponent of rules (that of inflexional morphology), whereas Czech lacks the more restricted apparatus of article-formation. Of course, either of these properties – having a morphology and having articles – may interact with – depend on or predict – other properties. There are correlations between different parts of the system: Keenan (1978:297), for example, suggests a partial correlation between possession of articles and word order type. But such correlations can only be established on the basis of a typological taxonomy of properties and their inter-connexions; the ascription of a type to a language presupposes a taxonomy erected on a cross-linguistic basis. And these correlations, and the taxonomy they manifest, are not part of the descriptions of particular languages but of linguistic theory.

Let's now look at what seem to me some of the benefits, as well as at some of the problems of establishing a typological taxonomy – the problems being such as go some way to explaining the primitive state of typological studies. As to types of property, we can in the first instance distinguish between NECESSARY properties of any language, *ie* universals proper, and properties that are merely POSSIBLE but which may be absent from particular languages. Within the latter we gan go on to differentiate between those properties that are SIGNIFICANT and those which are simply LEGAL, as compatible with the content of linguistic theory but not embodying a distinct generalisation. Finally, there are properties that are strictly IDIOSYNCRATIC, whose recurrence, if any, is coincidental. Let me try to illustrate these in turn, though we shall as we go along digress to introduce distinctions along other dimensions.

One might hypothesize, for instance, the universality of the argument-predicate construction in syntax. More specifically, given this, universals of argument-type can be proposed: these might be, for example, the case relations of case grammar. At the opposite pole lie properties which are specific to a particular language, which are not predicted by a general law. Most obviously, of course, lexicalisation, the minimal sign relation itself, is typically arbitrary. We can conceive of a restrictive typological hypothesis as seeking to limit such properties to the minimum; a strong, highly predictive typology would restrict the idiosyncratic to this area of lexical realisation. I think this is not too far from Hjelmslev's view. Genetic relations, on the other hand, according to Hjelmslev (1970:21–2) – though not expressed in these terms – essentially involve generalisations concerning lexical realisations: the pairwise constant correspondence between sign expressions that he designates the 'expression element function'. Typological relations involve rather the 'category function' (p.95), relating the categories of different languages on either the content or the expression plane.

There are other non-idiosyncratic properties that are not necessary, merely varyingly general. And amongst these there are some that warrant the designation significant rather than merely possible or legal. By this I'm not simply referring to how common the property is in the languages of the world. (We return to a brief consideration of this question below.) Let me try to illustrate what I do mean in relation to the phenomenon of grammatical agreement or concord, specifically verb agreement. By verb agreement I simply mean variation in the form of a verb in agreement with the semantico-syntactic class of an argument. The argument may be absent superficially. But the presence of the argument must be compatible with that of the alleged agreement; otherwise, we have a pronominal clitic, say, rather than a marker of agreement.

Verb agreement with an argument is merely possible, legal: particular predications and particular languages may lack it. A predicate may have associated with it, *via* agreement, a number of different properties of an argument. If *any* property is available for agreement, then no significant generalisation is available; if, however, agreement is limited to semantic or grammaticalised subclasses of nouns, or perhaps even some subset of these, then a generalisation concerning concord properties has to that extent some significance. Similarly, if the argument-type that triggers concord is restricted in some way, so that not all a priori possible triggers are allowed, then triggerhood for agreement may be significant.

Consider the latter aspect in a little more detail: this will bring us to a further distinction. Concord on a predicate may be triggered by a number of arguments simultaneously, by only one or by none at all. There is a restriction on triggers that is valid for a large number of languages, which I give as (1):

(1) VERB AGREEMENT GENERALISATION: In a predication showing verb-agreement, only terms of grammatical relations (GRs), NPs bearing the GRs subject, direct object and indirect object, trigger agreement; and then in accordance with the hierarchy of GRs, such that indirect object agreement implies direct object agreement implies subject agreement.

That is, for instance, agreement triggered by a direct object is excluded if the subject does not also trigger concord in the same sentence; and agreement triggered by a non-GR-bearing argument is excluded.

This is a very restrictive generalisation; in form, it constitutes a couple of linked IMPLICATIONAL universals: concord is only with terms, and concord implies concord with subject. Even though concord itself is not a necessary property of a language, the existence of concord in a sentence necessarily involves subject concord. This introduces another dimension of our typology of properties and their relations: as well as recognising necessary or invariant properties, we should also allow for the possibility of invariant relationships between non-universal properties, such as are familiar from the work of Greenberg. Hjelmslev captured this same distinction (1948:§ 1.3) in terms of a difference between 'propositions nécessaires' concerning language *vs* 'propositions générales', the latter being valid only for a language of a certain specified structure. Thus, (1) is true only of languages which show verb agreement.

It is at this point, too, that other distinctions made by Hjelmslev are relevant. For implications can be either UNIDIRECTIONAL, *ie* Hjelmslevian 'determinations', or BIDIRECTIONAL, Hjelmslev's 'interdependencies'. And implications, as well as universal properties, may comprise either SYNTAGMATIC or PARADIGMATIC relations, relations in either 'process' or 'system' (*cf* Hjelmslev 1963:23–5). However, rather than developing these already familiar notions further here, let us proceed with our consideration of generalisation (1). Let us merely note that almost all the implications currently under debate are paradigmatic (or at least formulated as such), given the subordination of 'process' to 'system'.

(1) is quite compatible with the existence of only subject-triggered concord in a large number of languages, as well as with, *eg*, concord with both direct object and subject in transitive sentences in languages such as Hungarian. And so on. However, it is infringed in various ways by a number of languages. These infringements cast some interesting light on the range of distinctions that must be recognised by any typological taxonomy. (1) is the particularly strong kind of prediction we can designate as PREDICATION-BOUND (or 'process-bound'). The second part, for example, requires that for a predication that shows object-agreement there must also be subject-agreement in that same predication. This prediction is falsified by languages like Palauan.

Consider the sentences of Palauan given in (2) (from Josephs 1975):

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| (2) | (a) | a | Droteo | a | lilęchęs–ii | a | babier |
| | | P | Droteo | P | 'wrote'–3rd+nh+sg | P | 'the letter' |
| | | ('Droteo wrote the letter') | | | | | |
| | (b) | a | Droteo | a | lilęchęs–Ø | a | babier |
| | | P | Droteo | P | 'wrote'–3rd+nh+pl | P | 'the letter' |
| | | ('Droteo wrote the letters') | | | | | |

(3rd = third person; nh = non-human; sg= singular; the particle glossed with 'P' is apparently a marker of the initiation of a major constituent that is specific). The verbs in (2) are perfective; there is no agreement in the case of the corresponding non-perfective. These perfective verbs show agreement with their objects; in this instance, the only indication of the number of the object is in the form of the agreement suffix on the verb. There is no subject agreement.

However, a rather weaker version of (1) can be maintained even for Palauan and similar languages, if we allow the formulation to refer to a language as a whole rather than to particular predications in a language. Let us refer to this as (1'):

(1') VERB AGREEMENT GENERALISATION: In a language showing verb-agreement ... (as in (1)).

That is, the second part of the generalisation (in particular) is satisfied if in a language some of whose sentences show object-agreement there are also sentences which have subject-concord. This weaker, LANGUAGE-BOUND (or 'system-bound') formulation is in fact satisfied by Palauan and the other languages I know of which infringe the predication-bound requirement (1). In certain constructions in Palauan we do indeed find subject-triggered agreement; compare the sentences in (3) (again from Josephs 1975):

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| (3) | (a) | a | Droteo | a | męnguiu | | a | hong |
| | | P | Droteo | P | 'be reading' | | P | 'the books' |
| | | ('Droteo is reading the books') | | | | | | |
| | (b) | a | Toki | a | diak | longuiu | a | hong |
| | | P | Toki | P | 'not' | 3rd–'be reading' | P | 'the books' |
| | | ('Toki is not reading the books') | | | | | | |

In the positive sentence (3) (a) there is no subject-agreement. (Since the verb is also non-perfective, there is no object-agreement either.) However, in the presence of the negative *diak* we find on the verb an agreement prefix whose presence is triggered by the third person subject; a first person subject would require the verb form *kunguiu*.[2]

Versions of 'relational grammar' I am familiar with incorporate a language-bound 'agreement law' of the character of (1') (*cf eg* Johnson 1977). However, even this is too strong; specifically, in relation to the basic generalisation incorporated in the first part of (1). For there does not appear to be a universal correlation between concord and GRs: the first part of (1) is also too strong. Consider in this respect the control of concord in Amharic.

