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22nd Annual Round Table

Linguistics: Developments of the Sixties
—Viewpoints for the Seventies



Georgetown University
School of Languages and Linguistics

***Report
Of The Twenty-Second Annual
Round Table Meeting
on Linguistics
and Language Studies***

***Richard J. O'Brien, S.J.
Editor***



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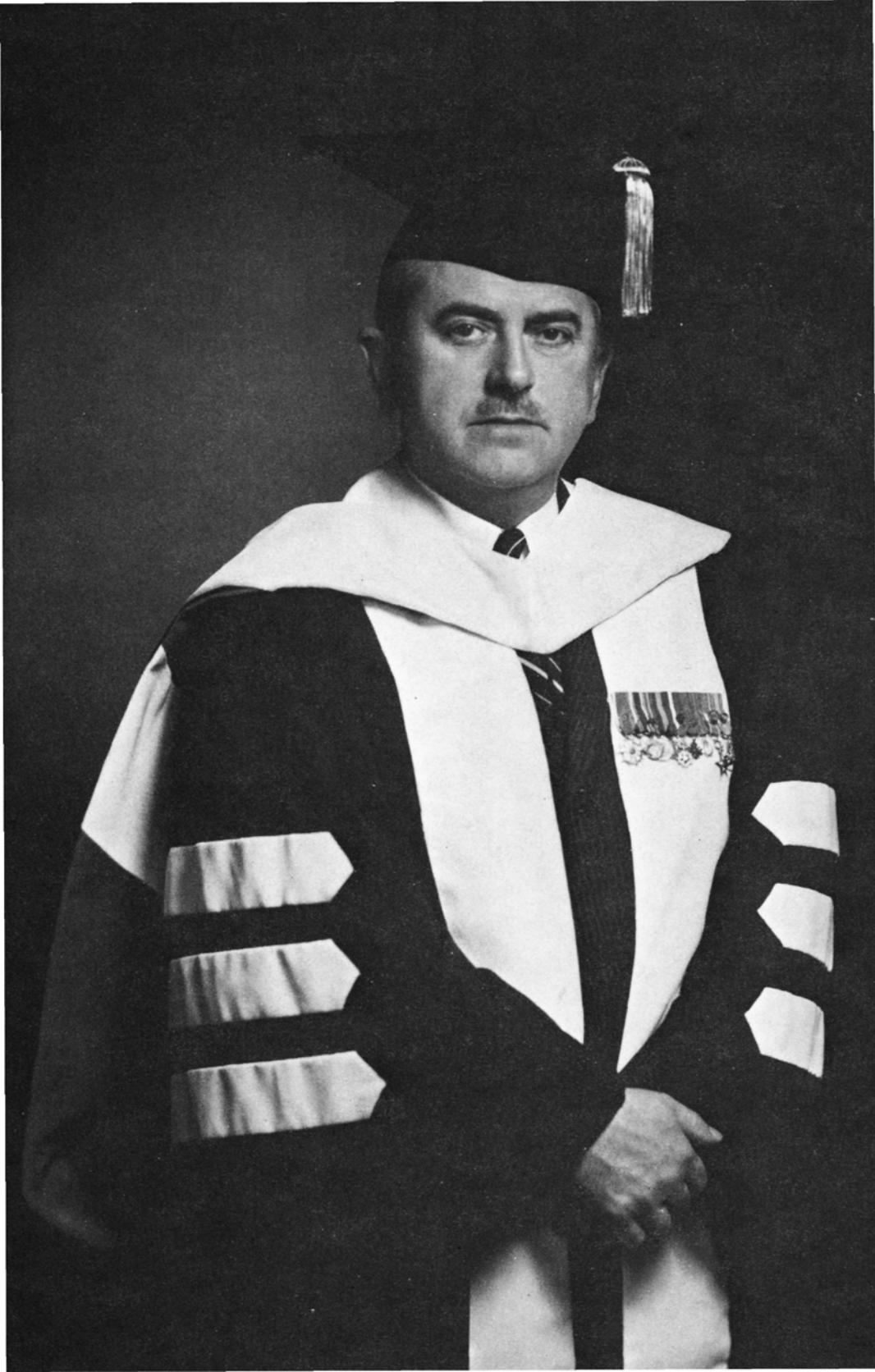
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IN MEMORIAM
LÉON DOSTERT

1904–1971



LÉON DOSTERT

1904 - 1971

Léon Dostert was born on May 14, 1904 in Longwy, France. He was brought to the United States in 1920 by American soldiers for whom he served as an interpreter during World War I. These men saw him through high school. He began his studies at Occidental College and later transferred to Georgetown University. He was awarded his Bachelor's degree from the Georgetown School of Foreign Service in 1928. In 1930, he received two further degrees from Georgetown, the Bachelor of Philosophy from the College of Arts and Sciences, and a Master's degree from the Graduate School.

From 1930 until 1942, he taught French at Georgetown, and then entered the United States Army as a Major. Among his wartime assignments were duty as liaison officer to General Giraud and Interpreter in French for General Eisenhower. He was decorated by the American French, Moroccan and Tunisian Governments and left active service in the Army as a Colonel in 1945. After the war, he became Chief of the Language Division of the Nürnberg War Crimes Tribunal.

He then took up duties as Director of Simultaneous Interpretation at the United Nations and, from 1948 until 1949, he was Administrative Counselor to the International Telecommunications Union in Geneva, and Secretary General to the International High Frequency Broadcast Conference in Mexico City.

In 1949, he was appointed first Director of the then Institute of Languages and Linguistics at Georgetown University. Under his direction, the Institute pioneered in the use of integrated language laboratory instruction, in training in simultaneous interpretation, and in the development of mechanical translation. He inaugurated the Round Table Meetings in Languages and Linguistics in 1950. In 1959, Professor Dostert relinquished the post of Director of the Institute and became Director of Research on Mechanical Translation and of Special Projects. These special projects included at various times training in foreign languages for blind students, English language programs in Yugoslavia and Turkey, English Teacher Training Programs in Turkey, and a literacy

program also in Turkey. It is estimated that through the efforts of those trained in this last program, over a half million young Turkish Army recruits were given the benefits of literacy in their own language.

Professor Dostert published widely on languages and linguistics and was the recipient of many academic honors for his work in those fields. He received honorary doctorates from Franklin and Marshall College (1957) and from Occidental College (1960), which he had attended as a young student. Professor Dostert received an honorary LL. D. from Georgetown University on October 13, 1958. In 1964 he left Georgetown and returned to Occidental College as Professor of French and Linguistics, and as Chairman of the Department of Languages and Linguistics. He became Professor Emeritus at Occidental in 1969.

In March 1971 Professor Dostert attended the 22nd Annual Round Table Meeting, whose proceedings are reported in this volume and in response to the spontaneous and continued applause of the participants and audience finally stood to acknowledge their recognition and appreciation of his many original and lasting contributions both to his profession and to Georgetown University which remembers his years here with lasting gratitude and affection.

While attending a linguistic conference in Bucharest, Rumania, Professor Dostert suffered a stroke and died on September 1, 1971.

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INTRODUCTION

For the last twenty-two years, Georgetown University's Annual Round Table Meetings on Languages and Linguistics have brought together scholars in linguistics and related disciplines to report on their latest research and to discuss current problems. The present volume represents the proceedings of the 22nd Annual Round Table Meeting. The theme of the meeting was: 'Linguistics: Developments of the Sixties--Viewpoints for the Seventies'. Seventeen papers were delivered at the meeting's four sessions which were held on March 11, 12, and 13, 1971. The discussion which followed the presentation of each set of papers was recorded and is included in the present volume.

In any discipline, it is important to pause now and then to survey recent developments and to consider what viewpoints they provide for the future. For linguistics, the sixties have been an extremely active and productive decade. To attempt to capture all the linguistic strands of the decade within the confines of a single meeting is clearly impossible. At most it is possible to report on some of the significant insights of the sixties and to relate them to the emerging problems of the seventies.

The viewpoints expressed in the papers and discussions included here cover a wide spectrum of the principal developments of the sixties and give promise that for linguistics the seventies will be a fascinating decade.

As Chairman I would like to express my thanks to the following graduate and undergraduate students of the School of Languages and Linguistics for their assistance as ushers at the 22nd Round Table: Etienne Zé Amuela, Maria Ibba, John Albertini, Cynthia Beuerman, Ron Sears, Al Rey, Joe Bellino, Janine Farhat, Elissa Bell, John Kalkbrenner, Kenneth Bradley, Linda Hersman, Irene Scouras, Fred Perry, and Zora Perry.

I owe a special debt of gratitude to Mr. David Strickler, the chairman of the student committee on local arrangements, to Mr. Leslie Hanzely who recorded the proceedings, to Miss Melody Kalkbrenner and Mr. Thomas Sission who mailed announcements and recorded

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registration, to Mrs. Marian Higgins who took care of local transportation, to Miss Marieluise Baur who prepared the photocopy and helped supervise the food services, and to my colleague Professor Neil J. Twombly, S. J. who proofread the final copy.

Finally, I wish to express my warmest thanks to Mrs. Dorothy Mills of the Publications Department who assumed the burdensome and bothersome task of general supervisor and coordinator of all the local arrangements.

Richard J. O'Brien, S. J.
Editor

WELCOMING REMARKS

ROBERT LADO

Dean, School of Languages and Linguistics
Georgetown University

Friends, on behalf of Georgetown University and the School of Languages and Linguistics I am honored to welcome you to the 22nd Annual Round Table on Languages and Linguistics which this year is under the able and devoted direction of Professor Richard J. O'Brien who in keeping with the highest traditions of the Annual Round Tables has brought together an exciting array of scholars and scientist who are making history in linguistics.

We wish to thank the participants for their generous gift of sharing with us their ideas and research, the registrants for coming here to learn and to contribute to the discussion, and the staff for their splendid cooperation with the Chairman.

I would also like to announce that we are delighted, thrilled, and pleased to have with us as a registrant here tonight the man who inaugurated these Annual Round Table Meetings in 1950 and who was the first Director of the Institute of Languages and Linguistics, as our School was then known. Professor Léon Dostert, would you please stand. [Sustained applause.] And now the Round Table.

WELCOMING REMARKS

RICHARD J. O'BRIEN, S. J.

Chairman, 22nd Annual Round Table Meeting
Georgetown University

The very large number of people gathered here this evening demonstrates how much all of us appreciate the kindness of our participants in accepting Georgetown's invitation to come to this Round Table Meeting on Languages and Linguistics and share with us their insights into the developments of linguistics during the sixties and their views on the prospects of linguistics for the seventies. Let me both express my deep appreciation to those who accepted the invitation to present papers and extend a very warm and sincere welcome to you all.

SYNTAX SINCE ASPECTS*

EMMON BACH

The University of Texas at Austin

1. Introductory. The impact of Chomsky's Syntactic Structures on the linguistic world of 1957 has been often described. By way of background to my remarks on the present scene in syntactic theory I would like to mention briefly some aspects of the Chomskyan revolution. 'Establishment' linguistics (in America at least) was caught up at that time in sterile and unresolvable debate centering around the attempt to find 'rigorous' methods for deriving grammars from corpora. Chomsky successfully cut the Gordian knot of linguistic theory by asking two questions: (1) What exactly are the assumptions about grammars and languages that underlie current descriptive practices? (2) Once we have made these assumptions clear, do the data of the linguist suffice to decide among the distinguishable theories? In other words, first we have to know what question we are trying to answer and then we have to ask whether we can answer the question on the basis of available data. The first question led to the exact definition of a number of alternative theories. The second question led to an enlargement of the range of data considered relevant to validating or refuting various hypotheses made precise by the first kind of work.¹

Let us consider a familiar example. The mathematical study of types of phrase-structure grammars made it possible to judge the probabilistic Markovian models of language of such workers as Shannon and Weaver (1949) or Hockett (1955). Merely by looking at the kind of data that linguists were considering at the time--that is, open-ended sets of texts or native speaker judgments about grammaticality--it was possible to invalidate the theory of finite-state grammars. But when linguists turned to considering whether context-sensitive grammars could be valid models of linguistic competence it was necessary to go beyond such data to other judgments of native speakers (in Chomsky's phrase,

to move to questions of descriptive adequacy). As is well known, the general conclusion of Chomsky and his co-workers was this: of the various relatively well-defined linguistic theories that had been proposed or that seemed to underlie descriptive work of the time only the transformational theory of syntax offered hope of achieving an adequate general account of human linguistic capabilities.

Chomsky's Aspects of the Theory of Syntax (1965) represented both a summing up of work that had gone on in the years since the inception of transformational theory and a presentation of new directions and changes in the basic theory. In the years since Aspects we have seen the development of another impasse that can be broken only by asking again the questions that Chomsky asked.

I shall be concerned here with showing how the mathematical study of transformational grammars by Stanley Peters and Robert W. Ritchie can be used to demonstrate the basic defects of the theory of Aspects.² Then I will argue that the developments in syntactic theory that center around the generativist-interpretivist controversy represent weakenings of an already fatally weak theory. Finally, I shall sketch some ways in which recent research has attempted to remedy the defects of the theory by putting substantive constraints on transformations and extending the range of data to include facts about a variety of languages.

2. Aspects of Aspects. In Aspects, Chomsky sketched a general theory of syntax,³ which we may outline as follows: A grammar (taken in the most general sense) consists of three main parts: a syntax, a semantics, and a phonology. The syntax consists in turn of two main parts: (1) the base: a set of recursive context free branching rules, and a lexicon of complex elements and (2) a set of transformations which operate cyclically from the 'bottom up' in the familiar fashion and which carry out a 'filter function' in blocking some structures available from the base when conditions of compatibility between parts of a phrase-marker are not met. The deep structures are the phrase-markers from the base that successfully run the gauntlet of the transformations and the phrase-markers that have undergone the transformations are the surface structures. The deep structures determine the semantic representations of sentences by the operation of the rules of the semantic component. Similarly the phonological rules operate on the surface structures to determine the final phonetic form. This theory we may call the 'standard theory' (following Chomsky, forthcoming).

The standard theory is an elegant, intuitively satisfying theory of syntax. But there are at least two problems with the theory as presented in Aspects. First, the mathematical properties of the system of grammars implicit in the (largely informal) discussion of Aspects

were completely unknown.⁴ As we will note in a moment, this loophole has been closed in the last several years by work that has been largely and unfortunately ignored by the linguistic community. Second, a number of aspects of the theory of Aspects were uncritically presented and accepted.⁵ Let us notice one example. In the theory of Syntactic Structures no complex or compound sentences were generated by the base (PS) rules. Thus, embedded sentences in complements and relative clauses, as well as conjunctions of sentences were all derived by so-called generalized transformations. A major change in the standard theory was the hypothesis that all these types of sentences are derived by base rules which yield so-called generalized phrase-markers literally containing all the sentences from which the surface structure is derived. But no real evidence for this view was given in Aspects. To my knowledge the only evidence that has ever been offered for this view is that given by David Perlmutter (1968).⁶ Perlmutter argued that the existence of deep-structure constraints showed that it was necessary to have the embedded sentences in the generalized phrase-markers at the point when lexical insertion takes place. But Perlmutter's argument holds only for complement sentences and not for relative clauses and conjunctions. It is quite possible that the correct theory falls between that of Syntactic Structures, with no base-embedded and base-conjoined sentences, and that of Aspects, with all embedded and conjoined sentences in the base.⁷

In both of these cases important preliminary work was neglected. Major changes were presented without a thorough examination of reasonable alternatives, as above, and more generally, without a study of their mathematical consequences. A purely mathematical study may be necessary to see just what the implications of the theory are. And to make our conclusions plausible--to get beyond statements that something can be accounted for in a certain way to statements that it should be accounted for in that way--it is necessary to look at all the reasonable alternatives.

3. Mathematical aspects of the theory of Aspects. Early work in mathematical linguistics was concentrated on simple systems known to be empirically inadequate. The work was important for linguistic theory since it made it possible to pinpoint exactly where the inadequacies of various theories lay. It is a great relief to be able to write off some proposed theory for the very strong reason that it can be shown to be equivalent to some system known to be weakly or strongly inadequate. At the time of writing of Aspects, the mathematical properties of transformational grammars were unknown.

In the last few years this shortcoming has been remedied. In a series of extremely important papers Stanley Peters and Robert W.

Ritchie have made the syntactic theory of Aspects precise and have proved a number of critical theorems about systems of transformational grammars.⁸ This is not the place to explicate the results in detail, but I shall try to indicate the importance of the results for general syntactic theory.

The first result of Peters and Ritchie was that transformational grammars defined in the spirit of Aspects are weakly equivalent to Turing machines. That is to say, any language that can be defined by a Turing machine or unrestricted rewriting system can be defined by a transformational grammar and vice versa. This result is somewhat disconcerting. It shows that claiming that transformational theory provides a theory of possible natural languages is making no stronger claim than that natural languages are systems of some sort. Clearly this tells us nothing that we don't already know. Proponents of the standard theory of transformational grammar could retort that weak generative capacity was not very interesting anyway. But the next result showed that looking at other aspects of descriptive adequacy was of no help.

Peters and Ritchie undertook to study the effect of restricting the standard theory in various ways, starting with restrictions on the base. The first startling result that they were able to prove was that putting extreme restrictions of various sorts on the base made no difference whatsoever in the generative capacity of transformational grammars. Among the restrictions that they studied was one that was directly relevant to a hypothesis that is entertained by many linguists. The so-called Universal Base Hypothesis⁹ makes the claim that all languages have the same set of base rules. Peters and Ritchie were able to prove that the question whether all languages share the same set of base rules was not empirically decidable given (1) the overly powerful standard theory, and (2) the range of data that linguists considered relevant to judging grammars ('descriptive adequacy').¹⁰

This result has devastating consequences for the study of syntax. One corollary is that it is impossible to give any convincing arguments about the correctness of a set of base rules for a particular language. For if it were possible to give such arguments, then we would be able to refute the Universal Base Hypothesis by looking at the base rules of several languages, shown to be correct by the putative arguments, and noting that they were different. But then the Universal Base Hypothesis would have some empirical content. And that is just what Peters and Ritchie proved it does not have. Another corollary is that the controversy between 'generative' and 'interpretive' theories of semantics is unresolvable. Before backing up that statement, let us consider the controversy itself.

4. The generative-interpretive controversy. The mathematical study of phrase structure grammars led to the demonstration that they were inadequate as theories of natural language since they were too weak in their generative capacities. The mathematical study of transformational grammars has led to the result that they are too powerful to qualify as theories of natural language. On the one hand they fail to distinguish natural languages from arbitrary recursively enumerable sets. On the other hand, they are so powerful that they make it impossible to answer certain questions that ought to have empirical content; for example, do all languages share a common set of base rules?

It follows that systems more powerful than the standard theory will fail in the same way. Yet a major part of the research of the years since Aspects has gone into the development of theories that are even more powerful--i.e. weaker in their assumptions about possible grammars and languages--than the standard theory.¹¹

In the following paragraphs, I am not denying that the controversy between generativists and interpretivists has uncovered many interesting facts or that there are empirical issues involved in the controversy. What I am claiming is that within the framework of a theory that is powerful enough to encompass generative or interpretive semantics, it is logically impossible to resolve the issues. We need to restrict the theory or look to new data, or both.

The developments in syntactic-semantic theory since Aspects can be conveniently divided into two kinds: those which make the general theory less restricted, and those which provide more restrictions on the theory. The controversy between generativists (e.g. G. Lakoff, J. R. Ross, Paul Postal, J. McCawley) and interpretivists (e.g. Ray Jackendoff, Ray Dougherty, A. Akmajian, N. Chomsky) has been carried out with a concomitant weakening of the theory of syntax.

The standard theory incorporated a rather strict definition of transformations as well as a stringent hypothesis about their ordering. All transformations were presumed to operate cyclically. Since Aspects it has been argued that there are in addition to ordinary cyclic rules, rules that operate before the cycle (Lakoff, unpublished) and rules that are constrained to operate only on the last cycle (Ross 1967a) as well as rules that can operate at any point in a derivation (Ross 1967b). In addition to the constraints on insertion of lexical items countenanced in the standard theory and the rule features added to the theory by G. Lakoff (1965) it has been claimed that there are deep-structure constraints and surface-structure constraints (Perlmutter 1968). More recently G. Lakoff has argued that transformations are just the limiting case of much more general (and powerful) derivational and even transderivational constraints (G. Lakoff 1970). On the interpretivist side, the restriction of semantic projection rules to operation on deep structures (as envisaged in the standard theory) has given way to a

proliferation of types of interpretive rules: surface structure rules, cyclic rules, rules that add interpretations that were not represented in the deep structures at all (Jackendoff 1969, Dougherty 1969).¹²

It is thus evident that whatever troubles we are in by virtue of the excessive power of the standard theory are even worse in the main lines of recent developments. But it is possible to use the Peters-Ritchie results to show the insolubility of the controversy more directly.

One tenet of the generativist theory is that there is no distinction between semantic representations and so-called deep structures, that the base rules directly generate semantic representations and that they are moreover phrase-markers (G. Lakoff, forthcoming). Now, if we assume that it is possible to say anything--i.e. represent any conceptual structure--in any language, given time and patience and the possibility of paraphrase; then it will follow that for the generativist there must be a universal set of base rules. We could refute the generativist position by showing that there is no such universal set of base rules. It is thus important for proponents and antiponents of this view to find evidence for or against the universal base hypothesis. But Peters and Ritchie have shown that this is impossible.¹³

5. Restricting the power of transformations. Some of the research that has gone on since Aspects has been devoted to narrowing rather than extending the power of transformations. Thus Joseph Emonds' thesis (1969) argued that except for a certain well-defined class of 'root' transformations, all transformations were 'structure-preserving' in the sense that their outputs had to conform to the possibilities given by the base rules. John Ross's investigations (1967a) into constraints on transformations were of the same sort in spirit. What Ross attempted to do was to limit the power of transformational grammars by removing some ad hoc restrictions on particular transformations and giving them once and for all as general constraints on a class of 'movement' transformations. Postal's (1971) crossover constraint worked in the same way. More recently some linguists have attempted to limit transformational grammars by excluding deletions (Jackendoff 1969), but unfortunately the proposed limitations have gone together with an unknown amount of increase in the power of so-called interpretive rules.

Most of these proposed limitations on the power of transformations have been purely formal. Although it is quite possible that such formal restrictions may lead to fruitful results, it seems reasonable to ask whether we ought not also to place substantive restrictions on transformations. In the remainder of this paper I would like to report on some preliminary results that indicate that such substantive restrictions are necessary.

6. Why substantive restrictions on transformations are necessary.

It has been noted by many transformational grammarians that languages share many similarities at the level of base structures.¹⁴ But there are pervasive similarities among languages in their surface forms as well, as noted by such studies as Greenberg 1963, Allen 1964. Let us consider a few examples.

As far as surface order is concerned, there seem to be three basic types of languages, Verb-first, Verb-second, and Verb-last. That is, there are languages with the dominant surface orders VX, NP VX, XV but apparently none of the other logical possibilities: XV NP, NP NP VX etc.¹⁵ Since there is nothing in the formal definition of transformations that prevents us from rearranging constituents in any way we wish, the standard theory or its weaker variants make no prediction about surface orders. It is also evident that Emonds' theory, by itself, makes no such predictions. In that theory, it is possible to have any kind of deep structure, hence also any kind of surface structure. Emonds predicts that there are no languages in which there is a difference between the order of major constituents in base structures and all surface structures and clauses, embedded or not. Moreover, if differences between root and subordinate sentences do occur it is the order of elements in the subordinate sentences that will correspond to the order of the underlying base structures. The order of root sentences will be derived by the root transformations. Both of these predictions seem to be incorrect, and therefore also the theory on which they are based.¹⁶

It is evident that restricting deep orders will not make it possible to predict possible surface orders unless we restrict the possibility of re-ordering by transformations. Moreover, since the restrictions we seek must result in restrictions on the order of substantive categories like V, NP, the restrictions must refer to these categories, in other words, they must be substantive in nature and not purely formal.¹⁷

What form could a theory of substantive constraints on transformations take? One particularly strong and simple hypothesis is that there is a fixed finite list of major transformations made available to every language by the general theory. This idea is implicit in traditional crosslinguistic identifications of construction types like relative clauses, passive, conjunction, or imperative. It is necessary to make some such assumption for many arguments like those of Ross 1967b about Gapping (see Bach 1971). Moreover, it is trivial to show that the results of Peters and Ritchie do not follow in such a theory.

Let us see how a theory of universal transformations could be used to explain the facts about surface order just noted. Let us suppose that among the universal transformations there are two in particular: Topicalization and Subject-formation.¹⁸

Topicalization is a rule that allows the preposing of any NP in a sentence (subject to general movement constraints like Ross's complex

noun-phrase constraint, or Postal's crossover constraint). Thus Topicalization might have some such form as this:¹⁹

$$\begin{array}{ccc} X, \ NP, \ Y \\ 1 \quad 2 \quad 3 \Rightarrow \\ 2 + 1 \ \emptyset \quad 3 \end{array}$$

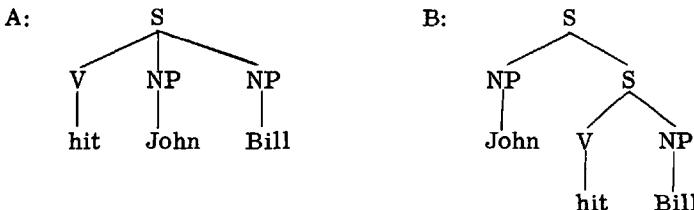
By Topicalization we will get sentences like these in English, German, and Japanese:

- (1) Mary I saw yesterday
- (2) Marie habe ich gestern gesehen
- (3) Mary wa boku ga kinoo mita

Subject-formation on the other hand is a much more limited rule. It allows the preposing of an NP that stands in the configuration V NP at the head of the sentence. By Chomsky-adjunction a new S-node is created to create a constituent corresponding to the traditional VP-constituent, which we may define (if need be) relationally. Thus the rule might look something like this:

$$\begin{array}{ccc} V, \ NP, \ X \\ 1 \quad 2 \quad 3 \rightarrow \\ 2*1 \ \emptyset \quad 3 \quad (\text{with * symbolizing Chomsky-adjunction}) \end{array}$$

Thus by Subject-formation we derive a structure like B from a structure like A:



One more assumption is necessary to our argument. A number of linguists (McCawley 1970, Muraki 1970, Bach 1971) have given evidence that languages like English and German have underlying structures with the verb in initial position. If we generalize this result (as does McCawley) then either all languages have an underlying VSO order, or languages belong to just two types in underlying structure: VX (English, German, Tagalog, Amharic) or XV (Japanese, Korean). I shall return to these two alternatives in a moment. Let us adopt the second

temporarily and assume that there are only two types of underlying base structures: VX and XV.

Now it is possible to derive the facts of surface order as well as some other facts, which constitute independent evidence for our assumptions. Let us list our assumptions:

- (1) Every language has a deep order XV or VX.
- (2) There is a rule of Topicalization with the SI: X, NP, Y
- (3) There is a rule of Subject-formation with the SI: V, NP, X and the effect of creating a VP constituent.
- (4) No other rules of Topicalization or Subject-formation are possible.

From these assumptions it will follow that the possible surface orders for arbitrary languages are these:

- (A) VX (no subject-formation rule)
- (B) NP V X (subject formation)
- (C) XV (no subject formation since the SI is never met).

A number of further consequences follow from these assumptions.

First, it will follow from the formulation of the Subject-formation rule that languages like Japanese will have no constituent corresponding to the VP constituent of English. Arguments that this is correct have been given by Masa Muraki (1970). Japanese does have a Topicalization rule but apparently no Subject-formation rule.

Second, it will follow that the sentences of every surface SVO language derive from underlying structures of the VSO type. As mentioned above, several linguists have given arguments to this effect. Our hypotheses lead us to expect further evidence for the correctness of this view. Let us consider one further piece of evidence for German.

Ross (1967a) argued that variant orders in Gapped sentences indicated that languages like Hindi and Amharic differed in their underlying structures and surface structures. Gapping could occur either before or after the rule of Verb-final. Hence there would be two possible derivations:

	S V O S V O		S V O S V O
Gapping	S V O S O	Verb-final	S O V S O V
Verb-final	S O V S O	Gapping	S O S O V

I do not know whether Gapping is a special case of Conjunction reduction (as has been argued by Sanders 1971, Koutsoudas, forthcoming). But in any case an argument quite parallel to Ross's can be used to

show that German has an underlying VX order. Whether or not conjunction reduction is different from Gapping, it applies in a directional way. If a string of conjoined constituents has identical elements on left branches then deletion is from left to right, if the identical elements are on the right, then deletion goes in the opposite direction. Thus we have in English:

John saw Bill and ~~John~~ heard Mary
 John saw ~~Bill~~ and heard Bill

Now consider some facts about conjunction reduction in embedded and non-embedded questions in German:

- (1) Ich frage, ob Hans komme oder Annchen
 'I asked whether Hans or Annchen was coming'
- (2) Ich frage, ob Hans oder Annchen komme
- (3) Kommt Hans oder Annchen?
 'Is Hans or Annchen coming?'
- (4) *Hans oder kommt Annchen?

If conjunction reduction operates as we have said, then it must be the case that we have two different orders at two different stages of a derivation in the embedded questions, but not in the non-embedded questions. If the underlying order for questions in German were NP V, both (3) and (4) should be grammatical as in these derivations:

	Hans kommt oder Annchen kommt
Reduction	Hans oder Annchen kommt
Verb-first	*Hans oder kommt Annchen
	Hans kommt oder Annchen kommt
Verb-shift	Kommt Hans oder kommt Annchen
Reduction	Kommt Hans oder Annchen

Since only one order is grammatical in the non-embedded questions like (3), we can conclude that verbs and subjects have in fact stood in only one left-to-right order throughout the derivation. Furthermore, if the underlying order were NP VX we should have only one possibility in the subordinate clauses.

	Ich frage, ob Hans komme oder Annchen komme
Reduction	Ich frage, ob Hans oder Annchen komme

But since both (1) and (2) are grammatical we must conclude that it is in the subordinate clauses that we have had a shift of order.

Notice that in Japanese only one order is possible in a reduced sentence:

Hans ka Ännchen ka kimasu ka

*Hans wa kimasu ka Ännchen ka

Since Japanese presumably has a verb-final order to begin with, this is what we expect.

Third, given our assumptions and the additional (independently motivated) assumption that rules are ordered, we can explain another difference between English and German surface order on the basis of rule ordering. Notice that when Topicalization applies to a sentence before Subject-formation, then the structure index of Subject-formation will not be met since the verb no longer stands at the head of the sentence. On the other hand, since Topicalization apparently has nothing to do with the position of the verb, applying Subject-formation before Topicalization will have no effect on the applicability of the latter rule.

Now compare English sentences like (1) with German sentences like (2).

- (1) Mary I saw yesterday
- (2) Marie habe ich gestern gesehen

In the English sentence two NP's precede the first verb. In the German sentence only one NP may precede the verb. We can account for this difference by the assumption that in English Subject-formation precedes Topicalization, but in German the opposite is true. After Topicalization, Subject-formation is blocked in German. This implies that the position of a subject NP in German at the front of a sentence can result from two possible rules. This consequence seems to be correct. Compare the two different intonations and possible contexts for German sentences like this one:

Hans ist nach Hause gegangen ‘Hans went home’
 (Wer ist nach Hause gegangen? ‘Who went home?’)
 (Was hat Hans getan? ‘What did Hans do?’)

Thus, we see that there is some independent evidence for the assumptions we have made to explain different surface orders on the basis of assumptions about two universal transformations.

I noted above that there is some question whether we should assume that all languages have an underlying VX structure or whether some like Japanese have an underlying XV structure. We are far from being

able to answer this question in any satisfactory way. But there is a certain amount of indication that accounting for the surface order of languages like Japanese by means of a transformational rule is not a totally wild speculation. Notice first of all that there is a certain asymmetry between VX and XV languages that remains unexplained. Why should the two underlying types be VX and XV rather than VX and NP VX or X V NP? Why should there be languages that turn from underlying VX order to surface XV order but not the converse? I have suggested elsewhere (Bach 1971) that the reason for this is that the list of universal rules has a Verb-final rule but not a corresponding Verb-first rule, but we still need to ask why this should be the case.

Let us recall that in the standard theory as revised in the last few years there are two kinds of transformations: cyclic and last-cyclic. It can also be shown that in the two languages known to have a Verb-final rule, Amharic and German, Verb-final must be a last-cyclic rule (Bach 1970, 1971). In fact we can account for the difference between Amharic and German by assuming that the rule is last-cyclic and ordered before and after the rule of Performative-deletion, respectively.²⁰ But it is possible to account for all the facts about Gapping and Conjunction-reduction in Japanese by the assumption that Japanese has an underlying VX structure, like all other languages, and that the rule of Verb-final in Japanese is cyclic. At the moment I do not have any way of distinguishing the two hypotheses about Japanese, except on the general grounds that we have mentioned. If all languages have the same types of underlying structures, then we have clearly stated a stronger hypothesis than if we allow two basic types. We could perhaps explain on this basis why languages like Japanese and English differ just in the position of the verbs and not the other constituents (see Muraki 1970). But that would require much more detailed theories than we have at present.

7. Conclusion. In the preceding pages I have tried to do several things. First and most important, I have emphasized the importance for syntactic theory of the mathematical studies of transformational grammars by Peters and Ritchie. I have argued that we are at present in a situation very similar to that of the early fifties in that a great deal of debate is carried on about essentially unresolvable issues. But the work of Peters and Ritchie can show us in which direction to move: we need to find heavy restrictions on the power of transformations and we need to widen the range of relevant data. I have suggested that placing heavy substantive restrictions on transformations can help us out of the present impasse. And I have shown how a theory of universal transformations can make data across languages available for settling questions of linguistic theory.

NOTES

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¹For a review of the backgrounds of the Chomskyan revolution see Bach 1965a. Readers who think that the differences between Chomsky and his predecessors have been exaggerated would do well to read the protocols of the Third Texas conference on English linguistics (Hill 1962).

²In drawing parallels between the present situation and that of 1957 I have drawn upon remarks made by Stanley Peters in several lectures. I would like to acknowledge here my debt to him. Many of the ideas of this paper owe their origin directly or indirectly to discussions with him.

³In fact, several. I select one of the two main models for the following discussion. Selection of the other does not change the principal conclusions in any way.

⁴See for example Chomsky's remarks on pp. 60-62 and fn. 37, p. 208 of Aspects.

⁵Chomsky himself makes clear the speculative nature of many parts of Aspects.

⁶Perlmutter's argument is not quite as strong as he says. He argues that it is impossible to state deep-structure constraints in a theory that operates with transformation-markers rather than generalized phrase markers. But without severe restrictions on the possible relations between transformation-markers and semantic rules this argument simply does not go through. At best it is possible to argue that the theory with generalized phrase markers rather than transformation-markers is more restricted and hence preferable. This point is made in the author's preface to the Japanese edition of my Introduction to Transformational Grammars, translated by Kazuko Inoue, Tokyo 1969.

⁷There have been repeated attempts to show that relative clauses are to be derived from conjoined sentences, e.g. Bach 1965b, Annear 1967. Most of these attempts fail on the question of assigning a particular order to the conjuncts and in making a plausible choice of conjunction. But perhaps they are to be derived from sets of sentences.

⁸See Peters and Ritchie 1969, forthcoming a, forthcoming b, and Peters 1970.

⁹Enunciated or speculated about by such linguists as Lakoff 1965, Bach 1968, and Fillmore 1968.

¹⁰Peters and Ritchie (forthcoming b) proved that it follows logically from the standard theory of transformational grammar (1) that there

is a grammar with a fixed (universal) base that generates any recursively enumerable language (of which natural languages must form a proper subset), and (2) that given any language and any requirements of descriptive adequacy of the sort customarily set by linguists, there is a grammar with a fixed base meeting these requirements. If the existence of the universal base follows logically from the theory, then the Universal Base Hypothesis cannot have any empirical content.

Notice that we cannot appeal to simplicity in the technical sense, since we have no *a priori* notion of the correctness of a simplicity metric and, crucially, the correctness of a simplicity metric must be judged on the basis of whether or not it leads us to choose descriptively adequate grammars.

¹¹There is an unfortunate confusion that results from the pairs of opposites 'weak' and 'powerful', and 'weak' and 'strong'. One theory of language is weaker than another if it makes fewer assumptions about languages--assuming they are comparable at all. Thus, the theory of transformational grammars is weaker than the theory of finite-state grammars in this sense. But in the other sense a finite-state grammar is weaker than a transformational grammar, since there are languages that can be defined by the latter but not the former.

¹²I do not wish to give the impression that these writers are unaware of the problem of trying to restrict the theory in some way to compensate for the added power of the various new devices; indeed, some--e.g. Perlmutter 1968, Jackendoff 1969--explicitly discuss this problem.

¹³Notice that the assumption that translation is possible amounts to no more than the claim that all languages are equally complete--in Archibald Hill's formulation (1958)--in their potentials of expression. It says nothing about what is an obligatory or optional category or about what can be thought by speakers of one language or another. Notice too that if one takes the position that the semantic representations of different languages are necessarily different and that the semantic representations are directly given in the base then one is committed to a denial of the Universal Base Hypothesis. But either position is devoid of empirical content at present.

¹⁴See e.g. Chomsky 1965, Bach 1968.

¹⁵A possible counterexample, according to James Tai, personal communication, is Mandarin Chinese. Tai has considered the hypothesis that Chinese is a basic Verb-final language with a surface order derived by postponing indefinite NP's.

¹⁶In Bach 1970 I presented evidence that Amharic is a language with an underlying VX (or NP VX) order which is transformationally changed to XV by a rule putting the verb to the end of all clauses. In Bach 1971 I argue that facts about questions are incompatible with the assumption that the verb-end order of subordinate clauses in German is the underlying order.

¹⁷The standard theory does make some predictions about deep order if we include in the theory the restriction that the basic grammatical relations must be definable on the deep structures of arbitrary languages, as seems to be suggested by Chomsky 1965. But the predictions appear to be wrong. If 'subject-of' is defined as the NP dominated by S, the first rule of every grammar must have one of two forms $S \rightarrow NP\ VP$ or $S \rightarrow VP\ NP$. Taking this prediction with the two possibilities for verb-phrase rules, the standard theory predicts that we can have languages of these possible deep order types: SVO, SOV, VOS, OVS. Of these, there seem to be no examples of the last two and there seem to be examples of VSO, a type which is ruled out completely by the theory.

¹⁸Fillmore 1968 proposes something like the following hypothesis about Topicalization and Subject-formation but within a different framework. My hypothesis differs from his in two respects: I do not follow the theory of case grammar for the base structures, and more crucially I assume that the base structures of all languages have a specified left-right order. Moreover, it is necessary to our claims to disallow as well as to allow certain putative rules, see Bach 1971.

¹⁹I leave open the question whether Topicalization involves an intermediate step in which a pronoun is left behind, as is argued in Sanders and Tai, forthcoming.

²⁰See Ross 1970 for defense of the view that sentences like John is a thief are derived from structures like this, I ASSERT to you that John is a thief.

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P R E L E X I C A L S Y N T A X

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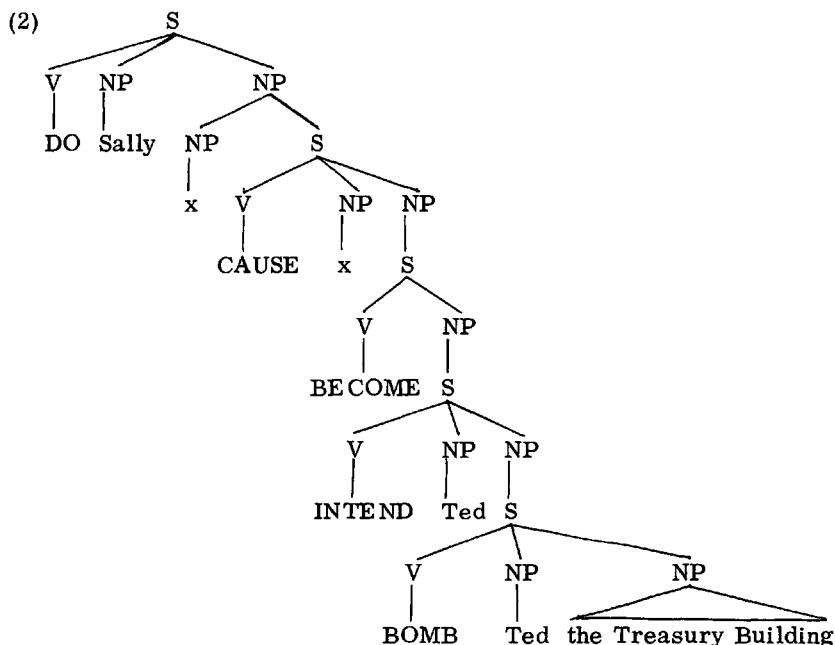
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Abstract. The 'generative semantic' conception of grammar, which rejects the familiar syntax/semantics and transformation/semantic-interpretation-rule dichotomies and treats the relation between content and surface form as mediated by a single system of 'derivational constraints', is sketched informally and illustrated, with special attention given to (i) the notion of 'possible lexical item' and interlinguistic differences as to what lexical items are possible, (ii) cases where an adverb modifies a semantic constituent that is not directly represented in surface structure, and (iii) verbs that refer to means and instruments.

This paper treats the questions of what kinds of rules are needed to specify how the content of sentences is related to their surface form and and of how those rules interact with each other. There is fairly general agreement that the surface structure of a sentence is appropriately represented by a labeled tree, i.e. that the surface structure of a sentence consists not merely of a sequence of morphemes but of the larger units which those morphemes are grouped into and that the category membership of those units is significant. There is no general agreement as to the nature of semantic structure; but I will maintain that the content of a sentence token is likewise appropriately represented as a labeled tree, though the ultimate units of a semantic structure will not be morphemes but rather some kind of semantic units. The semantic units which are encoded in a lexical item need not be all together in semantic structure, as they would be if the conception of the content of a lexical item as a bundle of feature specifications were correct. For example, the elements of meaning encoded by the word persuade in a sentence such as

- (1) Sally persuaded Ted to bomb the Treasury Building.
are combined semantically with different constituents of semantic

structure. Assuming for the moment that it is correct to speak of persuade as contributing notions of 'doing', 'causing', and 'intending' to the content of (1), those notions are not combined in a homogeneous fashion. (1) does not make reference to disembodied intention but to intention on Ted's part; it does not make reference to disembodied 'doing' but to Sally's doing something; and it does not make reference to disembodied causation but rather to what Sally does causing Ted to intend to bomb the Treasury Building. The elements of content to which I have been referring are not features of the sentence but are relations between items of content that figure in the sentence, and to specify the semantic structure of a sentence, it is necessary to not merely indicate which such elements are present but also indicate what items those relations relate. A tree diagram such as¹

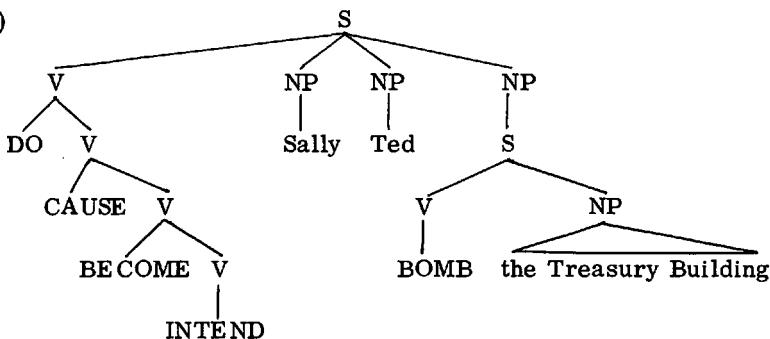


accomplishes that.

The relationship between (2) and any syntactic 'deep structure' for (1) that has been proposed by those who accept a dichotomy between syntax and semantics, which I do not, is extremely complicated. However, the relationship between persuade and (2) is far from arbitrary and has much in common with the relationship between other verbs and the semantic structures of clauses of which they are the main verb. The elements of meaning that are encoded in persuade are 'DO', 'CAUSE', 'BECOME', and 'INTEND', each of which is the main predicate

of the complement of the preceding one in (2). There are many other verbs which encode two or more predicates, each of which is the main predicate of the complement of the preceding one. For example, kill encodes 'DO CAUSE BECOME NOT ALIVE' and apologize encodes 'REQUEST FORGIVE'. The common characteristic in the relationship of each of these verbs to the semantic structures which it is used in expressing can be captured in the form of a rule which relates stages in a derivation that leads in steps from semantic structure to surface structure, specifically, a rule that allows the predicate of a clause to be adjoined to the predicate of the next higher clause. Successive application of that rule to (2) would convert it into²

(3)



which contains a constituent 'DO CAUSE BECOME INTEND' which corresponds to the verb persuade. I note that this rule, which is known as Predicate-raising, does exactly the same kind of thing as do the 'adjunction transformations' of syntactic studies that assume a distinction between syntax and semantics, only it does it to a structure whose ultimate elements (or at least, some of them) are not morphemes but semantic units. It remains to be seen that all combining of elements of meaning into lexical items can be reduced to the interaction of a limited number of transformations of the types (permutation, copying, adjunction, deletion, insertion) which are normally recognized. Finding a system of transformations that yield combinations of semantic material which are exactly the possible lexical items of English is a program of research of which so far only fragments have been carried out. However, the hypothesis that an adequate grammar of any language will involve such a system of transformations currently appears to me to be the only approach to linguistic structure that has much chance of providing an explanation of the contribution of individual lexical items to the content of sentences in which they appear.

In the last paragraph, I used the term 'possible lexical item'. This term is worth dwelling on before I proceed. Since Predicate-raising and the various other transformations that combine semantic material

can interact in an infinite number of ways, there is an infinite number of combinations of semantic material that they can yield, although only a finite number of them correspond to actual lexical items of English. The theory that I am sketching here predicts that those combinations which can be derived by Predicate-raising, etc. but to which no actual lexical item of the language corresponds are accidental gaps in the lexicon: combinations of semantic elements which purely by accident do not correspond to any existing word and for which a word could be introduced if the need arose. One example of such an accidental gap is the combination of semantic elements which could arise if the constituent 'Bill is alive', which I said was contained in the semantic structure of John killed Bill, were replaced by 'Bill is bowlegged'; while there is to my knowledge no English word meaning 'cause to cease to be bow-legged', there is nothing I can see to prevent such a word from being introduced into the language, and I indeed would not be surprised if I were to find that such a word did in fact exist in the technical jargon of orthopedists. The theory predicts that those combinations of semantic elements which cannot be derived by applying the transformations of the language to well-formed semantic structures not only do not correspond to existing lexical items but indeed are systematically excluded from the lexicon. Certain impossible lexical items are impossible because of principles governing what transformations can do that are valid throughout the language. For example, Ross's 'complex NP constraint' (Ross 1967), which disallows derivations in which a transformation moves something out of a 'complex NP', not only explains why one cannot say

- (4) a. *How many movies have you met the man who directed?
- b. *Which official do you believe the rumor that Phil plans to assassinate?

but also explains why there is no lexical item meaning 'kiss a girl who is allergic to', i. e. why there is no word *flimp which appears in sentences like

- (5) *Bert flimped coconuts.

that would be paraphraseable as 'Bert kissed a girl who is allergic to coconuts'. The impossibility of *flimp follows from the fact that any application of the transformation needed to combine 'a girl who is allergic to' with 'kiss' would cause the derivation to violate the complex NP constraint. There appear also to be impossible lexical items whose impossibility follows not from some general principle that would exclude the transformations needed to derive them but rather from the details of what prelexical transformations the language happens to have.

Here what I say is highly tentative, since I do not have a clear picture of what the relevant transformations are, but there are systematic differences between Japanese and English as regards what lexical items are possible. For example, Japanese but not English has compound nouns whose elements each have their own reference, e.g. (examples from Chaplin and Martin 1967):

- (6) a. Ano tihoo wa kansyo dotira mo hidoi 'In that region, the heat and cold are both severe'.
- b. Yoku nite iru oyako desu 'It is a parent and child who resemble each other a lot'.
- c. Koosi o kubetu suru 'We distinguish public and private'.

While English has compound nouns that are paraphraseable by a coordinate structure, e.g. secretary-treasurer and radio-phonograph, the two parts of the compound have the same reference: a secretary-treasurer is a single individual who is both secretary and treasurer. However, Japanese oyako refers to one individual who is oya 'parent' and another who is ko 'child'. If my judgment that this fairly common compound type in Japanese is systematically excluded in English is correct, then Japanese and English differ in the way in which they allow coordinate structures to be combined into a single lexical item.

I should also note that prelexical transformations include not only transformations which combine semantic material into possible lexical items, but also perfectly ordinary transformations such as EQUI-NP-Deletion and there-insertion. One piece of evidence that EQUI-NP-Deletion applies prelexically is that apologize has Equi-NP-deletion under identity with its subject rather than its indirect object: in

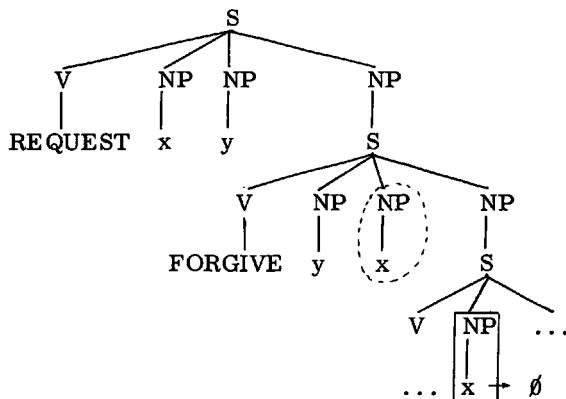
- (7a.) Tom apologized to Sue for kissing Lucy.,

the deleted subject of kiss is Tom, not Sue. If apologize has the semantic structure proposed by Fillmore (1969), namely

- (7b). x apologizes to y for S = REQUEST (x, y, FORGIVE (y, x, S)),

then the subject of the lowest sentence would be deleted under identity with the indirect object of FORGIVE.³ But since the indirect object of FORGIVE is identical to the subject of REQUEST and thus to the subject of apologize, there appears to be EQUI-NP-Deletion under identity with the subject of apologize:

(7c).

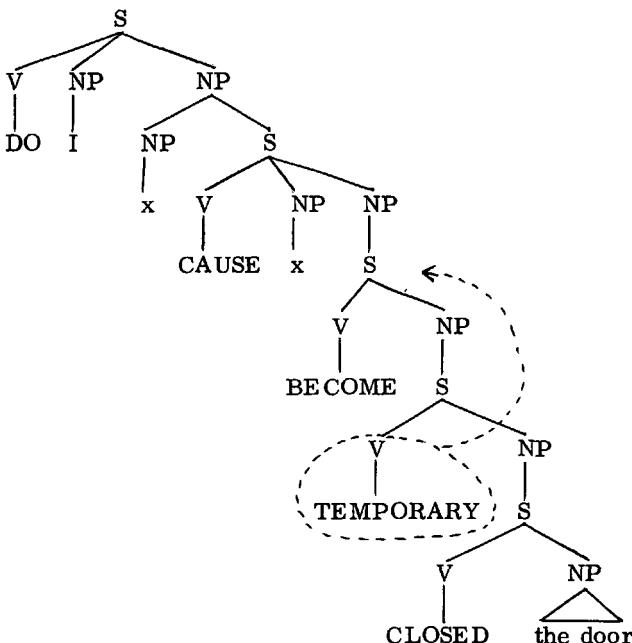


I have so far said nothing about the important topic of how one justifies putative semantic structures.⁴ One important type of evidence that can be offered in support of a hypothesized constituent of a semantic structure is the possibility of having an adverb that modifies that constituent rather than any constituent that is present in surface structure. Consider, for example,

- (8) a. The door opened, and then I closed it again.
- b. *The door opened, and then I kicked it again.
- (9) a. I closed the door temporarily.
- b. *I kissed Susan temporarily.
- (10) a. I lent Tom my bicycle until tomorrow.
- b. *I showed Tom my bicycle until tomorrow..

Again in (8a) does not modify the whole clause I closed it but rather the clause ‘it is closed’ which would be posited in an analysis of the transitive verb close as the causative of the stative adjective closed. (8a) presupposes that the door was once closed but does not presuppose that I or anyone else had ever closed it before. By contrast, kick is not open to an analysis as a causative of anything, and the clause I kicked the door again only allows an interpretation which presupposes that I had kicked the door before; (8b) is odd because again is not in a context which would provide grounds for that presupposition. (9a) confirms the analysis of transitive close as a causative of the stative adjective closed: what (9a) asserts to be temporary is the door’s being closed, not my action in closing it. For there to be derivations of sentences like (8a) and (9a) within the framework adopted here, it is necessary that there be a transformation which may apply before predicate raising and which raises ‘adverbial’ elements over certain predicates:

(11)



If this approach is correct, then the distribution of temporarily will be predictable without reference to idiosyncracies of any lexical items: it will appear in those sentences which are derivable from a well-formed semantic structure (thus, one in which 'TEMPORARY' is predicated of something that can coherently be said to be temporary, i.e. either a state or an activity) by moving adverbs and combining predicates in the ways that the grammar allows. Similarly, until tomorrow will occur in those sentences that are derivable from a structure containing a clause which until tomorrow can coherently modify and which allow it to be moved into the appropriate higher clause. (10a) provides evidence that lend encodes a structure containing 'POSSESS': until tomorrow in (10a) gives the time during which Tom's possession of my bicycle is to take place, not the time during which the lending takes place. The analysis of until tomorrow as originating in a clause 'Tom possess my bicycle until tomorrow' also explains why until tomorrow can occur in a clause with a past tense verb in (10a), even though it normally requires the tense of its clause to be future:

- (12) a. *Jack stayed in his room until tomorrow.
 b. Jack will stay in his room until tomorrow.

By the same token

- (12) c. The sheriff of Nottingham jailed Robin Hood for four years.

is ambiguous as to whether for four years modifies the whole clause (in which case it would refer to repeated jailings) or modifies the 'Robin Hood be in jail' part of the semantic structure that would be set up under the analysis of the verb *jail* (Binnick, *in press*) as 'cause to be confined in jail'. There is an interesting syntactic correlate to the property of modifying a clause which disappears through its main verb being incorporated into another verb, namely that such adverbs cannot be moved to the beginning of the clause.

- (13) Again I closed the door.

can only refer to a second action of closing the door, not to merely the door's being again closed:

- (14) Temporarily I closed the door.

if possible at all, can only be a past habitual (e. g. I worked temporarily as a doorman) and cannot refer to my making the door be temporarily closed;

- (15) *Until tomorrow I lent Tom my bicycle.

is ungrammatical; and in

- (16) For four years the sheriff of Nottingham jailed Robin Hood.

for four years can only be the period over which the sheriff (repeatedly) jailed Robin Hood, not the sentence which Robin Hood was to serve. The analysis proposed here at least distinguishes the prepositional adverbs from the nonprepositional ones, and, depending on details of the adverb-raising transformation that are not yet worked out, it may even explain why adverbs that modify part of the meaning of a word cannot be preposed. Keyser (1968) argues convincingly that adverb-preposing moves an adverb to a position on the same 'level' of IC-structure. Adverbs originate in the clause immediately above the clause that they modify. If adverb-raising makes an adverb a constituent of the next-higher clause, then after adverb-raising, an adverb which modifies a piece of the meaning of the main verb of a clause will be lower in the tree than an adverb modifying the whole clause would be.

I turn now to a topic, the fragmentariness of my understanding of which may or may not reflect inadequacies in my theory, namely of verbs that make reference to means and instruments. I will discuss especially hammer and nail. First, a word about the relation between those verbs and the homophonous nouns. Hammering need not be done with a hammer, but nailing has to be done with nails:

- (17) a. He hammered the nail with a rock.
 b. *He nailed the proclamation to the door with rivets.

Thus, the meanings of the verb hammer and the noun nail must be more basic than those of the noun hammer and the verb nail. In what follows, I will treat the meanings of the verb hammer and the noun nail as if they were unanalysable, since, while the verb hammer at least is surely analysable into more basic predicates having to do with striking and repetition, whatever internal structure they may have plays no role in what follows here.

Besides the transitive verb nail, there is also something that looks like its past participle

- (18) The proclamation is nailed to the door.

but really is not, since something is nailed to the door just as long as there are nails through it that hold it to the door, regardless of whether anyone nailed it. If the nails had just appeared out of the blue through divine intervention, it would be correct to say that the proclamation is nailed to the door; but it would not be correct to say that God had nailed it there: the latter would be appropriate only if God had assumed material form and driven the nails in by hammering them. The range of things that can appear with nailed is as follows:

- (19) a. The proclamation is nailed to the door.
 b. The boards are nailed together.
 c. The door is nailed shut.
 d. The flagpole is nailed at a 45° angle to the wall.
 e. The boards are nailed into a rectangle.

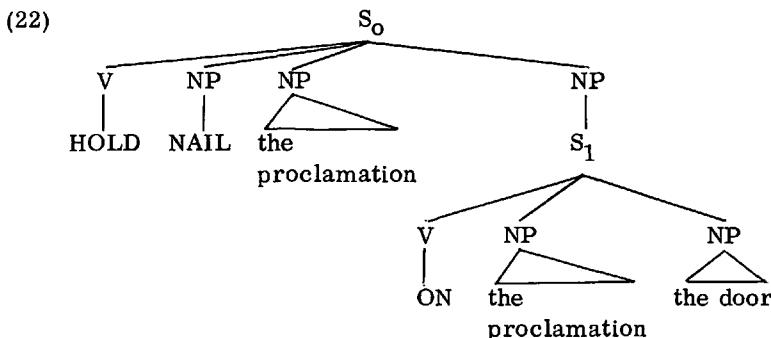
The items that can appear with nailed appear to be any and all items that make up sentences describing a configuration that is maintained thanks to the nails:

- (20) a. The proclamation is on the door.
 b. The boards are together.
 c. The door is shut.
 d. The flagpole is at a 45° angle to the wall.
 e. The boards are (form?) a rectangle.

The subject of nailed must be something whose stability is maintained by the nails; compare (19d) with

- (21) *The wall is nailed at a 45° angle to the flagpole.

I thus propose that the semantic structure of the sentences (19) is along the lines of:



where 'HOLD' is a three-place predicate relating an instrument, an object to which the instrument is 'applied', and a state in which the instrument maintains the object. The prelexical transformations involved in the derivation of (19) are EQUI-NP-Deletion, which deletes the subject of S_1 under identity with the object of S_0 , and some transformation which combines the instrument, NAIL, with HOLD. It is tempting to conjecture that there is prelexical passivization here, yielding an intermediate stage of the form 'the proclamation is held by nails on the wall', and that NAIL is then combined with BE HOLD by the same prelexical transformation that combines generic objects with verbs in the derivation of occupation terms such as dentist, if one accepts an analysis in which dentist encodes a semantic structure containing 'treat teeth'. The only evidence I know that has any bearing on that conjecture is the fact that not only in nailed but in the entire range of such words, the adjective in question consists morphologically of the noun for the instrument plus the past participle morpheme, and the past participle morpheme is the normal accompaniment of the passive construction. At any rate, there is an apparently productive relationship between nouns denoting instruments that can hold things in a fixed configuration, homophonous transitive verbs, and adjectives that are homophonous but for the extra past participle morpheme:

- (23) nail, screw, rivet, glue, paste, tack, staple, (paper) clip, pin, skewer, seal, tape, stitch, muzzle, shackle, chain, fetter, gag, handcuff, manacle, strap, lock.

The productivity of this relationship can be captured in a grammar by allowing the transformations sketched above to combine not just complexes of semantic material with HOLD but actual lexical items, and having a rule that would delete HOLD, thus yielding a derived verb

homophonous with the original noun. The transitive verbs appear to allow, besides an agent NP, exactly the same material that the corresponding adjective allows:

- (24) a. He nailed the proclamation to the door.
- b. He nailed the boards together.
- c. He nailed the door shut.
- d. He nailed the flagpole at a 45° angle to the wall.
- e. He nailed the boards into a rectangle.

It is thus reasonable to analyse (24) as some kind of causatives of the corresponding sentences (19). The observations made above about miraculous insertion of nails show that they are not just causatives. Indeed, even within the realm of human capacities, it seems funny to use (24a) to describe one's inserting the nails by any means other than hammering them, e.g. getting the nails into the board by putting a large solid object on the heads of the nails and sitting on it. Similarly, it would be funny to use

- (25) He pasted the photograph into the album.

to describe an outlandish way of causing the photograph to be pasted into the album, e.g. placing the photograph on top of a page in the album and then bombarding it with subatomic particles in such a way that its rear surface turns into paste. To paste a photograph into an album, one must not merely do something that causes it to be pasted into the album but do a standard kind of thing. I will leave up in the air the question of how to indicate this notion of 'standard action' in semantic structure and also the question of whether it may sometimes or perhaps even always be predictable on the basis of other things that an action must be 'standard'.

The range of constituents that can be combined with hammer is much broader than the range that can be combined with nail:

- (26) a. He hammered the nail into the board.
- b. He hammered the nail through the board.
- c. He hammered the metal smooth/flat/thin/soft/shiny.
- d. He hammered the flowerpot (in)to pieces/smithereens.
- e. He hammered the metal into a cylinder/disk/frying-pan.
- f. He hammered a groove into the metal.
- g. He hammered the dent out of the fender.
- h. He hammered the gold onto the sign.
- i. He hammered the shine off of the fender.
- j. He hammered a hole in(to) the wall.
- k. He hammered a 6-inch disk out of the wall.
- l. He hammered the boards apart.

In all of these sentences, hammer is combined with constituents that match the parts of a sentence which describes something that is brought about by hammering the object in the appropriate way:

- (27) a. The nail is in the board.
- b. The nail is through the board.
- c. The metal is smooth/flat/thin/soft/shiny.
- d. The flowerpot is in pieces/smithereens.
- e. The metal is (has become) a cylinder/disk/frying-pan.
- f. A groove is in the metal.
- g. The dent is not in the fender.
- h. The gold is on the sign.
- i. The shine is not on the fender.
- j. A hole is in the wall.
- k. A 6-inch disk is no longer in the wall.
- l. The boards are apart.

Curiously, certain states which can be brought about by hammering an object in the appropriate way cannot be referred to by sentences of the form (26). For example, it is possible to hammer something in such a way as to make it beautiful or ugly or dangerous or safe (by either creating or removing sharp edges); but the following are ungrammatical:

- (28) *He hammered the metal beautiful/ugly/dangerous/safe.

The acceptable adjectives appear to denote ‘objective’ properties, the unacceptable ones ‘subjective’ properties. While the adjectives denoting shapes, which surely are ‘objective’ properties, are unacceptable:

- (29) *He hammered the metal cylindrical/circular/spherical.,

the possibility of sentences such as (26e) suggests that this may be merely a case of suppletion involving adjectives and corresponding prepositional phrases. One weird restriction about which I have nothing to say is illustrated by

- (30) He hammered the rod straight/*bent.,

and one fact which may indicate that my mention of ‘objective’ properties in connection with (28) is wrong is the ungrammaticality of

- (31) *He hammered the reflection of City Hall off of the fender.,

which may indicate that hammer requires a property of the object itself, not of the object in relation to its environment.

Essentially the same range of items appears with the verbs (32), with the qualification that certain combinations are impossible due to the impossibility of performing the action so as to bring about the effect in question, e.g. one cannot sift flour out of a disk or to smithereens:

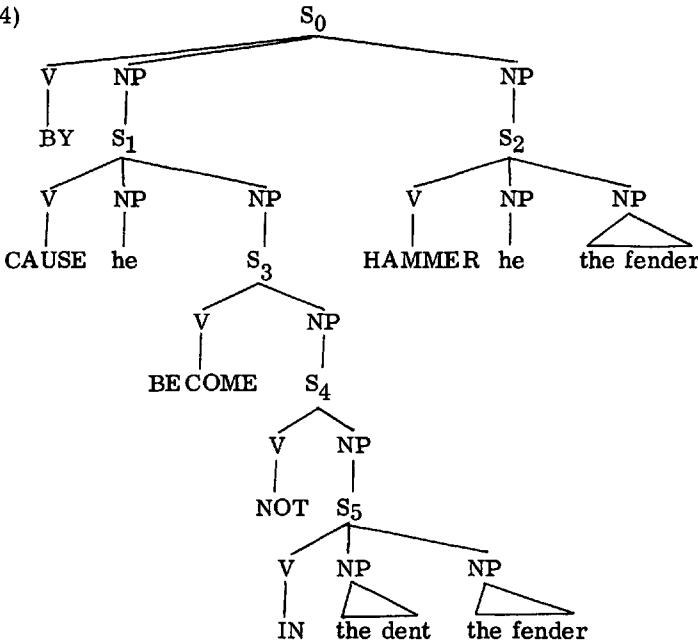
- (32) hammer, file, sand(paper), saw, sift, plane, iron, roll (as with a rolling pin), brush, comb, rake, mop, sponge, pound, knead, beat (eggs), polish, sweep, scrub, wipe.

Under the assumption that the meaning of the verb hammer plays the same role in all the sentences of (26), the semantic structure of those sentences would have to be something along the lines of 'x causes S by hammering y', e.g. 'he causes the metal to become smooth by hammering it'. One interesting characteristic of these sentences is the prepositions that appear in them: into in (26f), out of in (26g), onto in (26h), and off of in (26i). These are the same prepositions that occur in sentences referring to motion:

- | | |
|----------------------------------|---------------|
| (33) a. He ran into the kitchen. | BECOME IN |
| b. He ran out of the garage. | BECOME NOT IN |
| c. He jumped onto the table. | BECOME ON |
| d. He jumped off of the roof. | BECOME NOT ON |

In (26f-i), the groove, dent, gold, and shine do not move. The one thing that is common to (33a-d) and (26f-i) is that they involve locative relations coming into being (in (26f) it becomes the case that the groove is in the metal, and in (33a) it becomes the case that he is in the kitchen) or ceasing to be (in (26g) the dent ceases to be in the fender, and in (33b) he ceases to be in the garage), and the prepositions match the relation that comes into being or ceases to be: into is used for the 'in' relation coming into being, out of for its ceasing to be, onto for the 'on' relation coming into being, and off of for its ceasing to be. Thus, subject to the qualifications mentioned in connection with (28-31), the range of sentences involving hammer and the other verbs of (32) can be predicted on the basis of derivations that I conjecture are along the following lines, although here the analysis is highly tentative. Given a semantic structure such as (34), predicate-raising combines IN with NOT and then combines NOT-IN with BECOME, the resulting combination being realizable as the preposition into. Subject-raising raises the dent into S_1 , thus making it the derived object of CAUSE, EQUI-NP-Deletion (as in He made the metal smooth by hammering it) deletes the subject of HAMMER, a highly suspect transformation deletes the object of HAMMER under God knows what identity condition with S_3 , Predicate-raising combines CAUSE with BY, and a transformation hereby christened Means-incorporation replaces BY-CAUSE by the remaining verb (HAMMER) of S_4 .

(34)



I have discussed hammer and nail in the hope of thereby making clear what will have to be done to decide on the merits of the conception of grammar that I have sketched here or of any alternative theory. The analysis of hammer that I gave requires detailed justification, and I gave none because I so far have none worth speaking of. One kind of justification needed is a demonstration that the various steps into which I decomposed the relation between (26) and their meanings would recur in the relationships of other types of clauses to their meanings; until analysis of other lexical items shows that to be the case (or does the same for an alternative and superior treatment of hammer), my claim that the means which a language has at its disposal for combining semantic units into lexical items is a system of transformations will not have much support. In addition, the restrictions on hammer noted in connection with (28-31) and the restriction of transitive nail to a 'standard action' do not appear to follow from anything in my analysis, and it is not even clear that they can be expressed within my framework. Any analysis which shows these restrictions to follow from something else has a lot going for it.

To adequately account for the relationship between content and surface form, a grammatical theory must provide an answer to the question of how languages allow semantic units to be combined into lexical items, and if some alternative theory which upholds a distinction between syntax and semantics allows a correct answer to be given to

that question which the theory sketched above does not, the division of a grammar into syntactic and semantic parts will (at last) receive some justification. I have rejected the distinction between syntax and semantics and between transformations and semantic interpretation rules not so much out of certainty that they are wrong as because they have been assumed gratuitously, they have been applied arbitrarily, and even if they are correct, they can be shown to be correct only on the basis of the consequences of not accepting them.

NOTES

¹Capitalization is used to distinguish semantic elements from corresponding morphemes. I have ignored the semantic structure of the various NP's. I have also snuck into (2) an item which did not appear in the above discussion, namely 'BECOME', which appears as a consequence of my claim (McCawley, *in press*) that the notion of 'causing' which is relevant here is a relation between two events rather than between an event and a state; Ted's intending to bomb the Treasury Building is a state, and its coming to be the case that he intends to bomb it is an event. Regarding the verb-first order of the nodes of these trees, see McCawley 1970.

²The subject of BOMB is missing from (3), since Predicate-raising is in the cycle, and thus EQUI-NP-Deletion would have applied to the INTEND-clause before INTEND was adjoined to BECOME.

³This statement rests on the assumption that EQUI-NP-Deletion is in the cycle. For an elementary treatment of the notion of 'cycle', see the first section of McCawley 1970.

⁴A fairly detailed treatment of this topic is found in McCawley, *in press*.

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SOME PROBLEMS FOR CASE GRAMMAR

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1. Several years ago, from this platform, I presented a paper with the title 'A proposal concerning English prepositions'.¹ That was the first public exposure of an effort that a few months later resulted in a longish paper called 'The case for case'.² I suggested in these papers that a new order of concepts should be incorporated into the theory of transformational grammar; I spoke of 'deep structure cases', and my hope was that their existence could be discovered and justified by syntactic criteria and that their presence in underlying representations of sentences would have the effect of reducing the burden of the semantic interpretation component of a grammar. In spite of an over-exuberant final section in 'The case for case', I thought of my work, not as a proposal to eliminate deep structures altogether, but as an effort to find a level of syntactic structure which was deeper than that offered by the then standard theory. My position was what would now be called 'deep structure interpretivist'; and since my efforts were largely directed toward the classification of lexical items and the analysis of complement patterns of ordinary verbs and adjectives, it was of the sort that today would be called 'lexicalist'.

In his chapter on 'residual problems' near the end of Aspects of the theory of syntax,³ Chomsky reminds us of the failure of the theory presented in that book to deal with the fact that 'in some unclear sense' there is something in common between the me of John strikes me as pompous and the I of I regard John as pompous. There are semantic functions of noun-phrases which are not assignable to their syntactic positions on either the deep-structure or the surface-structure level. My suggestion in those early papers was that the notion of deep structure could be recast in such a way that certain sorts of semantic functions of noun-phrases could be represented directly and that the

structuring of sentences according to which they can be said to have subjects and objects should be taken care of by means of the transformational apparatus of the grammar; my hope was that these semantic functions would turn out to include those mentioned by Chomsky in the 'residual problems' chapter.

The deep case proposals derived more directly from an interest in languages that have case systems in their noun morphology. I was familiar with the classical grammar tradition of identifying one at a time the cases in which nouns could be inflected and listing with each case the 'uses' to which it could be put. As a generative grammarian looking at this tradition, I surmised--in the way that generativists do--that where our ancestors went wrong was in confusing what was 'to be explained' with what ought to be taken as 'given'. In that earlier view, what was taken as given was the information that the language has such-and-such cases, and what the grammarian needed to explain was how each of the cases could be used. We should reverse this, I assumed, and should take the 'case uses' as basic and regard the observable 'case forms' as derivable from them by the rules of the grammar.

I found encouragement in this ambition by the observation that the case uses had a lot in common between one language and another: one man's 'Dative of Person Affected' was another man's 'Accusative of Person Affected', and one man's 'Ablative of Personal Agent' was another man's 'Dative of Personal Agent'. Because of this apparent commonality across languages, it seemed to me that the case uses should be posited for all languages, including then those which lacked morphological case inflections altogether. By this being done, the same sorts of underlying semantic functions could be seen as realized in the form of case endings in one language, as prepositional or postpositional constructions in another, or in some quite different way in a third.

I have learned a few things since those days: I now know what 'ergative' means; from a number of extremely polite colleagues I learned about the kāraka theory of Pāṇini; I have become somewhat more conscious of the importance which semantic functions of the sort which have interested me have had in non-transformationalist but multi-level theories of grammatical structure; and, more importantly, I have in the meantime encountered an exceedingly large number of descriptive problems that turned out to be intractable within the model as I had been conceiving it.

I believe to this day that the basic ideas were not all wrong, in spite of the fact that most of the specific analyses I proposed in those first papers were bad ones. These days, partly as a kind of intellectual exercise, and partly out of nostalgia or stubbornness, I am in the process of preparing a version of 'case grammar' with some of the snags worked out and some of the details worked in. That study is far from

complete; what I hope to do in this paper is simply to expose some of the difficulties 'of fact and principle' which the model faces, and maybe even to suggest, from time to time, that the proponents of alternative views are not always clearly better off with respect to these problems.

2. I see a transformational grammar with a case base as having in general the following properties. The propositional core of a simple sentence consists of a 'predicator' (verb, adjective, or noun) in construction with one or more entities, each of these related to the predicator in one of the semantic functions known as (deep structure) 'cases'. The cases identify the roles which the entities serve in the predication, these roles taken from a repertory defined once and for all for human languages and including that of the instigator of an action, that of the experiencer of a psychological event, that of an object which undergoes a change or movement, that of the location of an event, and so on. (I recognize the emptiness of this assumption in the absence of a coherent grammatical theory in which the cases play a crucial role. I will address myself to this question shortly.)

The cases exist in a hierarchy, and this hierarchy serves to guide the operation of certain syntactic processes, in particular that of subject selection. It figures in subject selection by determining which noun-phrase is to become the subject of the sentence in the 'unmarked' instance. That case in a sentence which, according to the hierarchy of cases, outranks the others, is the one which has the noun-phrase it is associated with selected as the subject of the sentence.

Certain predicators have their own lexically determined subject choices, and there are furthermore certain subject choice options provided by the language--among them that provided in English by the passive transformation. A grammar must therefore provide some way of re-ranking the cases for particular sentences. (My present practice is to reflect the subject choice hierarchy in the left-to-right order of the cases in the deep structure representation of individual sentences, and to allow the subject selection process merely to select the leftmost noun-phrase in the list. The transformations which re-rank these elements then are transformations which move some initially non-leftmost element into the leftmost position in the list of cases.)

The surface cases in case languages, and the prepositions or postpositions or other syntactic function indicators in other languages, are determined by various sorts of information about the sentence, just one of these being the identity of the deep-structure cases; others have to do with the operation of the subject and object selection processes, facts about definiteness and animateness and the like, and, for nouns that enter into the various types of locative constructions, the dimensionality of the entity being designated.

The lexical items in a language which are capable of serving as predators--and this set includes not only all contentives but most connectives--can be classified according to the possible arrays of cases that they can occur in construction with. Lexical items can be further described by identifying the grammatical processes which are triggered by or made possible by their presence in a sentence.

Sentences that are embedded in underlying representations are embedded as occupants of some case role. By processes that are familiar if not well understood, embedded sentences can have complementizers attached to them, they can be nominalized, they can have some of their constituents 'promoted' to become constituents of the sentences into which they have been embedded, and so on.

Very briefly, then, these are the main characteristics of a transformational grammar whose base component specifies the case structure of sentences. I have left vague the way in which the case identity of a noun-phrase is to be symbolized, because that, as it happens, is one of our problems. I have left vague the relationship between the 'entities' that have case roles in what I described as the structure of simple sentences, and the noun-phrases that show up in particular positions in sentences, because that is everyone's problem.

3. The whole thing makes sense only if there are good reasons to believe that there is an irreducible number of role types by which grammatical theory makes its contribution to semantic interpretation; if it turns out that this number is small; if there are reasonable principles according to which these role types can be identified; and if grammars in which they are incorporated into underlying representations are superior to those in which they are not. There are certain criteria that I have appealed to in attempting to determine the cases, and I will speak of them now. They are not outstandingly confidence-inspiring, given the fact that I have changed my mind so many times in the past few years about the analysis of a number of sentence types; but I believe there is something to them nevertheless.

First of all I make the assumption that there is in a single clause at most one noun-phrase (which may, however, be compound) serving a given case role. If we accept this one-instance-per-clause principle, we are required to deal with apparent counter-examples either by showing that the putative identical case roles are in fact distinct, or by showing that the construction is better treated as an instance of clause embedding.

Let's consider first a situation in which the embedding analysis is preferred. Suppose that one of the case roles that we intuitively recognize is that of the Agent, and suppose that in a sentence like John compelled his son to stab the usher, we perceive agency in both what John does and in what his son does. The one-instance-per-clause principle

requires us to analyze the sentence as being clausally complex, and it compels us not to analyze compel to stab as a single discontinuous verb. (If all languages were like English, with the elements of compel to stab distributed in different places in the sentence, we could say that this application of the principle is of use in beating dead horses with straw men. The principle takes on some interest, however, in a language in which the notion 'compel to stab' has surface lexical unity.)

Let's consider next a situation in which we will allow ourselves to change our minds about the case identity of two noun phrases in a sentence. Take a sentence like John resembles Fred. It might be believed that in this sentence the two nouns John and Fred have the same role. One reason for believing such a thing is that if the two noun-phrases straddling the verb resemble both designate entities which are more or less equally observationally accessible, it must always be true that if the first resembles the second, the second resembles the first. Since the analysis as a complex sentence does not suggest itself in this instance, the one-instance-per-clause principle gives me the responsibility of showing that the semantic roles of the two nouns are distinct. I would have to say that the two entities are somehow taken in different ways. I might begin by suggesting that the sentence John resembles Fred involves the judgment that certain properties 'observable' in John are relatable to properties 'attributable' to Fred, with the second noun-phrase serving to identify a standard according to which the entity named by the first noun-phrase is assigned some sort of a position. This being so, it should follow that the two roles associated with resemble can be occupied by instances of different types of noun-phrases, or by noun-phrases having different assumptions about existence or observability associated with them. It should be possible, in other words, to put in the second position, but not in the first position, noun-phrases which are generically understood or which designate non-existent entities, even when the noun-phrase in the first position is a referring expression. This prediction is borne out, because the two noun-phrases cannot be interchanged in properly understood readings of the sentences That donkey resembles a unicorn, John resembles a horse, or John resembles his famous ancestor.

So much for the first principle. Now sometimes a single predicator takes noun-phrases of different cases, occurring in one sentence with one choice of cases, in another with a different choice. Since in English every sentence has to have a subject, one place to look for the variety of cases is in subject position. We find that the relation which a subject has to its clause can vary from one predicator to another, naturally, but it can also vary in different sentences with the same predicator.

By illustration, take sentences containing the adjective warm. A subject noun-phrase with this adjective can name: an experiencer of the sensation; something which when used can result in someone

experiencing the sensation; a time period during which sentient beings can experience the sensation; or a place in which they can experience the sensation. If we want to assign names to these functions, we might speak of Experiencer, as in I am warm; Instrument, as in This jacket is warm; Time, as in Summer is warm; and Location, as in The room is warm.

My second assumption, then, is that if one takes a predicate which is intuitively seen as assigning different semantic functions to noun-phrases that occur in specific syntactic positions with respect to it, there should be a natural 'stopping point' in any attempt to classify these semantic functions. If that turns out to be true, and if it is also true that one finds comparable lists of functions in the analysis of noun-phrases that occur with other predicates, we can believe that we are on the right track. We might be encouraged, for example, if we tried an analysis of the subject roles occurring with the adjective sad, because it is not unnatural to claim that for sentences like John was sad and The movie was sad, the emotion-experiencer role of John in the former is analogous to the sensation-experiencer role of I in I am warm, and that the experience-elicitng role of movie in The movie was sad is analogous to that of jacket in This jacket is warm.

It is one thing to see if there is a stopping place in the attempt to list the semantic functions that go with any given predicate, another thing to see if the lists of semantic functions found for different predicates have enough overlap to make it believable that there is a small list for grammatical theory in general. It is still another thing to inquire whether the functions that by this process we take as distinct are in fact 'emically' distinct, and for that we need to find other sorts of evidence. I believe that such evidence can be found, though it requires an appeal to syntactic constructions which are not in themselves perfectly well understood. When the comparative construction compares two noun-phrases and when the regular coordinate conjunction construction unites two noun-phrases, the noun-phrases which are brought together must have the same case role in the sentences in which they occur. With sad it is possible to compare two Experiencers, as in John is as sad as Fred, and with warm it is possible to compare two Instruments, as in My sweater is warmer than your jacket; but such mixtures of cases as that suggested by Lately I've been sadder than 'Love Story' or My jacket is warmer than Texas will not do. Similarly with conjunction, it is all right to say John and Fred are both sad or My sweater and your jacket are both very warm, but not John and the movie both became very sad near the end, or My sweater and I are both nice and warm.

The assumptions that I've mentioned so far are for determining when we are dealing with distinct cases with given predicates, and I may refer to them as principles of 'contrast'. Next we can consider a

principle of 'complementarity'. (Those of you who are over forty will be familiar with these terms.) Sometimes we find in different sentences semantic functions which in detail are partly alike and partly different, their differences being systematically relatable to differences in the semantic properties of the lexical material they are in construction with. (I refuse even to mention the terminological horror of speaking here of 'allo-cases' of the same 'caseme'.) With verbs of motion, like for example go, we can specify a starting point and a destination, as in a sentence like He went from the top of the hill to the cemetery gate; for transformation verbs we can specify the earlier state and the later state, as in a sentence like He changed from a 96-pound weakling into a famous football hero; and for verbs of temporal lapse we can talk about the starting and ending point of a time period, as in The pageant lasted from sundown until midnight. My inclination is to refer to the two points identified in all of these earlier/later indications as different instances of the same cases, namely Source and Goal. Depending on the type of predicator, the Source and Goal are interpreted as earlier and later locations, earlier and later states, or earlier and later time points.

Having come upon such a decision, we must immediately figure out what to do with certain apparent counter-examples. As my sample motion verb I deliberately chose the verb go, because it is one which is a motion verb pure and simple. The verbs go and come and move are just about the only motion verbs in English which have associated with them no understanding of manner, means or medium. In sentences with other verbs of motion, however, it might indeed look as if we need to distinguish as separate cases temporal Source and Goal from spatial Source and Goal. To see what I mean, consider the fact that we can say either He walked from the top of the hill to the cemetery gate or He walked from noon until sundown. If we say that the verb walk can occur with either temporal or spatial Sources or Goals, we are then required to come up with special explanations of why they cannot all occur in a single sentence, and why they cannot be mixed in the same sentence. That is, we cannot say He walked from the top of the hill to the cemetery gate from noon until sundown; nor can we say He walked from the cemetery gate until midnight or He walked from noon to the zoo. To account for these facts we must either (i) increase the number of cases by positing both spatial and temporal Source and Goal cases and introduce some constraints on their cooccurrence possibilities in single clauses, or (ii) reanalyze sentences with walk, swim, run, drive, etc., in a way that will allow them to be treated as referring either to types of activities, describable in terms of their durations, or to types of movements, describable in terms of their paths. The question of which of these choices is preferable is one of the problems I will shortly be discussing.

4. The principles I have just been talking about are fairly vague, they seldom lead to beautifully unambiguous results, and they are always subject to other sorts of considerations. Be that as it may, I have lately become comfortable with the following cases: Agent, Experiencer, Instrument, Object, Source, Goal, Location, and Time. There is one more, but I'm saving that till later. I used to talk about 'Datives', but I have reanalyzed the old Dative by spreading it around among the other cases. Where there is a genuine psychological event or mental state verb, we have the Experiencer; where there is a non-psychological verb which indicates a change of state, such as one of dying or growing, we have the Object; where there is a transfer or movement of something to a person, the receiver as destination is taken as the Goal. I no longer confuse selection restrictions to animates with true case-like notions.

There are certain difficulties in stating exactly what one ought to mean by 'Agent', but I am willing to leave those unresolved for now. I take the Instrument, for which I would be happy to find a better name, as the case of the immediate cause of an event, or, in the case of a psychological predicator, the 'stimulus', the thing reacted to. When the Instrument role is occupied by a sentence, that sentence identifies an event which is understood as having some other event or state as its consequence. The Object case is that of the entity which moves or which undergoes change, and I still use it as a wastebasket. Sentences embedded to Objects can serve to identify, for example, the content of a psychological event, as with verbs of judging or imagining. Source and Goal are used in the ways I suggested earlier, and in a few other ways as well. Since the Goal case is used to indicate the later state or end result of some action or change, it can absorb what I used to call 'Resultative' or 'Factitive'; that is, it specifies the end-result role of a thing which comes into existence as a result of the action identified by the predicator, as in I wrote a poem or I constructed a bridge. A 'sentence' embedded as Goal, therefore, is one which identifies the resulting state or event in a causative construction.

The case hierarchy is that of the order in which I listed them: Agent, Experiencer, Instrument, Object, Source, Goal, Location, Time. The case in a given sentence which occurs first on this list determines what is to be the subject of the sentence in, as I said, the 'unmarked' instance. For psychological verbs it is important to notice that the Experiencer precedes the Instrument (or 'cause') and the Object (or 'content') and will therefore be in first position in the deep structure. The so-called Psych-Movement verbs are verbs which require a transformation which moves the highest non-Experiencer noun-phrase into the first position. The Passive transformation is a more general re-ranking transformation, having the effect of putting an original Experiencer or Object or Goal noun-phrase into first position, inducing a modification

in the form of the verb, and associating the preposition by with the noun-phrase that got demoted. (I once associated the preposition by with the Agent noun-phrase, but that was wrong. It is introduced as a result of the operation of the Passive transformation and is associated with whatever noun-phrase was in highest-rank position in the deep structure.)

5. There are innumerable problems that come up in any effort to fill in the details of a grammar like this, and I will devote the rest of this paper to a discussion of some of them. The first that come to mind are those that have to do with the notion of agency. What should we understand about a sentence if we know that one of its cases is Agent? How do we determine whether a verb obligatorily or optionally takes Agent noun-phrases? In what way are notions like movement, intention, causation and result related to understandings of sentences containing Agent noun-phrases?

The model allows only two cases for noun-phrases that can appear in subject position in simple caused-event sentences, requiring both a special account of the analysis of sentences that say something about things caused by natural forces and a special explanation of situations in which there is a chain of causation. To take the second issue first: there are many events in the world which involve chains of causation. If my claim about the case structure of sentences is right, it should follow that where there is a causation chain, with one thing leading to another, the grammar of simple sentences allows mention of only the principal cause and the immediate cause, and does not allow mention of any of the intervening elements. I believe this is so, and I'll use an example offered by Donald Davidson to illustrate it. Suppose a man swings a baseball bat and the bat hits a baseball, suppose the baseball moves through the air and impinges on a window, and suppose that as a result the window breaks. The grammar of simple sentences in English allows us to say The man broke the window or The baseball broke the window, but not, as a description of the situation I just described, The bat broke the window. The nouns that can appear as the subject of the transitive verb break name either the principal cause, the Agent, or the immediate cause, the Instrument, but not any intervening cause. Furthermore, if we wish to express the role of both Agent and Instrument in the sentence, we can say The man broke the window with the baseball but not, as a description of this situation, The man broke the window with the baseball bat.

I believe, therefore, that I can justify having at most two cases related to sentences involving causation; but the next thing to consider is how one decides which of these two cases should absorb the role of phenomena which are not subject to anybody's control but which cause things to happen, as when we speak of things being caused by lightning, tuberculosis or erosion.

The possibility of positing a new case, say 'Force', seems unnecessary, since this putative Force case never occurs in contrast with either Agent or Instrument. (I recognize, however, the force of a suggestion of Rodney Huddleson's.⁴ One way of describing the difference between the intentional and accidental interpretations of John's involvement in actions identified by the sentence John broke the window is to say that on one reading John is Agent, on the other John is Force. On the Agent interpretation, we think of John as a sentient being; on the Force interpretation, we think of John as a force of nature.)

The question is, if Force should be grouped with either Agent or Instrument, which one should it be? Let us suppose that we decided to link forces with agents. The 'principal cause' interpretation of the Agent case seems for many sentences to be quite adequate: if thunder frightened the baby by the baby's having perceived the thunder, then the thunder can be certainly thought of as the principal cause of the baby's experience. But there are a few problems associated with this assignment. For example, the case hierarchy puts an Agent always in first position, making it in general possible for sentences having Agents to contain Instrument phrases as well, but impossible for sentences having Instruments as subjects to contain Agents as well. If our putative case Force were absorbed into the Agent case, it would then be necessary to add the special information that Agent noun-phrases which represent acts of God or changes in nature fail to occur in sentences which contain Instruments or instrumentally construed by-clauses. This is to account for the fact that we do not find sentences like Air pollution killed my petunias with cyanide or The thunder frightened the cattle with lightning. If, on the other hand, the Force were grouped with the Instrument rather than with the Agent, such facts would turn out not to be special facts about force-of-nature sentences, but would already be explained by a combination of the one-instance-per-clause principle and the case hierarchy.