Sentences of Amharic such as those in (4) are apparently unproblematical:

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| (4) | (a) | Almaz | bet– | u– | n | bə– | mətrəgiya– | w | tərrəg– | əcc |
| | | Almaz | 'house'– | 'the'– | acc | 'with'– | 'broom'– | 'the' | 'clean'– | 3rd+f |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| (b) | bet– | u– | n | Almaz | bə– | mətrəgiya– | w | tərrəg– | əcc– | iw |
| | 'house'– | 'the'– | acc | Almaz | 'with'– | 'broom'– | 'the' | 'clean'– | 3rd+f– | 3rd+m |
| | ('Almaz cleans the house with the broom') | | | | | | | | | |

(Haile 1971:102; f= feminine; m = masculine). Both sentences show subject-triggered concord, and this is a general property of Amharic sentences. (4) (b) also has a second concord suffix, in this instance trig-

2. Sidney Lamb has pointed out to me that at least the predication-bound agreement generalisation (1) is also violated by Uto-Aztecan languages like Monachi: in main clauses (at least) we find only object agreement. And (1) is also, and systematically, violated by languages whose concord triggers are determined on an 'ergative' basis – if one insists on interpreting such systems in terms of subject, direct object and so on. Consider the sentences from Avar given in (i) (Tchekhoff 1972: § 2.6):

| | | | | | | |
|---|---|---|---|---|---|---|
| (i) | (a) | dica | dir | emén | v– | atula |
| | | 'I'+instr | 'I'+gen | 'father' | m– | 'find' |
| | | ('I find my father') | | | | |
| | (b) | dica | dir | ebél | y– | atula |
| | | 'I'+instr | 'I'+gen | 'mother' | f– | 'find' |
| | | ('I find my mother') | | | | |

and (ii):

| | | | | | |
|---|---|---|---|---|---|
| (ii) | (a) | dir | emén | v– | ac'ula |
| | | 'I'+gen | 'father' | m– | 'come' |
| | | ('My father comes') | | | |
| | (b) | dir | ebél | y– | ac'ula |
| | | 'I'+gen | 'mother' | f– | 'come' |
| | | ('My mother comes') | | | |

Here, agreement is with the class (m(asculine) or f(eminine)) of the argument that is translated as a direct object in a language like English in the case of sentences like (i); whereas in (ii) it is the 'subject' that triggers agreement. (For some comparable examples from other languages, see *eg* Moravcsik 1978: § 2.2). Now, it may be that subjecthood is relevant to other aspects of the syntax of Avar, but the systematic violation of generalisation (1) by its markers of concord suggests that the appropriate specification of triggers in Avar is simply not in terms of GRs. In that case neither (1) nor (1') is relevant as such. Unless subjects and absolutives (the argument relevant to the triggering of concord in Avar and the like) can be related to some more general common notion, and (1) etc. reformulated accordingly, then (1) etc. are simply possible implications (members of a disjunctive set of implicata). See further note 3.

Notice finally here that there have also been a number of claims that there are languages which violate (1) and perhaps even (1') by virtue of indirect objects taking precedence as triggers over direct (*cf eg* Keenan 1976a, 1978). However, this (and particularly the latter – *ie* violation of (1')) is difficult to establish firmly in the absence of an adequate characterisation of this particular relational distinction, and without detailed investigation of the languages concerned in this particular respect. In his description of Mezquital Otomi, for instance, Hess (1968) states that 'Transitive verbs have prefixes indicating aspect and subject, and suffixes indicating indirect object' (p.20). But since Hess does not provide a morphological analysis, and given the uncertain status of 'indirect object', this statement of his and the significance of his examples are difficult to evaluate. Crucial examples of arguments labelled IO (indirect object) by Hess have arguably undergone dative movement (whereby in terms of RG the initial IO becomes direct object) even if they are initial IOs (see the examples in ch.IV, §§ 2.2 and 2.4). And it is not clear that direct object agreement is in fact excluded elsewhere. In 'indefinite subject' sentences agreement is triggered by direct objects (though such sentences are arguably passive). And consider the still less equivocal (iii):

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| (iii) | bimɨki | ra | xą'í | de | ma | hai | ndómingo | ra | pa | 'ą |
| | 'he–grabbed–me' | 'the' | 'people' | 'of' | 'my' | 'country' | 'sunday' | 'the' | 'day' | 'it' |
| | ('My countrymen seized me on a Sunday') | | | | | | | | | |

(example and gloss from Hess 1968:85).

gered by the direct object, which is masculine. However, the specification of what can be a trigger for this second concord suffix does not in fact crucially involve GRs. Notice, for instance, that as well as the sentences in (4) we also find such as (5):

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| (5) | bə– | mətrəgiya– | w | Almaz | bet– | u– | n | tərrəg– | əcc– | ibbət |
| | 'with'– | 'broom'– | 'the' | Almaz | 'house'– | 'the'– | acc | 'clean'– | 3rd+f– | 3rd+m |

with the same cognitive content, in which the trigger is an oblique argument. Again, as in (4) (b), the argument that controls the second concord occupies initial position in the unmarked instance.

These three Amharic sentences differ in the assignment of topichood (as is discussed in some detail in Haile 1971): control of the second concord is exercised by a topic which is not the subject (*ie* controller of the first concord). This means that (1) and (1') cannot be maintained as a universal; triggering of concord is not limited to NPs bearing a GR, though most concord triggers will bear some GR (rather than being oblique). We can allow for the Amharic situation by weakening (1') to (1''), say, such that:

(1'') VERB AGREEMENT GENERALISATION: In a language showing verb agreement, at least one concord will be triggered by terms only; ... (as in (1)).

This can be maintained as an implicational universal, with respect to this range of evidence. And we can add (6):

(6) VERB AGREEMENT GENERALISATION 2: A second verb agreement will be topic-triggered if it is not triggered by terms

as a subsidiary implicational universal. (1'') and (6) together are weaker than (1) or (1'), but they retain implicational universality for the specification of triggerhood.

There is, however, some evidence, though it is controversial, that neither of the two concords of Swahili involves a trigger essentially specified as term of a GR, but rather notions to do with 'topic' and 'focus' (Maw 1976; Anderson MS). If this is so, then even (1'') cannot be maintained, and triggering of agreement specified in terms of GRs is merely possible rather than a universal property. (6) can in these circumstances be made more general by omission of the restriction to 'second'. But this in itself is less restrictive. At best we can establish a disjunctive implication concerning control of verb agreement:

(1''') VERB AGREEMENT GENERALISATION: In a language showing verb agreement, the trigger of each agreement is a term of either a grammatical or a communicative relation ...

If, on the other hand, we can arrive at a grammatical characterisation whereby, on independent grounds, subject and topic can be classed together, as manifestations of some more general notion, then a more restrictive universal might after all be extracted from a combination of (1") and (6) (Anderson MS).[3] The concord phenomena, I suggest, illustrate rather nicely the complex interaction of hypothesis building and empirical investigation that must underlie the development of a testable typology.

Even in the event that there is no agreement universal, it nevertheless seems to be possible at the very least to impose significant constraints on the range of possible agreement triggers. I want now to turn to some other phenomena where what is at issue is rather whether some possible property is even significant or not. This will also lead on to a further kind of distinction that must be allowed for by our typology.