Another reason one might have for absorbing Force into the Instrument case is that then the natural-force noun-phrases would be seen as having the same role in sentences about their typical event-causing function and in sentences about situations in which they are controlled by some agent after all. It is well known that one can control phenomena in nature either by being God or by being trained and equipped in such arts as cloud-seeding and germ warfare. The assignment of natural-force noun-phrases with the Instrument case would also be consistent with my view that it is possible for the Instrument case to be occupied by a sentence, but not possible for the Agent; the benefit here is that a great many of the natural-force noun-phrases can be thought of as being derived from sentences.

There are languages in which the forces of nature are sometimes thought of as animated or deified by the speakers of the language; for such languages, we might be advised, the force-of-nature nouns should

be assigned the Agent case. I don't believe that will be necessary. If it turns out that 'all' natural phenomena are thought of as personified, then it seems quite unnecessary to make such an interpretation, for the simple reason that we could just as well say that we are talking about the beliefs of speakers as that we are talking about the properties of their grammars. If, on the other hand it turns out that some forces of nature are personified while others are not, then we could indeed agree to assign the nouns the Agent case in certain sentences, but we would do so by assuming that here the words are functioning in fact as proper names and refer to things like the god of thunder or the spirit of fire rather than to the phenomena themselves.

Talk about Agents and Instruments having a role in sentences that have something or other to do with causation raises the question of the case structure of the English verb cause. I recall once hurriedly writing that the verb cause is one which requires an Agent, but that is clearly false. In sentences like The glare of the sunlight caused the accident or The accident caused the revolution there is no allusion to agency, and it would obviously be necessary to attribute Instrument-hood to the subjects of these sentences (in the sense of Instrument that I have been discussing). We can see, therefore, that the Agent case is at least not obligatory. Is it then optional? Can we say that in a sentence like She caused the accident by screaming we have as Agent she and as Instrument a by-clause coming from She screamed? The reasons for suggesting that must be justified independently of the process by which the subjects of by-clauses can assume a role as subjects of cause quite independently of their being understood as Agents, as in sentences like She caused the accident by having left her drapes open. There will be more to say about the verb cause below.

6. The recognition of the need to deal with causation as a consequence relation between two events comes up in the problem of determining the case structure of certain kinds of 'impingement' verbs--that is, verbs of impact like hit and strike, and verbs of pressure like push and shove. It has been through an attempt to give a uniform case structure analysis of these verbs that I have been forced to give up the lexicalist position I started out with and to recognize more indirect sorts of relations between deep and surface structures than I had been originally willing to countenance.

Suppose that we would like to characterize certain facts about impingement verbs in terms of their similarity to verbs of motion, and suppose that we view them as expressing the situation in which there is something which moves and there is some destination or goal or direction which further characterizes this motion. The thing which moves, as in the straightforward analysis of motion verbs, is the Object, and the thing to which it moves, or on which it impinges, can

be thought of as the Goal. In sentences like John hit the fence with his cane or John hit his cane against the fence, John is the Agent, the fence is the Goal, and John's cane is the moving Object. In John pushed against the wall with his cane, John is the Agent, the wall is the Goal, and again John's cane is the Object. These sentences are thus seen as having a certain similarity with sentences like John dropped the dishes onto the floor, the detailed differences in the ways in which we interpret the cases being related to the different semantic properties of the verbs. (This analysis differs, by the way, from one given in my paper on 'The grammar of hitting and breaking', just recently published but written a long time ago.⁵)

The analysis seems quite adequate in sentences in which one speaks of the thing which is impinged on as merely being there, but a problem arises when we consider how to analyze sentences like I hit the ball over the fence and I pushed the table into the corner. What we are dealing with here are situations in which the impinged upon thing itself moves. If there were reasons for treating the impinged upon thing as the Goal in the earlier analysis, there are reasons for treating it as Object in these sentences and for treating over the fence and into the corner as exemplifying the Goal case. Either these verbs have to be given different analyses for their occurrence in these different sentences, or the second set of sentences needs to be reconstructed in such a way as to allow the same entity to be both Goal and Object.

This last choice requires us not only to recognize sentences about hitting the ball over the fence or pushing the table into the corner as complex, but as complex in a way which requires some sort of association between clauses that cannot be thought of as compounding the two together or as embedding one into the other. We need to be able to recognize that the latter sentences involve an understanding of event causation, according to which the occurrence of one event has the occurrence of another event as its consequence. In I hit the ball over the fence we would have to posit something like (clause i) I hit the ball and (clause ii) The ball went over the fence, the two clauses embedded to a higher predicate that has a meaning suggested by the word cause, predicating the event-causation relation between the two clauses. The first clause is embedded as Instrument, in its immediate-cause function; the second clause is embedded as Goal, in its resulting-state function. In the first clause the ball is Goal, in the second clause it is Object.

The consequence of this decision is the acceptance of a model of grammar in which the rules for transforming deep structures into surface structures will be fussier than I used to want to think, and the admission of prelexical transformations that are in fact a bit more complicated than McCawley's Predicate Raising transformation.⁶ We have here a situation in which one event serves as the immediate cause of

some other event. Somehow the transformations which will convert a structure meaning something like My hitting the ball caused it to go over the fence into I hit the ball over the fence will have to form out of all three verbs a lexical construct of the form by hitting cause to move and will have to 'conflate' (to use Leonard Talmy's term) the two constituent clauses into one. In the absence of detailed and principled proposals for designing a grammar which incorporates rules which do what I think needs to be done, all this is quite unsatisfactory; but I know that when and if it is done, it will serve to make English look a little bit more like those languages in which the only way to say I hit the ball over the fence is to say something like I hit the ball; it went over the fence, and the only way to say I knocked the man down is to say something like I hit the man; he fell down.

The restructuring processes that I have been alluding to appear to be governed by specific lexical items, and that suggests that the conflation process should indeed be construed as one which creates complex lexical constructs in a way suggested by McCawley's Predicate Raising principle, with the lexicon specifying which of these creations have been lexicalized in the language. It is possible to push against a table and as a result to have that table move into the corner. English allows us to say He pushed the table into the corner. It is possible to lean against a table and as a result to have that table move into the corner. English does not allow us to say He leaned the table into the corner. One way of capturing such facts is to say that the lexicon of English contains the information that push substitutes for by pushing against cause to move, but it fails to specify a lexical item capable of substituting for by leaning against cause to move.

Notice that it was my attempt to preserve certain principles of case structure that forced me to consider this possibility. I want to believe that there is a basic sense of verbs like push and hit according to which they can be assigned their deep-structure case frames, that the case frames associated with verbs of motion include the Source and Goal cases in their change-of-location functions, and that both the semantic and syntactic additional properties of sentences in which these verbs suggest the notion of resulting movement can be accounted for by the kind of process that I have in mind. The model will have to point out, for push, that the Goal noun-phrase takes the preposition against in the unconflated clause, but that the lexical item push which replaces the construct formed from the conflated clause takes that same noun-phrase as its direct object. This not only accounts for the fact that clauses with push against do not occur with location-changing Source and Goal expressions while clauses with just push may, but it also accounts for the fact that the idea of resultant motion exists also in the superficially simple sentence John pushed the table.

7. If we agree that there are reasons to reach these conclusions for cause-to-move verbs like push, hit, etc., we might then ask questions along a similar line about the analysis of another class of verbs involving both the notion of movement and the notion of manner, means or medium of movement--verbs like float, ride, swim, slip, etc. Each of these verbs looks as if it can be given two case analyses, depending on whether it is interpreted as a verb of motion or not, the two analyses requiring furthermore that the spatial and temporal interpretations of Sources and Goals lead to an addition to the total number of cases. That is, we can say either He swam from noon until 2 o'clock or He swam from the end of the dock to the shore. This, you will recall, is one of the contexts which challenged the use I wanted to make of the complementarity principle for the Source and Goal cases.

To use examples borrowed from Leonard Talmy, we can speak of a bottle floating on the water, and we speak of the bottle floating into the cove. In the one case there is just the matter of some object being suspended by its medium; in the other case there is the additional matter of its moving from one place to another. Grammatical theory needs to provide some way of separating these two aspects.

A semantic reason for wanting to be able to deal separately with the motion and manner aspects of certain expressions containing these verbs is that under certain conditions we can focus on one or the other of the two. Take for example permission-seeking sentences involving the verb swim. Suppose you are the guard at the entrance of a cave that a stream flows into, and I am going to ask you for permission to enter the cave swimming. Suppose in the first instance that I am already in the water and swimming. In this case it is simply known in advance that I am swimming, and what I need to ask permission for is to enter the cave. In this first case what I would say is May I swim in?, with heavy stress on in. In the second instance, suppose you have already given me permission to enter the cave, and what I am after is your consent to do so in the water. In that case what I must ask is May I swim in?, this time with heavy stress on swim. Verbs which do not have this sort of double-barreled interpretation, verbs like come and go, do not have this variety in stress placement potential either. I can say May I come in?, but not May I come in?. The stressing for swim when it is 'used as a verb of motion' is the same as that of the pure motion verbs. Possibly what we need, then, is an analysis by which the motion-verb swim is really complex, being a substitute for something like by swimming go, with the stressing of in in the surface sentence determined according to whether the underlying sentence contains a go-clause or not.

Grammatical theory, then, must provide some way of recognizing an association between two clauses such that the one designates what one might roughly call the manner in which the event mentioned by the

second clause takes place. In this instance, having the two clauses embedded to Instrument and Goal and commanded by the verb cause does not seem particularly natural. In defense of the possibility of calling on some sort of causal notion for the analysis, however, it should be pointed out that the English verb cause has not only the interpretation by which one event has another event as its consequence, but has other uses as well. That is, there is both a stative verb cause and an active verb cause. The active verb appears in a sentence like Susan's screaming caused Fred to drop the tray; the stative verb appears in the sentence Susan's living nearby causes me to prefer this neighborhood. A not particularly elegant way of using an analogous analysis for the manner-of-motion verbs as I suggested for the cause-motion verbs discussed in the last section is to embed the manner clause in the Instrument, the motion clause in the Object, and have both clauses be commanded by cause--this time, the stative verb cause. (The difference is that the use of the Goal case for the hit and push verbs suggested that the motion clause indicated a consequence or result of the action indicated by the Instrument clause.) Now we at least have some way of talking about the two senses of cause, we have set up structures which will require our poorly understood but by now familiar process of conflation, we have created the need for lexical rules of the form 'Substitute swim for the lexical construct move by swimming', and we have underlying structures for English which look something like what we will need for languages which do not allow conflation in these situations but which require surface sentences to keep the verbs separated (as in Spanish entró flotando 'entered floating', or Japanese aruite kita 'came walking').

In the next section I will suggest that what might have looked like straightforward instances of causatives requiring nothing more than McCawley's Predicate Raising might really involve something more like the conflation processes I have in mind. In particular I will propose that kill will turn out to be the lexical substitution for the construct by doing something cause to die rather than for the construct cause to die.

8. I have said nothing so far about the two cases that I call Location and Time. That is, I have said nothing about place and time notions independently of expressions about changing or moving. One possibility for dealing with these cases is that of saying that they are optional complements of essentially any predicator. Another possibility is that of saying that clauses that are capable of designating actions or events or situations which can be located in space and time are themselves to be embedded into higher sentences containing as their main verb something like occur or happen, with the understanding that it is this higher verb which takes Location-and-Time-introducing cases.

(Some verbs take Location and Time complements directly, as for example be in one of its uses, live, and spend, as in The beer was in the garage yesterday, I lived in Milwaukee in the forties, and Jeffrey spent Tuesday afternoon at the beach.)

One reason one might have for accepting a Location-and-Time-introducing higher sentence with occur is that its presence can serve to explain conditions under which the conflation process is blocked. I'd like to illustrate this point by considering the analysis according to which kill is taken as a lexical substitution for cause to become not alive. On McCawley's analysis there is a single chain of embedding in structures yielding the verb kill. If for my sentences about pushing tables into corners and hitting balls over fences there were reasons to separate the clauses which designated the causing event from the clauses which designated the resulting event, there may well be equally good reasons for assuming the same for verbs like kill. That analysis, however, would require that the Instrument or causing clause contain a verb that never shows up on the surface, something having the meaning of act or do something. An analysis we might give to John killed the rat, would be something like John's actions caused the rat to die. The verb kill, then, substitutes for the conflated-clause construct by doing something cause to die.

Since we are dealing here with two distinct events, each will have, in the world in which it occurs, its own separate place and time coordinates. If either of the clauses designating these two separate events has its own time and place coordinates specified, by being separately embedded to occur, the conflation is not possible. If I was standing on the Ohio side of the border on Tuesday of last week and shot an arrow at a cougar on the Indiana side, and if the cougar then wandered into Illinois and died of the wound on Friday, I cannot say that I killed a cougar in Ohio, or in Illinois, or in Indiana, or that I killed it last Tuesday or last Friday. I can say, however, I killed a cougar in the middle west last week, and that is because the conflation process is possible if the event-chain sentence is left intact but embedded as a whole to the higher verb which assigns the location in space and time to the whole sequence.

9. There are now some additional problems with clauses that indicate movement. The first thing to notice is the fact that Source and Goal, the starting point and the destination, do not exhaust the complement possibilities for verbs of motion. In addition to the complements of Source and Goal, there is the complement type that David Bennett has called 'Path',⁷ exemplified in the last phrase of He walked from the cemetery gate to the chapel along the canal. A particularly interesting property of the Path (or 'itinerative'?) case is that a sentence with the path designated can contain an unlimited number of Path

expressions, as long as these are understood as indicating successive stretches of the same path. This can be seen in a sentence like He walked down the hill across the bridge through the pasture to the chapel.

Superficially, at least, the Path case requires a qualification of the one-instance-per-clause principle. As it happens, the Location and Time cases do, too. Consider a sentence like He was sitting under a tree in the park on a bench Tuesday afternoon about three o'clock, a type of sentence discussed by Bennett. It's clear that we have in this sentence just one place specification and just one time specification, so on the semantic level the one-instance-per-clause principle is not violated; but I cannot say more than that. There are paraphrases of these constructions by which all of the noun-phrases that need to be linked together can be linked together by means of relative clause embedding and conjunction, but since such a way of dealing with the problem does not seem applicable to the problem of the multi-phrase Path, there may be other ways of seeing what is going on.

10. But now, what about all these prepositions? If the cases indicate the basic semantic functions of nouns, how does the case apparatus play a role in determining the selection of specific prepositions like at, on, in, to, onto, into, from, off of, out of, via, across, through, as well as along, under, beside, and the rest. The principles of contrast suggest that, for example, to, onto, and into are all instances of the Goal case, because although expressions containing them can occur with Source expressions, they cannot occur in the same sentence with other Goal expressions. But the principle of complementary distribution when based on surface evidence fails to show their identity. That is, we can speak of something as being located at the corner, on the corner, or in the corner, or as moving from the corner, off of the corner, or out of the corner. The only way we have for preserving the complementarity principle for the selection of individual prepositions and for claiming that the prepositions that we would intuitively like to group together are markers of the same case, is to impute certain differences to the underlying structure of the associated noun-phrases and say that these deep differences are what determine the selection of individual prepositions. Following work by Geoffrey Leech,⁸ we might want to say that nouns that occur in locative expressions can have imputed to them such properties as that of being a point or a surface or a volume, or that of being a part of a surface or a volume, or that of being a point or an area above or below or behind or in front of or to the side of some object, and so on. Innumerable ways of representing this information suggest themselves; whatever means we come across eventually for showing these distinctions in underlying representations, I assume at least that there won't need to be any changes in our understanding of the case relations themselves.

11. Expressions of duration and distance introduce new orders of problems for a case analysis of verbs of movement and change, because they somehow seem to combine the Source and Goal notions into a single unit, a 'hypercase' as it were. That is, we can say He lived there from March until September or He lived there for five months, but combinations of these are not possible in simple sentences. We cannot say He lived there from March for five months. Similarly we can say He walked from Palo Alto to San Jose or He walked thirty miles, but not He walked from Palo Alto thirty miles. I have no proposals in mind for capturing this fact, and I recognize that when I acknowledge this as a problem for the theory, I must also acknowledge the seriousness of the proposal that there might be some 'hypercuse' that similarly covers the Agent and Instrument case.

12. I have concentrated mostly on matters of space and time and movement in this paper, but let me now just briefly mention one or two other conceptual problems that the case grammarian faces. Just about every time that I have listed what I took to be the case notions needed for grammatical theory, I added, as if under my breath, 'and possibly Benefactive'. There are some unhappy facts about Benefactive constructions that suggest that the case status of the associated noun-phrase is simply not like those of the others. Benefactive constructions occur only in sentences with Agents, and only when the Agent's role is thought of as being deliberate or voluntary. To add Benefactive to the list of cases would thus require that the theory be complemented with a system of redundancy principles regarding the selection of cases for sentences, and would require furthermore that an understanding of the expression of intentional or voluntary acts be accounted for within the case apparatus. Since I am unwilling to face that possibility, my alternative is to reconsider the semantics of sentences with Benefactive phrases. It seems to me that a sentence of the form John did it for me can be understood as involving three basic notions: the one who does something, the Agent (John); his action or 'offering', the Object (John did it); and the 'direction' or receiver of that action or offering, (me), the Goal. It can be given a higher-sentence analysis, in other words, with Agent, Object and animate Goal, with the deed performed for somebody's benefit being expressed as the sentence embedded in the Object case. The obligatory presence of the Agent case is accounted for by the embedding context, and the intentionality of the performance on the part of the Agent can be built into the semantic structure of the higher verb. Verbs which satisfy these case frame and semantic conditions are verbs of the type give or offer. I propose, then, that sentences with Benefactives in them really come from more complicated constructions in which it is spelled out that somebody offers some deed to somebody else, and I posit for this an abstract verb of giving. The

clause-conflating principles then, however they are to be stated, will have the effect of changing something like I give you (I do it) into I do it for you; for some languages, like for example Mandarin and a number of the languages of West Africa, the conflation process does not take place, and we get on the surface something like what I've proposed for the deep structure.

13. I have said that the experiencer of a psychological event is represented by a noun-phrase in the Experiencer case, and that some other case will indicate the cause or the content of that psychological experience. In a sentence like I imagined the accident, I am inclined to call the accident the Object, and say that it identifies the content of the experience; in a sentence like The noise frightened me, I regard the noise as the Instrument, where I have in mind that sense of Instruments which covers the stimulus or reacted-to situation in the description of a mental event. Sometimes both Instrument and Object cooccur in the description of some mental event, as in The noise reminded me of the accident; that is why I believe both Instrument and Object are needed, in addition to Experiencer, in the description of psychological-event predators.

These are intuitive decisions, and for a number of sentences my intuitions fail. In a sentence like John loves Mary, is Mary the cause or the content of John's experience? Do fear and frighten differ only in that the latter requires Psych Movement, or is the non-Experiencer case for fear the Object, that for frighten the Instrument? Understandings that can be assigned to the separate cases might then explain why we allow ourselves to conclude such different things regarding the inner world of somebody who says I used to fear the devil as opposed to somebody who says The devil used to frighten me. Regrettably, I do not know how to answer these questions.

14. So far I have spoken only about certain conceptual problems associated with the effort to reconstruct a transformational grammar along the lines of a case grammar. You may have noticed that I have so far failed to give tree diagrams or any other sort of explicit symbolic representations of the structures I have been so cavalierly talking about. That failure stems not merely from a desire to save space. I simply have not found an acceptable notation for the sorts of things I want to be able to represent.

The main problem is how one can indicate the case role of noun-phrases and embedded clauses in the sentences of which they are constituents, and what consequences the choice of notation has for the operation of the grammar.

One possibility for a notation is the one by which cases are indicated as features on nouns. For a sentence like John gave the flower to Mary,

the complex symbol associated with John contains the feature + Agent, that associated with flower contains the feature + Object, and that associated with Mary contains the feature + Goal. I find this inadequate, first of all because the notion of case has nothing to do with 'properties of nouns', but rather with relations or metarelations which nouns have with the rest of the clause in which they occur. A second reason for finding it inadequate is that it forces all instances of clause embedding to be treated as instances of adjunction to nouns. This might be workable in some contexts, but not, I think, in all. Thus, John's screaming caused the accident can be interpreted as The event of John's screaming caused the accident, and That John loves Mary amuses Mary can be interpreted as The fact that John loves Mary amuses Mary; but it is not so easy to see what can be done for the embedded Object sentence in, say, I suspect that John loves Mary.

A second possibility is that of assigning case features to verbs, and just saying that for each verb we specify as its 'valence' a collection of case relationships, that the number of noun-phrases the verb can occur in construction with is determined by the number of cases specified in its valence feature, and that the association of the individual noun-phrases with the individual cases is to be achieved by counting from left to right and by checking off the cases in accordance with the case hierarchy. This too might be workable, but it introduces at least two complications: the first being that keeping track of the case identity of noun-phrases will become difficult after movement transformations are applied, especially if your theory is not wealthy enough to own derivational constraints; the second being that the theory will need to have special ways of distinguishing valence information associated in the dictionary with the lexical item and valence features occurring with those lexical items in individual sentences, in just those cases where the item is compatible with any of several combinations of cases.

These are both, in one way or another, fairly bad notations for case grammar. There is one that is still worse, however, and that is the one by which the case roles of noun phrases are indicated by means of labeled nodes dominating the associated sentence or noun-phrase. The cases are clearly not categories, though in this notation they are treated just like grammatical categories; the theory that represents them in this way needs therefore to distinguish two types of category symbols and needs to have variables ranging over case labels; and the theory needs devices for changing case labels, devices for deleting case labels and restructuring what is left, and so on. The proposal could not be taken seriously enough to be included in this discussion were it not for the fact that it is the practice which I have followed; that transformational rules stated in its terms are fairly easy to conceptualize; that it follows the tradition in transformationalist studies by

which labels are assigned to verb-phrase constituents and co-constituents that are not subjects and objects, such as Manner and Extent and Time phrases; and that since case constituents sometimes need to be built up with the addition of complementizers and prepositions and the like, the case labels at least provide foundation nodes onto which these enlarged structures can be built.

Actually the notations which are most pleasing to me on the deep-structure level are unfortunately notations that lend themselves least to the view that deep structure configurations and surface structure configurations belong to the same species. I have in mind a kind of dependency notation which makes use of kernel trees or 'stemmas' each containing one root node, one or more labeled branches, and a variable or index symbol at the leaf end of each branch. The node is a complex symbol containing semantic, phonological and rule features information, as well as the case valence. The branches are labeled with case labels, and are ordered from left to right according to the case hierarchy. The variables at the leaf end of the branches represent the entities which bear case relations to the predicator represented at the node. Any sentence has at base a collection of stemmas of this type, plus information about identities involving the variables; either there can be co-reference among the variables, or some of the variables can be identified with some of the stemmas. That much identifies the semantic interpretation of the sentence.

As input to the transformational rules, in place of the notion of deep structure, there is what one might call the 'composition plan' of the sentence, the plan by which the various stemmas are to be incorporated into each other to construct the surface sentence. The general effect of the composition plan will be to indicate which variables are to be replaced by lexical items and which stemmas are to be taken as nexus for which other stemmas. Using Sandra Thompson's examples,⁹ the two sentences I know a girl who speaks Basque and A girl I know speaks Basque will differ only on the level of the composition plan. The transformations will provide for lexical insertion and lexical modification, and will somehow provide for the construction of the surface sentence from all this.

I have a few proposals abrewing on how such a grammar can operate, but problems associated with deletion, topic/comment, quantification, and the representation of manner and degree adverbs seem at the moment fairly overwhelming. Being now a Californian, I have become acquainted with some people who know a lot about magic and witchcraft. I am counting on their services to help me complete this research.

NOTES

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²Fillmore, Charles J. 1968. 'The case for case', in Emmon Bach and Robert T. Harms, eds., *Universals in linguistic theory*, 1-90. New York, Holt, Rinehart and Winston.

³Chomsky, Noam. 1965. *Aspects of the theory of syntax*, 161-3. M.I.T. press.

⁴Huddleston, Rodney. 1970. 'Some remarks on case grammar', in *Linguistic inquiry*, I. 501-11; the suggestion referred to here is on p. 505.

⁵Fillmore, Charles J. 1970. 'The grammar of hitting and breaking', in Roderick Jacobs and Peter Rosenbaum, eds., *Readings in English transformational grammar*, 120-33. Ginn and Company.

⁶McCawley, James D. 1968. 'Lexical insertion in a transformational grammar without deep structure', in Bill J. Darden et al., eds., *Papers from the Fourth Regional Meeting*, Chicago Linguistic Society, 1968, 71-90.

⁷Bennett, David C. 1970. 'Some observations concerning the locative-directional distinction', mimeo.

⁸Leech, Geoffrey. 1969. *Towards a semantic description of English*. Longmans.

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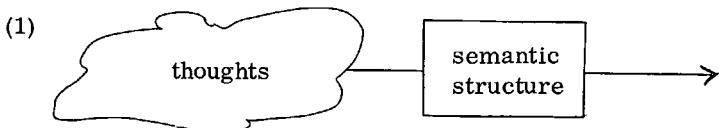
LINGUISTICS AND HUMAN KNOWLEDGE

WALLACE L. CHAFE

University of California, Berkeley

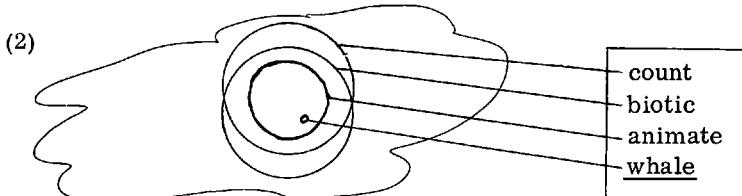
What I want to talk about is the relationship between people's knowledge of the world, as it is often called, and semantic structure. Semantic structure is something I conceive of as an integral part of language. In fact, there is some reason to say that it is the single most important component of language, since the well-formedness of discourse is determined in the first instance by the well-formedness of semantic structures. Now, I would like to think that semantic elements and relations and the constraints that govern their cooccurrence can be systematically stated. At the same time, I believe there is good reason to regard semantic structure as a formalization of human knowledge--if not of all human knowledge, at least of that much of it which can be talked about, which is certainly a great deal. However, the one fact about human knowledge that seems beyond question is that it is vast, complex, and variable, and these properties have almost always led to instant despair whenever linguists have thought about the matter. If, on the one hand, we regard semantic structure as a formalization of at least a major part of what people know, but if, on the other hand, what people know is so dishearteningly intractable, just where do we stand? It's a question that is going to confront linguists more and more, as they turn more and more of their attention to semantics. I don't pretend to have a final answer to it, but I want to suggest where I think the answer might lie.

What I say will assume a distinction between, on the one hand, thoughts or ideas--what actually goes on in the mind and is susceptible to introspective observation--and, on the other hand, semantic structure, the initial filter through which thoughts must pass before they can eventually be converted into sound. The relation between the two might be pictured in the manner shown in (1):



In order to convey our thoughts to someone, it is necessary that we arrange them in a way that conforms to the semantic resources made available by our language. We might think roughly in terms of plugging our thoughts into whatever semantic structures are most appropriate to them. It is useful to imagine thoughts as being present in a huge and multidimensional conceptual space. Semantic structure is then a kind of grid that allows us ultimately to convert at least part of what is present in conceptual space into sound. Probably there are other parts that do not fit this grid so well, but presumably we are interested here in those thoughts which language allows us to communicate easily.

I will assume that, as part of their semantic resources, people have available to them a large stock of what I have called lexical units, such as house, whale, swim, eat, and so on. Each of these lexical units can be regarded as corresponding to a small area of conceptual space. Thus, the lexical unit house corresponds to our concept or idea of house. Furthermore, I will assume that every lexical unit has associated with it a certain bundle of other semantic units, which I have lately been calling 'inherent features', although earlier I called them 'selectional units'. These semantic units can be imagined as corresponding to larger areas of conceptual space than the lexical units--roughly, as involving conceptual areas of varying size within which the lexical units find their place. Thus, for example, the lexical unit whale may correspond to a concept which is located within the larger area defined by the inherent feature animate, the latter being included within a still larger area biotic ('having life'), and perhaps at the same time within an area which can be labeled count, and which includes concepts that have to do with discrete, individualized objects. The picture I have in mind is shown in (2):



The conceptual relations between the various semantic units within semantic space are indicated. In semantic representations I have

followed the convention of setting off lexical units, like whale, by underlining them, and of writing their inherent features, like animate, biotic, and count, above them.

It can be seen from (2) that, because of the way our knowledge of the world is laid out, whale necessarily implies animate, and animate implies biotic and count. In other words, the cartography of conceptual space imposes certain constraints on the cooccurrence of semantic units. In formalizing semantic resources, therefore, it is necessary not only to identify semantic units--and to characterize them as lexical units, inherent features, and in other ways--it is also necessary to state a large variety of what may be called semantic constraints. Such constraints are generally of the form that semantic unit X requires the accompaniment of semantic unit Y; we may say, more concisely, that X implies Y. It is possible to state such constraints in the manner suggested in (3):

(3) whale \Rightarrow animate

$$\text{animate} \Rightarrow \begin{bmatrix} \text{biotic} \\ \text{count} \end{bmatrix}$$

Thus, when the lexical unit whale is present, it must be accompanied by the inherent feature animate, and the presence of animate requires the presence of biotic and count. The picture in (2) shows how these constraints are determined by the nature of our conceptual landscape.

We are going to be particularly interested here in inherent features--those larger areas of conceptual space within which lexical concepts are found. We will see that inherent features are not all of the same kind. First of all, we might take note of the fact that there are some semantic units which occur sometimes as inherent features, but also sometimes as lexical units in their own right. To put it the other way around, we could say that there are certain lexical units which double as inherent features. For example, the semantic unit big, which is found as a lexical unit in sentences like That's a big dog, also occurs as an inherent feature of various lexical units like whale and elephant. In other words, it is part of our knowledge of whales--in addition to our knowledge that they are animate and so on--that they are big. Thus, in addition to the constraints listed in (3), there is also a semantic constraint of the kind given in (4):

(4) whale \Rightarrow big

Whenever the concept whale is communicated, therefore, at least the semantic configuration shown in (5) must be present:

- (5) N
 count
 biotic
 animate
 big
whale

It may be noted that, although count, biotic, and animate are never anything but inherent features and do not, for that reason, ever have any surface representation, big does have a surface representation. Not only does it show up in surface structure when it is a lexical unit, as in That's a big dog, but sometimes it has a surface representation even when it occurs as an inherent feature. Thus, under appropriate circumstances, one might say either (6a) or (6b) and convey very nearly the same thoughts, the only difference being that in (6b) the inherent feature big is highlighted, as it were, and not postsemantically deleted as inherent features usually are:

- (6) a. There's a whale on the beach.
 b. There's a big whale on the beach.

It must be understood that big in (6b) is pronounced with weak stress and low pitch, as befits an item that conveys redundant information.

We can consider, then, that part of our knowledge about whales is formalized through the constraints listed in (3) and (4), which allow the sort of semantic configuration shown in (5). Probably everyone shares this much knowledge about whales. It would be strange if a person didn't conceive of whales as inherently animate and inherently big. Suppose, however, that there are certain people who are extremely fond of eating whales, and who conceive of them as inherently delicious. Such people would then have in their semantic resources the constraint given in (7a), which would allow them to use the semantic configuration in (7b), which would in turn allow them to say things like (7c):

7. a. whale \Rightarrow delicious
 b. N
 count
 biotic
 animate
 big
 delicious
whale
 c. There's a delicious whale on the beach.

Clearly not everyone has the constraint (7a) in his semantic resources;

I certainly don't, although I might have an analogous constraint for blueberry pie. I use this example to illustrate that people's semantic resources, as captured in part through semantic constraints, may vary. Of course, this variability simply reflects the fact that people's knowledge of the world varies. There is nothing particularly distressing about this fact. It means that we can't expect to state semantic resources in such a way that they will hold for all speakers of a language, let alone for all people in the world. But variation of this kind is surely exciting to study, and central to our understanding of differences in 'world view'.

It is obvious that knowledge varies not only from person to person, but also through time, so that what anyone knows at one point in time is not totally identical to what he knows at a different point. I would like to focus now on the fact that what a person knows changes during a discourse, a fact which can be formalized in terms of changes in semantic resources brought about by the actual semantic structures that are made use of as a narrative or conversation proceeds. One evidently starts a discourse with a particular fund of semantic resources, which is then continually modified by what is subsequently said. The point that is of interest to us here can be made with relation to the occurrence of the semantic unit definite. Definite is neither a lexical unit nor an inherent feature, but belongs to a third class of semantic units. The term 'contextual feature' seems appropriate as a label for such a unit (earlier I used the term 'inflectional unit'). The meaning of the contextual unit definite, in so far as it relates to count nouns, can be paraphrased approximately as 'the speaker assumes that the hearer already knows about a particular object or set of objects of the kind specified by the lexical unit to which definite is attached; furthermore, the speaker assumes that the hearer knows that this is the object (or these are the objects) presently being talked about.' Thus, for example, in (8a), where the speaker has not said anything about this whale before, he cannot make it definite; he is introducing this whale for the first time:

- (8) a. There's a whale on the beach.
- b. Let's go look at the whale.

In the following sentence, (8b), he is able to assume that the hearer now knows about this whale, and this assumption is captured through the attachment to whale of the contextual feature definite. The presence of this semantic unit, of course, is reflected in the presence of the definite article in the surface structure. (For other reasons, the whale in (8b) would usually be pronominalized to it, but that need not concern us here.)

Tentatively and incompletely, we might try to account for the presence of definite in (8b) by means of the contextual rule stated in (9):

- (9) 1 : 1 \Rightarrow definite

This rule is to be read, 'The occurrence of a lexical unit (such as whale in (8a)) introduces into the semantic resources available to this discourse a new semantic constraint, to the effect that that lexical unit now requires the accompaniment of the contextual feature definite.' From now on in this discourse, therefore, the constraint stated in (10a) will be present, and will result in the lexical unit whale being accompanied by the several inherent features and the contextual feature indicated in (10b), where I follow my usual practice of writing contextual features below the lexical unit:

- (10) a. whale \Rightarrow definite
 b. N
 count
 biotic
 animate
 big
whale
 definite

Actually, there are many modifications which must be added to the contextual rule in (9). I believe it is possible to state these modifications accurately and completely, but to do so would require a rule of much greater complexity. What I am interested in here, however, are not the ways in which this rule can be improved, but rather the ways in which the constraint ' $1 \Rightarrow$ definite' may be introduced into the semantic resources of a discourse where this constraint has not been initiated by the occurrence of the lexical unit itself, or at least not by its occurrence as a lexical unit. For example, having mentioned the presence of a whale on the beach, suppose I subsequently say something like (11):

- (11) Let's go look at the animal.

Such a statement is possible if I assume that you and I know that a whale is inherently an animal; in other words, if our semantic resources contain also the constraint shown in (12a), and if, therefore, an occurrence of whale as in (8a) involves the configuration shown in (12b):

- (12) a. whale \Rightarrow animal
 b. N
 count
 biotic
 animate
 animal
 big
whale

The fact that we can say (11), with definite animal, subsequent to (8a), in which we have overtly mentioned whale but not animal, indicates that our contextual rule (9) applies not only to lexical units which are brought into the discourse in their basic role as lexical units, but also to lexical units which are brought into the discourse as inherent features of other lexical units--as animal is here brought in as an inherent feature of whale.

Suppose, now, that you and I were interested in collecting the teeth of whales. Having said (8a), it is clear that I could also go on to say something like (13):

- (13) Let's go get the teeth.

How is it that, having mentioned a whale, we can then treat the lexical unit teeth as definite? Can we say that teeth is also an inherent feature of whale? In the two cases so far mentioned we were able to say that a whale is inherently big and that a whale is inherently an animal, but certainly we can't say that a whale is inherently teeth. However, it does appear to be the case that one of the things we know about whales (or perhaps, more properly, about a larger class of animals) is that they have teeth. In other words, there seems to be good reason to posit the existence of a type of inherent feature which captures the various parts which an object is known to have. Associated with whale, then, will be various parts like those listed in (14a), and the occurrence of whale in a semantic structure involves now a configuration of the kind suggested in (14b):

- (14) a. whale \Rightarrow having teeth, eyes, mouth, tail, ...
 b. N
 count
 biotic
 animate
 having teeth, eyes, mouth, tail, ...
 animal
 big
whale

In consequence, after the lexical unit whale has occurred, as in (8a), rule (9) will introduce definiteness not only for whale and for animal, but also for teeth, eyes, mouth, tail, and whatever other parts a whale may be known to have. Similarly, having mentioned a house, we can go on to talk not only about the house but also about the kitchen, the front door, the roof, and so on.

It should now be evident that in order to account for the occurrence of the contextual feature definite it is necessary to take quite a large store of information into account. In particular, for every lexical unit whose meaning involves an object that has parts, it is necessary to have a list of what those parts are. Such lists may, in some cases, be quite large. Nevertheless, they seem to be within the range of things that are manageable, and if we had a computer large enough we could imagine storing a complete fund of all information of this sort, at least as it represented the knowledge of one individual at one time. However, there is an added complication in the fact that knowledge of this kind, like that of other kinds, varies. In talking about houses in the particular neighborhood where I live, for example, I can assume that one of the parts of a house is an atrium, so that simple mention of a house allows me to go on and talk about the atrium. Having an atrium would certainly not be an inherent feature of house in any neighborhood where I lived previously. Variability in time rather than space can be illustrated with the fact that some years ago, having mentioned a car, we were able to talk about the running board. Having a running board is certainly not an inherent feature of car these days. Again, we ought to find such variability stimulating rather than depressing, even though it means that semantic resources can't be put in a neat package once and for all.

So far, then, the formalization of human knowledge in terms of semantic resources seems manageable, if we are willing to cope with very large amounts of information and with a certain amount of variability in both space and time. There is, however, another way in which definiteness may be introduced that makes the formalization of the relevant semantic resources seem considerably more problematic. What I have in mind is the fact that definiteness is sometimes brought to a noun by a relative clause attached to that noun. For example, suppose I say to you that I visited the Smithsonian yesterday. As my next sentence, either of those in (15) might be semantically well-formed:

- (15) a. I saw some false teeth that were worn by George Washington.
- b. I saw the false teeth that were worn by George Washington.

Even though I have not said anything about false teeth previously, it is evidently quite possible in these circumstances for me to treat this

item as definite, as in (15b), though I am not forced to do so, as is clear from (15a). What is it that creates this definite option? Evidently it is the relative clause attached to false teeth, the clause that shows up in the surface structure as that were worn by George Washington. The question of how definiteness is determined by relative clauses is a large one, and I don't know the complete answer to it at the present time. It seems beyond any doubt, however, that a host of very specific pieces of knowledge are involved. As an illustration we might consider that, although (15b) is a perfectly fine thing to say in the context given, (16b) is not:

- (16) a. I saw some shoes that were worn by George Washington.
- b. *I saw the shoes that were worn by George Washington.

The only change is that shoes have been substituted for false teeth, but now the relative clause in question is no longer capable of definitizing the noun. I think it may be apparent that the reason here is that we know that a person may have only one set of false teeth in his lifetime, whereas he must have more than one set of shoes. If we had the peculiar knowledge that George Washington wore but one pair of shoes throughout his life, then there would be nothing odd about (16b). I think I'm correct in assuming that we don't have such knowledge, but that we do know that he may very well have had only one set of false teeth, and that it is the difference between these two pieces of knowledge that is responsible for the difference between (15) and (16).

What we want to formalize, then, is the knowledge that, so far as false teeth are concerned, only one set may be worn by one person in his lifetime. We can imagine an inherent feature for false teeth that captures this knowledge, as suggested in (17a), which leads to semantic configurations like that shown in (17b):

- (17) a. false teeth \Rightarrow may be one set to one person
- b. N
- :
- may be one set to one person
- false teeth

The interaction between this inherent feature and the semantic units contained in the relative clause will then permit the definiteness exemplified in (15b). It seems to me possible that inherent features of this kind can be formalized in a satisfactory way, and that the interaction between them and the semantic structures of relative clauses can be formally described in such a way that the definiteness in (15b) can be systematically accounted for, while the lack of the definiteness option in (16) will be systematically explainable by the lack of a similar

inherent feature for shoes. However, the number of complex inherent features of this sort that would be required to account for all analogous cases must be staggering. The particularity of the kinds of knowledge that are called for can be illustrated even more vividly with the example in (18):

- (18) a. Yesterday I met a man who crossed the Atlantic in a rowboat.
- b. Yesterday I met the man who crossed the Atlantic in a rowboat.

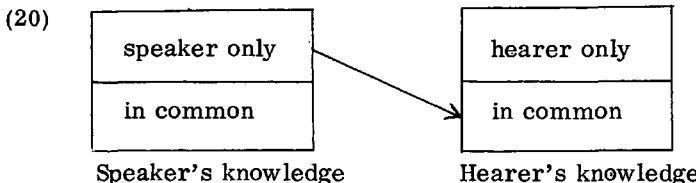
Evidently I would say (18b) if I assumed that my hearer already knew about a particular single event in which a particular man crossed the Atlantic in a rowboat. If I didn't assume such knowledge, I would say (18a). But it is now much harder to imagine how the relevant knowledge would be captured by any formal device, even if we allowed such a device to be monstrously large and complex. Should we say that there is an inherent feature of Atlantic of the kind suggested in (19a), or perhaps an inherent feature of rowboat as suggested in (19b), or both?

- (19) a. Atlantic \Rightarrow crossed by a man in a rowboat
- b. rowboat \Rightarrow used by a man in crossing the Atlantic

Or should we perhaps say somehow that the idea of a particular man emerges only when the lexical units Atlantic and rowboat cooccur in a particular way? It is clear that some people have this knowledge, but it is not so clear how it can best be represented. I am doubtful that it should be represented in the form of the semantic structure of a specific sentence, for example the sentence A man crossed the Atlantic in a rowboat. I have the impression that such knowledge is not tied to particular sentences, but that it is instead capable of being plugged into a variety of semantic structures of particular sentences, of which (18b) is but one of many possible examples. But perhaps more general ways of representing such knowledge can be found--ways that are still commensurable with the elements and relations of semantic structures, so that their interaction with the semantic structures of particular sentences can be formally described. It would be discouraging indeed to find that such knowledge can't be accommodated within the semantic universe at all, for then there would be a disturbing discontinuity between the explanations of definiteness given prior to sentence (18b) and the explanation of definiteness in that sentence. For the time being I would like to hope that the difficulty in explaining (18b) is only a matter of degree--that a formal explanation along similar lines is possible, but that it calls for a formal apparatus of almost unimaginable size and complexity. (Perhaps this is the time to point again to the likelihood

that some knowledge is not so easily plugged into language, and is not, therefore, so easy to talk about. As linguists, however, I assume that we are primarily interested in knowledge that does plug in easily, and I hope that even this last example belongs in that category.)

The situation, as I see it, can be summarized with reference to the picture in (20):



The two boxes represent the knowledge held by the speaker and the hearer. Possibly the larger portion of this knowledge is held in common, but certainly a sizeable part of it is held only by the speaker, and part only by the hearer. What language is all about is suggested by the arrow; namely, an attempt by the speaker to change some of the knowledge that is initially his alone into knowledge that is shared. That's why the speaker speaks. To the extent that its object is achieved, language reduces the store of knowledge that is private to the speaker and increases the store that is held in common. Of course, if the participants are engaged in a dialogue the hearer will subsequently become the speaker, and some of his private knowledge will then be transmitted in the other direction.

But a speaker rarely if ever transmits nothing but knowledge from the speaker only category. Evidently a hearer is unable to assimilate new knowledge unless it is presented within a matrix of things he already knows. What a speaker is obligated to do, therefore, is to reach down into the knowledge that is held in common, and to mix the new knowledge he wants to communicate with a generous proportion of old knowledge, so that the hearer can tell where this new knowledge fits within the store of things he already knows. All of my previous discussion can be seen in this light. For example, the definiteness of whale in (8b), of animal in (11), of teeth in (13), of false teeth in (15b), and of man in (18b)--this definiteness is a way that the speaker communicates to the hearer that this is a whale, an animal, teeth, false teeth, or a man that is already in their fund of common knowledge. The speaker is saying something further about this already known object or objects. I have sketched just a few of the ways in which the presence of such prior knowledge might be accounted for. We have seen that some of these ways are not too hard to formalize in reasonably general terms, while others seem to call for a formalization of the myriad specific pieces of information that make up the bulk of the in common area in

(20). Near one extreme we have the definiteness of whale in (8b), rather easily predictable through a rule of the sort indicated in (9), even though that rule would have to be complicated in certain ways. At the opposite extreme we have the definiteness of man in (18b), whose prediction seems hardly feasible. Perhaps the definiteness of teeth in (13) lies somewhere in between.

I find it helpful to view the currently popular notion of 'presupposition' in this light. One kind of thing we could say about (8b) is that it presupposes prior knowledge of this whale. Similarly, regarding (18b) we could say that it presupposes prior knowledge of this man. That kind of statement, however, really does nothing more than describe the meaning of the contextual feature definite. For a concept to be specified as definite means that there is prior knowledge of that concept. Calling this meaning a presupposition doesn't really add anything. More interesting, however, would be to say that (18b) presupposes that a particular man crossed the Atlantic in a rowboat. What such a statement does is actually to turn the problem we have been considering on its head, whereupon it may deceptively seem no longer to be a problem. Instead of asking what kind of knowledge allows us to treat man in (18b) as definite, we ask instead what the definiteness of man in that sentence tells us about underlying knowledge--what it 'presupposes' about such knowledge. We don't ask how knowledge influences what we say, but instead how what we say provides clues to what is known. But it should be observed that this presupposition approach doesn't really solve the problem of formalizing human knowledge. It just says that we tackle the problem one sentence at a time--considering for any sentence we might want to consider just what that particular sentence tells us about knowledge. It's a way of making the best of a difficult situation by saying that, since we can't do all of the job, we'll just do as much as we need to to explain what we happen to be interested in at the moment. Our basic problem remains.

I would like to close what may have seemed a pessimistic presentation with a note of optimism. What we need to give up, I think, is the idea that we can fully account for the well-formedness of discourse in the foreseeable future, the idea that it is the kind of problem we can wrap up within our lifetime. The enormous store of information that must be dealt with precludes such a satisfying solution, no matter how pleasant it may have been to contemplate. If we resign ourselves to the enormity of the task, we can for one thing, as I think we have been doing, focus most of our attention on those parts of human knowledge that are particularly salient or general, not worrying too much about such minor details as the man who crossed the Atlantic in a rowboat. In addition, however, and here I think there is particular room for optimism, we can search for the general principles by which

knowledge is organized, principles from which a viable theory of semantic structure can be composed. That we can discover many of the most important principles of this kind during our lifetime is not so out of the question. Furthermore, the search for such principles is to my mind much more important than any other task we might undertake, in our desire to understand the workings of the human mind as evidenced through language.

DISCUSSION

SESSION 1

Peter Maher, Northeastern Illinois State College: I'd like to address my remarks first to Mr. Chafe. First, the 'biggest' thing. There are whales that are not toothed, e.g. the baleen whale, which have whalebone instead of teeth. Aside from that, don't you think, as far as George Washington's false teeth are concerned, that the definite article here stems from the fact that this is a conversation piece: his having had false teeth in those days is a curiosity in our eyes. It's a historical fact that he had a famous set of choppers.

Wallace L. Chafe, University of California, Berkeley: Are you saying the definiteness results only from that? Actually I didn't know it. It's interesting but I don't think the definiteness depends on that.

Maher: If you were George Washington you could probably prove the point, but I think this clouds the issue. (My point here, which I add at editing, is that grammatical analyses must be consistent with objective facts known to the authors of the utterances we are parsing.)

The old distinction of 'count' nouns and 'mass' nouns has troubled a lot of people. Isn't the feature 'count' inherent in anything biotic? I mean, that's how you get plants and animals. You get only individuals.

Chafe: That may very well be true. You could say it the other way around, that 'biotic' is included in 'count'.

Maher: The question has been asked about grass and coral which are not 'count' but they do consist of individual plants and/or animals. My son at the age of two was saying 'grasses'. I think it's a simple cultural fact that 'grass' could be singular in some languages and plural in others.

Chafe: This is a kind of question of exactly how we should map what I was calling conceptual space, and it does seem that most things, as

you say, which are 'biotic' would be included within 'count', but not all. In other words, the two circles would intersect rather than one being included within the other.

Maher: Then you could further specify they eat whale without a dichotomy between 'count' and 'mass' nouns; you would have the partitive of biotic things.

Chafe: Well, I regard that as a different whale, if you like. The way I treated it, it's a 'mass' noun that is derived from the 'count' noun. I think of whale as being basically a 'count' kind of concept, but that there is this 'mass' concept of whale which is derived from it. This is true in a great many instances.

Maher: I have a proposal. I don't know how naive it is or whether it would solve any problems, but both in regard to your question in characterizing the two sentences about I saw a man who crossed the Atlantic in a rowboat or I saw the man who crossed the Atlantic in a rowboat, can they be specified in the terms of the different wh questions that each answer--like 'who did you see?' versus 'which man did you see?'

Chafe: Yes, those things are certainly all tied up together. I think 'which' goes along with definiteness. I mean that there is a known set, these individuals that you already know about, and you want to know which one from among them.

Maher: From there I would lead into Prof. Fillmore's paper. Wouldn't it be possible to label your sentence roles with the wh question that each answers? For instance, W. C. Fields in 'International Hotel' ended that film with deliberate misinterpretation of a question by his companion (a blond). They were flying out of this hotel in an autogyro together in a completely improbable scene. The blond sits on the co-pilot's seat. There happens to be a brood of kittens there. They're all shapes, sizes, and colors; and she says, "I wonder what their parents were?" W. C. said, "careless, my dear, careless." Now, can't we conceive of a wh question like 'what category, what kind, what race?' versus a wh question that is answered by 'manner'?

Charles J. Fillmore, Ohio State University and Center for Advanced Study in the Behavioral Sciences: I don't quite understand the proposal.

Maher: I'm proposing to label sentence roles or cases with questions of the wh sort. Just as school grammar in German, for instance, will speak of Wer-fall, Wem-fall, Wes-fall, Wen-fall. The difference

between I wonder who's kissing her now and I wonder who's kissing her cow is that cow answers 'what' while now answers 'when'.

Fillmore: I take it just as a proposal for labeling cases with wh words. I don't care what words you use. If your proposal also involves the claim that the set of interrogative words in a language matches the set of cases, then it's more interesting. But I don't know if that's what you're saying. Anyway, it's not true.

Maher: That was my intended point.

James D. McCawley, University of Chicago: This is also on Chafe's paper. First, one relatively minor thing, and then something a little more substantial. I think your choice of delicious rather than good tasting in example 7 was unfortunate. I have in mind a rather peculiar property of delicious as opposed to good tasting, namely that it is normally used only in commanding. There's a delicious whale on the beach really sounds like the kind of thing that a Japanese would say rather than an English speaker. It's also peculiar to use delicious in a sentence like this meat isn't delicious, although good tasting is possible in such sentences.

My more substantial comment has to do with what you said about the definite article. There is a fair range of things which are done with definite articles and definite noun phrases which weren't mentioned and which are worth bringing out. Keith Donnellan in 'Reference and definite descriptions' (Philosophical Review 75:280-304) discusses the important difference between what he calls 'referential' and 'attributive' noun phrases. An example of a referential noun phrase would be something like the following: you're talking to somebody at a party and say the man in the corner with the martini in his hand is having an affair with my wife. There the truth or falsehood of the sentence has nothing to do with whether the guy has a glass in his hand or a grenade or whether the glass has got a martini in it or chicken soup. It is true or false simply according to whether the person you are referring to is having an affair with your wife. A 'referential' noun phrase is merely used to single out the guy you happen to be talking about. By contrast, the NP in The man who committed this crime should be hanged is part of what is being asserted. This is what Donnellan calls an 'attributive' definite description and it is the kind of definite description that logicians have generally talked about. Distinct from both of these is the anaphoric use of a definite noun phrase, as in your example (11), let's go look at the animal.

There are some rather interesting restrictions on what noun phrase you can use in which situations. You mention that let's go look at the animal is a little bit awkward. If you put in place of animal other nouns

that would be applicable to whale like say mammal or cetacean, you get something truly weird. I think what usually happens with that kind of use of a definite noun phrase is that except in cases of contrast (e.g. you're talking about two different things, one's a mammal, and one isn't, and for some reason that's the dimension you use to distinguish between them), the noun used is what Lakoff calls a 'species'. I wish I had a better name than species. The idea is that there are certain words which correspond to that point in the taxonomy to which one refers when an exact description is not called for. Lakoff notes that while a huge range of nouns could be used to describe some specific object (e.g. vehicle, car, Chevrolet, compact), except in cases of contrast, if you're referring to it anaphorically, you pretty much have to say the car rather than the Chevrolet. You would use a more specific term only in cases of contrast and would not use a less specific term (vehicle) anaphorically at all.

An interesting grammatical problem has to do with the choice of genders when a pronoun is used without an antecedent in a language with grammatical gender. Lakoff, on the basis of a small amount of field work with French and German speakers, conjectures that the pronoun normally used has the gender of the relevant species noun, so in particular if you're talking about a car, the gender of the pronoun would be that of the word for 'car' rather than of the word for some special type of car.

Chafe: Yes, there seem to be certain points in the taxonomy that are more salient, that would stand out and are usually the ones that are used in the absence of some reason for using one of the others. I was wondering if the reason you wouldn't say let's go look at the mammal or things like that isn't also partly because mammal is a kind of technical word. It isn't part of our folk knowledge but rather part of our superimposed school knowledge.

Ursula Oomen, Georgetown University: My question to Prof. Chafe concerns the relation between inherent features and surface structure. I wonder whether we do not have to distinguish between the inherent feature 'big' and big in the surface structure. In the surface structure we can contrast big whale and small whale. In these examples it would seem that big adds some additional information to those features inherent in whale. Big in a big whale is then no longer a surface structure expression of the inherent feature 'big' which is attributed to whale.

Chafe: You're saying it with a stress on big. Is that what you mean?

Oomen: That's right. Is that your idea?

Chafe: Yes, that's true, but I'm saying it with the unstressed one. I mean that this unstressed thing occurs in cases where you know some inherent property of this object.

Werner Winter, University of Kiel: I was wondering about these inherent features said to be placed in a hierarchic order. This I think is one of the really troublesome things. One has the impression that to a certain extent these things really occur in order--one implies the other and you have a sequence. But let us take up the whale again and consider one piece of information which I think is much more common than the information that a whale is a mammal, namely that which tells us that the whale is a marine animal. Now where in the arrangement of features would we place marine? Would we place it before animal or after animal? Obviously not all animals are part of a group of marine animals, nor are all marine things part of the group of animals. So obviously we do not have an hierarchic arrangement even within the inherent features. Now how could we handle this problem? This has been disturbing me a great deal.

Chafe: Well, that's certainly true that not all these things are arranged hierarchically. That's obviously an oversimplification. I would think that in this case you'd have two circles which intersected--one in which there would be 'marine things' and the other in which there would be 'animals'.

Wallace Ceyak, Georgetown University: This is also directed to Dr. Chafe. I've only had the pleasure of reading parts of your book, but I notice that you refer to certain overriding qualities which certain word-types have that allow them to occur together--in other words, cooccurrence restrictions, or cooccurrence allowances. I was just wondering how far you've gotten (since you've written your book) in establishing the crucial qualities of words which allow them to occur together. For example, when you talked about delicious whale in your prior experience. We of course know that a whale is edible and therefore has the potential of being delicious. But more than that, certain terms can contain other terms as subsets. Just how far has your work progressed in getting down to those crucial qualities that words have which allow them to occur together in certain structures?

Chafe: I guess the answer to that is very simple--not very far.

Ceyak: And one more question. If we are going to get into this semantics business, as linguists of course we're going to have to tell how certain words can occur with each other, but when you talk about George Washington's teeth and George Washington's shoes and when you

talk about the man who went across the Atlantic in a rowboat, you're signaling a certain amount of presuppositions that depend on a great deal of prior knowledge. For example, with reference to the man who went across the Atlantic, you feel that he went across the Atlantic recently, and that it was part of a newsworthy event; but is it really a property of linguistics to go that far? Isn't there a semiotic point here--a truth or falsity, or an idea of when things can occur together? Isn't there something outside our studies or something that is just beyond our range?

Chafe: Yes, I think that's the crucial question. I was suggesting that maybe there's no way to draw a line like that and say that certain things are outside, certain things inside. At least I don't know where the line should be drawn. As far as the false teeth are concerned, I don't think that necessarily depends on any knowledge about George Washington.

Ted Higgs, Georgetown University: I would like to ask Prof. Bach a question about syntax. It seems to me that one of the major contributions of Chomsky's 1957 version of transformational grammar was that certain relations which were intuitively recognizable among certain sets of sentences could at last be systematically described and explained. In that model, the relating of sentences was explained by describing the class of 'kernel sentences', that is, sentences to which only obligatory transformations had been applied, and then using the underlying structure of the kernel sentence as the input or structural description for a number of optional transformations which generated the various types of interrogatives, emphatics, passives, and the like. By making explicit the semantic-syntactic relations that clearly held between the kernel and the set of sentences which could be derived from it, Chomsky was able to show very systematically how, and to what degree these sentences were related. This concept also led into the idea that sentences which at some important point in their derivation had the same deep structure also had the same constructional meaning. In the 1965 model, however, the whole notion of optional transformations was abandoned in favor of introducing into the Phrase-marker itself--in the form of 'triggers'--obligatory instructions to perform such transformations as NEG, IMP, Q, etc. An immediate consequence of this is that at a most fundamental level, John went and Did John go no longer have the same, or perhaps even a comparable deep structure. I wonder if, given this putative simplification of the grammar, it is still possible to relate formally say an active-transitive sentence and its passive, or if, having established a new kind of theoretical deep structure construct in the form of the triggers, there is any way to (1) distinguish such constructs from grammatical categories

and grammatical relations, and (2) state in some way that sentences which have deep structures different only with regard to these new constructs are clearly and intimately related, while sentences which have deep structures different in any other kind of deep structure elements are somehow less closely related.

Emmon Bach, University of Texas at Austin: Well, I think the first answer to your question is that in the model of Aspects, that is, taken with the particular analyses that were defended and referred to in Aspects, mainly from Katz and Postal's book and so on, in which those tricks were played in which special kinds of markers were put in, that the sentences were not shown to be related in any kind of systematic way but only by a kind of ad hoc way. Katz and Postal, I think, did try to get at this in the sense of talking about certain universal properties for these various markers like IMP for imperative and so on. But I think the kind of criticism implicit in what you are saying is essentially correct. As far as the present kinds of analyses go, I think that there is a sense in which the analysis using abstract performative verbs and so on as carried out by George Lakoff and Robin Lakoff, Haj Ross, and others, makes it possible perhaps to reconstruct the notion of relations between sentences in a way that has some sort of natural interpretation. That's a very hazy kind of answer, but that's the best I can do.

CRUCIAL QUESTIONS IN THE DEVELOPMENT OF TAGMEMICS--THE SIXTIES AND SEVENTIES

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Abstract. The sixties began, for tagmemics, with publications answering the question: What kind of high-level generalization can be made to characterize all units of human behavior or knowledge? A large number of studies of grammatical pattern and of phonological hierarchy grew out of this approach during the sixties.

By 1961, a crucial new question under attention was: Can there be a grammatical chart (a grammatical feature matrix) comparable to a phonetic one, and which is itself a unit? This first led to studies in the relationship between clauses, and then to integrating them with morphology, and morphology to some principles of historical change.

In 1963, a further question arose: Can matrices be developed to show the relation between situational role (case) and grammatical role? And can these be related to discourse structure? Becker and Wise, after the middle of the decade, worked on these questions, studying also the work of Fillmore which was beginning to contribute heavily to case analysis. Wise carried the underlying question further, to the study of the lexemic structure of discourse, while Longacre contributed vigorously to the study of grammatical constructions at that level.

By 1964, questions concerning the generativeness of tagmemic formulas, under the stimulus of transformational grammar, had been discussed by Longacre, and later followed up by Cook.

By 1968, a new question arose: Could group theory, from mathematics, aid in the description of pronominal sequences in embedded quotations, and in discourse structure itself? Pike and Lowe joined in this endeavor.

For the seventies: (a) How continue the mathematizing of discourse structure (Lowe, Wise, Grimes, Hale, Pike)? (b) How relate hierarchies (Longacre, Wise, Pike)? How show the relation between various

current theories? (c) How continue studying 500 languages so as to gather data to set up, inductively, substantive universals, and to test the validity of those guessed at deductively?

A theory can be characterized by the questions which it chooses to ask.

The first crucial question:

(1a) Who was Mr. John Doe III, born August 14, 1938, on Evans Way, Boston--have I ever met him?

This question is normal, rational, reasonable. Its validity is a presupposition of all rational discourse. If the question is in principle not answerable, then all human behavior as we know it must cease, all intuitive tests for validity are lost, and knowledge itself evaporates into skepticism.

This affirmation may be viewed as the epistemological starting point of tagmemic theory--a theory which in 1960 reached a crucial point in its development with the appearance of the last volume of the first edition of my Language in Relation to a Unified Theory of the Structure of Human Behavior. Tagmemic theory staked its claims on the belief that essential to the description of human behavior as we live it must be the ability to recognize a friend even though he has just had Wheaties for breakfast, cut his long hair, and replaced his necktie. Most of us do not know the shape of the molecules inside our friend's liver, nor even the length in millimeters of his small intestine. But lack of complete knowledge does not prevent us from knowing John Doe III.

We are able to recognize some objects in spite of the fact that we do not know everything about them. Similarly, and by direct epistemological parallelism, we must be able to recognize events, and we must be able to discuss rationally whether or not two particular utterances are or are not instances of the same poem, or of the same clause, or of the same word, in spite of changes in them over time, variability in context, and similarity to other--but contrastive--forms. All of this, then, leads to Question (1b), treated in the volume already mentioned:

(1b) What kind of high-level generalization can be made to characterize all units of human behavior or knowledge?

What is it, that is, about any person, event, situation, object, concept, which allows us to call it a unit, and to describe it adequately? By 1960, tagmemics (in the volume referred to) had finished answering this question in general terms: (a) A unit--any unit--in order to be well described must have had specified those characteristics which differentiate it from every other (and these same features allow it to be identified in further contexts); (b) its range of variability and its necessary physical components must have been given; and (c) the range

of contexts in which it may appropriately occur (its distribution in class, in sequence, and in system) must be stated. (Or, its feature mode, manifestation mode, and distribution mode must have been specified.)

But it was from a related question, which I first asked in 1948 and for which provisional answers began to emerge in 1949, that I date the beginning of tagmemics:

(1c) Is there a unit of grammar which would be as important to us--if we could find it--as the phoneme (or--rephrased for some scholars in the 1970's, perhaps--as the phone)?

Late in the Spring of 1949 an affirmative answer began to emerge--developed as tagmeme-in-syntagmeme by the 1960 third volume. The sixties, however, were a drab climate within which to continue this affirmation. As the decade began, constituents--as units needing mutual relating by means of a generalization about units as such--were in general repudiated or ignored by theory, even while cropping up as undefined terms in practice. Fortunately, as the decade closed, however, attention to the validity of constituents, and to some other units, began to receive more explicit attention. At the beginning of the sixties, however, tagmemics--on the theoretical front--stood alone in any attempt to capture the generalization directly; but by tagmemists the concept was being heavily and usefully exploited. (See annotated bibliographies of Pike 1966b, and Brend 1970, for monographs and articles by--among others--Pickett, Waterhouse, Blansitt, and Brend for grammar; Eunice Pike, Scott, West, and others for phonology of upper hierarchical levels; Grimes for some early incorporation of both tagmemic and transformational views.)

These studies involve another question, however:

(1d) How are these different elements hierarchically related?

Once the concept of unit had been firmly accepted by the theory, the hierarchical nature of some of these units proved inevitable, and was heavily developed in the 1960 volume.

The relationship between the hierarchies received initial attention, so that by this third volume specific sections were beginning to explore the relationships: phonology to grammar, phonology to lexicon, grammar to lexicon. Specific studies, also, were developed. Among them, for example, was one on Auca (Pike 1964b) in which a most elegant pattern showed a chain of alternating stresses on a verb beginning from the first syllable of the stem, moving towards the end of the stem, and a second chain of stresses beginning from the end of the suffix chain working back towards the stem; the addition of another syllable (another morpheme) affected the place of stress. Elegant rules led to adjacent stresses when the stress chains clashed at the juncture of stem with suffix.

In spite of some such studies, however, the question of the interlocking of the hierarchies was in general given only minor attention. It was left to other theories during the sixties to focus heavily on this point (such that transformational grammar attempted to cross over the hierarchies--in our view--by way of rules, and stratificational grammar did so by networks). Their contributions pose further questions of tagmemics for the seventies: Can tagmemics utilize the extensive results of the two mentioned theories to refine the limited statements of the early tagmemic approach, while retaining its own unit contribution?

While other theorists were dealing with these matters, however, the attention of the tagmemic theoretical front moved to a different question--our leading theoretical question for the first half of the sixties. It was this question which opened my Presidential address to the Linguistic Society of America in 1961 (see Pike 1962a):

- (2a) Is it possible that there is some kind of grammatical chart (or 'matrix') comparable to a phonetic chart, with contrastive features for rows and columns, but with grammatical units in the cells?

This question had as a pair of presuppositions both the presence of grammatical units (just as a phonetic chart has a presupposition of some kind of phone, whether or not one grants the phoneme as a unit) and the relevance of contrastive features of grammatical constructions (analogous to distinctive features of voicing, aspiration, and so on, of phones).

By 1961, in a workshop in Peru, this question was solved in a preliminary form. Mildred Larson provided us with a contrastive chart of Aguaruna clause types, with imperative versus stative, active, and equative as features of one parameter, and transitive, intransitive, nominative as features of the other (in Pike 1962a:222). This development opened up a whole new area for effective presentation of total structure in a simple, easily graphable fashion. Relations between clauses (or between sentences, phrases, etc.) could be easily seen as a function of the change of one or more sets of features. It opened the door, furthermore, to an easy way of specifying a relation between one whole set of grammatical constructions and a different set of constructions merely by the addition or subtraction of a set of features. Thus, one could start with a set of kernel constructions, and (a) show derivation of a second set from such a kernel (if one wished to use derivational terminology, which for some purposes continued to prove valuable) or (b) one could specify the two sets as mutually related by having comparable cells for many of the same contrastive features of their respective charts (without specifying a derivational base). Regardless, that is to say, of the theoretical statement of the relationships, this display of the relationships is clear and helpful.

In addition, the matrices themselves can themselves be units which are hierarchically related, such that a matrix of verb stems may involve some of the contrastive-feature sets which later differentiate the clauses--as in Larson's situation. A series of such charts at various levels of the grammatical hierarchy is helpful if one wishes to take advantage of the theory in order to get a quick insight into a total pattern. For example, just as in the Bloomfieldian era various scholars, including Bloomfield himself, would utilize or publish phonetic charts even while repudiating their theoretical relevance, so today a matrix of grammatical constructions is an extremely useful tool for insight into a system even for those people who ignore or repudiate the validity of the analysis of the grammatical matrix as a unit.

(2b) Can a system of grammatical constructions, seen as a matrix of units, itself be a unit?

The tagmemic answer is a firm yes. It is not only actual behavioral events which can be units (along with physical objects or John Doe III himself), but systems as well. If we ask: A unit for whom? Then one may answer, if one chooses: A unit in the output of the analyst (rather than in the behavior of the speaker). At this point, tagmemics is quite ready to grant that there may be units in the behavior of the analyst which are not necessarily in the behavior of the non-analytical speaker. We merely add that an analyst is human--and, if one of his features is a matrix-creating capacity, then it is a human capacity of interest to our theory. Here tagmemics does not commit the epistemological fallacy in which a theory covers all statements about the world except the statement about the statements.

(2c) Can such feature-matrices of various types be extended profitably to nonverbal situations?

The matrices of Bock (1962; see also 1964, 1967, annotated in Brend 1970), developed independently of ours, shows this to be feasible, with space-time-role arrays (discussion in Pike 1967:667-78, 673-74).

The feature-times-feature matrix, however, was not the only one which gave positive results. There was also the one of unit-times-sequential components, which led to various kinds of syntactic paradigms. Here, the question of the matrix was:

(2d) Can redundancy be utilized to display illustrations of a syntactic system in a compact form, such that illustrative material will vary only when the structure demands it?

The result was a paradigm in syntax comparable to paradigms in morphology, but integrated with them: successive paradigms moving down from the clause into morphological structure were helpful. (See, for example, Pike 1962b on Kanite, and 1966a on African languages.) Out of these matrix materials, however, a third major questions arose in the first of the sixties:

(3a) How can we handle relations of form to meaning in a morphological system when the relationship of form to meaning is not one-to-one?

Here a technique was made explicit which, when used at all by other scholars, had been used intuitively but without an explicit heuristic which could be generalized and taught in the classroom (see Pike and Jacobs 1968). The device was to record the affixed forms of ordinary paradigms in some kind of a feature matrix. Having done so, one by one, rows were permuted so that, for example, row two might become row four, while four became two; columns were similarly permuted, with the goal of bringing into a connected solid block all those segments (called 'formatives') in the cells which were phonologically alike.

The resultant formative blocks turned out to have considerable importance. On the one hand, they represented the logical intersection of features from two or more parameters (instead of semantic features being represented through a single row, consistently, by some phonological trait). A formative block would represent only those places at which certain specific features from the rows intersect with specific features from the columns. As a result, the classical morpheme (containing a form and its meaning), became a special instance of a matrix formative--a formative in the shape of a single row. Morphophonemics, on the other hand, turned out to be a device--seen from this perspective--to make a row appear to be uniform when, in fact, it was not. That is, by the replacement of certain formatives with another formative from the same row, the row itself appeared symbolically uniform--i.e. was morphophonemically written--so that the morpheme itself then had a single phonological representation in the morphophonemic formula.

But the extreme opposite to the classical morpheme was the formative which occurred in one cell only. In this instance the formative signalled two (or more) semantic features simultaneously--one from the row and one from the column. The formative was multisystemic. If the whole matrix were of this type--which I called an 'ideal' type as over against the 'simple' one of classical morphemics--every formative would have more than one meaning, and no simple morphemes would occur.

When we tried to understand, however, how such patterns of formative blocks could come about, other questions arose:

(3b) How does change in a morphological set of affixes appear over time, seen from the perspective of morphological matrices?

The answer here was that there must exist two driving mechanisms: the one is a drive towards phonological fusion in a high-level phonological unit pronounced at high speed; i.e. smearing of phonemes and phones in a high-level phonological wave. As the smearing continues, classical morphemes begin to smear and fuse into single-celled

formatives. If this were the only driving mechanism, language would end up as completely unintelligible, made up of a million (plus) separate items. There has to be a mechanism which induces a 'counter-flow' toward re-establishing some degree of morphemic simplicity of the classical type. This counter-flow can be seen as working through matrix analogy, which brings to bear various kinds of systemic pressures. If rows and columns of formatives which were incomplete or irregular were to be extended and made uniform, there would be re-established the simpler kind of system. Thus, the historical principle involves phonological fusion leading to the loss of classical morphemes, combined with matrix regularization leading toward the reestablishment of classical morphemes. (See Pike and Scott 1963, Pike 1965, and bibliographies.)

At the same time, agreement irregularities within a system had to be described, which led to the following question:

(3c) How could concord be handled simply in a way which avoids listing all the details every time--whether the detail be of a feature specification or of a phonological-context specification?

Here our answer again was in terms of the morphological matrices. (a) If there were two units in a sequence in which some or all of the same contrastive features were used, comparable matrices should be developed at those points, with features lined up in the same order. (b) Then, to show concord, it would not be necessary to list details--nor even rules of detail--but it would be only necessary to list the number-index of the matrices involved, with a rule saying that one must pick from each matrix indexed in the relevant string the topologically equivalent cell--i. e. a cell from the same numbered row and column. Thus a high degree of concord could be handled by a simple indexing of matrices. (See, for German, Pike 1965.)

Such a use of various kinds of charts led to the attempt to classify tagmemes themselves in a systematic way. From the first tagmeme publication, I had had subject tagmemes contrasting by elements such as agent versus goal. This led, specifically, to a 'distribution class of tagmemes' which included the subject-as-actor tagmeme in contrast to the subject-as-recipient tagmeme in active and passive clauses respectively. (See Pike 1954:Sections 7.3, 7.321, 7.43, 7.6; or see revised edition 1967, same sections, pp. 196, 219, 231, 246-48.) In the early sixties, however, I wished to develop some kind of array in which we would have the 'logical' or 'structural-meaning' component as one parameter, and the grammatical elements as another. This led to one of the most important tagmemic questions of the sixties:

(4) How are the 'situational' roles such as agent or goal related to 'grammatical' roles such as subject and object--and how are these related to discourse structure?

The question was answered programmatically in a paper on discourse analysis presented to the Linguistic Society of America in the summer of 1963, and published a year later (Pike 1964a). In it, on the one hand, were several kinds of matrices, including one with situational role (now, since Fillmore, usually called 'case') versus grammatical role and, on the other hand, there were tagmemes made up of combined situational and grammatical role, as one parameter, over against certain kinds of observer involvement (focus, emphasis, surprise), over against a grammatical hierarchical level. This article developed in a workshop in the Philippines where I was struggling with problems of topic (focus) and its rearrangements in clause structure pointed out by my colleagues of the Summer Institute of Linguistics. In this article I also pointed out that if the same event were reported from several points of view--with different people telling the same story--the situations would be constant, but the grammatical roles relative to the narrator-participant would vary. Within the tagmemic literature, three more-or-less independent approaches have attempted to grapple with this kind of data, and have carried these programmatic suggestions much further.

In the Philippines itself, Myra Lou Barnard developed an elaborate tree diagram of a narrative (from discourse to morpheme), with a black-outlined tree giving grammatical structure and a red-outlined, printed overlay giving the tree structure of the lexemic pattern (see Longacre 1968, volume 3, inserted folded appendix). In addition, she co-authored with Longacre a section of his report (1968, now printed in Vol. 1, Part 3, of Longacre 1970) discussing details of the interlocking of grammar and lexicon--or that which I now call lexemics--on levels of discourse, paragraph, and sentence.

A second development, on a more restricted topic, the subject of English clause, occurred in a dissertation by Becker (1967) who discussed four components of a tagmeme: a grammatical form (subject); grammatical meaning (agent); lexical form (noun phrase); and lexical meaning (single male human). Before completion of his work, however, he had access to Fillmore's early work on case (1966), and was able to discuss some of the similarities between the approaches. The need for further study of the relation between the implications of tagmemics and of case grammar comprises a major topic of concern for the seventies; at this point, tagmemics should benefit greatly from the work of the case grammarians. (See also Cook 1971.)

A third stage in the development of a tagmemic view of the situational versus grammatical role material, however, was given in the dissertation of Wise (1968). She had gathered material in Peru in order to check on my suggestion about permutations of participant relations (in situational versus grammatical roles) in the multiple tellings of a story from different viewpoints. The size of the problem became so large,

however, that she had to limit her first goals, so that some of her discourse material was delayed. (See Question (7b) where I return to her more recent work.) She made a major advance, however, by insisting (a) on the retention of form-meaning relations in setting up these lexemic units, so that certain lexemic elements were not treated merely as abstract semantic components; and (b) that lexemic constructions occurred at all hierarchical levels, from word to discourse. This was crucial, since it allowed us to insist again on the necessity of form-meaning composites, hierarchically structured, which had been basic to tagmemics from the beginning. The alternative of abstracting the semantic component away from any necessary form, so that one had some kind of deep abstraction versus surface objectification, was unacceptable to us both. The view which Wise developed appeared to me to tie into the epistemological outlook of tagmemics which keeps us always in touch with both physical and psychological reality at every stage in the generative or representational process. (The minimum physical component of every unit--including that of a false concept--would be some kind of neurological one.) At the same time, Wise carried further the relation of meaning to the observer viewpoint, to the plot, and to the social setting. For the seventies, I have already begun to explore the extension of this approach in various directions. (For a related view, with less hierarchical emphasis on the lexemic component, see now Ballard, Conrad, and Longacre 1971.)

The linguistic climate of the early sixties faced the question:

(5a) Is a tagmemic formulaic representation generative?

Longacre (1964) demonstrated that it indeed was so, implicitly, but that its generative power could--and should--be made explicit. Considerable study on generativeness of tagmemic formulas has also been handled since then by Cook (1967). The presence of this generative implication of tagmemics should not be unexpected by anyone who reflects for a moment on the fact that tagmemics grew up in a context where translation theory and practice was in view. Most people working on tagmemic material were expecting to use these formulas as a formal aid towards idiomatic translations. If the formulas were not, in fact, productive in this sense, they would eventually prove to be worthless for these purposes. The future survival value of tagmemics can, therefore, in part be measured by the degree to which this generative aspect helps nonnative speakers to cross language boundaries for translation purposes. It should be clear, then, that Longacre's insistence was not an ad hoc defense of an armchair theory, but rather an explicit component in the answer of the following question:

(5b) How is translation possible--and how is it related to tagmemic formulas?

It should appear that the grammatical formulas of tagmemics pose grammatical constraints on all possible sequences in a particular

language, and that the lexemic formulas of Wise impose various kinds of situational constraints. For the seventies, also, it should be clear that translation interest continues for many of us dealing with tagmemics, but that the focus has shifted to the higher levels of discourse structure. (Considerable hybridization of tagmemics with stratification theory is occurring here--especially with the work of Gleason (cf. Gleason 1968)--e.g. Grimes and Glock 1970.) That is, we may ask:

(5c) What kind of discourse formulas can be generative of the grammar of narrative, or of dialogue, or of hortatory presentations, and the like?

In the last years of the sixties, major advance in the grammatical phase of discourse analysis came from Longacre on the basis of work on a couple of dozen Philippine languages (1968), and he is carrying on in the early seventies by his work on languages of New Guinea. Emphasis on the difference between grammatical construction and lexemic construction on the discourse level, however, has been largely the contribution of Wise (1968), as we have indicated.

Such materials, in combination with a tagmemic emphasis on unit and on perspectives of particle, wave, and field, were built into an approach to the teaching of composition through tagmemic theory (Young, Becker, and Pike 1970). This suggests the development of further applications to the study of literature in the seventies.

We turn back now to discuss one other crucial idea extended during the second half of the sixties:

(6a) Can grammar be viewed as a 'wave', profitably, much as high-level phonological units and stress groups were sometimes viewed by tagmemics as waves, and sometimes as particles?

In the sixties, the development of phonology as wave was extended. There was an attempt to exhaust, as an underlying general principle, the kinds of things that could happen to any high-level phonological unit (Pike 1962b); and numerous studies on phonological hierarchy were related to this (see bibliographies cited--especially works by Eunice Pike). For a long time, furthermore, I had been interested in the way that phonological fusions led to grammatical problems (see, for example, discussion above about fusion in morphological matrices). On the basis of experience in Africa it was clear that it was not merely phonological units which changed relative to their place in the nucleus or margin of a phonological wave, but that grammatical units could change according to their place in reference to their closeness to the nucleus of the grammatical construction--viewed-as-a-wave. The closer to the nucleus the unit under attention came, the fuller and more contrastive and greater the set of freedoms the unit might enjoy, including modifiability by other tagmemes. The farther from the nucleus, the more the unit might be abbreviated or lose its freedom of distribution and of

semantic variability. (This material was presented when I was last at Georgetown--see Pike 1968.) There were sharp differences between phonological fusion, where the phones themselves could be modified, and grammatical fusion, where it was degrees of constraint on freedom of membership of morphemes in classes, presence or absence of additional modifying tagmemes, special phonological rules, and special semantic elements which were in general involved.

This material still awaits extensive development; it is difficult to handle since it requires simultaneously a theoretical commitment (a) to the presence of units and (b) to indeterminacies between them or their changing manifestations. This question has lain dormant, therefore, for half a decade. In the meantime, however, it fills in a major point in the speculative pattern of the tagmemics of the sixties--and conceivably could develop into a major point of interest in the seventies.

(6b) To what extent is it useful to view language and other human behavior via perspectives of particle, wave, field?

It should be clear that, in the fifties, ending with the publication of Pike 1960, the emphasis of tagmemics was clearly on defining the unit, which in this context we may call a particle. On the other hand, it should be equally clear that with the development of the matrix material in the sixties, an alternative view could be held--a view which I called 'field', or a matrix of relational, grammatical characteristics. With the development of the viewpoint of grammar as wave--supplementing the earlier phonological materials--this third possible perspective joins the other two for language and behavior as a whole.

It has proved impossible to handle all the materials of interest to me from any one of these three perspectives. Nor is it possible for me to integrate them all into a single statement. Rather the differences represent differences in observer standpoint; and as the analyst-observer changes his stance, he sees things from a different perspective, and describes them differently. It is this observer stance which allows representation of the same materials either as a set of particles (with variants), or as a set of waves, or as a point or points in a set of intersecting features (as a field).

This multiple viewpoint has proved extremely useful. I do not know of any more exciting set of relationships which I have ever studied. Again and again it has proved fruitful in driving me to look in profitable directions. Nevertheless, the metaphor has been troublesome. To a theoretical physicist and mathematician such as Ivan Lowe, for example, the metaphor of field seems misplaced. He and Peter Fries have urged me to define the terms 'wave', 'field', and 'particle' in reference to other terms, so that the metaphor could be in principle dropped. So that one of our tasks for the seventies (already under way) is to answer the question:

(6c) How can the terms particle, wave, and field be defined in multiple sets of postulates by nonmetaphorical terms, such that the resemblances (or identities) between them can be seen, the terms themselves can be avoided, and the alternative viewpoints formally exploited?

Since the areas covered by the terms overlap (inasmuch as any element can be viewed as particle or as wave or as point in a field), there are redundancies in the system. Unless these are formally specified, we may end with confusion. In addition to refining my first postulate set, however, I hope to be able to show that with alternate sets of postulates we can begin from other viewpoints and retain all of the contributions of wave and field without certain of the difficulties currently involved in them.

But a radically different question arose toward the end of the sixties:

(7a) Is it possible to use standard mathematical group theory, with its standard axioms, to apply to some phases of natural language; and to develop mathematical theorems from them which are mappable on to natural language?

Specifically, I became interested in the problem of the use of pronouns in direct quotations, on the basis of certain problems arising in African languages (Pike 1966a:86-92, where, for example, in a story a chief will be quoted directly but a commoner indirectly, to give status to the former. It occurred to me that the relationship between three persons Abe, Bill, and Charlie, in their relationship to the pronouns I, you, and he, appeared as if in some kind of game of 'musical chairs'--with three chairs named ', you, he', in which A, B, C found seats in various arrangements. Closure occurred (since the number of arrangements was limited and recurring), so that group theory of mathematics might conceivably apply. Eventually, a mathematician--Ivan Lowe--teamed up with me in preparing a paper (Pike and Lowe 1969) in which not only is group theory used (with its standard axioms as I had hoped), but also the development was carried by Lowe to the point where he could set forth an interesting theorem which he proved by standard mathematical procedures of induction. This theorem can be used to generate to the nth embedded direct quotation the relationships between cast (Abe, Bill, Charlie), person (I, you, he), and case (agent, goal, undefined--or particular grammatical subject-object relationships). In a further paper, Lowe (1969) extended the theorem to include inclusive and exclusive plurals.

This material has led directly to one of our major questions for the seventies:

(7b) Can components of discourse structure be mathematized?

While I was working on the early stages of this material, I had wondered if the same kind of mathematical group theory might somehow be brought to bear upon the changes of relation of *dramatis personae* to

the narrative, when different participants told the 'same' story--see Question (4). Wise now took the data gathered in a Peruvian language for the purposes of the earlier question, and studied it with Lowe to see if together they could arrive at its mathematical description in terms of group theory. Certain phases of this work were completed successfully in 1970. Various scholars (Wise, Lowe, Grimes, Hale) are working to extend the area of application of this approach.

I am myself working intensively to show that the pronominal structures referred to can be used to define not only sequences of quotations within sentences, but to define certain characteristics of conversations as wholes--the structured order in which different persons are allowed to speak to other persons. Yet one serious difficulty plagued me early: there were different ways to represent each of the six speaker-addressee axes (each representing a monologue) which were possible when three people were involved in the conversation (A-B, i.e. A speaks to B; B-A; A-C; C-A; B-C; C-B). But if each of these axes were treated as a terminal of some kind, then it followed that we had multiple ways--twenty-four of them--to generate this set of six terminal strings. In some cases the mathematics was quite different (non-communicative versus communicative groups which were abstractly different--having different multiplication tables). In other cases, the mathematics was similar (abstractly same) but concretely different (having same multiplication tables but different Cayley diagrams). And some groups were completely the same mathematically (both abstractly and concretely same), but different in terms of the socio-logical assignments of the mathematical symbols. Of these various groups some were totally alien to any normal conversational constraints which I have been able to imagine; others specified certain constraints which were present in some degree in different styles or conditions of normal conversation. This led to a further question:

(7c) Granted a large number of alternative mathematical models for a simple linguistic situation, what kind of sociolinguistic evaluative measure could be found to aid in the choice of the appropriate model or appropriate models?

In this particular instance my evaluation of the alternative models is made in the following way: (a) Each model grants to each individual in the set of three individuals, starting from an initial axis arbitrarily chosen, a set of 'rights': He may be allowed to speak or may not be allowed to speak; to reply or not to reply; to shift attention to someone else or not to shift. In addition, (b) there may be general negative constraints which grow out of the sum of the rights of all individuals in the system. A person may be told: 'Don't speak until you're spoken to', or, on the contrary, he may be chided: 'Answer the gentleman--didn't you hear him speak to you?' On the other hand, (c) one may study the characteristics of a conversational 'system'--that is, the total set of

all possible sequences of conversational interchange which may be allowed within the pattern of initial rights granted to individuals.

These different patterns vary to an astonishing degree: By the most restricted pattern which I have generated, after twelve conversational monologues (i. e. after there have been twelve stages of the conversation, each stage being a different axis from the preceding one) the total system has exactly one path which may be followed; this results from the fact that at each change of speaker-addressee axis there is exactly one choice available to the system as a whole. This is in contrast to a different pattern which generates precisely the same six axes, but which allows 531, 441 alternative paths to reach the six axes of the 12th stage, varying from 89, 939 to 87, 891 paths to each axis.

In addition, at each stage of the generative system various alternative patterns may emerge. Under one model at one stage only half of the axes are available, whereas at the succeeding stage the other half is available to the system; this alternation continues permanently. Under any other system, after the fifth stage, all six axes are available at each stage. These different patterns allow us to specify something about the 'flexibility' of a system and its social relevance. For example, in a conversation, where there are rights for more than one person to speak (providing he can get the floor by shouting louder), there are more paths than in a system in which the presence of the prior axis determines strictly the only one person allowed to choose to speak at the next stage.

The explication of alternate sets of these sociological rules, following up the work of Pike and Lowe (1969) and Lowe (1969), is much too extensive to be given here, but is in preparation elsewhere (Pike 1971). I shall, however, try to let the audience 'hear'--not merely see--the formalism of a few of these patterns, by representing them as condensed formal abstractions in a different medium. The way I shall do this is to replace the members of the cast--Abe, Bill, Charlie--each by a note on the piano. A pair of notes in sequence then represents a monologue (of any duration) in a conversation in which one member of the cast is speaking to another--i. e. it establishes a speaker-addressee axis. A second pair of notes shows that the axis has shifted to involve the third member of the cast in some way, as speaker or as addressee. Symbols of the mathematical representation are 'generators' of the group elements. (The multiplication tables and Cayley diagrams that justify certain of these operations are found in the articles referred to.)

Figure 1 illustrates¹ the musical abstraction when the starting point (the mathematical 'identity') is chosen as the relation 'A, calling himself I or me, is speaking to B, whom he calls you'. The operation of r[eply] reverses the speaker-addressee axis, so that B becomes the





FIGURES 10 - 12

speaker and A becomes the addressee; a second application of the r generator flips the axis and gets us back to the same axis we began with (i.e. rr = r² = I). This can go on indefinitely, with the alternation forming the basis of a representation of a 'dialogue' between Abe and Bill.

In Figure 2, the generator s[hift] allows the speaker to shift away from his starting addressee in order to address the third member of the cast. If this operation is applied again, it--like r--brings us back to the starting point (i.e. ss = s² = I). Similarly, if the addressee is retained, but the speaker is shifted, then we see (and hear) the result of this in Figure 3 (with t² = I).

When r and s are both used, and when r² = s² = (rs)³ = I, six elements will be generated (I, r, s, rs, sr, rsr) which can be mapped against the six axes A-B, B-A, A-C, C-A, B-C, C-B. See Figure 4. (But on the figures, I give below each axis only the particular generator which is being applied to the preceding axis, which in turn has had a generator affect the preceding state, and so on; I am not listing here the cumulative products of these elements.)