If we consider predications that contain both a subject and an object, then these two arguments and the verb may appear, if combination is unrestricted, in six possible orders relative to one another, *viz* those in

(7) SOV, SVO, VSO, VOS, OVS, OSV

All of these are attested in some sentences in some language or other. Thus each order is simply a possible one for any arbitrary language: all are legal, given S, O and V. Such word order variants become significant if, say, it can be shown that certain word orders may or may not be found in the same language. That is, rather than all possible orders co-occurring (the presence of each order is a possible concomitant of the presence of every other: they contract the relation of 'constellation' (Hjelmslev 1963:23–5) unrestrictedly), certain combinations are required or disfavoured. Thus Greenberg (1963: Universal 6) found that 'All languages with dominant VSO order have SVO as an alternative or as the only alternative basic order'. This constitutes an implicational universal, as does the claim made by Schwarz (1971:160) that languages in which SOV is the predominant order lack OVS as an alternative. In the context of this latter restriction, other, permissible combinations – say, predominant SVO and OVS – are significant. Unfortunately, this particular hypothesis – concerning the incompatibility of OVS and predominant SOV – is falsified by the syntax of such languages as Wichita (Pullum 1977:268–9) and Galibi (Derbyshire and Pullum to appear:§ 3). The notion of predominant order brings us, however, to a further respect in

3. Elsewhere (1979a) I have proposed a notion PRINCIPAL RELATION, such that absolutive is the intial principal and the cyclic principal in a non-subject-forming sub-system; subject is the cyclic principal in a sub-system showing GRs; topic is postcyclic principal. As principals they are pre-supposed by other relations at the same level: absolutive is the only obligatory initial relation (or CR); a sentence containing a direct object necessarily contains a subject cycle-finally; topic (and its variant focus) constitutes the principal post-cyclic relation as the only one positively specified (in the absence of topic there is no topicalisation structure). If the notion 'principal relation' is well-founded, then we can formulate an implicational universal concerning verb-agreement such that triggers are restricted to arguments that at some stage contract the principal relation. (The direct object in a subject-forming subsystem is an absolutive denied subject position by an ergative; indirect objects are embedded ergatives and/or subjects – see Anderson 1977, 1978, 1979c.)

which word order might, in principle, be significant.

For many languages, at least, it is possible to establish what Pullum (1977) terms a BASIC WORD ORDER; *ie* that found in simple declarative sentences that are unmarked stylistically and in terms of discourse. Thus, English is in these terms basically SVO, Japanese is SOV, Welsh is VSO. Basic word order is typologically significant if we find to be basic only some of the six possibilities – or seven, if we allow for 'free word order' as a (null) possibility. Various claims have indeed been made in this connexion. Vennemann (1973), for instance, assumed on the basis of Greenberg's work that only the three orders in (8) occur as basic:

(8) SOV, SVO, VSO

*ie* those with subject before object. Pullum (1977) added a fourth, *viz* VOS:

(9) SOV, SVO, VSO, VOS

exemplified by Malagasy. The basic order for transitives and intransitives in Malagasy is illustrated in (10) (from Keenan 1976a):

(10) (a) manasa ny lamba Rasoa
'wash' 'the' 'clothes' Rasoa
('Rasoa washes the clothes')
(b) lasa ny mpianata
'have gone' 'the' 'students'
('The students have gone')

(On the syntax of VOS languages, see further Pullum 1977:254–9; Keenan 1978.) But Pullum continued to maintain that the other two 'do not occur at all' (1977:269). Greenberg himself was rather careful to avoid committing himself on the status of the three orders not included in (8); he suggested merely that they 'do not occur at all, or at least are excessively rare' (1963:61). In fact it now seems reasonably clear that they are rare rather than non-occurrent (largely as a consequence of work in which Pullum has played an important part). VOS is by now rather well attested.[4] And recent work of Derbyshire and Pullum (Derbyshire

4. One possible objection to Keenan's discussion is that little argumentation (except in the case of Malagasy – see Keenan 1976b) has been offered in favour of interpreting the relevant sequences in these languages as necessarily GR-based, rather than involving, say, V-abs-erg and V-abs as opposed to VOS and VS. This would mean that, as in the majority word order types, the argument bearing the principal relation (subject or, as here, absolutive) is leftmost. The absence of nominal morphology leaves the status of these sequences in doubt in this respect, as does, in a number of instances, the absence or, alternatively, comprehensiveness of verb-agreement indices. Of course, this ergative possibility is removed in principle if ergative syntax (including word order) must be associated with ergative morphology (Dixon 1979). But given the paucity of serious discussions of syntactic ergativity, the viability of this generalisation remains doubtful. Notice, for instance, that ergativity in derivational processes, at least, is characteristic of languages (like English) which lack an ergative inflexional morphology. Moravcsik (1978) and Comrie (1978: § 7.4.3), for example, discuss such formations as those in *-ee* in English which are associated with absolutives: *employee, escapee*. And Moravcsik (1978: § 2.3) provides some specific evidence that word order patterns may be determined ergatively in a language (Chinese) which lacks ergative morphology. This suggests at least that the possibility of ergativity should be investigated in the languages cited by Keenan.

1977; Derbyshire and Pullum to appear) has revealed a number of plausible candidates for basic OVS status and a rather smaller number of OSV to add to the VOS languages described by Keenan (1978).

(11) are sentences illustrating the basic order for Hixkaryana, a Carib language of Brazil:

| | | | | |
|---|---|---|---|---|
| (11) | (a) | kana | yanimno | biryekomo |
| | | 'fish' | 'he caught it' | 'boy' |
| | | ('The boy caught a fish') | | |
| | (b) | manhotxowi | hawana | komo |
| | | 'they danced' | 'visitor' | collective |
| | | ('The visitors danced') | | |

(Derbyshire and Pullum to appear: § 1.1). This order, (O)VS, is not only statistically dominant but also arguably basic on syntactic grounds. The only significant alternative order is SOV, *ie* with S in initial position. But other elements than S can appear in this position, and their occupation of it is mutually exclusive with S being preverbal: *ie* neither XS(O)V or SX(O)V is possible. This suggests that all these elements, including subjects, are basically post-verbal, and fronting is limited to one sentence element (*cf* Derbyshire 1977; Derbyshire and Pullum to appear: § 1.1).[5]

Attested OSV languages are even fewer; and a number of claimed examples have proved to be not such (*cf* again Pullum 1977; Derbyshire and Pullum to appear). Derbyshire and Pullum do, however, discuss several plausible instances, including Apurinã, an Arawakan language of Brazil, illustrated by (12) (a):

| | | | | | | |
|---|---|---|---|---|---|---|
| (12) | (a) | anana | nota | apa | | |
| | | 'pineapple' | 'I' | 'fetch' | | |
| | (b) | anana | n– | apa | nota | |
| | | 'pineapple' | 'I'– | 'fetch' | 'I' | |
| | (c) | nota | apa– | ry | anana | |
| | | 'I' | 'fetch'– | 'it' | 'pineapple' | |
| | (d) | n– | apa– | ry | anana | nota |
| | | 'I'– | 'fetch'– | 'it' | 'pineapple' | 'I' |

(12) (b)-(d) illustrate that any deviation from the basic OSV pattern of (12) (a) necessitates the introduction of an affix in concord with the displaced element: the VOS example in (12) (d) has two concord affixes on the verb.

So the three potential basic orders not represented in (8) are not non-occurrent. There is no significant generalisation to be made concerning the simple occurrence of the different word orders of (7) as basic: all the possibilities are represented. Nevertheless, some of them remain not *well* represented in the range of languages studied in this respect; and, indeed, in the case of OVS and OSV, are rather limited in geographical and

5. There is an alternative to such an account of the word order possibilities of Hixkaryana; *viz* one in which SOV is basic and OVS derived, obligatorily if some other sentence element is fronted. This is, however, arguably more complex syntactically, as well as being counter to the observed textual distributions.

phylogenetic range. With the possible exception of Haida (if it is basic OSV), all the potential OSV languages reviewed by Derbyshire and Pullum are spoken in the Amazon basin; and seven of the eight potential OVS languages examined by them belong to the Carib family. We should certainly not make too much of this. As Pullum (1978) has recently reminded us, linguistic demography as such is the result of a wide range of non-linguistic factors. Some of the global dominance of SVO and SOV is the result of various eras of colonialist expansion. However, even given this, in a simple language count VOS, OSV and VOS remain scarce and areally and genetically limited. Say, too, these limitations were to coincide with evidence from language acquisition and loss and from language history,[6] then it may be that certain word orders will have to be recognized as universally unmarked or PREFERRED (just as for individual languages a basic or preferred order is establishable). This introduces another potential dimension to our typology, involving not absolute but relative preferences.

There are in existence by now a wide range of claims concerning (relatively) preferred properties and implications. These are difficult to establish, and may be subject to genetic skewing.[7] Nevertheless, a number of plausible arguments for unmarked status for certain properties have been constructed on the basis of evidence from the range of empirical domains noted above: consider some of the discussions in vol. 2 of Greenberg 1978. Thus far an argument of this sort – that certain possibilities are preferred on a principled basis – has not been successfully constructed in the area of word order; and the necessary investigations have, as far as I know, not been carried out. This is true both with respect to basic word order types and to types of word order variation allowed to a particular language (*cf* Steele 1978). However, let us pursue a little further the assumption that such a preference can be shown. This will lead us into a brief consideration of a final typological distinction, one of a fundamental and distinctive character.

6. Say, for instance, that the minority orders always arose via a very specific mechanism; OVS arose only as a historical development from the alternative synchronic analysis outlined in note 5; or S-final languages in general always arose from the decay of an ergative system in which in transitive sentences abs (later object) preceded erg (later subject). (Some of the languages discussed by Derbyshire and Pullum also display ergativity quite prominently.) Such might be taken to support according these possibilities a marked status. However, anything like this is far from being established, and may indeed not be establishable, given the available evidence. (On the testing of proposals concerning preferred properties (or 'relative universals') see *eg* Smith and Wilson 1979: ch. 12).

7. As observed above, the global dominance of certain language families must be taken into account. And there is a further problem, of a rather different kind. Certain properties, though relatively uncommon in the languages of the world, are nevertheless common in particular families (even where members of the family are not in contact), as with OVS and Carib; and even persistent or recurrent through time, as with front round vowels and Germanic. Lass (1976) refers to these as 'family universals'. How are we to interpret the existence of such 'family universals', if they exist? How, for instance, are they transmitted? The explanatory gap here is similar to that associated with 'drift' (Sapir 1921). How, on the other hand, are we to account for the distribution of these properties if not in terms of some such concept?

Various explanations have been offered for the supposed occurrence of only the variants in (8) as basic orders. Vennemann (1973), for instance, associates the requirement that subject precede object in basic orders with the fact that subjects constitute an unmarked topic slot. Clearly, such accounts cannot be maintained as such, given the evidence we've just surveyed: all possible basic orders are attested. Say, however, some are preferred or unmarked; a range of evidence from different empirical domains points to subject-before-object basic orders as unmarked. The question arises: is this an independent generalisation, or is it, at least in part, REDUCIBLE? This preference may depend on some other property, linguistic or extralinguistic. Indeed, all preferences, including absolute preferences, *ie* universals, are potentially dependent or reducible; explicable, at least intuitively, in terms of more general 'laws'.[8] We can, for illustration, contrast a dependent and independent view of the set of case relations. The set of case relations earlier envisaged by Fillmore (1968) was independent; they apparently embodied an independent property of language.[9] In a localist theory of case relations (Hjelmslev 1935/1937; Anderson 1971) a restriction is imposed on the content of the set of case relations: the set of case relations is coterminous with the set of argument types necessarily displayed by simple predications of location and change of location. This restriction, if appropriate, may be in part reducible to the cognitive primacy of spatial percepts.

In the present context of generalisations concerning word order variation, reductions of the kind proposed by Vennemann (1973) with respect to supposed universal restrictions remain appropriate for investigation in the context of (relative) preferences. Thus, in somewhat similar vein, Keenan (1978: § 6.3), for example, attempts to provide a cognitive basis for the paucity of VOS languages, an account which, if well-founded, applies equally well in the case of OVS and OSV. He argues that a basic VOS structure is problematical cognitively and pragmatically even in simple sentences and that this is compounded in complex sentences. Of importance in his argument is again the notion that subjects are frequently topical, and the assumption that there are advantages to having topics presented early in an utterance: languages, indeed, possess rules, or constructions, that bring about just that. Early presentation of the topic in an utterance, Keenan suggests, aids the hearer in assessing the relevance to him of what is being said. If something like this is valid, then the scarcity of object-before-subject languages may indeed be directly associated with pragmatic considerations. However that may be, what is involved here is perhaps the most crucial, and difficult, problem in establ-

8. I have in mind here reductions which refer to generalisations established in some independent empirical domain. An account of markedness such as is offered by Chomsky and Halle (1968:ch.9), on the other hand, represents no more than a notational strategy with no independently established content (Lass and Anderson 1975: App. 1, Lass 1976, Anderson 1979b).

9. This characterisation does not seem to be true, however, of more recent work by Fillmore (*eg* 1977).

ishing and interpreting a typological taxonomy: the question of the relative independence or irreducibility of linguistic generalisations.

We can, unremarkably, conceive of typology as being ultimately concerned with establishing the dependencies that hold between the various universal and possible properties and implications. In these terms, we can characterise the 'radical naturalist' position as reducing all linguistic generalisations to dependence, ultimately, on generalisations that are not simply linguistic; the converse view is the 'radical autonomist'. Various schools of thought can be associated with degree of inclination towards one or other of these poles. But to an extent these remain *personal* inclinations, until and unless we can provide more-than-intuitive content for such notions as I've labelled here reducibility. This most fundamental aspect of the organisation of a typological taxonomy remains perhaps the most mysterious.

I offer, in conclusion, the following tabulation of some of the distinctions we have considered and their hierarchisation:

(13)

<table>
<tr><td colspan="2"></td><td colspan="2">property</td><td colspan="2">implication</td><td></td></tr>
<tr><td colspan="2">universal</td><td></td><td></td><td></td><td></td><td rowspan="2">preferred</td></tr>
<tr><td rowspan="4">possible</td><td rowspan="2">significant</td><td></td><td></td><td></td><td></td></tr>
<tr><td></td><td></td><td></td><td></td><td>non-preferential</td></tr>
<tr><td rowspan="2">legal</td><td></td><td></td><td></td><td></td><td>preferred</td></tr>
<tr><td></td><td></td><td></td><td></td><td rowspan="2">non-preferential</td></tr>
<tr><td colspan="2">idiosyncratic</td><td></td><td></td><td></td><td></td></tr>
<tr><td colspan="2"></td><td>language-bound</td><td colspan="2">predication-bound</td><td>language-bound</td><td></td></tr>
</table>

The reader may derive some amusement from attempting to assign linguistic phenomena to appropriate cells therein. He may find that questions arise concerning the independence of the individual cells from each other, and their interaction with the reducibility dimension, as well as, of course, concerning the precise characterisation of the notion of reducibility – or, to put it even more tendentiously, of explanation. But that's a whole other board game.

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## Panel and open discussion

The main point raised in connection with Coseriu's introduction was the question of the empirical significance of the distinction between *System* and *Norm*. It was pointed out, in clarification, that Coseriu's *Norm* corresponds to Hjelmslev's 'usage', and that Hjelmslev's 'norm' is equivalent to Coseriu's *System*. The importance of the distinction between typology and classification was emphasized and also related to Hjelmslevian theory.

The discussion around Seiler's contribution focused on the categories of gender and number, specifically with respect to the semantic functions of the genders in Semitic, generally with respect to the semantic interactions of gender, number and countability. Terminological and cognitive parallels were drawn between Seiler's (German) terms for individuating principles (*Zusammenfassung* (= 'subsumption') and *Ausgliederung* (= 'sorting out', 'order')) and Hjelmslev's 'concentrated' and 'diffuse' as the terms for the cognitive content of gender ('Om numerus og genus' *Festskrift til Christen Møller* Copenhagen 1956).

Data was presented (from Monachi and Greenlandic Eskimo) which violated one or more of John Anderson's agreement generalizations; *cf* footnotes * and 2. above, pp. 179 and 184, in which JMA discusses some of the points made.