When r and s but not t are combined, the 'right' of the speaker is to shift addressee, the right of addressee is to reply--upon which, having

become speaker, he may himself at the next monolog-stage shift addressee--but the listener has no speech rights (but can listen in without being rude), being under the interdict of 'don't speak unless spoken to'. Nevertheless, a substantial number of possible sequences of tone paths result from this combination.

In Figure 5, the t is given again, but is applied to r after r has been applied to I; in Figure 6, t is applied to s for a slightly different result. Then in Figure 7, r, s, and t are all given, but with the choice of sequence determined by the flip of a coin.

In Figure 8, however, a much more restricted pattern is given, in which it takes six stages--six monologs--for the initial axis to reappear. Exactly this one path is allowed, where $\underline{g}^6 = \underline{I}$, and where \underline{g} is defined in turn as the effect of the double operation of the r followed by the w seen in Figure 10.

In the latter, note that w leads to a cycle of A-B, B-C, C-A, A-B, but that when r reverses any one of these axes, the cycle direction continues with the same pairs of the cast involved, but with their axes reversed to B-A, C-B, A-C. (Here, $\underline{w}^3 = \underline{r}^2 = (\underline{rw})^2 \underline{w} = \underline{I}$, and $\underline{rw} = \underline{wr}$.) In Figure 9, on the other hand, $\underline{m}^3 = \underline{r}^2 = (\underline{rm})^2 = \underline{I}$ (but $\underline{rm} \neq \underline{mr}$). As a result, the cycle starts just as it does for w, but when r enters the sequence, the reversal of the one axis simultaneously reverses the direction of the cycle for m.

Even though the patterns sound different, with different generators (or different sequences of application of the same generators), they have one thing in common: each of these mathematical groups generates precisely the same six terminal points--the same six relations between A, B, C. The difference between them, then, lies not in their ability to generate different 'terminal strings' of such a small unit as a monolog, but in their output of different monolog-sequence types as characterizing different conversation structures. Each of the formalisms of these sets is equivalent in being able to generate all and only the correct terminal strings of monolog size, with respect to those pronominal features under study (but not treating, here, gender, for example).

On the other hand, the groups differ radically in the degree to which the structures which they generate approximate natural conversations. Any of the groups which lack an r, for example, do not reflect a characteristic which is basic to every dialog--the ability of some addressee under some circumstances to reply to the speaker. Natural conversation includes the right to reply, and the mathematics chosen to map natural conversation must reflect that fact.

It should be clear in the seventies, therefore, that sheer formalism is not enough. The fact that one has a mathematically-firm mapping of his results is not enough--nor even the fact that it maps all and only the correct linguistic sentence elements without contradiction. There

must also be a cultural evaluation of the results to see if, in fact, the social situation presupposed by the mapping is represented naturally.

Similarly, it should be clear that a conversation is not merely a string of sentences joined together by man or by any simple coordinating device. Rather a conversation may of itself be a highly structured element, in which each monolog is controlled in its relation to the next monolog by a tight sequence of sociological rules determining linguistic rules--and the formalization of the linguistic changes as the social situation changes. Conversations between king and commoner follow different rules from a conversation between pals.

But the speaker not only speaks to John Doe III of Evans Way, Boston. He turns to speak to him.

Query:

(7d) How can the formalisms of our pronominal axes be paralleled by kinesic formalisms, so that a mathematical approach to gesture can bring it, too, under more rigid control?

This question, just beginning to be studied, might conceivably open the door of the seventies to the study of language-speaking human beings--or language formally studied in a context of the formal study of a unified theory of the structure of human behavior as a whole.

Finally, I might mention that the Summer Institute of Linguistics has now begun the analysis of the 500th language in its study program. It expects to continue the study of these languages in order to gather data which can be used to set up, inductively, substantive universals, and to test those guessed at inductively. The theoretical notions of tagmemics continue their heuristic impact in this area, and should be heightened in their coverage during the seventies by coming more closely into relation with other theories by sociological means such as those represented by this Round Table.

NOTE

¹I am indebted to Stephen Pike for selecting the particular notes used to identify Abe, Bill, Charlie--occasionally varied arbitrarily from Figure to Figure for interest--and for the drawing of the Figures.

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THE CROOKED PATH OF PROGRESS IN COGNITIVE LINGUISTICS

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Abstract. This paper briefly reviews the history of stratification theory, here called cognitive linguistics since it aims to provide a model of the information system that enables a person to speak his language. The theory began in the mid-fifties with a three-stratum model, and it added a fourth stratum, above the classical morphemic level, in 1961. During the sixties a series of revisions occurred, not all of which involved direct progress. The paper concludes with a brief introduction to the current model, which has three grammatical strata and one phonological stratum for the language proper, plus a conceptual system containing all of the individual's knowledge (other than his language).

0. Orientation. In the early sixties linguistic theoreticians spent a lot of their energy trying to persuade others that they were right. The most vociferous of such persuaders were the transformationalists, and they won quite a few converts. But many remained unpersuaded, or only partly persuaded; and it appears that during the late sixties, as different factions developed within the transformational school, a degree of tolerance--or perhaps it was just resignation--developed, and more and more linguists decided that they could go on living even if others held different points of view. Now for a linguist to allow others to disagree with him doesn't necessarily mean that he has decided to tolerate stubbornness, stupidity, or ignorance. Perhaps some linguists have been realizing that different objectives call for different formulations, so that the current variety of linguistic theories is quite acceptable as the reasonable outcome of the variety of objectives being pursued by differently motivated linguists. This point was first brought home to me by Halliday's paper 'Syntax and the consumer', presented at the first Georgetown Round Table in which I participated, in 1964.

His point was simply that you will get different syntactic theories for different purposes.

It has been common during the past decade to designate linguistic theories on the basis of some important feature of their descriptive apparatus. We have tagmemics, which makes use of tagmemes, transformational grammar, in which transformational rules play an important role, systemic grammar, which makes use of a device for which the technical term within that theory is 'system', stratificational grammar, which is stratified.

Now if it is correct, as I just suggested, that these different theories have resulted from different aims,¹ then we might alternatively designate them in accordance with these aims in order to clarify the situation and strengthen our tolerance for one another's differing formulations. Thus systemic grammar might be called functional grammar, since its approach was arrived at through a concern with the communicative functions of linguistic choices made by the speaker. Stratificational grammar can be called cognitive linguistics, since it is concerned with representing the speaker's internal information system which makes it possible for him to speak his language and to understand utterances received from others. I leave it to Pike and Smith, respectively, to designate what alternative names might be applied to tagmemics and aspectual theory. For transformational grammar the appropriate alternative term is already in use: It may properly be called generative grammar, or generative linguistics, as its aim is to provide a system of rules which can generate all the sentences of a language and only those.

0.1 Generative linguistics. A few years ago I would have thought it inappropriate for the term generative to be assigned just to the transformational school because I considered that stratificational theory, among others,² also had a generative aim. I did not state the aim in the same way for stratificational theory, since it seemed to me, following Hjelmslev, that the more sensible objective was to generate the texts of the language, including those longer than sentences. But this is of course also a basically generative aim. Since other schools of thought in linguistics have or have had generative aims, one could more accurately characterize transformational generative grammar as that which attempts to generate by the use of a particular notational system of production rules borrowed from symbolic logic. (Gleason has suggested that it be called prescriptive grammar.) But the simple designation 'generative linguistics' is not inappropriate, since this theory more than any of the others tends to let other aims be subordinate to the overriding generative objective.

But I must add to this that for the current version of stratificational theory the term generative would not be correctly applied in any case--

that is, the term 'generative-stratificational grammar' suggested by Karl Teeter in a recent talk to the Yale Linguistics Club (entitled 'Generative-Bloomfieldian Grammar') is a misnomer as applied to current stratificational theory. Some time ago I rejected as a practical impossibility the goal of generating all the texts of a language and only those, and I proposed that as an alternative we need a relative aim (Lamb 1966a:541-543). That is, we want a grammar that generates as many texts as possible (rather than all the texts) while generating as few as possible spurious texts. Since the absolute goal is impossible we would use this relative standard to choose between two competing grammars.

But my current position goes farther than this and rejects the generative goal as unrealistic even as a theoretical possibility, since that goal presupposes that there is such a thing as the set of grammatical sentences,³ as a well-defined set. But such a well-defined set does not exist (cf. Hockett 1968). In any real language, the boundary between what is grammatical and what is not is constantly shifting. And you can't avoid the problem even by assuming that you are stopping the language at a point in time in order to define the set of sentences at that point, since at any point of time there are semi-grammatical sentences. The constant flux of a real language entails that new grammatical devices are gradually coming into use, along with new idioms and the like, while others are gradually withering; here one is dealing with continuous rather than discreet phenomena. The 'boundary' between grammatical and ungrammatical is a continuum, not a sharp boundary. To try to enumerate the sentences of a language--even at a point in time--is like trying to enumerate the leaves on a tree at a point of time in June. How far formed does a newly forming leaf have to be to count as a leaf? At what point does a bug-eaten leaf cease to be a leaf?

Thus I for one am happy to leave the term generative as an exclusive designation for the transformationalist school. In any case, it appears to be the only school that attaches such overriding importance to the generative aim.

0.2 Cognitive linguistics. The other term that calls for comment is 'cognitive linguistics', which I propose as a designation for that branch of linguistic inquiry which aims at characterizing the speaker's internal information system that makes it possible for him to speak his language and to understand utterances received from others. Is it appropriate that this term should apply only to what has been known as stratificational theory? The reason I say 'yes' is that the basic requirement which a linguistic theory must meet to qualify for this term is that it take account of the basic fact about human beings who know a language. That fact is that they are able to speak it. And they are able to understand sentences received from other speakers. In short, they are

able to perform. Their knowledge of their language, their internal linguistic information system, is a competence to perform. That knowledge is capable of being used by them for speaking and understanding. Thus any linguistic theory qualifies as cognitive linguistics which aims to provide an account of a language that can be used as a basis for a performance model. As far as I know, stratificational theory is the only one that qualifies at present (cf. Reich 1968a); but we would be delighted to welcome linguists from other backgrounds to this fascinating and fertile area, if they can suitably modify their theories so that they can confront the fundamental fact that must be dealt with. My strong suspicion is that any such modifications will inevitably be in the direction of stratificational theory. Indeed, we have already seen moves of some transformationalists in this direction, and we welcome further such revisions.

1.0 Background and beginnings.

The developments of the sixties in cognitive linguistics cover almost the entire history of this theory. A preliminary model existed by the end of the fifties, but it had not yet been publicly announced, nor had the term 'stratificational' been used for it, until 1961. Various revisions and additions were made during the sixties, as hypotheses were formulated and tested, and as attempts were made to increase the scope of data accounted for. And one might perhaps be justified in saying that by 1970 the theory was no longer merely programmatic but actually existed, so that as the seventies begin it has finally become sufficiently developed to be proposed seriously as a relatively coherent theory of language.

1.1 The background. This brief review of developments must begin with the acknowledgement that cognitive linguistics did not originate out of the blue but was built upon the admirable work of its predecessors, of whom I should mention particularly Nida, Hockett, Hjelmslev, and Chomsky. These theorists of course in turn built upon earlier work, including that of Bloomfield, Sapir, Saussure, and others (and the list of those who influenced Bloomfield goes all the way back to Panini).

Now the neo-Bloomfieldian model I had been taught as a graduate student in the early fifties had two structural levels, phonemic and morphemic (cf. Nida 1949, Hockett 1954). A morpheme had allomorphs or might have just one morph, and a morph was composed of phonemes. Allomorphs might be either morphologically conditioned or phonologically conditioned and many alternations of the latter type were economically described by means of morphophonemic rules, but such rules were generally not considered to have any structural status, as the structural fact was that the morpheme had allomorphs. The morphophonemic rule was just a descriptive device for summarizing numerous statements specifying phonologically conditioned allomorphs.

1.2 Stage I. My first major disillusionment with this neo-Bloomfieldian model came when I tried to apply it to Monachi, a Utoaztec language of California I was attempting to describe. Despite what I had been taught I kept finding evidence indicating that there was another level of structure between the morphemic and phonemic levels. It wasn't economical to go from morphemes to phonemic forms in just one step. Rather, it was evident that there was one level of alternation involving morpheme-sized units, with morphological conditioning environments, and below it a level of phonologically conditioned alternation among phonological units.⁴

I therefore concluded that there was an intermediate level between the morphemic and phonemic levels of neo-Bloomfieldian linguistics. I called it (at this time) the morphophonemic level. (Later it got renamed--see below.) I also rejected the commonly held notion of the morpheme as a class of allomorphs in favor of the concept of the morpheme as an element existing on a different realization level from that of its allomorphs. In other words, the allomorphs were the realizations of the morpheme rather than its members. These allomorphs were composed of morphophonemes. The conditioning environments for alternations among allomorphs were morphological. Then morphophonemes were realized as phonemes, which were composed of phonological components, and alternations at this level were conditioned by morphophonemic environments. This treatment puts the morphophonemic rule into a different status than it had before, in two ways. First, it was a statement of realization, just like that between morphemes and allomorphs, or between phonemes and allophones, rather than a process statement. Second, it was given a structural status and not just considered a convenient descriptive device for summarizing statements about allomorphs.

This three-level model, departing from the earlier two-level view, might be taken as the beginning of stratification grammar, since it is hardly fitting to call a system stratified if it has only two strata. The term stratum and the term stratification were not used in connection with it, however, until the sixties. The three-level model was first proposed at a meeting of the Linguistics Group at Berkeley in 1957, and it was used in my dissertation, completed in the same year. I was not yet ready to present it more publicly.

While it had dealt with one serious defect of neo-Bloomfieldian grammar, this original stratification model remained less than completely satisfying in that it failed to deal with certain phenomena which suggested that there must be further structure above the morphemic level. For example, Monachi has some verb stems which come in semantically related pairs, one for singular agent, one for plural agent in each pair, otherwise having the same meaning. These are shown in Table 1. They are not to be compared with go : wen(t) or good : bett(er)

of English, since the Monachi stems are contrastive in identical morphological environments. Other examples pointing to the possible existence of a higher level appeared at about the same time in Chomsky's *Syntactic Structures* (1957), for example the shooting of the hunters and the active : passive relationship. But the device which Chomsky, following his teacher Harris, proposed for dealing with these data, namely the now-famous transformational rule, was unacceptable to me since it was a process formulation, involving mutation of forms on the same level; and while such a formulation could apparently account for the primary data it was not realistic as applied to encoding and decoding.

TABLE 1. Some Monachi verb stems

Gloss	Singular agent	Plural agent
'to wander'	nywi	moo
'to sit'	qa'ty	jy'k ^w i
'to stand'	wyny	qo'no
'to lie'	hapi	q ^w api

Rather, these and similar data made me suspect that there might be some additional level of structure above the classical morphemic level; but I didn't get around to exploring this possibility until the beginning of the sixties.

2. Developments of the sixties

Cognitive linguistics entered the sixties with exploration leading to the conclusion that there was indeed a higher level, and it adopted the name stratificational grammar. It then went through a series of refinements and revisions, not all of which, as we can now see, represented actual progress. That is why I call the path a crooked one.

Following Ilah Fleming (1969) we may recognize three stages of development during the sixties, Stages II, III, and IV. Each of these stages worked with a particular cognitive model of language, and minor model changes occurred within each stage. Ignoring these minor model changes we may designate the models as Model II, Model III, and Model IV, using the same numbers as for the stages in which they were used.

2.1 Stage II. Now of course it wouldn't have to be the case that an additional level of structure would be related to its lower neighbor in

the same way that the morphemic is to the morphophonemic, and so forth; but examination of the evidence indicated that this was indeed the case, for the essential properties. Thus, for example, calling the higher level the 'sememic', we may say, with reference to Table 1, that Monachi has a sememe 'to wander' which is realized morphemically as either nywi or moo depending on whether it is occurring with singular or plural agent. Similarly, we can account for the ambiguity in the shooting of the hunters by recognizing that of is the ambiguous realization of either the agent sememe or the goal sememe, both of which have other, but differing, realizations (Lamb 1964a:72-3).

There is a nice parallel here, between transformations and morphophonemic rewrite rules. The earlier treatments of morphophonemic alternations used process or mutation rules, applying to 'basic' phonemes to yield actually occurring phonemes. Neo-Bloomfieldians rejected the structural validity of such rules because they were process rules applying to units of one level to yield units of the same level in a fictional time span; they therefore lacked what we may now call cognitive reality. Other linguists found these process formulations so useful, since they could summarize in one statement the alternation of indefinitely many allomorphs, that they went ahead and used them and said, in effect, "let cognitive reality be hanged, I want economy!" Now the stratificational answer is to get both the economy and the cognitive reality, by setting up the morphophonemic rule as a realizational phenomenon rather than a process. Thus instead of saying that m is replaced by w intervocally, we say that there is a higher-level element, a morphophoneme, which has alternate realizations /m/ and /w/, of which the latter occurs intervocally, the former elsewhere.

Now we find a similar situation higher in the linguistic structure. And again the descriptive device which occurred first historically was a process formulation, developed by Harris and adopted by Chomsky, in this case called the transformation. Many linguists rejected the transformation on grounds which we may summarize by saying that it is cognitively unrealistic. Others said, in effect, "cognitive reality be hanged, look at all the things we can do with transformations!" And again the resolution of the conflict is to recognize that we are actually dealing with a higher level and with alternative realizations of the higher-level entities, the relationship being that of realization rather than transformation. In other words, we can account for the data treated by Chomsky and his followers without giving up cognitive reality.

This second model was the first one I considered ready for public presentation. It was proposed in a paper read at the Christmas meeting of the Linguistic Society in 1961, in which it was argued that we must recognize a stratum between the classical morphemic and phonemic levels as well as a fourth stratum above the classical morphemic.

It was further argued that the relationship between different such levels was different from the class-member relationship and the process relationship, with which it had generally been confused; and the term stratum was proposed for a realizationally defined level. Hockett (1961) had independently arrived at the same term at about the same time.

A short time later, Chomsky brought forth his revised conception, in which there was a deep structure and which no longer had any optional meaning-bearing transformations. This deep structure (the term is from Hockett 1958:246-52) comes part way toward the sememic stratum, but only part way, for the rules relating deep structure to surface structure were still transformations, process rules which perform operations on some items while leaving others unchanged. The consequence is that a 'deep structure' so formulated is really a selection of various surface features and is not in fact very deep.

An informally published Outline of Stratificational Grammar appeared in 1962,⁵ but the first formal published presentations did not appear until two years later (Gleason 1964, Lamb 1964a, 1964b). Two of the three papers which then appeared owe their existence to the Georgetown Round Table.

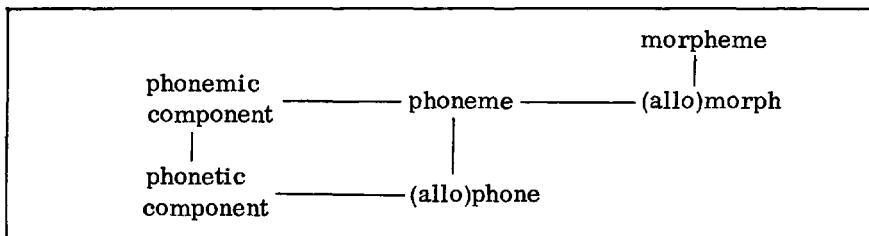
A further step taken during Stage II was an analysis of the relationship of realization into a number of more elementary relationships, which were called diversification, neutralization, zero realization, empty realization, composite realization, portmanteau realization, and anataxis (Lamb 1964a). In fact it was this analysis which was used to establish that the newly added sememic stratum was indeed another stratum, related to the next lower one in the same way that the more familiar strata are related to their lower neighbors.

In the early part of Stage II it was not clear whether each stratum had a tactic, but by 1963 it was apparent that there were indeed four independent tactic patterns. The problem of specifying the details of the interconnection of the tactic patterns with realizational elements, however, remained unsolved.

2.2 Terminological problems. It has always seemed wise to me for any terminology to stick as closely as possible to established traditions. Now if the tradition has terms for only two levels, and if some of these are used ambiguously or vaguely, then something must be added, but one does best to add as little as possible, and then in keeping with tradition as much as possible.

The terminology of the classical structural tradition can be represented as in Figure 1, in which the vertical dimension represents realization while the horizontal represents composition (cf. Hockett 1961).⁶ For example, a morph is composed of phonemes and is a realization of a morpheme.

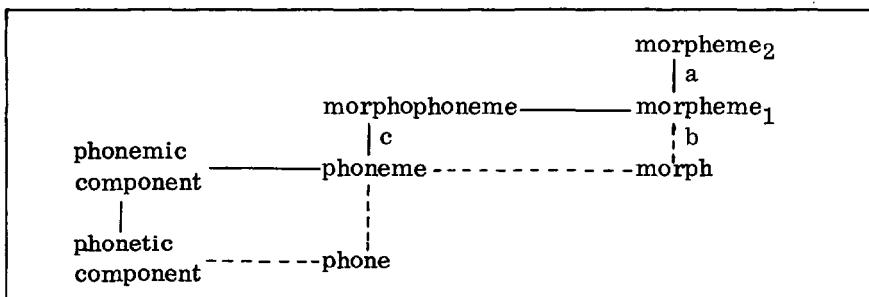
FIGURE 1. The classical model



Using the same diagramming conventions, we may represent the relationships among the units of Stage I as in Figure 2. In this figure the dotted lines represent relationships which, while they exist, need not be explicitly described in the grammar, since they are automatically (and more simply) defined by the specification of the 'solid-line' relationships. This model is described as having three strata since the lowest level, the phonetic, was not considered to be part of the linguistic structure.

FIGURE 2. Model I

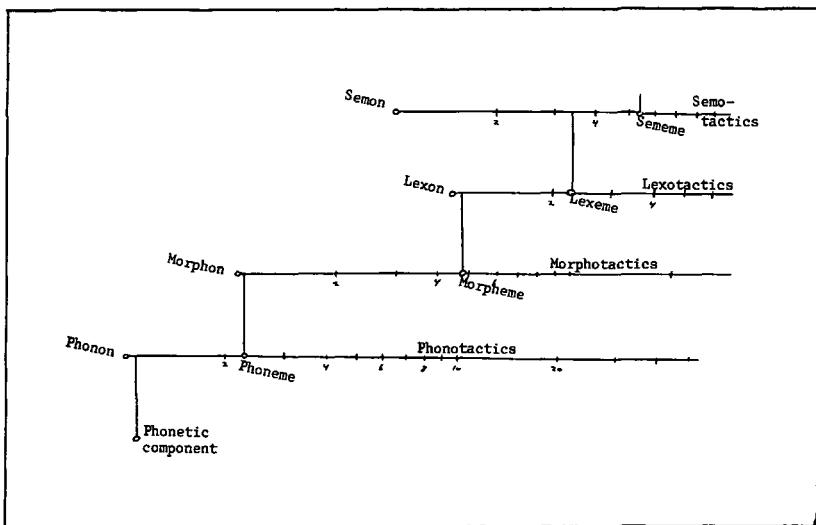
- a: Morphologically conditioned alternation.
- b, c: Phonologically conditioned alternation, to be described as (c) realization of morphophonemes by phonemes.



The terms given as 'morpheme₁' and 'morpheme₂' furnish our first example of a terminological problem. The tradition provides only one term for two structural units which must be distinguished. Of these, morpheme₂ is closer to the morpheme of the fifties, but morpheme₁ is closer to Bloomfield's morpheme, which was described as composed of phonemes. Those not familiar with the history of stratificational theory need not be bothered now with a discussion of what was done with the

terminology at Stage I since we get a better idea of what is needed if we go directly to Stage II (Figure 3).

FIGURE 3. Model II



Here we have the term morpheme for 'morpheme₁' of Figure 2, and lexon for 'morpheme₂'. The term lexon is formed with a suffix used for the elementary unit of each stratum, so it means 'elementary unit of the lexemic stratum' or 'component of a lexeme'. Similarly, the morphon is a component of a morpheme; and the term may also be considered a shortening of 'morph(oph)on(eme)'. Thus we add only one suffix to the terminology rather than a series of additional terms. The term lexeme is here used in roughly the sense it already had in the tradition extending from Whorf (1938) to Conklin (1962) (cf. Lamb 1966a:567-9). And the term sememe has an even longer tradition, having been used not only by Bloomfield but also by the Swedish linguist Adolph Noreen, who in the early part of this century proposed that a linguistic structure has three levels, phonemic, morphemic, and sememic, with units which he called the phoneme, the morpheme and the sememe (Noreen 1903-18, Pollak 1923).

In Figure 3 the minimal units (phonon, morphon, etc.) are placed to the left of the top of the appropriate vertical lines rather than right at the intersections as in the earlier figures. This calls attention to the fact that the minimal unit of each stratum does not always coincide with the unit having a realization on the next lower stratum. For example, the morpheme second is the realization of the combination of lexons two and -th, entering into portmanteau realization (Lamb 1964a).

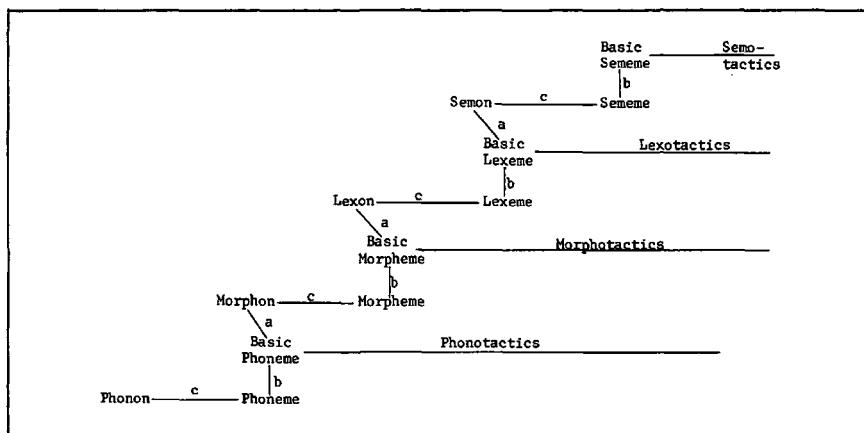
The vertical lines are placed to show some average estimated quantitative relationships for a typical language. For example, according to the rough estimate, there are about five morphons in the average morpheme. Semon is placed rather far to the left of the vertical line since it was considered at this time that there was an extensive amount of portmanteau realization at this level (Lamb 1966b⁷).

Another feature of Figure 3 is that the horizontal lines extend to the right of the 'emic' units (e.g. morpheme). The reason is that there are structurally significant combinations of these units, specified by the tactics of each stratum.

2.3 Stage III. The next step was to examine the interconnections among the seven relationships which are collectively involved in the difference between one stratum and the next. Upon such examination it soon became apparent that they are systematically ordered with respect to one another, so that a 'full stratum' of difference, say from lexons to morphons, consists of several little steps rather than just one big one with all the realizational relationships in a single bundle. In part this situation had already been recognized in Stage II, in that morphemes were present at a level intermediate between lexons and morphons; and Figure 3 also separates out portmanteau realization. But in Stage III further separation of the relationships was recognized, resulting in the scheme shown in Figure 4 (Lamb 1965). (Later work

FIGURE 4. Model III

- a: anataxis, zero realization, empty realization
- b: diversification, neutralization, portmanteau realization, zero realization
- c: composite realization



showed that this is not exactly the right way to sort them out; but if this was not a step straight ahead, at least the path was moving 'diagonally' forward at this point.) In this scheme there are two intermediate levels between the lexon and the morphon, and again we have a terminological problem in that a distinction is drawn between two units both of which are like the former morpheme. It seemed easiest to call them simply the basic morpheme and the actual or realized morpheme. The simple term morpheme could be used for short for the latter.

With the recognition of the new intermediate step it was no longer sufficient to say that the linguistic system just consisted of strata together with realizational relationships separating them. Rather, it became necessary to recognize the stratal system, a complex structure of which the stratum was just one part. Thus the morphemic stratal system included the morphons, morphemes, basic morphemes, together with their interrelationships, plus the morphotactics. The level of the morphons and morphemes could be called the morphemic stratum.

Perhaps the most noteworthy feature of Stage III is that alternation is recognized both above and below the tactic pattern of each stratal system. This meant, among other things, that four layers of alternation were recognized between the lexon and the phoneme, where neo-Bloomfieldian structural linguistics had only one.

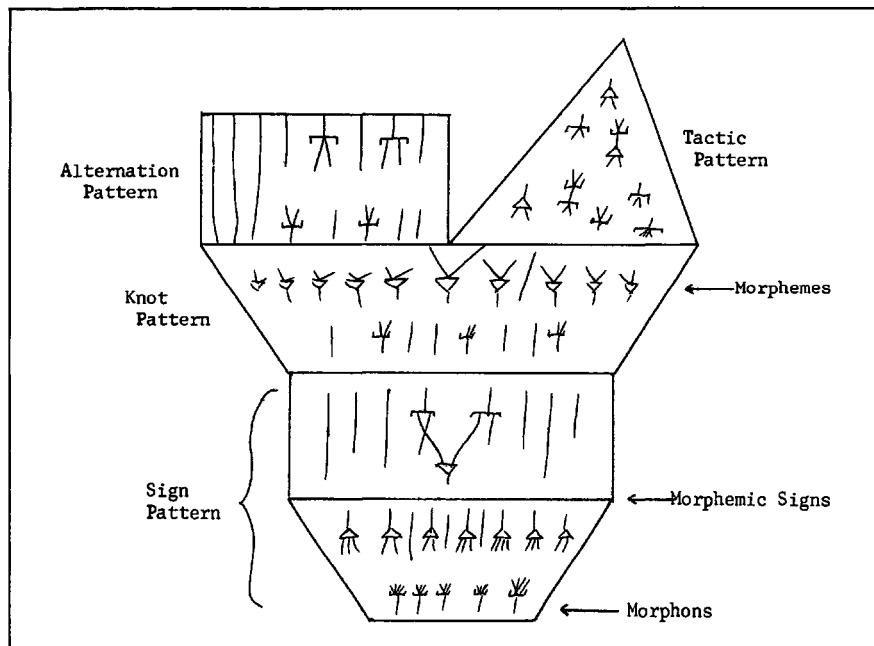
Another step taken at this stage was a refinement of the definitions of the realizational relationships, which (except for anataxis) were now analyzed as different combinations of the elementary oppositions and vs. or and upward (toward content) vs. downward (toward expression), as follows:

Name	Definition	Example
Diversification	downward or	past ppl. / -en, -ed
Neutralization	upward or	past ppl, past tense / -ed
Composite realization	downward and	second / s e k ə n d
Portmanteau realization	upward and	two -th / second
Zero realization	downward or with zero as one choice	pl. / -z, Ø, ...
Empty realization	upward or with zero as one choice	DO, Ø / do

This somewhat neater analysis was accompanied by the development of a graphic notation (here I was influenced by Halliday) and, as its conceptual correlate, the discovery that the whole of the linguistic structure could be represented as a network of relationships, rather than a system of items and their relationships (cf. Hjelmslev 1961:22-3). In other words, things like morphons and morphemes are not objects or symbols (although the English language forces us to speak of them in this way) but positions in a network of relationships. But the relational network notation was not used in print till the publications representing Stage IV.

2.4 Stage IV. Stage IV might be said to have begun in November 1965, as I was reworking some material for a lecture, with a revision of the model of the stratal system. The basic revision was to put most of the alternations of a stratal system above the tactics rather than just below the tactic pattern. The only instances of diversification left below the tactics in Model IV were those directly involved in portmanteau realization. The resulting organization of the stratal system may be represented as in Figure 5. Here the morphemic system serves as a typical example and the types of relationships found in each part of the system are indicated by nodes of the relational network notation.

FIGURE 5. Organization of the morphemic system in early stage IV

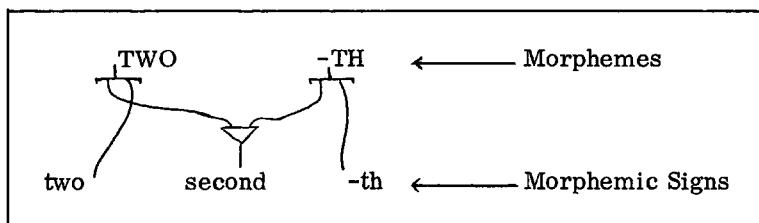


The morphemic signs of this model correspond to the (realized) morphemes of Stage III, but the morphemes and the morphotactics of Model IV are at a lower level (i.e. closer to expression) than the basic morphemes and morphotactics of Model III, because of their position with relation to the morphological alternation. Similarly, the basic lexemes of Model III are closer to semons of Model IV than to Model IV lexemes.

Why this difference in the treatment of alternation? As with most revisions, the basic motivation was economy. Diversification above the tactics has the property that the realization chosen in a given instance is that which fits the tactic environment. In other words, the conditioning environments are given by the associated tactic pattern and therefore need not be stated separately (Lamb 1966c:563; Lamb 1966d:55-6). Previously, the statement of conditioning environment had been a structurally significant part of the realization rule. This appears to be a considerable saving.

When I found for several instances of diversification in different stratal systems that they could apparently be accounted for more simply above than below the tactics, I proceeded to the conclusion that diversification in general should be treated this way. The only exception was that involving portmanteau realization, for which the conditioning environment was at the same time the other participant in the portmanteau realization, as shown in Figure 6, where -TH conditions and is conditioned by TWO, with the resulting realization second.

FIGURE 6. Portmanteau realization in early stage IV



The other major departure of Model IV was an increase in the number of stratal systems, from four to six. The additions were, very roughly speaking, at the bottom and the top. This feature of the model was perhaps its main target of criticism, and we can now look back at it as an example of the crookedness of the path of progress in cognitive linguistics. For in this instance the critics were right. How, then, did the model get that way? It was largely a consequence of the hypothesis as to how the stratal system was organized. In particular, Model IV allowed only one level of diversification (aside from portmanteau realization) and one level of composite realization in the stratal

system. When it became evident that one must recognize two levels of alternation in phonology,⁸ and it also appeared that the tactics of consonant clusters conflicted with syllable tactics, requiring two separate tactic patterns, I concluded that one must recognize two stratal systems in phonology, which could be called 'phonemic' and 'hypophonemic' (Lamb 1966c).

At the semological end of the network also there was evidence for two separate stratal systems. This was not new evidence, since it had already been partly accounted for in the sememic system of Model III, which distinguished basic sememes from realized sememes, with a level of alternation intervening (Lamb 1965). These basic sememes became the hypersememes of Stage IV, elements of the hypersememic system, which was set up above the sememic.

The resulting six stratal systems could conveniently be grouped as three pairs of systems, two each for semology, grammar, and phonology; but no structural evidence was found for such pairing--it remained a matter of convenience.

This early Stage IV model is the one presented in Outline of Stratification Grammar (Lamb 1966d). Although it was just a preliminary outline, it was published as a small book and was therefore reviewed in the linguistic journals. This occasion provided criticism of the model, some of which was valid, some of which was based on misunderstanding. The most serious defect of the Outline was that it was just an outline; it didn't go into detailed explanation or presentation of evidence. Thus the failures of critics to understand some points are largely excusable.

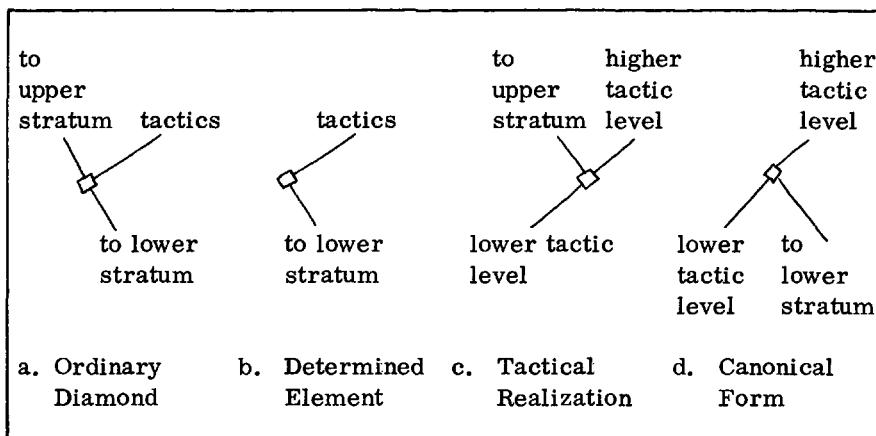
But the most serious defects in the model were those which I became aware of on my own and those which were brought to my attention by my co-worker Peter Reich.

2.5 Minor model changes in Stage IV. In 1967 we went through some minor model changes, which corrected some of the more serious defects.

One of the problems involved the interconnection of the tactic pattern with the realizational portion of the stratal system. In the Outline the connections took the form of the 'upward ands' of the 'knot pattern' (see Figure 5), on the grounds that a morpheme connected upwards both to the lexemic stratum and to the morphotactics. The assumption was that the tactic pattern is oriented in the same up-and-down dimension as the realizational portion. But that assumption turns out not to be valid. Rather, the whole of a tactic pattern is on one and the same realizational level, so it occupies a horizontal plane. It specifies the allowable combinations of elements of a single realizational level. Thus the connection of a morpheme to the morphotactics should be sideways rather than upwards. For this type of connection a new node was invented, called the diamond. Four types of diamond node are

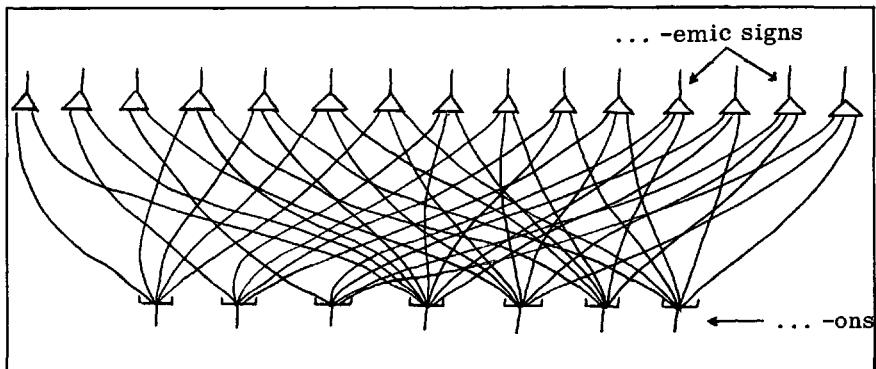
shown in Figure 7. As the third of these illustrates, it is possible, in the revised conception, for a line from the higher stratum to go into the midst of a tactic pattern, whereas in the earlier version all realizational connections to the tactics were at the bottom of the tactic pattern. (For more on diamonds see Lamb 1970.)

FIGURE 7. Types of diamond node



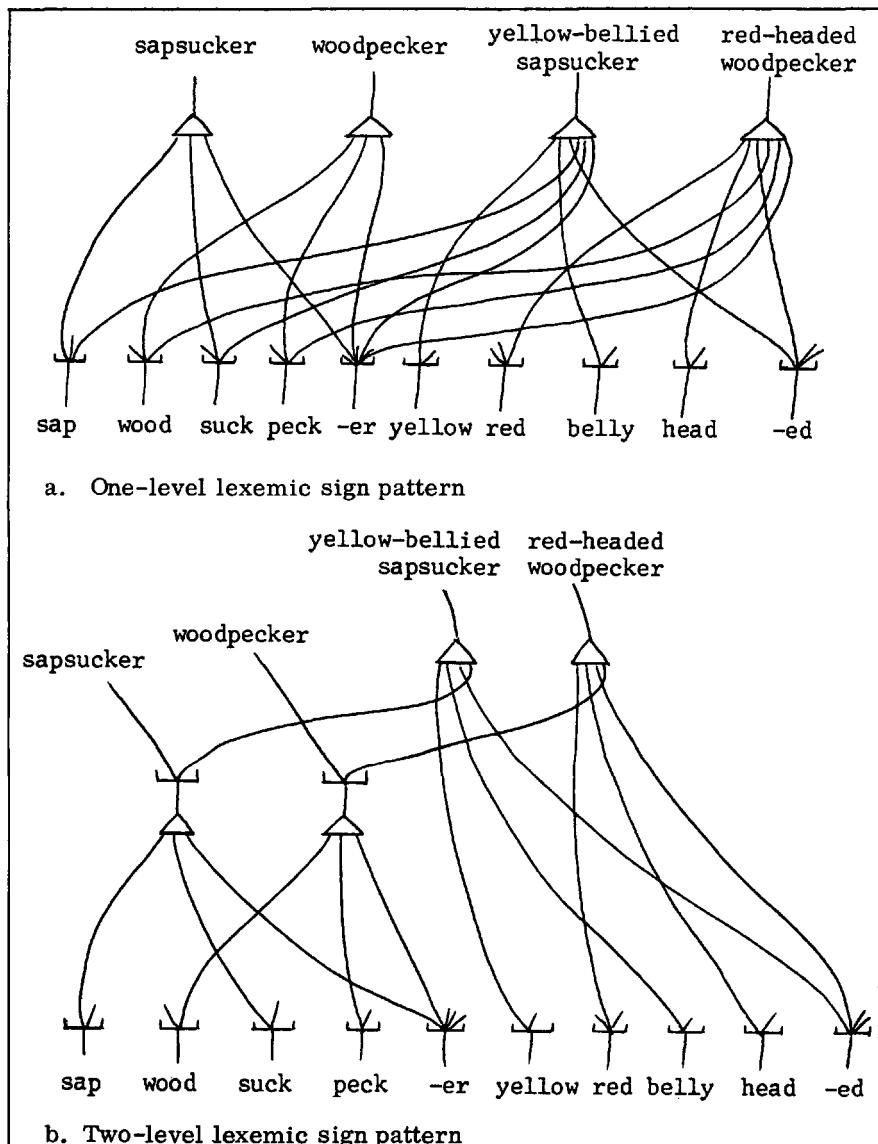
A second infelicity of Model IV (in its original version) involved the sign pattern, which had the form illustrated in Figure 8. It soon became apparent that the limitation to a single level of such patterning within each stratal system was unnecessary and that it entailed uneconomical handling of certain information. The solution was the multi-level sign pattern. Its attractiveness may be illustrated by an example

FIGURE 8. Basic form of sign pattern



from the lexemic sign pattern for a speaker of English who knows enough about birds to have the lexemes yellow-bellied sapsucker and red-headed woodpecker alongside of the lexemes sapsucker and woodpecker. Figure 9 shows the two analyses. Examples favoring a multi-level sign pattern in the morphemic system are provided by long morphemes such as lobster, dromedary, encyclopedia.

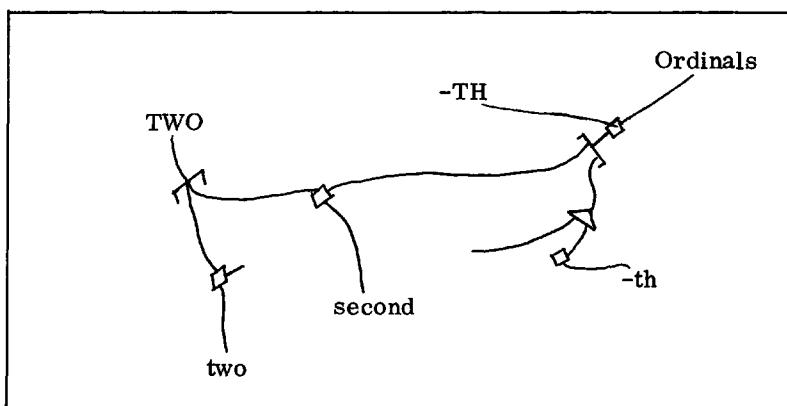
FIGURE 9. Lexemic sign patterns



Now the 'and' nodes in a sign pattern generally correspond to 'ands' of an adjacent tactic pattern. The tactic pattern generates new combinations of elements at its level; the sign pattern contains the fixed or prefabricated items that through repeated use have come to be treated as units. Thus the different levels of a multi-level sign pattern should correspond to the different levels of the associated tactic pattern. Because of this correspondence, one gets a more economical picture of the organization of the linguistic network if one places the sign patterns on a horizontal plane along with the tactic patterns.

The treatment of portmanteau realization was also subjected to alternative analysis, primarily in consequence of considerations arising from our work on performance models, in connection with the Linguistic Automation Project. I shall spare you from the details and proceed directly to the punch line, which is Figure 10, an alternative to Figure 6. In this treatment, which is made possible by the conception of the

FIGURE 10. Portmanteau realization in late stage IV



tactic plane as horizontal to the realizational lines and thus intersectable by them in its interior, the portmanteau realization is handled neither below nor above the tactics, but at the tactic level. Above it (shown as at upper left) we have the two lexons TWO and -TH; while just below the tactics (shown as at lower right) we have the single morpheme second, occurring as realization of these two lexons. The 'and' node in the tactics is the construction for ordinals--the first line leads to numerals, the second to the suffix -th. But as the ordered 'or' node provides, if the numeral is TWO, the left-hand line is taken instead of the construction, so that the 'prefabricated' ordinal second occurs.

(For semological examples, see Ikegami 1970.)

With the removal of portmanteau realization from below the tactic pattern, there would be no occurrence of diversification ('downward or')

at this level of the stratal system. But at about the same time our attention was drawn to some examples of free variation, and it was apparent that the model of early Stage IV had not properly provided for this phenomenon. The crucial property of free variation is that the alternants can occur in the same tactic environment. For this reason, it is uneconomical to account for such alternation above the tactics, since this would involve two lines from a 'downward or' connecting to a single tactic location. Elementary simplification procedures (Lamb 1966d:43-7) shift the 'downward or' to below the tactics, in keeping with the fact that the difference between the two alternants is of no tactic significance. Thus we introduced a subtactic alternation pattern into the stratal system (cf. Bennett 1968).

These revisions had been made by the Fall of 1967, and they could well have resulted in a new synthesis, which we might call Model V; but I was at the same time aware of additional problems for which new solutions were not so readily forthcoming, and of others for which there seemed to be several alternative solutions none of which seemed clearly better than the others. Thus from late 1967 till the end of the decade we were without a stable integrated model, since we had no successful synthesis of non-defective hypotheses.

The most perplexing of the problems was that there seemed to be needless repetition of diamond nodes for simple lexemes and simple sememes. A simple lexeme is one which connects to a single lexon rather than to a combination of lexons. For example, English has the morpheme wood. But according to Model IV it also must have a lexeme wood, since this element participates in the lexotactics right along with complex lexemes like woodpecker and yellow-bellied sapsucker. Moreover, the model required that we have sememes for these lexemes, since they played a part in semotactics right along with complex sememes like a stitch in time saves nine. It seemed clear that this was a case of excess surface information, but it was not clear how to eliminate it without getting into some very messy complications. A number of solutions were attempted, one of which was used in a paper I presented at the annual meeting of the Linguistic Society in 1967. But neither this nor the other attempts were fully satisfactory.

Another problem area, actually a whole dimension of problems, was that of formalizing the processes of encoding and decoding. This area was the primary concern of the Linguistic Automation Project. The work uncovered more questions than answers but contributed greatly to our understanding not only of these processes but of linguistic structure in general.⁹

And then there were suspicions that the model had too many strata. Each of them had been added only when the evidence demanded it; but perhaps some of the evidence could be differently interpreted.

Part of the evidence for stratification comes from alternation, and as we approached the end of the decade we found increasing evidence of alternation other than free variation below the tactic pattern; and it appeared that we might be moving toward a refined version of Model III.¹⁰ This is another illustration of the crookedness of our path, for it became apparent that not all of our earlier revisions had been in the direction of actual progress.

Meanwhile, up at the highest levels of the linguistic structure, Gleason and his students were working out principles of discourse analysis from a stratificational point of view. A number of valuable studies have resulted from this effort (e.g. Gleason 1968, Austin 1966, Cromack 1968, Taber 1966).

3. The Current Model

In late 1970 and early 1971 a new synthesis finally emerged. It resulted from breaking out of some previous conceptions of the organization of the overall network, considered as a series of stratal systems. As mentioned above, the six stratal systems of Stage IV had been grouped into three pairs, and although this pairing had no structural significance, there was a lurking suspicion that the overall structure did somehow consist of three major parts and that future revisions would reveal that the two members of each pair did go together. For example one speculation was that a more complex model of the stratal system would bring the phonemic and hypophonemic systems together into a single stratal system, and that corresponding coalescences could be made for the grammatical and semological pairs. As it turns out, this speculation was partly sound but partly misleading.

Continued study of alternation and its interconnections with the tactics and the sign pattern led to a model of the stratal system which comes rather close to that of Stage III. The stratal system in the new model has an alternation pattern both above and below the tactics. But the lower alternation pattern apparently overlaps with the sign pattern, since some alternations can be most economically accounted for in the midst of the sign pattern rather than above or below it. In the morphemic system, for example, this treatment allows us to deal efficiently with alternations among segments of specific morphemes, such as the vowel in do : did : done and the y in have : had (cf. Reich 1968b).

Now it is indeed possible to coalesce the former phonemic and hypophonemic systems into a single stratal system. This happy simplification is made possible in part by the new model of the stratal system and in part by a reinterpretation of the evidence which was formerly seen as forcing the recognition of two separate tactic patterns in phonology.

One is tempted to suppose that the morphemic and lexemic strata might be similarly coalesced, but this possibility does not work out. The essential property of a stratum is its distinctive tactic pattern. Now linguists have long recognized the obvious fact that languages have syllable structure. That is the essence of phonotactics. And if there is anything that is inescapable about linguistic structure it is that there are such things as prefixes, stems, and suffixes, which combine in characteristic ways to form words. That is the essence of morphotactics. And it is in clear conflict with phonotactics, in that tactic boundaries and constituent structure are different. So there are clearly two separate tactic patterns here. And if there is anything even more widely recognized by linguists of varying persuasions than stems and affixes, it is that clauses come in subjects and predicates, that subjects are, or contain, noun phrases, and predicates are, or contain, verb phrases. That is the essence of lexotactics. And its independence from morphotactics is shown by such conflicts as that in the queen of England's hat and that in English verb phrases, where, for example:

lexemic: 3-sg. perfect passive take

is realized as

morphemic: have -z + be -en + take -en +

with entirely different tactic structure. And I think that most of you in this enlightened audience will agree that there is still another tactic pattern, that at which, instead of subjects and objects, we find agents, goals, instruments, benefactives, datives, and the like. Its independence from lexotactics is clearly shown by such examples as active and passive pairs, which are grossly different at the lexotactic level while almost identical semotactically.

Now, what about the hypersememic stratum of Stage IV? When we explore it we find that this stratum has to contain all of the individual's knowledge of his culture, his personal history, his physical environment, in short everything he knows, save the language itself, which is already represented in the lower strata. In other words one is dealing here with information that lies outside the scope of what is traditionally considered language. And one is dealing with a system of far greater complexity than any of the lower stratal systems--indeed probably more complex than all of them put together. Therefore it seems appropriate to give a special status to this highest level--to consider it as outside the language proper (which does not mean for a moment that the linguist is prohibited from its premises) and to give it a more fitting name than the awkward term 'hypersememic'. Accordingly I now call it the conceptual system, or for those who would like another Greek term, the

gnostemic system; its basic units are the gnosteme and the gnoston. The gnoston is an elementary unit of knowledge, and this position in the system corresponds well to its Greek meaning, 'something which is known'.

With this revision the linguistic system proper, according to Model V, has the same four strata that were recognized back in Stage II: Sememic, Lexemic, Morphemic, and Phonemic.

But there is one further major revision, which amounts to a partial coalescence of the morphemic, lexemic, and sememic systems. It is a solution to the perplexing problem of repetition of diamond nodes for simple lexemes and simple sememes, mentioned above. Related to this problem is the fact that morphotactic classes which are needed to specify the occurrence of morphemes in derivational constructions are often semantically defined; this situation was left unaccounted for in Stage IV. It suggests that there must somehow be direct links from morphemes to meaning, bypassing the lexemic and sememic strata. Similar considerations at the lexemic stratum indicate that there must be connections from the conceptual system to the lexemic stratum, bypassing the sememic. Thus one arrives at a scheme in which some points of the conceptual network connect to the sememic stratum, some to the lexemic stratum, some to the morphemic stratum.

Now to say only this is not to answer the problem of how we nevertheless account for the participation of morphemes like wood in clauses, which are produced in the lexemic and sememic systems. The essence of the solution is a device that may be called the 'representative', since it functions like a representative in a legislature, who acts for the people he represents, at least in theory, so that their interests are served without their having to be present in the legislature. A representative in the lexemic system is a diamond node which connects into the lexotactics and downward to one or more (via 'downward or') locations in the morphotactics, leading to the morphological forms which it represents, both fixed morphemes and, in some cases, products of productive constructions. Such a representative functions as a single lexeme in the lexotactics, but it connects downward to several or many morphemes, which are distinguished from one another by their different connections to the conceptual system, as well as in part by different morphotactic properties.

Thus the morphemic, lexemic, and sememic strata do achieve a partial coalescence in that they share a common inventory of morphemes (while the lexemic and sememic strata also share a common inventory of lexemes), and in that all three are in part directly connected to the conceptual system.

According to the current model, then, the linguistic system as a whole, which is distinguishable from, but intricately interconnected with, the conceptual system, consists of two main portions, which

could be called phonology and grammar; and the phonology consists of a single stratal system, while the grammar consists of three partially independent stratal systems, whose three separate tactic patterns are concerned with morphology, surface syntax, and deep syntax.

Last Spring, at the end of my course on Language and the Brain, in which we had been considering, among other things, various of the uncertainties and unsolved problems of Stage IV, I told my students that I would be happy when I knew as much as I thought I knew in 1966. I am now able to report that I am happy.

NOTES

¹Actually I believe that this is correct only in part, that there are also other reasons for the differences. But let us concentrate on this one for now.

²As I have previously pointed out (Lamb 1967, 1966a), Hockett, a 'neo-Bloomfieldian', and Hjelmslev had both advocated the generative aim some years before Chomsky appeared on the scene.

³I shall now just use the term sentence, to make the discussion applicable to transformational-generative grammar as well as, by implication, to any type of generative grammar that would attempt also to generate units of discourse composed of sentences.

⁴Cf. Hockett 1961. Hockett considers a solution like that described here, but in the end rejects it.

⁵This Outline proposed the possibility that languages have five strata, but the four-stratum model was returned to by late 1962.

⁶It is true that neo-Bloomfieldians didn't always recognize phonological components, nor did they always systematize the relationships among their units to the degree shown here. But if one performs a logical analysis of their use of the terms and removes inconsistencies, one arrives at this scheme.

⁷This paper was published in 1966 but was presented at a conference in 1963 and revised in 1964.

⁸Prescriptive phonologists find evidence for more than two levels of alternation in phonology (e.g Chafe 1968), but mutation rules impose their own notational ordering requirements independent of the linguistic structure and therefore cannot be used as a basis for arguing about how much stratification is needed if one uses a realizational format (Lamb: in press).

⁹Two approaches were followed in this area. Peter Reich attempted to specify the operation of the nodes by defining each as a finite-state machine. A general network processor was constructed for the 7094 computer, using this approach (Reich 1968c). The other approach was to set up models of the internal structure of the nodes, specified by means of more elementary network components. At this more elementary level there are four basic node types, the branching, the 'and'

junction, the 'or' junction, and the blocking element; and all lines are one-way. A computer system for testing networks constructed in this system is currently being prepared.

¹⁰In connection with the treatment of alternation, some new network devices, including one-way lines and nodes, have been posited. These and many other matters touched upon here are described in Lockwood (in press).

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LINGUISTICS IN GREAT BRITAIN IN THE SIXTIES: PERSPECTIVES FOR THE SEVENTIES

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When Robins wrote his article 'Linguistics in Great Britain, 1930-60' (Mohrmann and Sommerfelt, eds. 1963) he gave an account of linguistics in Britain by concentrating on the work of Jones, Malinowski, and Firth. This general linguistic teaching, compared to later work in the field, can be viewed as a set of attitudes or perspectives about the nature of language and how to deal with it, rather than a system of linguistics. Firth (1957) declared himself an enemy of 'system' or 'systematization', if this implied the erection of a mechanical set of discovery procedures, particularly those based principally on distributional criteria. He held that the principal task of the linguist was to give an account of the meaning of language, and meaning, he said, was communicated by the entire range of interlocking systems and structures at all levels of analysis, phonetic as well as grammatical, lexical as well as situational.

Partly as a consequence of the centrality of meaning statements, he insisted on studying language, not in abstraction from its actual use, but in restricted languages, such as that used in drilling, or giving orders, or playing sports, for which activities contexts could be readily supplied. On the lexical level, he said that individual items acquire aspects of their meaning through their collocations or habitual use, with other lexical items; on the level of syntax, he discussed colligations, the cooccurrence of grammatical categories, so that for both lexical items and grammatical categories, one could discuss certain mutual expectancies. The framework within which all such relations were discussed were System (his expression for paradigmatic relations) and Structure (his expression for syntagmatic relations). His approach was polysystemic: that is, a given structure could be the locus of more than a single system. Where phonemicists posit, for

instance, a single vowel system for a language, Firth would set up as many systems as there are differences in the number of vowels that could occur in given positions in a word. He would not treat discrepancies in the distribution of such vowels in terms of defective distribution or neutralization. The elements of his phonology also differed from that of phonemics, in that he distinguished phonematic units identified through their phonetic composition and distinctive function at the lexical level, from prosodies, which are sound features usually more than a single segment long, including material dealt with in phonemics as suprasegmental, and sometimes segmental.

In contrast to Malinowski (1923, 1935), from whom he derived the basic orientation of studying language in a context of situation, Firth believed that all the categories he discussed were abstractions made available for the description of language, constructs the linguist has found useful, not entities actually existing in reality. For this reason, his own work could be interpreted either behavioristically or mentalistically, since he felt that adherence to either position was not required of the linguist, particularly since we claimed to know so little about minds.

The state of linguistics in Britain today is vastly different from the situation described by Robins, and it is not unreasonable to assign 1960, the date of Firth's death, as a useful point at which to begin to describe the change. Before that date, Firth was linguistics in Britain in a special way: he had long held the only chair of General Linguistics in the country and had been chairman of the Department of Phonetics and Linguistics in the School of Oriental and African Studies (SOAS) in the University of London since 1944. Since 1960, graduate and undergraduate programs in linguistics are found in very many more universities: St. Andrew's (Scotland), Cambridge, Edinburgh, Essex, Lancaster, Leeds, London's University College and School of Economics, Manchester, Newcastle, North Wales (Bangor), Reading, Surrey, and York. Most of these departments are not simply departments of linguistics, but, like Firth's, combine linguistics with phonetics or languages. Since not all Universities list professors of linguistics, it is not possible to be certain about the number of lecturers who offer the subject, but there has been at least a tenfold increase in the ranks of teaching linguists, with a correspondingly large increase in the number of students, particularly on the undergraduate level. Planning in British education is done on a five-year basis. In the British scheme of things, a discipline is recognized as mature when a chair and department are created in the subject. There are forty-six British Universities. In 1967, at the beginning of the last quinquennium, more than twenty of those lacking departments of linguistics applied for the establishment of one. Most of these requests were denied because it was thought that there would not be sufficient numbers of qualified personnel

to form strong departments, but it is likely that more centers will be added at the beginning of the next quinquennium in 1972. If there is a shortage of professional linguists, there is no lack of interest in the subject. This is attested at Oxford, for instance, by the superlative offerings in linguistics stocked by Blackwell's. For several years, linguistics, as represented principally by work in the transformational-generative approach, has been a subject of great interest for philosophers and psychologists. This year, Oxford has appointed Pieter A. M. Seuren its first Lecturer in Linguistics. This interest stems principally from the impressive work produced by the transformational-generative approach. Among undergraduates, interest has been stimulated by a pamphlet What is Linguistics? by David Crystal (1969). This was prepared to help the counselling of sixth formers, who want to know what subjects they can study at the University and is kept up to date by successive editions, of which the third is to appear. Crystal lists eight centers where there is undergraduate work in linguistics: Reading, York, SOAS, London School of Economics, London's University College, Edinburgh, North Wales, Bangor, and Surrey. In the planning of the University Grants Council, more stress has been given to the development of undergraduate, than to graduate programs.

Firth's work was of course not the only linguistic activity in Britain before 1960, but it deserved the prominence given it by Robins, because of its originality and because of the rather large number of linguists trained by Firth. But linguistics in Great Britain today is truly international and it is only in individual cases that one would want to characterize it as peculiarly British. First of all, it is clearly not Firthian; but on the other hand, there is no strong commitment to any approach. All of the departments I contacted considered themselves eclectic, although there were always individual professors strongly committed to a particular approach--TG, Stratification, Firthian, Systemic Grammar, Functionalism, Tagmemics, etc. These differences vary rather constantly with the age of the instructor and the source of his training. All of the younger ones--and they now form more than half of the roster--began linguistics when TG was the latest and most exciting work in linguistics. Those trained in American institutions are for the most part convinced of 'the mastery of M. I. T.', as one linguist explained to me. All departments agreed that the student deserves an introduction to all of the representative theories, and this naturally leads to a conflict between coverage and depth. The older linguists from the Firthian era find value in these new ideas, but many offer, as one put it, "sceptical acceptance" to the TG approach.

This 'sceptical acceptance' clearly reflects Firth's avoidance of commitment to the reality of the categories he employed to describe language, and there are other perduring characteristics of British linguistics that one notes. One is the strong interest in phonetics, fostered

by Sweet and Jones, which continues in extensive work in articulatory, acoustic, and physiological work in phonetics. Even the newest and smallest departments have remarkably sophisticated phonetic laboratories. Another characteristic is the continual production of descriptive materials drawn from that sphere of influence now known as the Commonwealth and from elsewhere. The phonological aspects were more stressed in the earlier work, but now the shift is to the grammatical. Another characteristic of British linguistics is seen in the formation of the instructional staff. Few of the present older generation of linguists did either their first degrees or their doctoral work in linguistics. There was next to no undergraduate study in linguistics available before 1960, and the majority of degrees done by this group were in fields such as Classics, Languages, Psychology, Anthropology, etc. Increasingly, the younger instructors will have done graduate work in linguistics.

The undergraduate programs being offered in British Universities typically combine the study of linguistics with other subjects, such as languages, mathematics, psychology, philosophy and social anthropology. Such interdisciplinary work is also found at the graduate level. This is particularly true of the University of Edinburgh, which is generally conceded to be the strongest center of linguistic work in Great Britain. Both formally and informally, there is cooperation and interchange among the disciplines concerned with language problems, such as shared seminars by the departments of Linguistics and the department of Machine Intelligence. There is also a School of Epistemic Studies at Edinburgh, whose function it is to invite distinguished scholars in various linguistic disciplines to lecture and consult at Edinburgh. It also offers short extracurricular courses in linguistics and allied topics, outside the formal degree requirements.

At the University of Reading there has been cooperation between the department of Linguistic Science and the speech therapists and pathologists in the local hospitals. This has now led to an even broader interdisciplinary effort, and attempts are being made to have a Child Communication Study Center funded. The departments of the University of Reading that are currently interested in the project include the departments of Linguistic Science, Applied Physical Science, Psychology, and Education. Engaged in the discussions are also representatives from the hospitals and university administrations. In addition to these, there are other departments with established connections with the hospitals, including Physiology and Biochemistry, Microbiology and Applied Statistics.

Another interdisciplinary project of great interest is found in London. Since 1963 research has been conducted in the London Institute of Education at the University by the Sociological Research Unit of the Institute. This is particularly interesting in the context of our discussion because of the presumed Firthian influence involved. This research is concerned

with differences in the verbal behavior in a large sample of middle and working class, five year old pupils, under controlled conditions. The analysis relies on the Scale and Category grammar of M. A. K. Halliday, who has collaborated closely with Professor Basil Bernstein, in charge of the project. Halliday also trained most of the linguists involved in the research. The results of the research are being published in a monograph series, 'Primary Socialization, Language and Education'. Twelve titles have been advertised and four of the monographs are already in print.

Halliday's work comes closest to being what one could call British Linguistics, not merely because the work was done in Britain, but because of the explicit appeal he makes to the basic ideas of Firth (Halliday 1961). Some professed Firthians see this work as a legitimate development of the original insights, but others either doubt or deny it. Those who see Halliday as continuing Firth's approach do so because of the centrality given system and structure, and to the study of language in use, what they would call the 'sociological' approach. Those who doubt the legitimacy of the developments stress Firth's bias against systematization and his predilection for the study of restricted language structure. Those who deny the logical development (Palmer 1968:8-9) would stress Firth's polysystemic approach, which Halliday's retention of the phoneme violates, and would claim that mere use of Firthian terminology is not enough to make the work truly neo-Firthian, e.g. 'exponence' (Palmer 1964).

In order to have a fixed theoretical framework for the research, it was necessary to choose a stable version of the developing thought. The formulation of Scale and Category grammar, which lies behind this research, is not the same as the more recent, 'Systemic Grammar'. One of the researchers finds no basic conflict between the two stages of the development of the theory (Turner and Mohan 1970) although the categories for the description were worked out in 1965. Bernstein considers the differences very important.

In Social Class, Language and Communication (Brandis and Henderson 1970) there is an account of the careful preparation for the research, the controls used and how the children and their mothers were tested for language use. The mothers were interviewed about their typical reactions to the child's questions and problems and a notable difference between the habits of middle-class vs. working-class mothers appeared: the middle-class tended to explain, exhort and motivate the child through language, while the working-class was more prone to simply give orders. Correspondingly, a finer gradation in the options used by the two groups was found, both between the sexes and between the classes. Using a model room, picture-story cards, scenic pictures, imaginative completion of stories, and the explanation of the games they played, the investigators found differences in the frequency of type-nouns vs. token-nouns,

adjectives, verbs, etc. used by the two classes of children. They found a tendency on the part of the middle-class children to be more cautious, using expressions like I think..., It might be... or It looks like... in assigning descriptions, whereas the working-class child would more generally define directly, It is.... They found this an indication of the middle-class child's ability to deal more patiently with the future as opposed to the working-class child's preoccupation with the immediate present. This difference in attitude, they felt, was important for success in schooling, which promises much for the future, but often offers little of relevance to the present.

In Talk Reform (Gahagan and Gahagan 1970) the authors describe the techniques they invented and employed to stimulate the children to use more options in their speech than they would probably use if left to their own resources. This derives from Bernstein's conviction that there is a difference between the codes of children from the two classes. One is a restricted code which exemplifies reduced options and therefore depends on familiarity with the context on the part of the listener. The other he calls an elaborated code, which shows a more structured syntax and which is less dependent on the context of situation. He is convinced that the difference does not lie in the availability of options, but in their realization. He concludes that these findings do not demonstrate that working-class children are inferior in competence, since they do have a passive grasp of the distinctions they rarely employ, but that they are not encouraged to make use of them. This encouragement, he thinks, is the prime responsibility of the school, and programs which stimulate discrimination, such as that described in Talk Reform, could do much in helping children who do not fully exploit their language resources, hence fail to socialize, and consequently do poorly in school.

In A Linguistic Description and Computer Program for Children's Speech (Turner and Mohan 1970) there is a detailed account of the categories employed in the analysis of children's speech, based on the 1965 Halliday's Scale and Category grammar. Since the findings were to be computerized, each element had to be made explicit and assigned a coded character. This also had the effect of allowing the investigators to use a more or less 'delicate' analysis, depending on how many categories and intersections they chose to consider.

While this research was in progress, both Halliday's work in grammatical theory and Bernsteins' sociolinguistic thought have evolved rapidly into positions beyond the scope which underlies the published work. A sample of Halliday's current thinking is found in the chapter 'Language Structure and Language Function' that he contributed to John Lyons' reader New Horizons in Linguistics (1970). This book, incidentally, was held by all the linguists I questioned in Britain to give a fair, if selective, picture of the kind of work and attitudes to be found there.

This chapter illustrates the general attitude toward language and its analysis which Halliday presented in his 1964 paper at the Georgetown Round Table, 'Syntax and the Consumer'. He holds that different models in linguistics are best considered to be appropriate for different aims, rather than competitors for the same goal. While rejecting the usefulness of a distinction between competence and performance, then, he does not think that this involves him in a refutation of the psychological presuppositions of TG, since what he studies (called 'performance' in that approach) can be handled without a previous study of competence. British linguists do not universally accept that view, of course, and Marshall and Wales (1966) reacting to the Round Table paper, point out the need for a previous study of competence, since there would otherwise be no norm for evaluating the results of performance studies.

Halliday views a speech act as a simultaneous 'selection' from among a large number of options. These options represent the 'meaning potential' of the language. Such options include the possibility of making a statement, issuing a command or asking a question, etc. The options are 'systemic', i.e. choices from a definite paradigm, and while the number may be enormous, they can be reduced to a limited number of relatively independent 'networks'. These networks correspond to certain basic functions of language.

Three main functions are discussed: the first is 'Ideational' and it is concerned with communicating the content of the speaker's experience, including his own consciousness. Like Firth, Halliday thinks this could be interpreted either mentalistically or behavioristically. Secondly, there is the 'Interpersonal' function, by which language serves to establish and maintain social relations (it is here that he quotes the importance of Bernstein's work on educational failure being traceable to language failure). Finally, there is a 'Textual' function. This enables the speaker to relate what is being said to what has preceded and will follow, and so to form texts or connected discourse.

In the remainder of the article, Halliday concentrates principally on illustrating how the ideational function (which others have called cognitive or semantic) can be studied in the clause. First, he considers the expression of processes (actions, events, states, and relations, the persons, objects, and abstractions associated with them). While there are a vast number of processes distinguishable in a language, he focuses, for the sake of illustration, on a system which opposes all processes as 'transitive' and 'intransitive'. In such a system, therefore, there are only two options.

Associated with each type of process are a small number of functions or 'roles' resulting from the parts the various persons, objects, and other phenomena play in the process. In the sentence Sir Christopher Wren built this gazebo we have a 'transitive clause' containing three 'roles': actor (Sir Christopher Wren), process (built) and goal (this gazebo).

The 'specification' of this clause would involve (i) the 'selection' of the option 'transitive' from the 'system' transitive vs. intransitive. This would then 'determine' (ii) the presence of the 'functions' 'process', 'action', and 'goal', and these are 'realized' by (iii) built, Sir Christopher Wren, and this gazebo respectively.

The 'roles' that appear in the expression of processes include (i) the process itself, usually represented by a verb, (ii) the participant functions of the persons and objects, and (ii) circumstantial functions, the associated conditions and constraints such as place and manner.

Halliday distinguishes between central and peripheral roles. Circumstantial roles are considered comparatively peripheral, although there is a difference between what he terms inner and outer ones. The 'place' element is 'outer' if an actor is present (as in He placed all his jewels in the wash) otherwise it is 'inner' (as in He lost all his jewels in the wash).

Analogous to this distinction is the notion of an 'Inherent' role. This is not called 'obligatory', since it may or may not be realized in expression. For example, pelt is considered inherently instrumental: They pelted him (with stones), just as give is inherently benefactive: I gave the best present (to him) although both the instrument and the benefactored may not be expressed.

Other options involve 'Voice'. The options are middle vs. nonmiddle; if nonmiddle, then active vs. passive; if active, then + or - goal; if passive, then + or - actor. The ground for choosing one of these options is the 'Textual' function of language, although which options are available depends on the transitivity of the clause. A middle clause has one inherent participant (John sneezed); a nonmiddle has two, actor and goal, although one or the other may not be expressed, as in Mary is washing (her clothes) or The clothes have been washed (by Mary).

There is much more detail in the rest of Halliday's article; but the picture that emerges is, that within the Firthian framework of system-structure, Halliday concentrates on systemic relations, from a functional point of view. From the systemic, the syntagmatic or structural elements appear as a set of options compatible with, or required by, the realization of other options. Given the option 'transitive', in the example above, Halliday says that this 'determines' the presence of the functions 'process', 'actor', and 'goal', and these are then 'realized', in this particular case, by Sir Christopher Wren built this gazebo. He has taken the expression 'realization' instead of the Firthian 'exponence' from Lamb because it is more widely known and appears to avoid the objections raised by Palmer concerning Halliday's, as opposed to Firth's, use of 'exponence' (Halliday 1966a, Palmer 1964).

Parallel with this shift in Halliday's linguistic theory are changes in Bernstein's sociological thought. Now that the monographs giving the

details of his research are being published (the research will stop in the summer of 1971) his sociological theory has attained a form that is not adequately instantiated by the earlier work. With the elaboration of the Network Theory in Halliday, Bernstein has found a more satisfactory conception of sociolinguistic investigation, since he can map the sociologically important choices in a context onto the linguistic choices made possible by the Network. In this way it is possible to point out which linguistic choices are sociologically significant.

Bernstein, like Halliday, is convinced that one selects a linguistic theory because of the uses to which it is to be put. Halliday's work enables him to study texts as units (Hasan 1968). Because it is socio-logically oriented, congruent with his own views, and not limited to the sentence level, Bernstein therefore prefers Halliday's approach to Chomsky's, which he considers restricted to the sentence level and psychologically, rather than socio-logically, oriented.

Bernstein sees the importance of his research in terms of the procedures developed, the conceptualizations formed and verified. The findings of the research, on the other hand, he would characterize as fairly trivial, since the findings will change in a few years; but the permanent accomplishment will be the methodology and its theoretical justification.

The major change in his sociological approach is, he thinks, the change from a context-free definition of codes to a context-dependent definition. In the Regulative Context, for instance, which involves persons of higher vs. lower status, such as parent vs. child, teacher vs. student, Bernstein had said that every child in every culture would have access to the linguistic markers of the logical distribution of meanings. These would include Hypotheticals (If you do that, you'll pay for it), Disjunctives (Either you do that, or else...) etc. But on this context-free definition, there is no way of distinguishing between the different regulative contexts. It is likely that in the instructional context, the hypothetical, disjunctive, and conditional expressions are less used than in the home situation. He has recently begun to apply Halliday's Network theory to the study of Regulative Contexts, which he analyzes into six subsystems with a number of choices. Such context-dependent definitions have led to a study of the relationships between 'code' which is a very high level abstraction, and 'speech realizations' in specified contexts. A consequence of this is that there cannot be a general linguistic definition of 'code', although Bernstein thinks that there is still justification for the distinction between 'elaborated' and 'restricted' codes. If the code is elaborated, a different pattern of meanings will be realized and this fact of itself will activate a different set of linguistic choices. Accordingly, the codes will define the meanings which are realized in particular contexts. He acknowledges that this involves very difficult and subtle

problems, such as attempting to spell out the macro features in a society that shift and change (e.g. the division of labor, the value systems of the society) and the specification of how the codes are evoked, generated, and maintained in micro interaction.

Bernstein thinks that two of his key concepts, restricted and elaborated codes, have been misunderstood. By the distinction, he was trying to point out that when the code is elaborated, the speaker is forced to a greater exploration of the syntax than when the code is restricted. This does not, he thinks, involve any judgment about the relative value of either code. The differing exploration of the syntax is correlated with the social factors behind the movement toward either elaboration or restriction.

As a corollary to this, Bernstein does not acknowledge a distinction between standard and nonstandard language. He would not call the 'educated speech' which Quirk's Survey is studying 'standard language' but merely a form of language peculiar to a particular context or set of contexts. In his opinion, communication fails only when speech is inadequate to the meanings it is seeking to express. While there is more to communication than sharing cognition, the 'more' consists largely of every kind of cleavage, involving some kind of invidious ranking of various forms of speech. As such, he has found nothing inherent in any dialect that prevents the communication of meaning, phonological barriers, perhaps, aside.

Another impressive research project that falls within the period under examination is the Survey of English Usage directed by Randolph Quirk at the University College, London. It was begun in 1960 and the data-collecting and technique-evaluation phases of the project will come to an end in 1974. The Survey describes 'educated English' and this is defined as 'the repertoire of the University educated adult', both in writing and speech. It will be based on a corpus of a million words, contextualized in a wide variety of uses. From the point of view of this study, Quirk's work is significant for several reasons. First, it is clearly conducted outside of the Firthian sphere of influence; secondly, it has already produced a considerable number of important publications (Bald, Carvell, Close, Crystal, Davy, Godfrey, Greenbaum, Kalogjera, Kempson, Quirk, Svartvik) and thirdly, it is publishing the largest and most detailed grammar of English ever produced in an English speaking country. Finally, its resources will be preserved as an archive for the consultation of students and scholars.

The corpus of the Survey consists of written and spoken materials classified and weighted according to elaborate criteria. For instance, the spoken section consists of surreptitious recordings of conversations, as well as telephone calls and conversations which the speakers knew were being recorded. These are distinguished according to the relationships among the speakers. Also included are spontaneous

and prepared oral commentaries on various topics. The written part of the corpus contains works prepared for oral delivery, both published and non-published types. Radio talks, readings of the news, stories, formally scripted speeches and drama were entered under the written-for-oral-delivery category. Letters to friends or strangers, on personal or business themes are put under the non-published class. In the published set, classes such as learned arts, sciences, fiction, and non-fiction, administrative and official documents, persuasive writing, and imaginative prose fiction are represented.

The Survey aims at assuring that there are examples throughout the entire repertoire range. These are then distinguished according to such norms as (a) the amount of linguistic differentiation they show and (b) the influence the variety is thought to have. The language of law, for instance, would rate high on the first norm, since it is self-contained to a high degree, whereas the language of the novel, which does not seek to draw attention to itself, would rank low. Conversely, the language of law would rank low on the scale of likely influence, while that of the novel would be ranked very high on that norm.

To make sure that this complete coverage was obtained, it was not possible to rely on written and spoken sources alone. Materials were also gathered by elicitation from suitable subjects, in writing and by recording. Considerable thought has been given to the techniques of elicitation and to the validity of its results (Quirk and Svartvik 1966; Greenbaum and Quirk 1970). The general conclusion is that no known elicitation technique gets completely natural results, but the reliability of data can be reasonably estimated.

'Elicitation' involves both the judgments and production of samples of language by the subjects. Sentences are presented and operations such as negativization, passivization, questioning, denying, lexical substitution, etc., are requested. There are also completion exercises which can be compared to grade the degrees of uniformity or hesitation among the informants. Judgments that are required of the subjects concern their estimates of grammaticality and similarity of aurally presented material, as well as their preference for forms presented visually. Careful check is kept of the times and intervals in presentation and completion.

Quirk is responsible for the grammatical viewpoint according to which the Survey is conducted. A product of English Schools, his initial general linguistic orientation was that of the Daniel Jones Phonetic School which, he says, stoutly opposed the Firthian prosodic-phonematic approach. His graduate work in linguistics began at Yale, under Bloch, and continued at Michigan with Pike and Fries. The predominant grammatical influences in Quirk's grammatical work, he says, from 1960 on, were the views of MIT, overlaying Pike, Fries, Bloomfield and Bloch, in that order.

In Quirk's view, the Firthian claim to be British linguistics during the period up to 1960 is subject to some distinction: it was the only coherent school that was indigenous to Britain, but there were probably more linguists teaching Bloomfield, and opposing Firth, than those who followed him. It was just the case that there was no other, peculiarly British school of linguistics in competition with SOAS. Even within SOAS, there have always been linguists who were not formed by or were not followers of Firth.

The format of the Survey, which will be preserved as an archive at University College, consists of a vast collection of 4 x 6 inch paper slips. These typically have about 300 words on them, grammatically contextualized according to an elaborate program worked out by the investigators and applied by research assistants. The spoken texts are transcribed and both the spoken and written texts are classified according to the approximately 170 grammatical categories employed. Each slip is reproduced 700 times; and after the application of the classificatory procedures, most of the slips have been filed.

The huge grammar which will be published next fall is the result of the cooperation of Quirk and three of his former colleagues on the Survey, Sidney Greenbaum (Wisconsin), Jan Svartvik (Sweden), and Geoffrey Leech (Lancaster). Over a period of three and a half years, they exchanged materials and had occasional meetings. They were only able to work fully as a team during the past summer, and the version that resulted from an intensive period of work was then circulated for criticism and suggestions among several linguists. As a result of this, the final version was started in November and finished in January of this year. The only grammars that are longer than this one are those by Jespersen and Poutsma and Kruisinga.

What are the prospects for linguistics in the seventies? Prophecy is a chaney business, but there are signs of what one can expect to be found in the plans and ongoing research in Britain. First, despite the likelihood of funding restrictions, it is the common opinion that more centers of linguistics will be started during the next quinquennium which begins in 1972.

Ongoing research, principally in Great Britain, is listed in Report No. 4 of the Linguistics Association of Great Britain as of November, 1970. Approximately 200 of the 600 plus membership of the Association returned questionnaires about their research, classified under 33 headings. All of the Universities with departments of linguistics are represented, as are 24 other colleges and Universities without them. According to this listing, which describes 343 research projects, the most represented are projects concerning the description of foreign languages (63). Next is Stylistics (31), and Historical Linguistics (31), followed by Language Laboratory Programming and Sociolinguistics (17 each). Although they are fewer in number, there are projects of

research into Linguistic Theory (13), Phonology (12), and Grammar (8). Among these, 5 projects in Prosodic Phonology are reported and 6 in Generative Phonology. Grammatical research breaks down into 3 in transformational-generative theory, 4 of other types and 1 in systemic grammar. In Linguistic theory there are 7 in TG, 1 Firthian and 1 Systemic project listed. Dialectology is represented by 14 projects, and Descriptions of English by 10. The rest of the headings have less than 10 entries (Linguistic bibliography and terminology, Communication theory, Computational and mathematical linguistics, Language typology and universals, Philosophy of language, History of linguistics, Native language acquisition, Speech and language disorders, Native language teaching, Phonetics, Lexis, Semantics, Contrastive linguistics, Foreign language learning and teaching, Translation, Multilingualism, Writing and reading, Lexicography, Use of English abroad, Testing, Onomastics, and Miscellaneous).

From these indications and from the discussions at the Spring meeting of the Linguistics Association, it is clear that there will be continued and increasing interest in the TG model, particularly in its semantic aspects. This is coupled with the work long done by the linguistic philosophers at Oxford and Cambridge.

Phonetic research, articulatory, acoustic, and physiological, will continue to be widely cultivated. There are projects not listed in the Association Report concerned with the synthesis of speech, not by the conventional method involving spectrographic analysis and drawing of magnetic lines on plastic, but through computerized rules.

At the University of Edinburgh, an ingenious electro palatograph is being developed by William Hardcastle. Using an enlarged photograph of his palate, Hardcastle places lights behind the relevant areas which correspond to the forty contacts he has built into a plastic artificial palate. With this, it is possible to correlate sound and motion picture recordings of the contacts made during continuous speech. It is planned to produce an artificial palate with 100 contact points for greater discrimination. At the University of Essex, there is extensive myographic work, reported in the Occasional Papers issued by the Language Center. One of the most sophisticated centers of Phonetic research is to be found in the University College of the University of London.

Dialect studies have long been cultivated and are continuing to receive increasingly refined treatment. The Institute of Dialect and Folk Life Studies at the University of Leeds is preparing a Linguistic Atlas of England and has already published several volumes. Similar work is carried on in Wales and Scotland, and a particularly interesting survey of urban speech in Gateshead and Newcastle upon Tyne is being carried out by Strang, Pellowe, Nixon, and McNeany at the University of Newcastle. It aims at a new typology of varieties and employs

statistical methods derived from microbiological methods of classification to reveal relevant traits.

While there are several projects in sociolinguistic investigation in progress, Bernstein is pessimistic about the future of his own brand of study. The chief reason for this is that Halliday is leaving University College, which will make difficult future collaboration. Another reason is that he is convinced that sociology must study transmissions in society, and that so far, little work has been done. The studies concerned with the facts of socialization within the family have not been concerned, as he thinks they should, with the processes of socialization and with the media that carry transmissions. In his view, Sociolinguistics is not so much a field as an area of problems.

The variety of work being done and the promise of still more workers in the field gives promise that Linguistics will continue to flourish in Great Britain. We can therefore expect an increasing flow of important work from that source; and if past performance is any indication, we can learn a good deal from it. In particular, we can learn a lesson from the British cool: they are notable for raising a flag without feeling the need to wave it.

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COMPARATIVE LINGUISTICS: CONTRIBUTIONS OF NEW METHODS TO AN OLD FIELD

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Abstract. The paper offered tries to discuss the significance of some modern approaches in linguistics to the field of comparative linguistics. A brief survey stresses the importance of structuralist notions for recent work in the comparative field. The major part of the paper is devoted to a critical evaluation of generativist contributions; their suggestions as to the nature of sound change in general and of specific problems such as the regularity hypothesis and the developments subsumed under the name of analogical change are discussed, and some problems of detail in publications of King and Kiparsky are subjected to further investigation.

Any history of modern linguistics will have to assign a prominent place to a discussion of developments in the field of Comparative Linguistics, for it was here that methods of scientific investigation of language in itself as a subject of scholarly inquiry were first developed and refined. Comparative Linguistics is the oldest branch of the language sciences; benefits and difficulties derive from this state of affairs. Among the benefits I would list the fact that few of our subdisciplines have had their data and their hypotheses subjected to the scrutiny of such large numbers of competent scholars who approached their material from often most divergent points of view; assumptions and interpretations that have stood the test of time and continuous criticism have a very good chance of being unfalsifiable and thus acceptable as scientific hypotheses. So much has been generally agreed upon that time and again scholars in the field lost sight of the true nature of the conclusions they themselves as well as their predecessors had been drawing: there was a tendency to overlook that what was communis opinio still was only a hypothesis or a network of hypotheses, so that new proposals conflicting with old views could be dismissed as

'mere hypotheses' rather than as hypotheses with a lesser probability ranking than the old ones (if that indeed did apply). Difficulties of the discipline also derive from its age: On the one hand, there are still remnants of the nineteenth-century view that comparative linguistics covered everything that was of a general nature and therefore of more than language-specific interest in the entire field of linguistics, so that for instance the need to have the inquiry into the general properties of language reflected in the curriculum of a university, was at least until rather recently widely believed to be adequately met by having a chair for Comparative, and even more specifically, Comparative Indo-European Linguistics, as one of the chairs that, taken together, made up the total of a Philosophical Faculty in a German university. This difficulty is, however, essentially a thing of the past, and one need not pay much attention to it. Another problem has much greater practical significance: The very fact that so many important results have been secured by the generations of scholars working in the field of comparative linguistics, has contributed to the impression that little truly original work remains to be done here. Many an observer is inclined to think that the discoverable has been discovered, and that what is left amounts to so little as to be uninteresting. If this assessment was correct, it would suffice to devote some cursory comments to the field, taking stock of achievements of the past and pointing out failures and the reasons for both, and then to devote one's time and energy to the truly urgent and significant facets of linguistics.

As a matter of fact, the assessment given does not seem to be valid. To be sure, the bulk of publications past and present is devoted to a painstaking discussion of details, but attention to details is not equivalent to occupation with trivialities. Very much through this work on detail, our understanding of the prehistory of the Indo-European group of languages has been enriched and deepened. New insights have been gained on the one hand through intensive investigation of some of the lesser-known languages of the group; to name just one point here, our notions about the Proto-Indo-European verb system and its internal development are now vastly different from those current half a century ago when an abstraction from Greek, Armenian, and Indo-Iranian data was thought to give a reasonable semblance of the oldest recoverable state. Evidence from Anatolian languages as well as from Tocharian indicates that the situation found in the 'classical' languages reflects the results of coalescence of two formerly coexisting verb types. Similarly, such presumably highly archaic features as the occurrence of three ablaut degrees in certain noun-declension types in the 'classical' languages no longer is projected back without further questions into the so-called parent tongue: the simpler pattern found in Latin and Tocharian now seems to have a good claim to being older.

Work on details would have been less successful if it had not been carried out with due attention paid to methodical advances outside the narrow field itself. Comparative linguistics of recent years owes very much to ideas developed by structural linguistics; the days when comparative research could be rightly accused of being atomistic are largely past. The question as to the place of a reconstructed feature in a reconstructed system is part of normal procedure; no longer will one be satisfied with the reconstruction of a feature at odds with the rest of the system, as was the case when Brugmann posited PIE \bar{b} to account for the correspondence Gk τ : Skt g found in Gk téktōn : Skt táksan ‘carpenter’ regardless of the fact that * \bar{b} remained an isolate in the Indo-European consonant system. Hypotheses about number and distinctive properties of the reconstructed so-called laryngeals can no longer be proposed in a vacuum; mere phonetic speculations, necessary as they may have been as a preliminary step, no longer suffice--witness the development of the argumentation from Sturtevant to Martinet (1958) and Andreev (1957).

Much more consciously than in the past, reconstruction will now attempt to recover as much as possible of entire systems; reconstruction of historical development through time will aim at the reconstruction of sequences of diachronically differentiated systems; let me mention here just the early work of W. P. Lehmann. One of the most beneficial side-effects of this emphasis on the systemic character of language has been a greatly increased awareness on the part of the investigators of the gaps in our knowledge of earlier stages of language and language groups; if a reasoned mapping of one's results is part of normal procedure, lacunae will assume much greater importance than if it is enough to include one's findings in an unordered list. I think that in many ways we have become more modest about our knowledge of past stages in the development of Indo-European languages, although as a matter of fact the amount of information at our disposal has greatly increased; but now we see our findings in proper perspective, and the white spots on the map show how unevenly our actual knowledge is distributed.