Finally, it was suggested (by Eric Hamp) that since OVS has been observed in Carib and OSV in Amazon it might be said that 'we have seen everything' and that 'a trace of almost everything is virtual'. Reflecting on the significance of linguistic rareties, and given the validity of a number of assumptions, he suggested that our task was to choose between parameters such that dependents (Anderson) or implications (Hjelmslev) exhausted even the rarest occurrences of particular traits. A language universal might perhaps then be seen as 'the perimeter of such a parameter set'.

Contributors to the discussion were SML, EH, WUD, EC, FJW, WUW, HS, JMA, Jørgen Rischel (chairman), Eli Fischer-Jørgensen, Michael Fortescue and Torben Thrane.

*Niels Ege:* **On Japanese** ***wa, ga, o***

I share Mr. Coseriu's concern about the indiscriminate use of terms like Subject and Object in establishing general language typologies (SOV, SVO, etc.).

Even as applied to non-ergative languages their status is far from clear, and it is not apparent that they are used, or can be used, uniformly as we move from language to language.

Still, Japanese does not seem to me to be particularly well chosen as an illustration of a language to which the standard notions of subject and object do not apply at all.

Jap. noun phrases with *ga* and *o* actually correspond fairly well to 'our' subjects and objects, respectively, as witnessed by the fact that roughly similar selectional restrictions apply in Japanese and in Indo-European vis-à-vis verb roots with similar semantic import. Whatever differences appear in actual usage between *ga* and IE subjects, on the one hand, and between *o* and IE objects, on the other, are due to differences between Japanese and Indo-European not as to the notions of subject and object as such, but to differences in other areas of grammar.

In particular, it is perfectly reasonable to state as a rule of grammar that Japanese subjects – when expressed – are always marked with *ga* and, conversely, that Jap. noun phrases marked with *ga* always have subject function. However, Jap. subjects need not occur at all superficially, and accordingly the occurrence of an explicit subject usually signals, additionally, either novelty (= indefiniteness) or emphasis (roughly corresponding to an IE cleft sentence), or else embedded predication.

Similarly, Jap. objects – when expressed – are always marked with *o*, and Jap. nouns marked with *o* always have object function. Jap. objects, again, need not occur on the surface (*tabeta* '(he) ate', but also '(he) ate (it)'). It is true that some verbs which may take a noun phrase with *o* in Japanese (*eg*, *miti o aruku* '(he) walks on the road') only rarely have transitive equivalents in IE – or take direct objects only in a transferred meaning or in idioms : *seinen Weg gehen*; *walk the plank*. But this is hardly a valid argument for not accepting the notion of object as a proper description of NPs with *o* : within IE itself the notion of transitivity is subject to considerable fluctuation. The fact that Japanese may preserve the *o* noun phrase in the so-called passive (*Taroo wa/ga tegami o nusumareta* 'T. had his letter stolen (lit.: 'T. wurde sein*en* Brief gestohlen')') is due to language-specific semantic and syntactic properties of the Jap. 'passive' – rather than to the Jap. *o*-form being different from our object constructions. Thus also, the ungrammaticality of the passive **miti ga arukareru* throws no light on the notion of subject (or object) in Japanese; it derives from the fact that in general a specific selectional restriction of animateness applies to *-areru* (making it, in fact, a passive in the literal sense).

The fact that subjects of IE sentences are rendered now by NP + *ga*, now by NP+*wa* in Japanese may seem to indicate that our notion of subject either applies quite differently in Japanese, or does not apply there at all.

However, there is no truth whatever to the claim, still so frequently made, that *wa* serves as an alternate subject marker ever. *wa* marks neither subject nor object, nor indeed any adverbal (case) function. *wa* is outside or above syntax in the narrow sense, being a general topic marker, applying not only to the entire following predication, but to the entire utterance. As such it may single out temporal, causal, locative, or other 'adverbial' (including non-finite verb) phrases, – as well as, indeed, noun phrases. In all these respects it is closely paralleled by *mo* (which however has the additional semantic element of 'also-ness').

The semantic relationship between the noun phrase to which *wa* is appended and the verb of the following predication does not derive from grammatical function or role, but from selectional restrictions : in *Jiroo wa kaita* 'J. wrote (it)', J. must be agent, in *tegami wa kaita* '(he) wrote the letter', 'letter' must be goal – in both instances for purely semantic reasons; similarly, *Taroo wa korosita* is ambiguous between the readings 'T. killed (him)' and '(he) killed T.' precisely because Taroo's role in an act of killing could equally be that of the performer and that of the victim.

If it is true that *wa* does not mark subject (or, in general, case) we should find simple sentences exhibiting both *wa* and *ga* at the same time – which, of course, is exactly what we have in *zoo wa hana ga nagai* 'the elephant, (its) nose is long'.

Finally, I see no evidence, syntactic or semantic, to support Mr. Coseriu's specific claim that Japanese particles like *ga* and *o* signify local relations rather than grammatical functions. To the extent that Jap. postpositions may derive from, or synchronically overlap with, indications of place or direction, such phenomena seem again fairly parallel to what we observe in Indo-European.

*Editors' note:* In reply to Niels Ege, Coseriu disagreed with the proposed analysis on the grounds that *ga* was not a subject-marker, but rather had specific reference, as opposed to *wa* which had a more general meaning. Neither was *o* an object-marker. It carried the sense of 'receiver'.

# 6 Summarizing discussion

## *Henning Andersen:* Introduction

0. When I accepted the invitation to introduce the final session of this symposium, I was confident that the topics around which the first five sessions were centered would touch on so many questions that could not possibly be done justice in the limited time available, and that it would be easy to single out a few that all the participants would find it appropriate to return to in the concluding session. I think everyone here will agree I was right in my prediction about the first five sessions. But it was foolhardy of me to think that it would be easy to choose a few points that all the participants would be sure to want to reconsider in this last session. The prepared contributions and the discussions today, yesterday, and the day before have ranged far too wide for this to be an easy matter. In the following remarks I will therefore be guided by my own predilections and interests to a rather greater extent than I had planned. I beg your forbearance for this subjective element in my remarks and hope that at least some of the issues that I have found particularly important will seem important also to you.

### 1. Rask and Hjelmslev

**1.0.** The contributions that were presented on the first day of the symposium – and the discussions they gave rise to – made it abundantly clear that Rask and Hjelmslev have very different statuses from the point of view of modern linguistics. The study of Rask belongs entirely to the historiography of linguistics and requires qualifications that we do not all possess. Rask's terminology – and his use of common parlance words – present difficulties even for the native Danish reader of today. His whole conceptual apparatus presents problems of interpretation which one cannot solve without a thorough knowledge of the ideas about language that were current in his day. Hjelmslev, on the other hand, belongs to our own period. He wrote in the language of our time, and his conception of language is modern. If Hjelmslev seems difficult to read, this is due to the complexity of his subject matter, with which he endeavored to deal in a rigorously consistent, explicit fashion. Rask's theoretical framework, by contrast, is to a large externt merely implicit. And we are not always certain that he was consistent.

**1.1.** The famous controversy between Hjelmslev and Diderichsen as to whether Rask was a misunderstood typologist or not is symptomatic of the relative opacity of Rask's works. It would be nice, perhaps, if we could conclude – as I think it was suggested – that both Hjelmslev and Diderichsen were right, but I am afraid this option is not open to us. Hjelmslev expressed himself so unequivocally in his 1951 lecture on Rask that he must be either right or wrong. Mr. Benediktsson showed in his

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very thorough analysis of Rask's writings that it is extremely difficult to substantiate the claim that Rask's aim in comparing languages was typological rather than genetic. I thought I remembered a fairly clear-cut example of a purely typological argumentation in the treatise on Icelandic in relation to the tongues of Asia (1932-1935:II 1–45), but this turned out, on second reading, merely to alternate between different types of arguments for genetic relationship, and so it illustrates how difficult it is to evaluate which – to Rask – was the more important criterion in establishing genetic relationships, structural similarities or sound correspondences.