Partly as a result of the increased awareness of our limitations, certain vigorously debated topics of the past have now suffered virtual eclipse. One of the most fascinating directions in Indo-Europeanist research of generations past was the attempted reconstruction of an Indo-European proto-culture; as a matter of fact, this aspect of the field was probably that with the strongest appeal to the layman. Nowadays, very little tends to be said about such topics as material culture, social organization, or religious beliefs of speakers of the Proto-Indo-European parent tongue, not because such topics no longer would hold any intrinsic interest, but because we have learned to differentiate in

much greater detail between various stages in the development of the language group, and we are no longer willing to draw any conclusions about the cultural implications of some linguistic features unless we can be certain about the assignment of these features to specific levels in relative time. We know that we know relatively little about the rules and regularities of semantic change, and we have become more reluctant to assign a basic meaning to a reconstructed lexical item, if the attested reflexes of this item differ in meaning. We know that, given substantial time depth, clear decisions between assignment to the class of native features or that of borrowed ones cease to be possible, with the result that we can no longer be certain as to what should be included in the lexicon of earlier reconstructed stages; thus, it seems questionable whether one of the stock items in every discussion of Indo-European vocabulary, **ekwos* 'horse' can be projected back beyond a relatively late stage.

All this makes Indo-European studies now a good deal less colorful than they were one or two generations ago; but on the whole I think that they have profited from being forced back to their proper domain. Indo-European studies have again become primarily a language science and ceased to be a rather vaguely defined branch of cultural studies. No doubt, in this way the field lost some of its wider appeal; but in return, I think it has assumed new importance in a linguistic context. To me, it seems significant that in the United States a surprisingly strong survival of Indo-European studies has occurred; I doubt whether the same would have happened if Indo-European studies had retained their cultural bias.

All told, the impact of structuralism upon comparative linguistics has been beneficial. The goals of comparative work have become more sophisticated linguistically--both systematic consistency and realism have grown to be necessary properties of the proposals made. Language per se, and no longer language as a vehicle of information about properties of the universe, has become the focus of interest again. It now no longer is the exception, even in such conservative areas as Central Europe, that a specialist in the field is also well-versed in general linguistics, so that comparative linguistics will have a chance of being subjected to methods and approaches found useful elsewhere.

So far, I have limited my comments to the results of an exposure of comparative linguistics to structuralist influence. It seems appropriate to ask now the question whether major contributions to the field of comparative linguistics have been made, or can be expected, from generative grammar.

My remarks up to this point have been extremely general in nature. I felt that such an approach to my topic was justified because the facts referred to can be assumed to be commonly known. In what follows, I intend to proceed rather differently; here I will discuss individual

contributions, appraise them and, where needed, criticize them. Before I can go into details, some introductory remarks seem called for.

One might ask the question whether generative grammar can be applied in the domain of comparative linguistics at all; the reason for such doubt can be that there obviously is no linguistic competence common to the speakers of the various languages compared, and that thus no grammar in the generativist vein can be written for these languages taken together. However, this objection is not valid if the purpose of comparison is the reconstruction of at least parts, and ideally the whole, of a real language, viz. the parent tongue of the group; there then is to be discovered an unbroken chain of languages to be described by an unbroken chain of grammars; these grammars can be correlated with one another along the time axis, just as grammars describing a sequence of stages in the development of individual languages taken by themselves can be correlated. Interlanguage comparison with resultant postulation of source forms is in essence not very different from intralanguage comparison of morphemes considered to be related and therefore usable as bases for the reconstruction of underlying forms. Therefore, if generative grammar can be successfully used in the field of intralanguage historical linguistics, it must also be considered applicable in the wider field of interlanguage historical-comparative linguistics. The question as to what to expect as contributions of generative approaches for the field of comparative linguistics can then be answered on the basis of results obtained in intralanguage historical linguistics.

Generativists claim to have provided a deeper understanding of the nature of linguistic change. They say that change is change in competence (and therefore in Grammar), not just in performance. This is fine--up to a point. That linguistic behavior must be adjusted by a speaker in such a way as to produce a changed form correctly, and that this adjustment can be described by changing the array of rules used to describe the systematic linguistic behavior of this speaker, can be accepted as a reasonable hypothesis. But the crucial question is not: How is the Grammar of a speaker adjusted to a changed form? but: How does a changed form come into existence? Why should the originator of the form all of a sudden garble his rules and produce something deviant (which by itself would not be anything extraordinary yet, since this after all is the anatomy of the mistake)--but why should this deviation from established patterns be codified forthwith in the Grammar of the originator and accepted readily by those whose Grammar, at this point, can be presumed to be intact? It seems much more reasonable to adopt the view that deviating, that is: changed, forms, occur constantly in every speaker's performance; that most of these forms do not receive a higher rating than that of slip or error and are therefore disregarded as potential base forms for reiteration; but that

some of the deviations, given favorable circumstances, are imitated both by the original speaker and by others. Once accepted, they no longer are deviations, and the behavior of speakers has to be adjusted so as to produce the forms now accepted in an adequate way. It seems unwise to single out only this very last step for consideration. To be sure, if competence alone is the subject of a linguist's inquiry, the decision makes sense; but the price paid is too high: the reason why a change came to occur cannot be given--brushing aside performance means foregoing a chance to explain what has happened, and why should we advocate a reduction in explanatory power if it can be helped?

Sound change taken as grammar change only treats language, as it were, in a vacuum. (The insight that sound change is grammar change is, by the way, trivial; if I assign phonology to grammar as one of its components, any change affecting phonology will automatically be a change in grammar; the statement is thus tautological.) The same strangely unrealistic attitude is also displayed in the attitude of generativists versus the role of children learning their native tongue as originators of change. It is said that a child, from observing language as used around him, constructs a complex of rules which serves as his Grammar. The array of rules will be largely identical with that which can be used for a description of the linguistic competence of the adult, but there is a tendency to simplify the rule system. This again is fine--as far as it goes. For it has to be noted that language learning is learning to produce utterances that will be acceptable to the community to which the child belongs. It is conceivable that a child could develop a set of subconscious hypotheses (if such a thing can be assumed to exist) which would account for a form observed in the speech of the community in a way simpler than that followed in the production of this form by adults: in this sense, a simplified segment of Grammar could be retained by the child indefinitely. However, the crucial point is whether the output of a set of rules used by the child is acceptable to the community; and here, it is only realistic to state that the odds are overwhelmingly against the community's adjusting its own Grammar to that of the child and almost as much against a child's resisting community pressure and retaining his private form. It is simply beyond reasonable imagination to posit a situation where some Proto-Indo-European child playfully or otherwise added the feature [+Continuant] to its set of features characterizing voiceless obstruents and the group around him not only tolerated this development, but permitted its adoption, whereupon Proto-Germanic had come into existence. The importance of a new generation as a source of innovation seems grossly overrated in the thought of generativists; children learn a language by imitating their elders and by being instructed by them; the new generation becomes a force in the community only at a time when allegedly the innate ability to reshape a Grammar by simplification has ceased to operate.

These seem to be the hard facts today; and from all that we know about other and earlier civilizations, the role of the child in a community seems to be even more subordinate than in our own society.

To sum up the last two points: Neither the identification of sound change as Grammar change nor the claim that Grammar construction by a child is the principal, if not only, cause of Grammar simplification (which in turn means: of all major change in Grammar) are sufficiently supported by evidence to be convincing. In both cases, the realities of language use are largely disregarded; language is treated as something purely abstract, while the data available indicate that it forms part of a complex communicative network.

The notion that sound change is Grammar change (Postal 1968, King 1969) is a necessary corollary of the inclusion of phonology among the components of grammar. Another result of this decision is the rejection of the principle of regularity of sound change in its purest form: if the phonetic shape of a form is determined by low-ranking rules which form part of the grammar, it makes good sense to assume that such low-ranking phonetic rules could at least in part be determined in their specifics by higher-ranking grammatical rules. Even with these premises granted, the suspicion that something might be wrong here cannot quite be suppressed: Why is it that the statistical frequency of all-pervasive, context-free (in terms of Grammar) changes is so much higher than that of changes delimited by grammatical categories? Does not such an observation seem to indicate that sound change operates first of all independently of non-phonological grammar, and that all we have to reckon with is interference from morphological, syntactic, and semantic patterns on a level which is basically autonomous? This question cannot be answered in the negative by pointing out instances of grammatical interference, certainly as long as the interference from semantic paradigms cannot be accommodated in the theory (it is interesting to note that generativistic discussion of analogy includes only analogy within what has traditionally been considered grammatical paradigms; such matters as the reshaping of a word for four after that for five or that for father after the one for mother, all well-known instances of analogical interference, remain untreated). For the time being, I can see no reason for rejecting a strict regularity hypothesis in favor of a weakened one which admits ad-hoc solutions on a par with overall solutions. The notion of major and minor rules offers no way out until the difference between the two is more clearly defined; at present, one cannot help gaining the impression that concepts like minor rule or idiosyncratic development and minor rule, which seems to reflect the difference between phonologically predictable and phonologically unpredictable change, offers small comfort; I cannot help suspecting that admitting the notion of a minor rule, with its apparent explanatory potential, one runs the risk of settling too easily for

dubious answers (a case in question is King's (1969) treatment of the Germanic weak preterite).

As long as there is full isomorphism between the arrangement of rules arrived at in a generativist frame of reference and the ordering of sound changes according to their relative chronology, findings of the two approaches will be the same; this means that the two can successfully supplement each other, which would be of relatively great interest because it should be possible to discover a change unrecoverable by way of the one approach by using the other. The more important becomes the question as to the relevance of alleged evidence for rule reordering and rule insertion. Closer inspection of the data seems required, since *a priori* a synchronic reshaping of diachronically based rule sequences cannot be excluded.

Since the point seems to be of considerable importance, I would like to discuss two problems from the literature.

King (1969:52 et passim) proposes to explain forms such as German Lob [lo:p] : Lobes [lo:bes] and Rad [ra:t] : Rades [ra:dəs] for older [lop] : [lobes] and [rat] : [rades] (both attested in Middle High German) by postulating a rule order (1) lengthening of vowel before voiced obstruent, (2) devoicing of word-final obstruent, although it is well known that the changes occurred in the sequence (2) - (1). The usual explanation of length in the nominative-accusative form is of course that the long vowel was transferred analogically from case forms with two syllables long after devoicing had taken place. At first sight, King's argument has considerable appeal. But then it becomes obvious that certain difficulties have just not been mentioned. King considers only forms from stage German, a highly artificial breed of Standard German. The apparent neatness and elegance of King's proposal rapidly disappears once actual data are considered in their variety and complexity. From my own native language, which is a variety of Northern Standard High German, two lists of words can be assembled with an alternation between [p t k (or x) s] in the nominative-accusative singular and [b d g z] in the nominative-accusative plural, one with long vowel in the monosyllabic form (as in King's examples), the other with short vowel:

I (long): Stab, Lab, Sud, Lid, Weg, Steg, etc.

II (short): Grab, Trab, Bad, Rad, Tag, Zug, Gras, Glas, etc.

Neither a rule ordering [1] - [2] nor the inverse sequence yields fully satisfactory results; [1] - [2] does not produce the numerous members of list II, [2] - [1], as rightly noted by King, fails to provide for an explanation of the long vowels in I. Matters are not made simpler by the fact that at least in one instance a word has to be listed in both I and II: for Zug 'train' I have both the pronunciation [tsu:k] and [tsux]. To try

to solve the problem by introducing a convention [- Rule 1] for members of the list II seems to offer no attractive way out, certainly not if the rule is inserted where King wants it. It seems to me that the good old solution of simply saying that in words of list I the long vowel of the nominative-accusative singular is due to intraparadigmatic analogical transfer still makes the best sense; to subsume the involved picture under one simple set of rules, one of which has to be invalidated straight away for the majority of forms, certainly does not contribute to an increase in our knowledge and understanding, but has little more value than a game. To say the least, no argument in favor of rule insertion or rule reordering as a grammatical device can be derived from the data when fully presented.

The same statement has to be made concerning Kiparsky's (1965) explanation of the Latin phenomena subsumed under the heading Lachmann's law. Lachmann observed that in Latin past participles (and forms derived from them) voicedness and voicelessness of the stem-final consonant of the verb was reflected in length vs. shortness of the stem vowel: āctus : agere :: factus : facere. Sommer (1948:122-123) proposed to explain this highly unusual phenomenon as due to secondary introduction of stem-final voiced stop in regularly formed past participles *aktos, etc., with subsequent loss of voicing before voiceless consonant; what he implies is apparently that before voiced consonant the vowel was automatically lengthened (as, e.g. in English bed : bet) and that this lengthening survived devoicing, with the result that now [a:] could no longer be identified as an automatic variant of /a/, but had to be reclassified as /a:/ (or /aa/, if this analysis seems more appropriate). Kiparsky suggests a different approach. He wants to insert a rule according to which a vowel becomes long before the sequence voiced obstruent plus voiceless obstruent. This does indeed appear to account for āctus or rēctus (although of course the rule in itself is highly unnatural and counterintuitive), but there are too many contradictory examples to make Kiparsky's suggestion worth retaining (cf. Sommer 1.c.). Most significantly, the rule fails to operate where a related form with voiced stop in stem-final position is no longer present in the language or where semantic change has obscured a relationship. Thus, the word for 'tired' is not *lāsus, as to be expected if Kiparsky's rule had validity, but lassus, and 'cough' is not *tūsis, but tussis. Likewise, 'worst' is pessimus, not *pēsimus. What is required is, that a paradigm has to exist in which forms with [b d g] are materially present (it is not enough that they can be posited for underlying forms, as would be the case with lad- and ped-), and it is further required that the voiced stop reflect Proto-Indo-European voiced stop and not aspirate, which is an important indication of the relative chronology of the development that led to āctus, etc. The requirement that voiced stops in postvocalic position should be present

in the paradigm of which the -t- form is part, clearly points toward spread of the voiced stop in a process of intraparadigmatic leveling. Kiparsky's suggestions do not take account of this fact, and they, too, have to be rejected.

It seems that some of the alleged arguments for the use of generative phonology as a supplement, if not a replacement, of traditional approaches to diachronic phonology have much less value than they were believed to have. Those discussed here, and others no less than these, show a remarkable weakness as far as complete coverage of the data is concerned. What is offered as a solution with supposedly far-reaching consequences for linguistic theory and practice proves to be adequate only for a portion of the material; to make matters worse, there is a tendency not to state fully and explicitly where the formulae offered fail to apply. Such severe shortcomings make it questionable that the contributions of generative phonology at least have much to offer at this time that will survive detailed criticism, except where generative phonology limits itself to restatements of earlier analyses. The proof that the new approach can open up new territory (and not just pave trails blazed by others) still remains to be furnished.

It seems only fair to ask the question whether more substantial contributions have been made in the principal domain of generative grammar, that of syntax. It is not without a certain irony that the section on 'Transformational syntax in historical problems' in King's book is extremely brief and contains little more than a recount of an article by Kiparsky (1968). The section is part of a chapter which opens with the sentence 'Syntax has always been something of a stepchild in the family of historical linguistics'. While this statement is not without justification, one cannot help wondering whether the slightness of generativist contribution to this field is, at least in part, conditioned by the failure of earlier approaches to provide ample data which could be reassembled under new viewpoints, which leads to the question whether generative method is well-suited for work on material not pre-analyzed, a question which of course keeps being asked by people concerned with primary analysis of languages living or dead.

But rather than pursue this question any further, I want to make some comments on Kiparsky's article. The author claims that his data support the assumption that the injunctive of older Indo-European languages, a form unmarked for tense and mood, first arose in conjunctions of verb forms of which only the first retained its marking. He then offers the suggestion that at a later stage the present tense took over as the unmarked form in such conjunctions, and that from conjunctions with a past tense a present form could be extracted as a free form functioning as a past, the so-called historical present. While this analysis seems a little oversimplified, the basic soundness of Kiparsky's central assumption is obvious. However, some rather

important questions remain: From the point of view of morphology, the present tense in Proto-Indo-European and the older among the daughter languages cannot be called unmarked: in a pair **bheronti* 'they are carrying': **bheront* 'they were carrying', the first form is overtly marked by the element *-i* 'non-past'. One would want to know how the author proposes to explain the rather surprising replacement of an unmarked form by the marked one; a reference to general remarks by Jakobson certainly is not enough--even if the present was unmarked elsewhere, this would not be sufficient to overrule the formal evidence of the Indo-European languages. But then, Kiparsky seems to be perfectly willing to disregard evidence of overt form in favor of some alleged general properties not formally signaled; witness his claim that in the pair nominative : vocative the latter is the marked member, in spite of the incontrovertible fact that wherever there is a formal difference between nominative and vocative in Indo-European languages, the latter is the morphologically less complex form (cf. Winter 1969). Simply to assert that the nominative universally is the unmarked case, is not enough; formal evidence always should have precedence over universalistic considerations whose applicability in a given case has to be proved, not merely claimed.

I have gone into some detail here for two reasons. One is that the basic observation made by Kiparsky seems sound (though a generativistic frame of reference is not required to formulate it), the other, that it seems important to point out one thing: Work in comparative linguistics is deeply data-bound; reconstruction is always concerned with meaning and overt form. The latter is not secondary in interest to some abstract deep structure; the degree to which generative grammar can become useful for comparative work depends on the degree of data-relatedness generative grammar can retain and it depends on the degree to which generative grammarians can in their formulations account for the data at hand. It will not suffice to select those items for which elegant explanations can be offered and to relegate everything else to some limbo; there is even less excuse for such attitude here than in work with living languages, for our corpora as a rule are not openended. A small problem discussed here, another small problem discussed elsewhere will be of little interest, no matter what the theoretical implications might be. What we need to make use of the contributions of generative grammar in comparative work, is a concerted effort to cover exhaustively at least whole segments of the grammar of languages which we work with; I think such effort will be worthwhile from a more general point of view on the part of generative grammar too if the material from dead languages would be given a treatment that deprived it of the character of a mere illustration of points originally made elsewhere.

At the opening of the second part of today's paper I said I would want to ask the question whether major contributions to the field of comparative linguistics have been made, or can be expected, from generative grammar. I think it is fair to sum up our survey by stating that what has been offered so far is, in spite of often rather strong claims, still of a very tentative nature. Attempts to give a new interpretation to such notions as change, regularity of change, analogical change have thus far not been successful. Contributions to points of detail have been interesting. Still, it is less what has been done for our field than what can be expected that makes generative grammar interesting for the comparative linguist. I, for one, look forward to things to come.

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DISCUSSION

SESSION 2

Kenneth L. Pike, University of Michigan: A comment on the paper of Fr. Dinneen. I was very pleased with the summary of the material. I would merely like to add, at one point when he was talking about needles, both a practical point of interest, and a theoretical question at the end.

Some years ago, talking with one of the phoneticians from Edinburgh, this particular professor was saying that you did have syllables which were necessary to the theory but that you needed nothing larger than the syllable--so that you could take things like I'm going to go tomorrow and handle it all in terms of syllables; stress groups were unnecessary from his physiological point of view. He, with a physiological crew, had been getting his results by taking a hypodermic needle, sticking an electrode down through it, pushing it into a muscle, then having people talk. The electric firing into the needle then showed the action potential. Since in my experience the syllable was radically insufficient to handle my data, I wanted to know why it was that his experimental data seemed to deny my experimental data where syllable, though necessary, was obviously not sufficient.

In order to test this, to see why there was this unexpected gap, I asked him if he used the following kind of data which I noticed first in South America. There, with the Aguarunas or some other groups in the northern deep jungle, if you're having a greeting ceremony, you might say Good morning, how are you? Hope you're fine. If anything happened to you while I was gone, don't blame it on me. But they would shorten this and say it very rapidly like this (with extremely short stress groups, heavily accented, sharply decrescendo) >Good morning, how are you? Hope you're fine. If anything>happened to you while I was>gone, don't blame it on>me. I said that it is inconceivable that this kind of material could be handled just by the syllable. There are other muscles than those referred to; put your hand on my belly and feel them, they're tough. Well, he did that and he felt the muscles, but he hadn't dealt with these data. Furthermore, the historical

implications of how change could occur physiologically if something like this were developing seemed to me to be obviously beyond either a phoneme or a syllable. So I said to him, 'Did you feed anything like this into your data or did you only study ordinary sentences which were just drably uttered?'

To which he replied, 'Well, no professor is allowed to experiment on anybody until he experiments on himself'. So, the physiology professor that he dealt with had pushed the needle into himself in the presence of the linguist who then said: 'Pardon me, professor.' The linguist walked out and fainted in the aisle. But then I said: 'Why don't you use this kind of data which is obviously contradictory to your conclusions?' He said, 'You don't realize how difficult it is to get informants'!

So the question. What about the possibility that since then (this was several years ago) the British scholars are coming to the conclusion, (which to me is obvious, important, necessary, inevitable) that not merely a phone or phoneme, not merely a syllable, but something beyond this must also be part of all respectable, responsible, useful linguistic theory?

Francis P. Dinneen, S.J., Georgetown University: I don't know that they're doing any experimental work on that at Edinburgh, but there is considerable work done by Crystal at Reading with acoustic means on intonation patterns. He's applying this in cooperation with the interests of the speech therapists because he thinks that if he can show that intonation patterns develop in a child's language before he masters the grammar and lexicon, it may be a way of allowing a diagnosis of deafness at an earlier stage than they can presently.

Pike: I have a question for each of the other panelists. Of Sydney Lamb I would like to inquire: I mentioned, (not as part of my written paper but ad-libbed into the manuscript) the fact that I thought that although we had many areas of deep agreement, that I thought there was one probable area of sharp disagreement, namely, that I had put attention on units, but I thought that he did not--or didn't have them in his system; and yet when he spoke, I kept hearing about units, so that it's quite possible I was wrong. I would like to be corrected and to have it corrected on the record, if I was wrong. Sydney, what about the theoretical status of unit? Would you please inform me.

Sydney M. Lamb, Yale University: You're right in saying that we have a disagreement on this point. To me the linguistic structure is a network of relationships and just relationships. All of the information that needs to be accounted for can be accounted for in this way, so that the system does not consist of items connected to other items by

various relationships, but relationships connected to other relationships and so on throughout the entire network. I discussed this point in 'Linguistic and Cognitive Networks' (1970). Now, I said today in my paper that although, for example, a morpheme is nothing but a position in a network of relationships, the English language forces us to speak of it as if it were an object. It would be terribly difficult in speaking English to always refer to it as a position in a network. I try to do so when I can, but in talking in ordinary English, I speak about 'units' and 'elements' and so on. But what I mean by 'unit' is a position in a network of relationships.

Pike: I'd like to ask a question of Prof. Winter. If I followed him, he was rejecting cultural elements in historical matters, rejoicing that we could now just talk about language. That was what I heard. It may not have been what he said. However, a bit later, when he was talking about changes, he was talking about behavior, as it were, not language at all--if we take language as an entity abstracted from culture and behavior, why is a particular sound changed? So, I need to be informed here as to how he avoids what to me came through as clash. One, that at the beginning I thought he was saying that we ignore culture, within which I would include behavior; second, a bit later he was including behavior, within which I would include change, and I can't put these views together. Would you help me, please?

Werner Winter, University of Kiel: Well, of course the first point had very much to do with the history of the field where in the 19th century one of the principal preoccupations was with the discovery, say, of features of material and spiritual culture of the Proto-Indoeuropeans. What I wanted to make clear at this point is that we have become a great deal more skeptical about the feasibility of this approach--not in principle but in practice, because we are much less certain about the levels in time and in development of the languages to which we can assign certain vocabulary items. I have, say, levels A, B, C, D and I have no possibility to decide whether a particular lexical item or particular set of lexical items belongs to level D and only level D, or to level C and D, but not A and B. I cannot make an attempt to reconstruct the cultural universe to which level B or C or D belonged. Of course what was done in the Wörter-und-Sachen approach and the like was essentially to use the information in the lexicon to say that, if people had a word for something they had to be familiar with what they designated with this word. I think we have become more skeptical. I tried to point out the uncertainty about the rules and regularities in semantic change, so that we are in a way confident about what meaning to project back only if the word means roughly the same thing in all the languages compared. Now if in addition the formal properties of an

item do not permit a safe assignment and thereby correlation with other lexical items at a specific given level, we cannot be so certain as our predecessors were about the cultural properties of the people or the cultural knowledge of the people speaking a language at a given level. This is what I mean by withdrawing from an emphasis on reconstruction of culture in this sense. I wouldn't be denying its importance. I'm all for it but I think we have become more aware of the innate difficulties there.

Lamb: I have not so much a question as a comment on Prof. Pike's paper. As he was talking, I got the impression that perhaps he could be characterized as the Norman Thomas of linguistics. In politics, Norman Thomas used to propose ideas, and all the other politicians said, 'Oh, this is horrible, this is awful', and then ten or fifteen years later, they started proposing the same things--not giving credit to Norman Thomas but proposing them as something new. Well, ten or fifteen years later the Democrats start proposing them and the Republicans say, 'Oh, this is terrible', and then ten or fifteen years after that the Republicans start proposing them.

Emmon Bach, University of Texas at Austin: With the permission of the chairman and the panel, I'd like to make one general comment and then three briefer comments on each of three of the papers and I think I'd like to do it all at once and then let the people involved respond as they wish after I've made my comments altogether.

First of all, several people have spoken about the fact that there can be a number of aims pursued in linguistics. I would like to say about this that it's all very nice to be tolerant and to say that we all love each other, and so on, and I'd like to say that I love everybody, too, but it seems to me when we're talking about ideas and ideas about general linguistics, that we can't do this in the same spirit that we might use in approaching other areas of our life, that we should in fact admit that there is really only one aim of general linguistic theory and that is to understand how language works. Insofar as our theories conflict with each other, we have to admit that we must choose between them.

Now about Prof. Pike's paper. I hope I don't sound impertinent if I say a few things which may sound rather sharp. I have a great deal of respect for Prof. Pike. I agree with Sydney that he has said many things in the past which we have only caught up with years later. What disturbed me particularly about the parts of the paper today was the following kind of consideration. Suppose we admit that we can describe various aspects of language and other behavior as instantiations or interpretations of some formal models, say group theory or something else. At that point I would have to ask myself, should we do this? In other words, it's not enough to show that we can use some model and

organize our knowledge about some facet of human behavior in line with this model, but we have to then ask the question should we?

To Prof. Lamb's paper on the crooked path of gnostic linguistics, I would like to make this comment: It seems to me that when you began your remarks you started out by making the assumption that you had some direct knowledge about cognitive reality and you said that you rejected various theories because from some known properties of cognitive reality we could say that such-and-such a theory was simply false. Well, to that kind of argument it seems to me that I could just as easily say I also have direct knowledge of cognitive reality and you're wrong.

On Prof. Winter's paper, finally, there are a couple of more specific things which I would like to say. First of all, I think the discussion about order in generative phonology could be clarified perhaps by distinguishing several different situations. Only one or perhaps two of them came up in the discussion. First, there's a general question whether two different related languages or two different dialects can differ by having two rules in opposite orders. There seems to be a fair amount of evidence that this is the case beginning with the review of Keyser on the Kurath volume in Language, and so on. The second question is: Can it happen in the history of a single language that a reordering of rules takes place? On this, one of your examples, the one from King, I think bears directly. I just want to say on this point and on the next one, namely, can rules be inserted out of historical order, many linguists, among them generative phonologists, have been questioning, in the way that you have, the particular examples that have been given and, I think, come to the realization that the cases for insertion 'out of order' are relatively few and far between and relatively weak. We're looking at present for a stricter way of predicting when something like that could take place, or perhaps ruling it out altogether. So in that sense King in particular is especially working on this idea of inserting rules out of chronological order. Another person at Texas, a student named Mary Clayton, has been working on the idea that there are fixed points at which insertion can take place and trying to give a richer picture of what a phonological system is like.

Finally, a comment on the very true observation that there's been practically no work done on the history of syntax as compared to studies of synchronic syntax. I would say there that the main problem is that we are simply so ignorant of the universal syntactic characteristics of languages that we can't get to first base in doing any kind of comparative work. By way of illustration, consider a comparative linguist who offered to the linguistic public the proposition that language A and language B were related because they both had a rule of nasal assimilation before stops. Well, any linguist would know that this is not really good evidence because languages simply tend to have rules of

nasal assimilation before stops. That's a universal fact about phonological systems. In syntax, we don't have the foggiest notion at this point about what, in the way of similarities among languages, we could attribute to universal characteristics of language as opposed to those things which would be explained by common origin or something like that.

Pike: First a comment on the general comment which--if I heard correctly--was 'There's only one aim for linguistics: that is how language works'. This, of course, I'm in violent disagreement with. This is a matter of belief--not of linguistics, or something like that. I would have a radically different belief which drives my behavior; namely I'm interested in truth about man, about how language is related to man, about how language is related to behavior. I wouldn't ever grant that I'm interested only in languages. I'm not.

Now, specifically, the question astonished me so, I'm not quite sure that I got it. If I rephrase it (which may not be his question, but it's the best I could do), it appears as 'Why bother using mathematics?' which I will interpret as saying 'Why bother with any formalism whatever?' I was a little bit startled after his paper from last night about mathematics so I will take it as if I heard him correctly, although I probably haven't. Now, however, I will answer the question 'Why use mathematics?' or more generally, 'Why use formalism?' (which is so closely related to mathematical logic and math that I assume for my purposes that it is equivalent).

Now I am interested in formalisms--and specifically standard mathematics as a formalism. First, I'm interested in standard mathematics, namely group theory, because I'm less likely to be arbitrarily ad hoc building something to fit some arbitrary element here and there and let it distort the facts. I'm preferring at the moment to use standard available math for which enormously important theorems are available, put that against my data and distort the data as necessary to look at it through the model--but at least I will have known it's distorted by the model.

Now I come to that. Why should we ever use models in science? To which I reply, 'The world is awfully big. Language and behavior are awfully huge. We cannot study it all at once or write it down. If the model is as complicated as the whole, we've done nothing. We must abstract while keeping something concrete in tagmemics--which is a neat trick if you can pull it off. But in the abstraction of the formalism, in my view, there's inevitable distortion. It's not a question of choosing one model which does distort and another one which does not distort. All of them distort. It's a question of which distortion you accept to what degree.'

In my view, therefore, a theory or formalism is a window. A theory is a window through which I can look in one direction only. If I wish to see other data (like the phoneme for example), I have to use a different theory from some now available. If I want to see other things I have to go to other theoreticians who are looking out of different windows and force me to see things which I don't see in looking in my particular direction. So, a model is a window. Formalism abstracts from total reality.

But I would like to know if I am consistent in my formalism. But curiously enough one of the mathematicians--Goedel specifically--has proved (for the purpose of the mathematicians, and I assume he's right) that no formal system as complicated as ordinary arithmetic is ever provable consistent by working within itself. It can be proved consistent if and only if something is taken from outside, beyond the system, by which to evaluate the system. Now if we have a formalism of any kind, of any theory whatsoever, if Goedel is right, it is forever, always, inevitably, without any exception, unprovable consistent (even if it's consistent) without using something from outside the formalism.

What is that something for linguistics? Behavior, culture, action, life--and hence I am interested in meaning, which is the set of sloppy, unorganized problems in competences and performances and views of that behavior before it is brought into the formalism. So in tagmemics we refuse to operate without reference to two things at once. One, some formalism. Two, some component outside the formalism, behavioral, which justifies the formalism by making it relevant, meaningful, etc. So, we have to have a form-meaning composite which is one of the inner basic assumptions of tagmemics.

Lamb: I think I'd like to comment first on the specific point. Prof. Bach asked if I was claiming direct knowledge of cognitive reality. This is not my claim. As I said in my paper, the requirement which a theory must satisfy, to be called cognitive linguistics, is that it take account of the basic fact about human beings who know a language. That basic fact is that they are able to talk. In other words, if there is anything at all that we know about human beings who speak a language, it is that they are able to speak, and they are able to understand other people who speak to them. Therefore, the information system which they have in their mind has to be usable for speaking and understanding. So this is the requirement. I'm not making a direct claim about cognitive reality. What I am saying is that stratificational grammar is trying to deal with that observation. In other words, we want to have a model of the language that can be used for both encoding and decoding. And as far as I know, so far stratificational theory is the only one that has been concerned with those processes.

Now, on the general point, that is about whether we should have just one aim or whether we have different aims. Here I am in emphatic agreement with Prof. Pike and in emphatic disagreement with Prof. Bach. I think it would be a terribly dull world if we all were pursuing the same aim. I'm very glad that we are not. I'm not even quite sure what it means--the aim that Prof. Bach put forth--to understand how language works. I certainly wouldn't identify this as my aim. My aim is to understand the human information system; that is, the information system of the human mind. I study language because it seems to be the best thing to study to enable us to get at the information system. But just as Prof. Pike likes to relate his linguistic inquiry to non-linguistic data, so I am interested in the mental information system generally. I like to find out things about it by studying language and by extending my formulations to see what other things besides language can then be accounted for.

Winter: I would like to answer in the order of Prof. Bach's comments. There is one difficulty I have about your first two comments. You say on the one hand it seems to be pretty much accepted that you do have rule reordering observable between two related dialects or two related languages and at the same time you say that the evidence now seems to be against the assumption of rule insertion in the history of a single language. Did I catch that correctly?

Bach: There can be differences between related languages or dialects in rule ordering, but it is questionable that rules can be inserted in grammars 'out of order'.

Winter: But did I misinterpret your comment to that effect? No. You see, if this interpretation of facts is right, I find it extremely difficult to explain how rule reordering, reversal of order, could come about anywhere if in the history of an individual language it did not happen. What should have triggered it--you see, maybe I misunderstood you. But I mean I think if reordering of rules is being ruled out for the history of individual languages, it will follow that it also has to be ruled out for the diachronic treatment of languages that split into dialects, and so on.

As far as the other point is concerned, the explanation given for the absence of syntactic studies, I would like to answer with a question. Couldn't it be that generative grammar proves sterile for comparative linguistic purposes because of its preoccupation with deep structures rather than with surface structures? Let me take your own contribution yesterday. I think I would probably contest in virtually every point your claim that German is a 'VX' language; I think I'm perfectly willing to demonstrate that that is wrong. But, even if this were not so, your

reconstruction of German as a 'VX' language would be, I would say, 100% useless for a comparative evaluation of German data for broader comparative purposes. I tried to emphasize in my paper that we need the meaning and the overt surface form to work comparatively, whereas generative grammar moves away toward, well, shall we say logical structures behind it. It is not the underlying semantic logical structures which can be compared, but the overt form and the meaning attached to this form which can enter into a comparative argument; maybe it is the degree to which overt form is disregarded by generative grammar which makes this type of grammar not particularly helpful at this point. I wonder whether this situation can be changed or whether it is an intrinsic property of the generative approach that makes it, as far as syntax is concerned, largely unhelpful for comparative purposes.

John Francis, Center for Applied Linguistics: I'd like to address my comments and questions to Prof. Winter's presentation, taking into account also the last comment made. It seems that one can look at the question of the potential contribution of generative grammar to the task of comparative linguistics in a somewhat different light. To the extent that generative grammar purports to provide a comprehensive synchronic description of a language, I see no alternative to the extent that those descriptions are appropriate--from the point of view of comparative linguistics to provide a characterization of, and a rationale for, the linguistic change framed by those grammars.

Let that be a general preface to four comments that I would like to make with respect to your presentation.

First, it seems to me not the case in describing fathers and sons and parents and children that in order for linguistic change to occur, the parents must adopt the speech of the children as you suggest they must. Jakobson, never afraid of the obvious, said that while parents and children must understand each other, linguistic change occurs in the direction of the allegro code which characterizes frequently the speech of the younger generation. That is, parents and children understand each other but they understand each other across differences which are coterritorial, idiosyncratic, personal and dialectal; and suggested that the evolution was in the direction of the allegro code.

Secondly, a rather powerful argument has been made that linguistic change is related to what Kurylowicz calls the generalization of the redundant morph in isofunctional complex morphemes. He has adduced considerable data to that effect. His theory has aroused controversy which I don't intend to enter here. Since Kurylowicz has addressed some of the facts which you discussed--Lachmann's law--perhaps you would comment on his explanation.

Thirdly, generative grammar provides a framework for discussing linguistic change beyond the rule changes in the change of domain of the application of a particular rule. This has been discussed recently by Henning Anderson in his treatment of the Ukrainian preverbs.

Finally, I think it would be our task as comparative linguists to consider for example why in English--if Chomsky's description of the English accentual system is reasonably adequate--a Latin stress has replaced a Germanic stress rule.

Winter: The question which disturbs me about the children's codes being retained is that whenever you observe children, they seem to be extremely conformist about linguistic behavior. Yes, this is indeed true! I mean you should see my son who is extremely independent-minded in non-linguistic behavior. He will accept every instruction as far as language is concerned from wherever it comes. So I think the chances for survival of a private code of a child are infinitesimally small. I would say, and this I tried to express in my paper, if a child finds a more economical way of arriving at the same output by his rule, then the simpler rules have a perfectly good chance of surviving because the rules would be below the surface and they would not show. However he arrived at the right form would not matter. But if he doesn't arrive at the right form, the simplified rule and its output will soon be eliminated; just see how long a child will retain a simplified term he goed; it will go down the drain very rapidly. I think this is the fact we have to cope with and this is the reason why I think we cannot be at liberty to use an alleged short-cut system of the child as a wholesale explanation of linguistic change. To be sure, I do not have a good alternative to offer. The hypothesis proposed is beautiful, but I think its claim just runs counter to experience.

This applies also to the third point that the changes introduced by a child will affect a different domain from the domain of the rule as used by his elders. I think the goed example again would be a case in question. The overextension of the domain of a rule by a child does not survive community pressure as far as I can see, or at least the chances are so small that one should not, I think, build a theory entirely on it.

ANTHROPOLOGICAL LINGUISTICS: RECENT RESEARCH AND IMMEDIATE PROSPECTS

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If it becomes desirable to formulate any cognition as science, it will be necessary first to determine accurately those peculiar features which no other science has in common with it.

Immanuel Kant

Introduction.¹ Anthropological linguistics is an integral component of cultural anthropology. Cultural anthropologists study cultures--those systematic networks of categories, attitudes, predispositions, and rules which the members of a particular society differentially share, contribute to, and transmit (particularly by means of language). The scientific concept of culture was and continues to be largely the creation and tool of cultural anthropologists, most of them also linguists or at least with a lively interest in linguistic models.² Most anthropological linguists and cultural anthropologists see language as a part of culture or as a corresponding system linked by thousands of underlying similarities in structure and points of content.

Anthropological linguists are personally excited about the intellectual systems of primitive and peasant peoples--particularly, of course, their languages. Because of this they remain typically committed to field work and the analysis of their field experiences in relation to whatever abstract models and theories will lend understanding. Anthropological linguists study fine-grained phonetics, morpholexical structures, contextual semantics and other problems within analytical frameworks that range from 'raw descriptivism', to semantically oriented structuralism, to 'words-and-things' palaeolinguistics, to impure

transformationalism, to humanistic studies of lexical symbolism. Anthropological linguistics, like anthropology in general, occupies an extreme position in tolerating or encouraging diversity, eclecticism, and intellectual pluralism; scientific fields, like cultures, differ in their value systems. This theoretical relativism has an interesting analogue in the principle of relativity in physics, and the humanistic position that every analytical model is to some extent a point of view, a metaphor of reality. Theoretical relativism is congruous with the interest in linguistically and culturally diverse gestalts, with what might be called 'the problem of the linguistic and cultural parallax.'³

A Brief Overview

With the above observations in mind, let us briefly consider some of the main developments and activities within anthropological linguistics--along with some passing and tentative guesses about the future.

The past decade or so has witnessed substantial developments in a number of areas. One group of scholars has continued historical-comparative work on the genetic groupings of language families, leading to the establishment of new groups, or to areal syntheses, as by Mary Haas and Carl Voegelin, or to the finer differentiation of existing entities such as Mayan, Athapascan, and Proto-Lolo-Burmese.⁴ The emphasis has been on phonology and, to a lesser extent, morphology. Such analyses overlap with several attempts to utilize linguistic and philological methods to reconstruct the social or ecological components of prehistoric semantics; Frank Siebert, for example, reconstructed the terms for fifty biota in Proto-Algonkian and then correlated them with the palaeobiological evidence in order to establish this section of the proto-semantics, and to shed new light on the question of the Algonkian homeland. A second set of studies has explored linguistic universals, with particular reference to marking and probabilistic syntactic constraints in the case of Joseph Greenberg, and to substantive semantic universals in the work of Brent Berlin, Harold Conklin, and Oswald Werner on genus, species, and other taxonomic concepts.

Much research has been 'sociolinguistic' or 'psycholinguistic'--or perhaps 'sociopsycholinguistic'--in some generic sense. One type has focused on empirical aspects of speech events and speech patterns. The speech event (or communicative act), including associated body motions, can be subjected to minute phonetic and cinematographic 'interview analysis', often in collaboration with psychologists. Alternatively, diverse modes and styles of speaking can be described in relation to speech situations and social contexts within the framework of a theory of communication; the 1970's should witness the completion of definitive empirical studies, by Joel Scherzer and others, to the area

which Dell Hymes has felicitously dubbed 'the ethnography of speaking'. A large body of related research has probed bilingualism and dialectal differences as they covary with dimensions of class, caste, and ethnicity--with much of the theoretical stimulation coming from William Bright, John Gumperz, William Labov, and Uriel Weinreich. A number of younger scholars such as Blount and Sanches have been carrying out field studies of the processes of language acquisition--inspired at times by Noam Chomsky, at times by Piaget, and at times by the data.

Many anthropological linguists have concentrated on internal or 'straight' linguistics, and it is often difficult (and sometimes invidious) to draw a sharp line between anthropological linguists with a major interest in phonology or syntax and, on the other hand, the linguists with a major interest in the language of a primitive or peasant society (and the necessarily associated questions of field work and the sociocultural system). Among these more purely linguistic studies it is morpholexical systems, to use Bloomfield's apt term (1939), that have continued to seem formally and semantically interesting--particularly in the many formulae for the production of long words. Outstanding morphologies were produced by M. Barker, Karl Teeter, and several others. There have been many attempts and some successes at handling phonology and syntax within various frameworks, including tagmemic and generative ones; in fact, the total bibliography or evaluation of such contributions is quite beyond the scope of this review. The coming decade will see more basic work on morphophonology ('deep phonology'), and on major problem areas in syntax such as pronominalization, subordination, conjoining, and embedding. The semantic distinctiveness of Amerindian and other 'anthropological' languages will stimulate relatively explicit treatments of underlying semantic categories, with fruitful collaboration between syntacticians such as Charles Fillmore and the culture-oriented semantics of the many anthropological linguists who have been investigating categories of animateness, alienability, and the like. One may expect coordinated studies of the categories which Roman Jakobson has called 'shifters'--categories such as person, mood, tense, and 'evidential', which combine purely symbolic syntactic functions with indexical ones relating outwards to the contexts specific to a given system of language and culture.⁵

At many points in the series of research problems outlined above, the linguistic anthropologist has played (and will continue to play) a role vis-à-vis general or abstract theory more or less analogous to the role of many cultural anthropologists vis-à-vis other social sciences. Just as the latter often broadens or enriches models from sociology or psychology by checking their fit with his field experience in typologically diverse cultures, so the linguistic anthropologist relates his field experience with typologically diverse languages to problems raised by culture history, structural and generative grammar, cultural anthropology,

and even social psychology. In this specific methodological sense, the institutional and intellectual role of anthropological linguistics remains characteristically pragmatic.