> [1] This remarkable number of derivatives, especially from verbs [in the 'Turanian' languages, *ie* Turkish, Tartar, Yakut, Chuvash, Circassian] conforms to the structure of the Finnic family, but is fundamentally different from the European. True, the endings themselves can rarely be compared to those of Finnish or Lapp, but it is important to note that the whole spirit that reigns in these two so extensive families is the same; for this makes it reasonable to assume a basic relationship between them. When you consider the southern classes of the Finnic family, *eg* Ugric, the similarity in words and structure seems so significant that no observer could deny it. Some of this may be due to admixture, [2] but when the letter correspondences in these languages have been investigated, and many of the correspondences of the northern tribes in this way have been traced all the way to Greenland, a basic relationship between Turanian and Finnic will not easily be denied. As for the European family, all similarities to it must be explained as the result of the intermingling of peoples, [3] for their linguistic system is so entirely different (p. 33).

Here Rask begins with [1] a structural comparison between two groups of languages. But hardly has he made his point before he acknowledges that [2] the relationship must be confirmed by sound correspondences, which – with his indomitable optimism – he is almost certain will be forthcoming. At the end of the passage, however, he slides back into [3] a purely typological argument against a relationship between the languages of Europe and this putative, far-reaching family of Asian languages. It is clear from this passage that the relationship to be established is a genetic one, and it seems that here Rask considers typological affinity a necessary, but not sufficient criterion for establishing genetic relationships.

The Turanian case, with its prima-facie structural similarity and the yet to be determined sound correspondences, has a counterpart in the discussion of Celtic in the same paper. Here, '[c]ertainly a very great number of words correspond with Icelandic, and such an old and remarkable language class must by no means be omitted from a comparison of the European family with the languages of Asia. But of course it must first be proven that it belongs to the European family'. With this in view, Celtic calls for 'a closer examination, especially the structure of the pronouns and verbs of Welsh and the Gaelic linguistic system in general'.

In both cases, Turanian and Celtic, features of structure are evidently assigned precedence over sound correspondences. The case of Celtic seems to confirm the conclusion that Rask was inclined to consider structural similarity a more reliable indication of 'basic relationship' than sound correspondences, but in practice as well as principle required both structural similarity and regular sound correspondences between two languages for them to be considered related. The Celtic case, too, leaves no doubt that Rask was concerned with genetic relationships.

**1.2.** It is striking, however, that although Rask strove to establish genetic relationships, and although he was well aware of the fact that the historical development of a language can be grasped as a sequence of periods, he was not sensitive to the diachronic dimension. This is why it was possible for him to draw up lists of correspondences between related languages without wondering by what changes they had come about, and hence without feeling constrained to being consistent as to whether *x* in one language had changed to *y* in the other or *vice versa* (*cf*, for instance, 1932–1935:I 68 or Holger Pedersen's comments in the Introduction, pp. xxiii, xxxv, xxxvii, li). One cannot explain this lack of concern for the diachronic dimension by claiming that historical linguistics is a later development, due primarily to Grimm (thus Hjelmslev 1951:154). That languages change was common knowledge long before Grimm (*cf* Diderichsen 1960:141). N. M. Petersen, who wrote the first historical grammar of Danish (1829–1830), and J. H. Bredsdorff, who published a treatise on the causes of linguistic change in 1821, demonstrated in their works a fine sense of the dynamics of language. These were close associates of Rask's from long before the prize essay was written, and Rask must have been familiar with their understanding of language change. One cannot escape the impression that Rask, while he had an almost uncanny flair for static patterns in languages – and among languages – was, so to say, time deaf. There is nothing extraordinary in this, of course. Much of the work in historical linguistics since Rask has suffered from the same sort of inability to transform static diachronic correspondences into realistically dynamic accounts of language change.

**1.3.** Ms. Bjerrum pleaded for a resumption of the study of Rask, whose total production, published as well as unpublished, is of considerable relevance for an understanding of early 19th century linguistic thought. Everybody would agree that this is an important task for the future. Not everybody would agree with Hjelmslev, who in his similar plea of 1951 advocated that Rask's works be analysed from an immanent point of view. Such an approach would be appropriate if Rask had been an autodidact who had only written for himself. But all of Rask's works, from his earliest student papers to his last lectures, as well as his voluminous private correspondence, bear witness that he was, and saw himself as being, a participant in a scholarly dialogue. In his writings he evaluated the works of others, and he expected in return that his publications be evaluated according to their merits. It would seem that it is in the context of the give and take of this scholarly dialogue that Rask must be understood.

I would like to emphasize here – since this was not done in the discussions of Rask in this symposium – the tremendous contribution Diderichsen made to a fuller understanding of Rask's works and of the continuity of the grammatical tradition between the 18th and the 19th century. Diderichsen's well-documented account of the sources of the young Rask's ideas and of the remarkable consistency of Rask's approach throughout his scholarly career will surely prove an excellent point of departure for future students of Rask.

My own investigation of J. H. Bredsdorff, who like Rask and N. M. Petersen was a pupil of Søren N. J. Bloch (1772–1862, one of the most active and creative Danish linguists during the first quarter of the 19th century) has convinced me that there existed in Denmark during the lifetime of Rask – undoubtedly for the first time in the history of Danish linguistics – a scholarly milieu with an ambience conducive to the discussion of old and new ideas about language, to their testing against new language data, and to their confrontation with ideas current in other fields of knowledge. The importance of this milieu for the development of Rask's thinking should not be ignored. As a possible illustration I would mention Rask's attempt to formulate a hierarchical system of classification reflecting degrees of relatedness among languages and employing the terms 'race, class, stock, branch, language, and dialect', which was mentioned by Mr. Benediktsson. Rask first presented this system of classification, explicitly characterizing it as new, in a letter (written to P. E. Müller from St. Petersborg) dated 29 January 1819, in which he also proposed criteria for a systematic classification of the known linguistic 'races' (Sarmatian, Semitic, Scythian, etc.). Later the same year, in a discussion of the Scythian linguistic race, he finds it practical 'in the same manner as the natural scientists' to distinguish between the strictly systematic classification and several 'natural families' of languages (1932–1935:II 253; the paper, 'A treatise on linguistics, in particular on the classification of the Finnic peoples' was dated 30 May 1819). Rask can have gotten these 'new ideas' from any of a number of sources, in fact he most likely learnt about them already when he was in high-school. But it is remarkable that they correspond to major points in the doctoral dissertation of his closest associate at the University of Copenhagen, the man he thought of as the only one who could act as his linguistic executor, in case one should be needed, J. H. Bredsdorff. Bredsdorff's dissertation was published and defended in 1817 and may not have reached Rask until the following year. The dissertation emphasizes the importance of consistently hierarchical classifications and the need for binary, preferably logically contradictory, criteria of classification, and discusses at length the principled distinction between systematic and natural taxonomies.

Be this as it may, it stands to reason that in a renewed study of Rask, the research that was initiated so brilliantly by Diderichsen should be continued and extended to include the other active linguists of Rask's time, with whom he interacted, whom he influenced, and whose work stimulated his own thinking. This seems to be the only way in which one can form an adequate conception of the development of linguistics in early 19th century Denmark.

## 2. Naturalness

**2.0.** It is very regrettable that Mr. Lass was unable to participate in the symposium. His planned contribution would undoubtedly have added perspective to the discussions devoted to the question of naturalness in language. But the two presentations by Mr. Dressler and Mr. Wurzel – individually and taken together – gave an excellent survey of the main theses of natural theory and a good indication of the directions in which the theory is being elaborated.

I have long felt convinced that language research done from the point of view of natural theory can contribute significantly to a delimitation of what is conventional and historically given in language, and what is dictated by the nature of man, the nature of communication, and the nature of language transmission, and hence, universal. Obviously, natural theory is subject to the same conditions of success as other theories about language: it will succeed in broadening our understanding of language to the extent that its explicit or implicit assumptions about its object of investigation are appropriate. Mr. Dressler acknowledged that important advances along naturalist lines have been made by structuralists of various schools. This clearly implies that natural theory shares some basic assumptions with structuralism. It would be interesting and useful to bring these affinities out into the open. I myself was struck by several points in the reports of Messrs. Dressler and Wurzel which indicate that natural theory is moving towards traditional structuralist positions. I will mention a few of them.

**2.1.** Mr. Dressler admitted that the strong claim that sound change is simply the result of natural phonological processes has to be abandoned. When an allophonic process is 'phonologized' (as Hyman 1977 put it), that is, changes its output from intrinsic to extrinsic allophones, this change is in some measure – Dressler recognizes – a denaturalization. It seems that this insight comes very close to an understanding that the natural processes which govern intrinsic allophones are powerless to explain the kind of event by which contextual variation is accorded status as part of the phonological norms of the language. There is, however, nothing 'unnatural' about this kind of event, by which contextual variants are integrated into a semiotic system by being assigned the value of conventional indexes (*cf* Trubetzkoy 1958:47, Jakobsen & Halle 1956:10). One would hope that natural theory will recognize it as entirely conforming to the nature of language.

**2.2.** Mr. Wurzel argued convincingly for the utility of the concept 'system-appropriateness' (*Systemangemessenheit*), to be understood as naturalness in terms of a specific system. It really seems impossible to understand the dynamics of language without some such notion, and it is no wonder that such a notion has always been explicit or implicit in diachronic linguistics. In synchronic linguistics the distinction between systemically motivated and unmotivated has appeared in various guises. The old distinction between productive and unproductive patterns was integrated with a structuralist approach by Karcevski (1927). Hjelmslev distinguished between system, norm, and usage (Da. *system, norm, usus*) in his attempt to resolve the Saussurean antinomy between synchrony and diachrony (1934). But nowhere has the difference between systemically motivated and unmotivated been discussed so eloquently and in such detail as in the works of Coseriu (1952; also 1968, 1969, Serebrennikov 1970). For all the clarity of Coseriu's contrast between system and norm, there remain a number of problems on different levels of description. Here one would welcome attempts on the part of naturalist linguists to elucidate the precise content of their notion of *Systemangemessenheit*.

**2.3.** Another notion which played a prominent role in Mr. Wurzel's contribution was that of iconicity, which however was applied in a much too narrow sense.

No one would quarrel with the naturalist concept of constructional iconicity. It is a concept which seems to be rediscovered by every generation of linguists. The specific example discussed by Mr. Wurzel, the Slavic genitive plurals with a zero desinence, has been the subject of a dynamic comparative study by Greenberg (1969), who has shown that on the whole there is a tendency for these counter-iconic zero desinences to be replaced by real desinences. It is important, however, in dealing with such examples to consider whole paradigms at a time. Specifically in this instance, the special relation between the genitive plural and the nominative singular (both of which can have zero desinences) and genitive singular and nominative plural (which regularly have identical desinences, but tend to differ in stress) needs to be taken into consideration (*cf* Jakobson 1956).

But the notion of icon is a much wider notion than the naturalists' constructional iconicity would let us suspect. In the sign theory of C. S. Peirce, icons and indexes are the natural sign types, as distinct from the conventional symbols. And among icons three subtypes are defined, which are essential in linguistic analysis. In the image, the signans has simple qualities which are similar to simple qualities of its signatum. In the diagram, relations in the signans reflect relations in its signatum. In the metaphor, the signans is allowed to represent its signatum by reason of an *ad hoc* recognized similarity between the two. I will not venture into a discussion of images and metaphors here, but would like to use one of Mr. Wurzel's examples, the paradigm of the Russian word for 'table', to show that only the examination of whole paradigms allows us to observe the diagrammatic relations between content and expression.

| | Sg. | Pl. |
|---|---|---|
| Nom. | stól –Ø | stol–í |
| Acc. | stól –Ø | stol–í |
| Gen. | stol –á | stol–óv |
| Loc. | stol –é | stol–áx |
| Dat. | stol –ú | stol–ám |
| Instr. | stol –óm | stol–ám,i |

Fig. 1.

As the paradigm shows, the two numbers are distinguished by different desinences in the six cases, that is, by different symbolic signs. But at the same time there is a consistent difference in length between the desinences of the singular and those of the plural, so that for each case the opposition 'plural' *vs* 'singular' ('more than one' *vs* 'unspecified number') is reflected as a difference between $n + 1$ segments and $n$ segments.

Some might cautiously prefer to view this regular diagrammatic relation between content and expression as coincidental. But when one considers the historical background of this paradigm, which is the outcome of an amalgamation of several different Old Russian paradigms with desinences of different lengths followed by a gradual generalization of some allomorphs at the expense of others, it seems possible to view the modern

| | Middle Russian | | Modern Russian | |
|---|---|---|---|---|
| | Sg. | Pl. | Sg. | Pl. |
| Nom. | Ø | 1, 2, 3 | Ø | 1 |
| Acc. | Ø | 1, 2, 3 | Ø | 1 |
| Gen. | 1 | Ø, 2 | 1 | 2 |
| Loc. | 1 | 2 | 1 | 2 |
| Dat. | 1, 3 | 2 | 1 | 2 |
| Instr. | 2 | 1, 2, 3 | 2 | 3 |

Fig. 2. Number of segments in the desinences of the Middle Russian paradigm variants corresponding to the paradigm of Modern Russian *stol* 'table'.

regularity as the telos of the numerous individual changes and the whole development as an illustration of what Rask called *Naturens Gang i Sprogene* (*cf* Andersen MS).

There has been a development in recent years within structuralism away from the early fixation on the arbitrariness of the individual sign and towards a fuller appreciation of the multifarious natural – and especially the diagrammatic – relations of representation in language, between content and expression (as in the example above), between the paradigmatic relations of the system and the syntagmatic relations in texts, and between language and the reality it is called upon to represent. This trend seems eminently compatible with the goals of natural theory. How could there, indeed, be any conflict between the search for naturalness in language and the recognition that the very principle of structure, and hence, as Jakobson put it 'the essence of language' (1965), is the natural sign relation instantiated by the diagram. There can be no doubt that the *réseau de fonctions* which to Hjelmslev lies at the base of every language should be viewed as an example of naturalness.

**2.4.** I would like to mention one more point, returning now to Mr. Dressler's contribution, in which a distinction was made between natural phonology, which is to be founded on the study of articulation and perception and their neurological bases, and natural grammar, which must have a basis in cognition. It seems to me that there are enough compelling arguments that demonstrate the primarily conceptual nature of phonology and thus speak against the artificial barrier that has traditionally been erected between phonology and the rest of language. Naturalists may be skeptical towards structuralist findings to the effect that, for instance, markedness values in phonology are in part language specific, which would indicate that they may be conventional rather than natural; or towards the notion of feature ranking, which can only be understood as the result of a mental operation performed on conceptual entities; or towards other results indicating that phonological systems are structured according to the same principles as systems of grammatical categories, and hence must be understood as conceptual in nature (Andersen 1975a, 1975b). But surely no naturalist can ignore the direct mapping of content categories onto phonological categories in phonaesthemes (*cf* Samuels 1972) or in size-sound symbolism (*cf* Ultan 1978), or in poetic texts, whether poetry or advertising. These phenomena show that the two articulations of language are of a piece.

Again, just as the content system of a language may be separate from, but is not independent of, the network of symbolic values that make up the cultural system of its speakers (*cf* Ivanov & Toporov 1974), so the interdependence between phonology and the value systems that form other experiential dimensions – an interdependence which is manifest in synaesthesia – testifies to the intimate relations between language and the other cognitive systems man has at his disposal (*cf* Fischer-Jørgensen 1967, 1978, Fónagy 1963, Jakobson 1979). The exploration of the physiological foundations of these relations, which is proceeding apace in the recently defined field of neurosemiotics (*cf* Ivanov 1978, 1979), promises to yield important insights into the genetically given prerequisites for the semiotic behavior characteristic of our species.

### 3. Genetic linguistics

**3.0.** The question, to what extent genetic-comparative classifications can be based on typological considerations, was approached from very different points of view by the three main speakers, but resulted by and large in a consensus. Mr. Egerod's impressive survey of some major problems in the classification of the languages of East and South-East Asia showed that where strict correspondences (element functions) do not suffice to establish genetic relationship – either because they are too few or because the extent or direction of borrowing cannot be determined – structural affinities cannot fill the gap, for they may be the result of adaptive change. The establishment of element functions is the essential first step, and typological comparison may be a valuable second step – potentially a barrier to erroneous claims of cognacy, according to Hamp – but only when it is supported by actual correspondences in matters of detail, in particular such morphological correspondences as those between the Japanese and Altaic causatives. As Mr. Hamp put it, it is the small, fine-grained, quirky facts that really count in genetic comparison.

**3.1.** Both Mr. Egerod and Mr. Hamp emphasized the importance of viewing putative language families in terms of specific diachronic hypotheses, of resolving the correspondences into relative chronologies (so Hamp) and of correlating the purely linguistic data with facts of textual attestation and with whatever historical or archaeological data may be relevant (Egerod). Since the topic of this session in the symposium was genetic classification, it is perhaps worth emphasizing that for the historical linguist the mere establishing of cognacy between two or more languages has no value in and of itself. For the historical linguist the regular synchronic (or achronic) correspondences serve as a point of departure for the formulation of diachronic correspondences between the attested languages and earlier reconstructed language states from which they have developed. And these diachronic correspondences, in turn, have to be construed as the outcome of diachronic developments which have to be reconstructed in as realistic terms as possible. While typological comparison can contribute only modestly in the determination of cognacy between languages, typological considerations are of the essence both in the reconstruction of unattested language states and in the reconstruction of the hypothetical developments which alone allow us to understand the divergence of the attested languages from a once unified proto-language. A genetic classification based on algebraic element functions has no utility in historical linguistics. Only by attempting to reconstruct the historical development of both a system and its manifestation in texts can we add to our understanding og language change. The same thing is true with respect to content categories. Without the attested or hypothetical textual collocations with specified referential value which mediate between different content categories tied to similar expression entities at different stages of a language, we lack the justification for defining element functions between the expression entities in question. It was a pleasure to hear Mr. Hamp's account of the linguistics of Holger Pedersen, for whom such a realistic approach to comparative and historical linguistics was a matter of course.

## 4. Typology

Before concluding I would like to comment briefly on Mr. Coseriu's important distinction between language type as a classificatory notion and type in the sense of the higher level patternment or plan of the individual, historically given language. This distinction, which one might refer to as the distinction between general type and specific type, has far-reaching implications.

Mr. Coseriu somewhat provocatively denied typological classification any scientific or theoretical value. This seems fair in those cases where 'das Klassifizieren eine rein empirische Operation der Anwendung des schon anders Festgestellten [ist], durch die man nicht mehr erfährt, als man durch die Sprachbeschreibung oder die Sprachgeschichte schon weiss'. Fortunately, however, few linguists would occupy themselves with such sterile exercises. Typological classification can – and in current practice usually does – aim to establish similarities and differences in the extent to which diverse techniques or processes are utilized in different languages, and its ultimate goal is to determine what are the principles of language structure and language use which govern all languages, what are the options available to languages given the existence of these principles, and how does the different utilization of these options in different languages reflect the interplay of different principles. Drawing up taxonomies of languages from different points of view is an efficient way of carrying out the systematic typological comparison needed to reach this goal. The results of this endeavor are a prerequisite for what Mr. Coseriu considers the chief goal of typological linguistics, the identification and description of the specific types of individual languages, though here, of course, due allowance must be made for the dialectic of general typology and description.

In Mr. Coseriu's conception, we should distinguish three levels of patterning in language, the norms of usage, the functional system, and the specific type. The norms of usage comprise what is historically realized and codified in the given language community. The functional system, as defined by Coseriu, corresponds only partly to the norms. For on one hand, the system, which comprises everything that is productive in the language, allows for usage which has not yet been realized, but exists *in potentia*; thus the system provides for future innovations which may be acceptable to the speakers and codified as part of the norms inasmuch as they are systemically motivated. On the other hand, part of the traditional norms – what is unproductive – may lack systemic support and will tend – since it cannot be re-created, but only reproduced – to fall into disuse. In this way the synchronic tension between norm and system will be realized diachronically as systemically motivated drift.

A similar tension may exist between the functional system and the specific type of a language. At a given stage, the system may comprise, say, different techniques of synthesis. But if the specific type of the language is, say, agglutinative, this discrepancy between system and type may be manifested as a diachronic tendency for fusional complexes to be replaced by sequences of merely juxtaposed morphemes, that is, as typologically motivated drift. Coseriu's conception, in effect, provides a solution to the problem of drift (Sapir 1921) by assigning what appear to be 'metaconditions on change' a place in the structure of language – and hence in the internalized grammars of the bearers of any language, the only place where such 'metaconditions' can exist in reality.

The implications this conception holds for various branches of linguistics are too numerous to be discussed here. I will emphasize only the implication it has for our understanding of language acquisition. It implies in effect that language learners beyond forming hypotheses about the functional system of their language – hypotheses they apparently are able to maintain even in the face of counter-indications in the observable usage that serves as their raw data – form superordinate hypotheses about the ideal type of their language, which may be only imperfectly manifested in its functional system. One might question whether such a view of language acquisition can be confirmed or disconfirmed in any way. I would suggest that Mr. Coseriu's conception precisely by integrating typology with language description and identifying the dynamics of diachrony with the tension between the different levels of patternment in synchronic states provides a principled basis for exploiting language histories as a source of knowledge about language states and their transmission from generation to generation. In particular it promises that a systematic study of developmental tendencies in languages whose history is known may shed light on the way in which specific types determine changes in functional systems (*cf* Andersen MS). It would be interesting to discuss other ways in which this view of language structure may have a bearing on the investigation of language in its different manifestations.

**5. Conclusion**

Looking back over the span of years that separates us from Rask's time, we can see a certain progression in the development of our science and a definite continuity in its basic ideas.

Rask believed that language could be viewed as an organism which is formed in accordance with the laws of nature, and which in its historical development follows a spiral course from simplicity of structure to complexity and back to simplicity, in accordance with laws of nature (thus already before 1807; *cf* Diderichsen 1960:107ff, 163f.). Hjelmslev attained an incomparably more sophisticated understanding of language as the semiotic prior to and above all other semiotics used by man, in principle independent of the substance in which it is manifested, but still based squarely on the general structural possibilities man bears within himself, and in its historical development doubly conditioned by the nature of man: from within, in the general sense that there can be no language states which do not conform to man's innate capacity for language and in the specific sense that tensions within a given linguistic system determine how it may change; and from without, in the sense that changes in the norms of a language, like changes in fashions, depend entirely on the will of the 'enigmatic and capricious being' man is.

This symposium has paid tribute to the lasting value of the insights of these two men, among other things, by focusing on some of their main concerns and demonstrating that they still command the attention of linguists today and continue to stimulate advances in our understanding of the nature of language and its dependence on the nature of man.

I think it would be appropriate to conclude by drawing a parallel between language and linguistics. One of the important implications of Hjelmslev's theory of language is that any science, and hence also linguistics, can be regarded as a semiotic – *mutatis mutandis* analogous to language. In such a semiotic, the substance of the science in question is given form by the system of terms that enter into its definitions, and any extension of its domain to new areas of substance implies an elaboration of the system of concepts that constitutes its form.

I am sure all the participants in this symposium share the understanding that language cannot be viewed as a corpus of ready-made formulae and patterns that the speakers of a language learn by rote, but must be grasped as the activity in which members of a speech community create and re-create speech, and through which they continually build up and expand their linguistic competence. In Humboldt's words, language is not a work (*ergon*), but an activity (*energeia*).

There have been linguists who were inclined to regard linguistics as ideally a fixed doctrine, supplemented by a set of procedures for reducing any corpus of texts to a grammar of the language in question, that is, as an *ergon*. It seems that linguistics is better understood as an activity, the dialogue – written and oral – through which members of a community of scholars share their knowledge and understanding of language, and in so doing continually re-create and refine the insigts of earlier generations of linguists and contribute to the advancement of their science by extending its field of inquiry and expanding their collective understanding of its object of investigation.

This view of linguistics as *energeia* has been amply corroborated by the spirit of this symposium.

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