The Lexical Symbol

Having superficially reviewed the long and sometimes thin front line of recent research, let us now make a ninety-degree turn and consider in greater depth one question to which scholars of varying motivation and orientation have contributed various components. I might well, incidentally, have focused on what could perhaps be called 'anthropological sociolinguistics',⁶ or also on the many interesting patterns of morphological analysis as actually practiced by expert morphologists such as Stanley Newman, Wallace Chafe, John Fought, or Ives Goddard--rather than as exemplified and programmed in general or introductory texts. I have chosen, however, to focus on a different aspect, one that brings together research by diverse linguistic and cultural anthropologists: the semantic description of lexical symbols, or, to put it more plainly, how lexical symbols and systems work and what they mean. Since I have used one idea--that of the lexical symbol--as an idea for integrating research of several kinds, this second section of the article will appear to the reader at once more abstract and also more personal.⁷

First, some explanation of 'lexical symbol'. The primary concern has been with what Peirce called the 'symbolic sign'--a conventional association between the signifying aspect of the sign and its semantic object. Relatively little attention has been paid to what Peirce called 'iconic and indexical signs' (the most notable exceptions being some studies of the usage of second person pronouns and kinship terms of address, and Watt's original analysis of cattle brands).⁸ In the second place, the studies to be discussed below mainly involve morphemes and words, both of which are 'products' of the morphology and the dictionary; in this sense the signs in question have been 'lexical'. Third, the research has generally assumed that the sign has three parts or 'corners': (1) the signifying form, such as a morph, (2) the semantic objects or denotational meanings, in their various contexts, and (3) the underlying interpretive system of semantic features and rules as they might be known by someone with a native intuition, or as they would seem to be shared by the members of the speech community. To simplify matters, I will use the term 'lexical symbol' for what has now been defined as the 'symbolic, lexical, triadic sign'. The specific studies in question have raised many new questions about subdividing or refining this lexical symbol, leading to an adaptation but not a replacement of Peirce's general model.⁹

Anthropological linguists have usually treated lexical symbols as inherent in the semantic code, and not as reducible to or replaceable

by systems from logic, psychology, or cultural studies--although, for the reason already stated, there is great interest in such outward relations, particularly to that of cultural symbolism. In structuralist terms, anthropological linguists have been largely concerned with paradigmatic, substitution relations between lexical symbols and between subsystems of lexical symbols and of cultural symbols.

Let us consider some of the contributions to our understanding of the lexical symbol.

One fundamental point within the relativistic theory of anthropological linguistics has been that the phonology and grammar of so-called 'primitive languages' are formally as complex and productive as, let us say, English or Latin; this has been validated by sophisticated analyses of Eskimo, Kwakiutl, Yokuts, and other languages. During the past decade a similarly relativistic position about the lexicon has motivated many anthropological grammarians and semanticists. While some of their dictionaries have been short and rudimentary, and mainly of value for comparative work, others have probed semantic complexity to new depths. Perhaps the outstanding overall work has been Robert Laughlin's monumental dictionary of Tzotzil, based on years of field work and practically native intuitions; the approximately 36,000 lexeme entries, from botanical entries to emotion-laden idioms used in religious contexts, are defined at unorthodox levels of sensitivity and accuracy. Laughlin's dictionary confirms empirically that speakers of Tzotzil have a lexicon as richly chambered and finely grained as that of English speakers. The work of such lexicographers leads to a thorough analysis of the roots and words current in the language, which is the necessary basis for an adequate statement of the categories and rules in the grammar. Laughlin's lexicography, since it contains an enormous amount of systematically coded information on cooccurrence restrictions and semantic categories, can be ignored by the syntactician of Tzotzil only at his intellectual peril. Finally, such description can lead to a theory of the lexicon that is pragmatic in being based on the lexical complexities of the human condition rather than on a logician's 'make-believes'. These lexical complexities reflect the system of a priori categories and relations in a given language, which is related, in turn, to the question of the epistemology of such categories and relations in human language generally.¹⁰

Unlike some other kinds of linguists, anthropological linguists are keenly interested in denotation (or reference), and often develop a theoretical position on the topic. This is in part a corollary to their interest in speech as behavior and in the associated context of the socio-cultural system. What is the status of denotational meaning in these theories and practices?¹¹

Some anthropological linguists have been arguing that the denotational meanings in any particular system are a subselection of some larger set

that is universal in a comparative and typological if not psychological sense; semantic theory then describes the underlying primitives and rules that 'account for' and 'generate' the denotata. At the other extreme it is argued that only the perceptual and cognitive categories of a specific system enter into the denotational meaning, and that it is, strictly speaking, impossible for the underlying semantic system to classify and generate a subset from a universal grid.

A second, vigorously contested question concerns the relation of the denotata to some external reality, be it social or biological. One group of scholars has argued that such questions of external reference are irrelevant because many semantic subsystems, such as those involving the metaphysical or syntagorematic terms, lack denotata that can be pointed at--except, of course, where the syntagorematic terms are used indexically. Semantics, as seen by these men, is essentially concerned with necessary, *a priori* relations between an underlying significational system, and overt lexical forms with their perceptual and cognitive denotata. Opposing this position, a second group has been operating with a relativistic theory--relativistic in the sense that the relation between the sound image and the semantic image is considered to be arbitrary because of the inherent contingency relation of the latter to some external reality: this is the familiar tale of how snow and camels are classified in Eskimo and Arabic. Despite a certain number of inadequacies or contradictions, the various positions along the above dimensions--and various combinations of such positions--have motivated empirical analyses of general theoretical interest. Beneath the diversity of opinion, almost all would agree that the system which governs lexical symbols is to a large and significant degree latent or covert, and specific to a given language. Moreover, there does seem to be a real value in recognizing the dichotomy, on which Bertrand Russell insisted, between denotational fields not susceptible of ostensible definition, and other materially objective fields of reference. In the case of these latter fields, the most fruitful assumption is of two layers or stages: the immediate denotation is purely perceptual or cognitive, and it mediates between the underlying semantic features, and, on the other hand, the denotata of the external, material world. Such an assumption of what might be called 'ecological or material semantics' is necessary if we are to deal adequately with referential and indexical meanings when formulating hypotheses about semantic process and change.¹²

At least as interesting as their relation to external reality, is the question of the internal organization of these sets of denotata or referential points--of what many philosophers call 'extension'. In one breakthrough analysis of the color terms in 98 languages, Berlin and Kay demonstrated that, despite great individual and cross-linguistic

variation in the marginal denotation and in overall fields of denotation, the primary denotation, or what they call the 'focus' of a term will always fall in the same spot on the spectrum: the 'truest blue' and Russian sámyj sinij approximately coincide on a chromatic grid. These objective foci and the primary terms themselves, moreover, were found to occur in a universally valid order: if a language has only two terms, they will denote white and black, the third will be red, and so on through the seventh color, brown--human perception thus imposing a discontinuity on the physical continuum.¹³ In a somewhat related study, Floyd Lounsbury selected a large sample of Crow-Omaha kinship terminologies from the truly enormous literature on the subject, and showed that the great and formally innumerable set of kin type denotata could be reduced to or generated from a small underlying set of primary kin types by the cyclical application of just three ordered rules.¹⁴ (The Crow-Omaha terminological type, incidentally, while perhaps uninteresting to someone embedded in Modern English, is realized in Old English and in Proto-Indo-European, and, in fact, in a large proportion of the world's semantic systems, generally at a deep semantic level and in significant articulation with the associated cultural system.) Both of these pioneer studies contain premises and conclusions that I am neither explaining nor pretending to defend, but both also explore in an interesting and partly conclusive way the differentiation between primary and nonprimary denotata: the denotata of a lexical symbol are not on a par or coordinate but, on the contrary, are arranged and ordered. This primacy of certain denotational subsets may be a matter of selection from universal sets of psychologically diverse status, or it may be internally motivated in terms of a given semantic system. Improved understanding of the relations between analytically ranked denotational meanings and associated cultural variables should facilitate more revealing studies of the motivation of semantic change--analogous to the correlation of phonetic and societal variables in some contemporary sociolinguistics.

The problems of referential or denotational structure are paralleled by the equally interesting one of the internal organization and differential status of the underlying features in signification, which resemble what some philosophers call 'intension'. Often the anthropological studies in question have been motivated by the indeterminacy of featural analysis, which is as salient in lexicology as in phonology. In some cases, one solution has seemed more economical in terms of semantic space, featural complexity, and similar criteria of parsimony. In others, one featural analysis has seemed to correspond more closely to what was proposed, on other grounds, as 'the native model', and scholars have sought to elaborate a semantic theory which would 'make explicit' the native intuitions.¹⁵ In a third category of solutions it was shown that a given set of semantic features corresponded closely

to cultural ones--a residential feature of patrilocal household membership, for example, emerging as preferable to a purely genealogical one. Finally, a number of interesting psychological tests were constructed in order to discover the differential cognitive reality of features and feature systems. One conclusion which has emerged from these and other attempts to validate analyses is that signification features are internally differentiated, whether the criterion be logical, psychological, or somehow cultural. Clearly; some of the same criteria for disambiguation could be used in morphology and syntax: one wonders, for example, about the internal subdifferentiation of verbal categories such as animate and evidential. The internal differentiation of signification (or intensional) features is an interesting analogue to the internal differentiation of denotational or referential 'points'; the primary analytical concern within anthropological semantics is not so much with the contrast between denotation and signification as with exploring more refined and subtle contrasts and complementarities within these general categories.

Some of the most interesting studies have explored an aspect of the lexical symbol that might be called stadal or processual: symbols always develop from others and are always developable into others. An essential aspect of the description of the meaning of such stock examples as 'bachelor' and 'father' would include their successive stages as categorized within the semantic system. This has been partly explicated by Charles Frake in his paper on disease terminology. In a similar study of soma in the Sanskritic texts of the Rig Veda, Wason demonstrated that the seeming contradictions in the epithets can be resolved if the physical (i. e. external ecological) denotata are assumed to be the successive stages in the natural growth of a certain species of hallucinogenic mushroom (the birch-dwelling Amanita muscaria) that is known to have been present prehistorically in part of the putative Indo-European homeland. The explanatory value, both structural and historical, of the processual or stadal assumption suggests that semantic theory should generally divest itself of a static view of the lexical symbol.¹⁶

A similar focus in research has been on the contextual aspect of lexical symbols. The cognitive anthropologist and linguist, Stephen Tyler, has criticized the contextless and purely genealogical orientation of many kinship studies and, in an exemplary analysis, worked out the covariation of Koya kinship terms with culturally specific social contexts such as 'extended patrilateral household' and 'wedding' that, in this case, are manageably finite and susceptible of rigorous and relatively simple definitions. He concludes with an elegant system of markers for such contextual or stipulative meanings. In a theoretically adequate model for describing sentences, such context markers would be added to the more familiar ones for syntactic features. In fact, Tyler concludes that 'context is part of semantics'.

The concomitant of contextual variation is polysemy; to put this more precisely, polysemy involves disjunctively defined features the selection of which covaries with culturally specific contexts of use. In a penetrating study that reveals his philological training, the symbolic anthropologist, Victor W. Turner, has investigated in detail the social, ecological, and religious denotation and secondary associations or, colloquially, 'connotations', of the three primary color lexemes in the Ndembu language: white, black, and red. They are closely associated in the explicit ethnosemantic system with meanings that are primary in a psychosexual sense. Turner concludes with a comparative and archeological survey of such associations, but maintains a balance between the role of universals, which are empirically motivated in this case, and, on the other hand, the polysemous associations specific to this particular system. On the one hand, the three primary colors are associated with concrete but universal images of semen and mother's milk (white), earth and feces (black), and, of course, blood in the case of red; on a more abstract but less universal plane, the three colors are associated with ideas of unitary (white), dead and impure (black), and, in the case of red, with a combination that ambivalently includes both fecundity and peril. On the other hand, the colors are associated with the concrete and specifically Ndembu values of the latex of the mudyi tree, the blackness of a liver, and the blood of circumcision. Turner's study, like that of Tyler, assumes that lexical symbols are inherently polysemous and that polysemy is the symbolic, language-internal facet of varying usage in varying contexts. Studies of this sort will enable us to advance beyond the truism that words are untranslatable nodes in incongruous networks, to the construction of explicit descriptions of the associational-contextual aspect of lexical symbols.

Much anthropological lexicology has dealt with the underlying arrangements and processes that govern lexical symbols; the main assumption is that overt forms are imperfect reflexes of underlying systems. Paradigmatic analysis has been concerned with dimensions that cross-cut each other and are 'simultaneously' united in nonlinear bundles to define lexical items. This is particularly well illustrated by grammatical categories and the so-called simple lexical systems of pronouns, and of kinship and color terms.¹⁷

Perhaps the most intense work has been on taxonomies, where the underlying relation is one of class inclusion: an initial taxon or 'unique beginner' includes one or more levels that succeed each other until the terminal taxa are reached. Of interest has been the repeated discovery that taxonomic depth depends on the semantic domain, on the semantic system as a whole, and even on cultural values and attitudes toward taxonomizing: the Navaho speak of themselves as 'great classifiers', just as certain tribes of Australian aborigines are notoriously explicit about genealogical relationships.

Taxonomic analysis can contribute directly to our understanding of logic, and its relation to language. In an analysis explicitly opposed to Russell and many linguists, Lévi-Strauss has argued that proper nouns are not only designations for unique individuals, but symbols for classes that can be defined by semantic features.¹⁸ In an original and laudably comprehensive taxonomic investigation, Hale and Casagrande found that their sample of 'about 800' Papago folk definitions could be classed exhaustively in terms of 13 'types of semantic relationships': attributive, contingency, function, spatial, operational, comparison, exemplification, class inclusion, synonymy, antonymy, provenience, grading, and circularity. They conclude with fundamental questions about the universality of these types, their relation to ethnographic definitions, to 'cognitive styles', to grammatical parts of speech, and to the types of definition in scientific logic. Werner, reanalyzing this inventory, has reduced it to four underlying definitional taxa, one of which is the taxonomic relation itself (class-inclusion).¹⁹ These and other studies by linguists and ethnosemanticists indicate that class-inclusion and thinking in terms of taxonomic schemes is an essential fact of the human condition. Lévi-Strauss has shown with irony the 'primitive mental' (e.g. taxonomic) character of Sartre's philosophical thoughts on history, and a similar point can be made about philosophically inclined linguists who have argued that taxonomies and their analysis are trivial and epiphenomenal.

Research on underlying arrangement, process, and definition indicates that the organization of features varies in an objective and discoverable manner from simple cross-cutting features; to relative products, as in the case of affinal kinship terms; to gradients, as in the case of color terms; to various kinds of taxonomic and transformational relations; to branching tree diagrams wherein features are ordered by the sequential contrast of one feature at a time. A large part of the vocabulary, particularly verbal elements and elements involving case-like relations of instrument, agent, and the like, have denotata of relational (also called 'sentential') form. The probability that formal variations are inherent in the lexicon is suggested by the bizarre and counter-intuitive results of dealing with the entire lexicon in terms of one analytical model. On the other hand, this same variation strongly suggests that such standard examples as 'father' and 'chair' are, in their meaning, both componential and relational-sentential: one signification of 'father' is indeed 'male, lineal, first ascending generation', but another is 'the man who impregnated the mother'.²⁰ In fact, all lexical symbols probably have a few or even many codefinitions and are ambiguous in this sense. Research during the coming decade promises the discovery of new structures and further insight into codefinitional ambiguity, and the into the multiplicity of intersecting structures and processes that underlie systems of lexical symbols.

As interesting as the form of lexical symbol systems is their substantive meaning. If one makes the structuralist assumption of an underlying system of oppositions, then lexical symbols are found to be organized in vast systems of correspondences which may classify different levels of social reality or, again, oppose realms of culture-related meanings--kinship statuses, totemic symbols, and so forth--to realms of material or natural phenomena, particularly biota. Among the Hopi, for example, words for colors, animals, birds, trees, bushes, flowers, and types of corn and bean, are cross-classified by their association with the cardinal directions, zenith and nadir. Such networks of semantic sets, in both primitive and technologically advanced societies, are related to each other by various transformations, including obvious ones such as the metaphor and other less obvious, such as the transformation of metaphor through metonymy. A crucial role in symbolic transformations appears to be played by the denotational and significational functions of semantic 'operators', but, despite masses of vivid exemplification in print, both the function of these mediating terms and the overall processes themselves are still poorly understood. Even in its imperfect state, the symbolic anthropology of lexical symbols is of linguistic relevance because, among other things, these semantic associations partly govern linguistic change--unless we adhere to the lexical atomism of the orthodox, second- and third-generation Neogrammarians. The primary relevance of such symbolic analysis of lexical systems, however, is probably not to phonology and grammar, logic, or syntax, but to personality psychology, literary criticism, political science, and other fields concerned with synthetic *a posteriori* judgments about the human condition. In fact, I have adduced this final type of contribution to our understanding of lexical symbols so that my present statement would phase out where anthropological linguistics itself fades into cultural, cognitive, and symbolic anthropology.

Conclusion

Let us now draw together some of these inter-connected conclusions, suggestions, and open-ended questions. First, the lexicons of primitive and peasant peoples appear to be as rich as had previously been alleged. Second, lexical systems involve diverse levels of reality and formally very diverse processes and structures. Third, lexical systems are inherently open, with constant increment and loss, often triggered by new contexts and new problems. Fourth, lexical symbols are inherently processual, often in the sense of containing stages in their meaning, and always in the sense of developing into and out of each other. Fifth, all lexical symbols are inherently polysemous; the polysemy covaries with other, associated symbols, and with the culturally specific categories of contexts which, although large, can often be

described rigorously. Sixth, the denotational meaning of symbols is internally differentiated; the primary denotata may belong to a universal grid, or be determined by semantic or cultural patterns specific to a given system. Seventh and last, significational features are also internally ranked and constitute culturally-oriented systems of differential semantic depth, with their own sequences of ordering, differential complexity, and levels of contrast and marking. Issues and questions such as these will continue to excite the curiosity of anthropological linguists during the coming decade.

NOTES

¹I am grateful to the following persons for their helpful criticisms and suggestions: Barbara Babcock, Robbins Burling, Margaret Hardin Friedrich, Stephen Tyler, Frank Wordick, and most particularly, to David Price.

²Cultural anthropologists with strong linguistic interests who have made contributions to the concept of culture include Franz Boas, Bronislaw Malinowski, Alfred Kroeber, Edward Sapir, Clyde Kluckhohn, and Cornelius Osgood, and more recently, Claude Lévi-Strauss, Victor Turner, Clifford Geertz, and David Schneider. A different tradition, with a different evaluation of behavior and material culture, includes Leslie White and Ralph Linton.

³This theoretical relativism is congruous with the 'five fields' model of general anthropology which crosses the boundaries between biological, psychological, social, cultural, and linguistic knowledge, and, temporally, between prehistory, history, and the structural present.

⁴Morphological theory and practice within anthropology is currently divided between American descriptivism, transformational-generativist, and a considerable number of eclectic models that include some of one or both of the two just named as well as components from the semantic structuralism of Jakobson, Benveniste, and others.

⁵I am indebted for this valuable idea to Michael Silverstein, whose research formulations are already at an advanced stage. The foregoing review could be considerably expanded to include, for example, the work of Charles Hockett on the evolution of language, and of Marshall Durbin on Mayan sound symbolism, and of Madeleine Mathiot on the relation between lexical and grammatical categories.

⁶'Anthropological sociolinguistics' differs from the usual sociolinguistics in a number of ways--for example, relatively greater interest in primitive and peasant society and in the cultural level.

⁷The present study is still primarily an attempt to give a theoretically integrated description of a number of studies by others; in the

near future I will spell out a relatively independent position on lexical symbolism.

⁸'Indices' or indexical signs involve an existential or physical relation to the object; the function of indices is to refer to some feature or object in the immediate environment of the interpretant (indexical symbolism, incidentally, overlaps with but must not be confused with 'ostensive definition' by pointing, and so forth). 'Iconic signs' directly 'represent exhibit, or exemplify' the structure--an onomatopoetic word, a statuette of the Virgin Mary, a diagram of a machine, or a mathematical formula (the latter directly representing a mathematical image). Peirce held that any general purpose language requires all three kinds of sign--iconic, indexical, and symbolic. Concomitantly, all three should be dealt with explicitly in a theory of natural language.

⁹The consensus (if partly implicit) is to include some idea of an interpretant, whether the intuition of an informant, an abstract but individual decision (e.g. Chomsky's 'competence'), or a sociocentric semantic system--or some combination of these three. For the above review I have not used the term 'designatum' (or 'designation'); my use of 'signification' or underlying meaning and 'feature' or 'component' corresponds roughly to the designatum and designata of Weinreich and many others (except that, like most anthropologists, I would assume an interpretant). The use of 'designatum' for the class or field of denotata strikes me as a superfluity. I have not used the well-known trichotomy of Charles Morris (from C. S. Peirce) of 'syntactic, semantic, and pragmatic', although for some purposes it might have been apt.

¹⁰These are ontological questions about the nature of things. While 'ontology' seems to be a word to eschew in anthropological and linguistic circles, it is also clear that much contemporary research is governed by premises that are ontological.

¹¹Until recently many 'straight linguists' have been amateur logicians (Chao, Bloomfield, Harris), whereas most of the anthropological linguists were indifferent to this field. A few, such as Hockett and Lounsbury, combined both interests, in a style that is spreading rapidly today, partly as a response to transformational-generative linguistics. The younger anthropological linguists with a major interest in both culture and logic include Paul Kay, Oswald Werner, Micheal Silverstein, Stephen Taylor, and many others.

¹²Because of space and the deadline I have had to limit myself here to an abstract summary; to fully document who stood for what would take an article. A good example of 'ecological semantics' comes from ancient Celtic religion, where the system of perceptual and conceptual denotata mediated between that of cultural symbolism and the botanical denotata of acorns, mistletoe, etc.

¹³I have certain specific quibbles about the Kay-Berlin hypothesis, but this is not the place to air them.

¹⁴Lounsbury's global rules have proven enormously fruitful in historical-comparative research; Wordick, for example, has posited an Omaha III for Proto-Indo-European and, by running through the transformations, generated a large number of fresh etymological questions, which lead to the discovery of new cognates.

¹⁵Cultivating linguistic sophistication in gifted informants and then discussing their grammar with them is of course an old tradition-- notably as exemplified by Sapir (I have spent scores of hours discussing with certain Tarascans in Tarascan the details of my analysis of the locative spatial suffixes). On the other hand, scholars who identify the 'real' system with the statements of their informants about distinctive features, whether of kinship or phonology, would do well to read David Schneider (1965).

¹⁶Quine has argued that the stadal components of signification are difficult or impossible to elicit in a simple stimulus-response situation.

¹⁷Much of the semantic analysis in anthropological linguistics has concerned so-called 'simple systems', doubtlessly skewing our knowledge toward 'componential' rather than 'relational' analysis. That the methodological trivium of pronouns, color, and kinship is not trivia, however, is suggested by the following: (1) many 'simple' systems are complex in many ways (e.g. Kariera kinship terminology); (2) it is always valid to begin the analysis of a large and complex phenomenon with the subsystems that are relatively simple; (3) definition by cross-cutting or paradigmatic features probably will always remain an important component in semantic theory.

¹⁸As Lévi-Strauss puts it (1966:215): "In so far as they derive from a paradigmatic set, proper names thus form the fringe of a general system of classification: they are both its extension and its limit.... The more or less 'proper' nature of names is not intrinsically determinable nor can it be discovered just by comparing them with other words in the language. It depends on the point at which a society declares its work of classifying to be complete.... This brings us to the root of the parallel mistakes committed by Peirce and Russell, the former in defining proper names as 'indices', and the latter in believing he had discovered the logical model of proper names in demonstrative pronouns.... each culture fixes its thresholds differently."

¹⁹Werner's other basic types are synonymy, attribution, and grading.

²⁰Contrariwise, 'chair' is not adequately defined by the relational statement, "A chair is a piece of furniture for one person to sit on" (Weinreich 1966:468-9), since this would cover a stool, an ottoman, etc., whereas it is adequately defined as a combination of taxonomy (a kind of furniture) and features (backed, elevated, flat top, etc.), and one relational feature of objectivity or instrumentality (for sitting on). On the other hand, I would adapt one point from Weinreich's brilliant paper of 1963 by treating the 'linking' of lexical items in

constructions and the so-called 'simultaneous concatenation' of features in one lexical item as two subcategories of simple featural definition (which contrasts with relational featural definition).

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CONTEMPORARY EMPHASES IN SOCIOLINGUISTICS

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During the past five years, there has been an increased interest in the relation of linguistic forms to social meaning. It is difficult to put an exact date on the formal establishment of the field of sociolinguistics, but the term has appeared increasingly in recent years in forms of courses, book titles, seminars, and even graduate programs.

It is in some ways curious that sociolinguistics should emerge as it has and when it has. The major interest of linguists in the early part of our century was in historical matters, the evidence of which was largely from literary texts. Within the last two decades, the greatest advances in knowledge about language have been made by linguists taking as primary data their ability to discriminate grammatical and ungrammatical sentences. With the exception of certain dialectologists and anthropologists, in fact, linguists have never really kept in very close contact with language as it is actually used. The separation of langue (language) and parole (speech) made by Ferdinand de Saussure has been well preserved by the generation which followed him although it still seems curious to some of us that langue, which Saussure thought of as the dimension of language shared by all its speakers, was considered so general that linguists could speculate about it from limited sources of speech (even their own) while parole, the individual dimension, was considered so variable that it would take large scale surveys to measure.

William Labov (n. d.:43) has recently observed that despite this historical separation of language and social meaning, however justified it may have been, there has been, in the past fifteen years, a "... noticeable movement away from the extreme asocial position in theoretical work towards a view of linguistic structure and evolution which includes the evidence of everyday speech outside of the university community." It is generally felt that this growing rapprochement of language form and social meaning has come about because of three motivating factors:

(1) the desire to find a sounder empirical base for linguistic theory, (2) the conviction that social factors influencing language use are a legitimate topic for linguistic investigation, (3) the response to the growing feeling that such sociolinguistic knowledge should be applied, if possible, to urgent educational problems.

Of these three motivations, the last has attracted the greatest amount of attention to date. However important basic grammatical research may be in the application of linguistic knowledge to education, certain assumptions of linguists and anthropologists seem to place them several steps ahead of educational psychologists who, for example, have put forth dangerous ideas about cultural and verbal deprivation of disadvantaged children. Even without research, linguists have found themselves in disagreement with the superficial analysis of a child's language that would permit a sentence such as He happy to be interpreted as logically deficient because it lacks the copula. Linguists have attempted to counter such analyses by citing other languages in which the present tense copula is optional and have given serious attention to the sociolinguistic situation involved in non-standard English.

Educators, as might have been expected, were among the earliest to assess the importance of a child's language. It is only natural that problems would be noticed first in the frustrations of teaching. As is often the case when there is a sudden national awakening to a social or pedagogical problem, the development of theory, materials, and the training of personnel relating to the general area of social dialects was dictated by expediency more than by any careful well-developed plan. As absurd as it may seem to produce classroom materials before establishing a theoretical base for their development, that is exactly what happened in this field. To complicate matters even more, sensitive teachers, realizing that their training had not been adequate for their needs, began asking for that training, preferably in condensed and intensive packages. And healthy as this situation appeared to be, it only triggered still another problem--that of finding adequately prepared professionals to provide this training.

In all fairness to both psychologists and educators, however, it must be admitted that researchers in these fields were among the first to tackle the problems which had been almost completely ignored by linguists and social scientists. To be sure, they charged into areas without adequate interdisciplinary breadth and descriptive depth; but this does not diminish the fact that they noted the problems and put forth solutions while other fields were still tending to internal matters.

Evidence of the importance of educational applications in sociolinguistic research can be seen, at least partially, in what appears to be recognition of elitism from within the field of linguistics. There is a growing uneasiness among linguists about pursuing research projects

which are totally irrelevant to contemporary social and political situations. The academic disciplines must be sensitive to such situations and be ready to plot new courses as conditions change. One of these conditions is the plight of American children and adults whose academic success and social mobility are severely restricted by the kind of English they use and by their difficulties in dealing with the written word. Recognition of this problem has motivated many linguists and students of linguistics to try to utilize the knowledge of linguistics in such areas and to explore newer avenues of linguistics which might bring us closer to solutions. They have recognized that many of the aspects of education are underlined by sociolinguistic principles and processes. Much of the English teacher's job, usually unknown to her, is at the very core of problems of language interference, identifying speech functions, switching rules, attitudes toward language diversity, language planning, etc. This seems to have become apparent to many sociolinguists, who are beginning to demand relevance of their field to real problems in the world out there.

It was under this sort of pressure that the graduate program in sociolinguistics at Georgetown is being developed. The attempt is being made there to create a strong program with emphases on linguistic competence, communicative competence, and educational applications. It is felt that the educational emphasis will serve largely a watch-dog function, insuring that studies of the two types of language competence serve at least some practical ends. On the other hand, the experience of sociolinguistics students with real problems in real life is also hoped to have a salutary effect on the students. The danger of such application, of course, is the standard danger that applied linguistics shares--that of overzealous attempts to get on with 'relevant' applications without sufficiently thorough theoretical grounding.

One example of the attempted educational emphasis of sociolinguistics may be seen in the recent interaction of sociolinguists from Georgetown University and the Center for Applied Linguistics with the Norfolk, Virginia Public Schools. With special funding from Emergency School Assistance Program monies, the Norfolk schools invited this group to help them solve problems involving, in the broadest sense, racial integration of their students and teachers, and, more specifically, the reading, writing, and speech of their students. A team of three staff members and five advanced students engaged in a seven month program involving first, a number of teacher preparation sessions and then, a series of visits and actual teaching interactions in the classrooms of these teachers. The major problem, planned and executed by the group as a whole, was in helping teachers appreciate linguistic diversity, teaching them to recognize different dialects as systematic, graceful, and logical and in helping them understand some of the educational pitfalls involved when sociolinguistic information is not regarded.

As might be expected, the teachers were generally naive about the linguistic features which serve to identify people of different socio-economic or educational status although they were quite accurate in their judgments of such status from a tape recorded speech sample. They were predictably unaware that the brand of English used by their black students had a pattern of grammar. In fact, each teacher unceremoniously failed the Black English grammar test which we administered near the beginning of the workshop. Since the assumption of most educators is that all varieties of language diverge from some standard norm, it was our task to try to instill the notion that different language systems are equally valid for the specific needs of their users, even to the extent of pointing out that it might be appropriate to play football in non-standard English. In addition, considerable time was spent on the relationship between cognition and speech, emphasizing in particular that the Black English code seems capable of expressing everything that standard English does.

The phase of the Norfolk project involving teacher preparation sessions concluded with several sessions on the implications of current social dialect research for teaching strategies and materials development. The notion of dialect interference in the reading, writing, and speech of students was explored and current research materials relating to these factors were described and evaluated.

The second phase of the attempted educational emphasis of sociolinguistics in the Norfolk schools involves working with the teachers in their actual classroom settings. It involves sociolinguistics students and faculty in such matters as helping the schools perceive the cultural and linguistic biases of standardized tests, in assisting teachers in evaluating student writing which contains predictable patterns derived from the oral language of the students, and in actually getting involved in the individualized instruction of children in the classroom.

The theoretical emphasis in sociolinguistics derives largely from the study of variability in language. This study of variability has led to very important modifications of linguistic theory, ranging from Labov's (1969) variable rule to DeCamp's (*to appear*) use of implicational analysis to Charles-James Bailey's (1969) contention that there is no real distinction between synchronic and diachronic linguistics. To take the variable rule as an example, its proposal is the result of the discovery that linguistic regularity penetrates deeply into the area of variable phenomena--an area previously dismissed by linguists as free variation, or, more recently, as mere performance. An impressive demonstration of the kind of linguistic regularity which is observed when variability is ignored is to be found in Labov 1969.

Another example of the phenomenon discovered by Labov is the case of the final consonant cluster simplification rule in English. The data to be discussed are taken largely from the Black nonstandard dialect,

but the rule can be shown to operate, at lower frequency levels, in the socially favored dialects of American English as well. The final consonant cluster simplification rule removes the final member of a consonant cluster, provided the final member is a stop and provided that either the final member is apico-alveolar or the first member is [s]. These constraints can be summarized as follows:

$C_1 C_2 \longrightarrow (C_1)$ Provided that C_2 is a stop and either C_2 is apico-alveolar or C_1 is [s] or both

It happens that simplification is variable, that is, not every instance of every word which meets the conditions just outlined undergoes simplification. Before careful studies on variability were performed, such variation would be dismissed as free variation or described by an optional rule. The degree of variation would have been considered outside the realm of linguistic competence. It can now be shown that there are a number of conditions which quite regularly affect the degree of variability--even though none of these conditions cause simplification to apply categorically, nor do any of them prohibit simplification entirely. Furthermore, the effect of the various conditions is hierarchically ordered; some conditions have a greater effect than others.

The cluster simplification rule applies less frequently if the following word begins with a vowel than if it begins with a consonant. That is, the word best would be more often pronounced bes' in the phrase best pear than in the phrase best apple. It applies less frequently if the final deletable consonant is [d] or [t] representing the ed suffix than if both members of the cluster are part of the same morpheme. For example, the pronunciation [mɪst] varies with the pronunciation [mɪs] whether the corresponding spelling is mist or missed, but the pronunciation [mɪs] is more frequent if the spelling is mist. The phonological features of the first member of the cluster are constraints, too. If the segment is sonorant (i.e. a lateral or a nasal), deletion is more frequent than if the segment is not sonorant (i.e. a spirant or a stop). In nonsonorants, continuance becomes a factor; continuants (spirants) promote simplification more than noncontinuants (stops). This means that the final [d] of roamed will be found more often deleted than the final [d] of roved and the final [d] of roved more often deleted than the final [d] of robbed. Finally, for past tense forms of verbs, it can be shown that verbs which undergo ablaut as well as suffixation in forming the past tense are more readily subject to deletion than are verbs whose past tense is formed by suffixation alone. That is, told, the past tense of tell, is more likely to be pronounced tol' than is tolled, the past form of the verb to toll. The effect on cluster simplification of each of these conditions is obvious to anyone who looks at the data, and also can be shown to be statistically significant at very high levels of confidence. These conditions are summarized in Figure 1.

FIGURE 1. Constraints on English final consonant cluster simplification

Consonant cluster simplification is favored when:	... so that:	... is more often pronounced:	... than:	... is pro- nounced:
Following word begins with a vowel	best pear	bes' pear	best apple	bes' apple
Both members of the cluster are part of the same morpheme	mist	mis'	missed	miss'
First member of the cluster is sonorant	roamed	roam'	roved robbed	rove' robe'
First member of the cluster is continuant	roved	rove'	robbed	robe'
Verbal base undergoes ablaut	told	tol'	tolled	toll'

Not only do each of these factors affect cluster simplification, but they affect it in hierarchical order. That is, the effect of the strongest constraint on simplification outweighs the cumulative effect of all the weaker constraints. This is illustrated in Table 1, adapted from Wolfram 1969. For all four social classes of speakers, simplification is

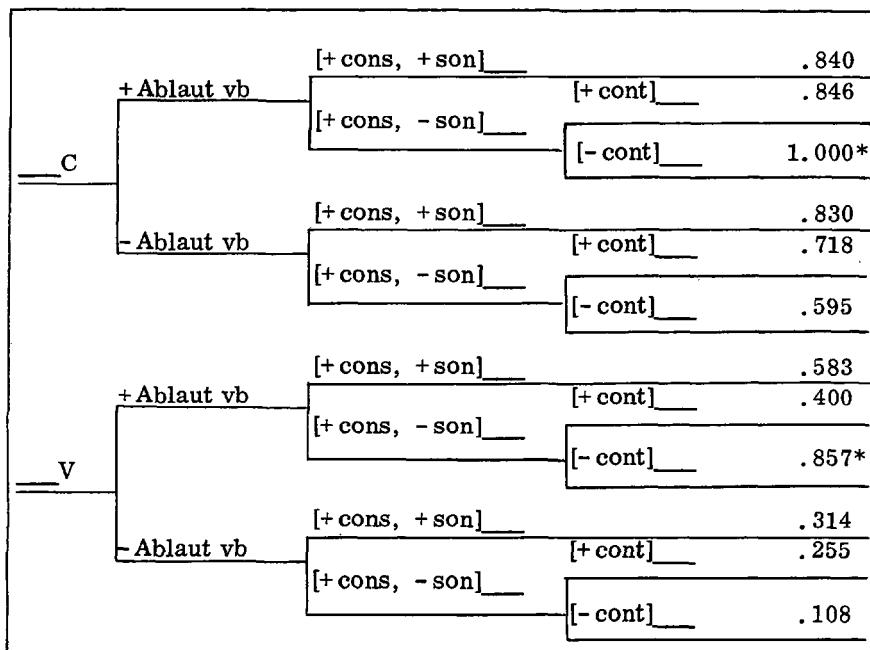
TABLE 1. Cross-products showing percent of consonant cluster simplification as affected by following vowel and by intervening morpheme boundary (adapted from Wolfram 1969:62,68).

Social Class	Upper Middle	Lower Middle	Upper Working	Lower Working
C#C##V	6.8	13.3	24.3	33.9
CC##V	22.6	43.3	65.4	72.1
C#C##C	49.2	61.7	72.5	76.0
CC##C	78.9	86.7	93.5	97.3

more frequent when the following word begins with a consonant, which favors simplification, and a morpheme boundary intervenes between members of the cluster, than when the following word begins with a vowel, which inhibits simplification, and there is no intervening morpheme boundary, which tends to favor simplification. Quite naturally, when both factors favor simplification, the rule applies most frequently of all; and when both inhibiting factors are present, frequency of simplification is lowest.

Cross-product data of this kind, involving only two constraints, are not terribly impressive, except for the fact that the pattern repeats for all four social classes. After all, when two cross-products are being compared, one is almost certain to outrank the other. However, the same pattern obtained in data on Washington, D. C. speakers for all the constraints summarized in Figure 1. That is, a near-ideal pattern of nested hierarchies was found as illustrated in Figure 2. The data in Figure 2 are recapitulated in Table 2. In both displays, each frequency figure is equal to or lower than the one beneath it except for the

FIGURE 2. Hierarchical effect of various constraints on cluster simplification in bimorphemic clusters



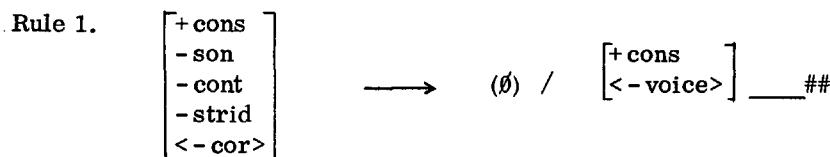
two starred cases. Both of these involve an ablaut verb whose base ends in a stop. The only such verb in the data is the verb keep (past

tense kept) and its failure to fit the model may well be idiosyncratic to that particular item. In addition, note that the 100% figure for this item when the following word begins with a consonant reflects the behavior of only two examples.

TABLE 2: Tabular display of the effect of various constraints on cluster simplification in bimorphemic clusters

Foll. env.	Prec. cons.	Ablaut	Example	no. abs./ no. exs.	%
C	sonorant	yes	told secrets	21/25	84.0
C	nonsonorant <u>continuant</u>	yes	left me	11/13	84.6
C	nonsonorant <u>noncontinuant</u>	yes	kept going	2/2	100.0*
C	sonorant	no	planned well	49/59	83.0
C	nonsonorant <u>continuant</u>	no	missed her	28/39	71.8
C	nonsonorant <u>noncontinuant</u>	no	worked fast	25/42	59.5
V	sonorant	yes	told all	7/12	58.3
V	nonsonorant <u>continuant</u>	yes	left on	2/5	40.0
V	nonsonorant <u>noncontinuant</u>	yes	kept after	12/14	85.7*
V	sonorant	no	planned it	16/51	31.4
V	nonsonorant <u>continuant</u>	no	missed out	14/55	25.5
V	nonsonorant <u>noncontinuant</u>	no	worked always	7/65	10.8

In traditional generative phonology, the final consonant cluster simplification rule can be accounted for only by some such optional rule as Rule 1, which says nothing about the constraints on variability.



All this rule specifies is that a final true stop may be deleted after a consonant, and if it is not coronal (i.e. not [t] or [d]), the preceding consonant must be voiceless. The specification [-voice] is sufficient to designate [s] since it turns out that [s] is the only voiceless consonant which can precede a non-apic-alveolar final stop in English. Using Labov's variable rule format, however, all the constraints on variability can be accounted for.¹

$$\text{Rule 1} \quad \left[\begin{array}{l} +\text{cons} \\ -\text{son} \\ -\text{cont} \\ -\text{strid} \\ <-\text{cor}> \end{array} \right] \longrightarrow (\emptyset) / \left[\begin{array}{l} +\text{cons} \\ <-\text{voice}> \\ \delta \text{ son} \\ \varepsilon \text{ con} \end{array} \right] \beta \# \underline{\quad} \# \gamma C^2$$

Without the sociolinguistic motivations for studying variable language behavior, the theoretical modification necessary to express this kind of regularity would never have been made.

Another important area of emphasis in sociolinguistics has to do with communicative competence rather than linguistic competence per se. That is, it has not so much to do with the kinds of syntactic and phonological mechanics which the variable rule accounts for as with the selection of those items from the speaker's linguistic repertoire which are appropriate to a given social situation. Perhaps the simplest example of communicative competence has to do with address forms. In American English, this means the competence Americans have in selecting First Name (FN) or Title and Last Name (TLN) as the appropriate address form for a given individual (Brown and Ford 1961). Susan M. Ervin-Tripp (1969) has worked out a set of rules in the form of the type of flow chart used in computer programming which accounts for the selection constraints she believes are relevant for academic persons (Figure 3). A questionnaire completed by members of the introductory sociolinguistics course at Georgetown University indicates that other factors may be relevant to the decision as to which address form to select. Table 3 shows a sort of hierarchical arrangement of three attributes of the addressee; membership in a religious order, acquaintance, and sex, as determining factors. There were 23 respondents; those who did not indicate that they would give or expect to receive FN as an address form indicated either TLN or avoidance of all address forms as alternative options.

If we interpret the differences among the students' reports of their address-form use as a kind of variability, these data emerge in a hierarchical order somewhat analogous to the variable rule data cited earlier. The most important constraint seems to be membership in a religious order; most of the students do not normally expect to address a priest or a nun by his or her first name, even if they are fellow-students. Acquaintance seems to be the next most important factor

FIGURE 3. An American address system (from Ervin-Tripp 1961:95)

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TABLE 3. Effect of three attributes of the addressee on address form selection as reported by 23 students

Addressee is a fellow student who is:			No. of students who expect to:	
Member of a religious order	Fairly close acquaintance	Of the same sex	Give FN	Receive FN
No	Yes	Yes	22	22
No	Yes	No	19	20
No	No	Yes	15	15
No	No	No	13	13
Yes	Yes	Yes	8	18
Yes	Yes	No	6	12
Yes	No	Yes	2	10
Yes	No	No	0	5

and sex the least. The hierarchy breaks down at only one point; 18 of the 23 students would expect a religious person of their sex with whom

they are well acquainted to address them by their first name while only 13 expected a non-religious person of the opposite sex with whom they are not well-acquainted to use their first name.

Another area in which speakers demonstrate a structural competence in communicative behavior which is not strictly linguistic is in the use of ritual speech events. William Samarin (1969) found that the Gbeaya of West Africa have a definite structure for well-formed insults. This structure can be formulated as:

(C)	B	(S)	where C = personal challenge
*1		*2	B = derogatory body-part comment
			S = simile

The use of numbers and asterisks is an adaptation of Langacker's (1969) mirror image convention and is to be interpreted as allowing the challenge to appear at the beginning or end of the insult, but not in both places. Some of Samarin's (1969:328-329) examples are these:

- | | |
|---|--|
| C I'm not going to do anything
stupid with you | B Awful watery eyes |
| B Your mouth is spread out | S Like dirty water poured in
the dump |
| S Like what kind of mouth? | |
| B Your mouth is crimson | C It's me you're looking for
trouble with |
| S Like a wan-gbara bird's arse | B Your mouth is wide |
| C Won't we fight today? | |
| B So what's that dirty thing whose mouth is wide? | |

A somewhat looser structural competence in communication is knowledge which speakers have of well-formed narratives. In his work with black and Puerto Rican adolescents in New York City, Labov and his colleagues (1968) (Joshua Waletzky, in particular) found that the narratives given in interviews had definite structural components. A fully-developed narrative will begin with a one or two clause 'abstract' summarizing the entire ensuing story. The function of the abstract seems to be to test the interest of the audience in the topic before committing oneself to telling the whole story. Next is found an 'orientation' in which the participants, time and setting are given. The 'complicating action' and the 'result' are the main body of the narrative and the only components which appear in all narratives. Many narratives end with a 'coda'--a statement such as and that was it, you know which closes the speech event. Intermingled with the body of the narrative are clauses of 'evaluation' which give reasons for telling the

story in the first place. In fight narratives, for example, the evaluation is designed to convince the hearer of the speaker's fighting prowess and of the justice of his side of the dispute.

There are numerous other areas of communicative competence displayed by speakers of all languages which are open to analysis. The structures of telephone conversations, introductions, bull sessions, sermons, and board-room meetings are only a few examples.

These three areas hardly exhaust the current topics of interest in sociolinguistics. There is a growing interest in the influence of social factors on language change. There is a wide and growing literature on multilingualism both from the point of view of the multilingual individual and from the point of view of the multilingual state. Sociolinguists continue to maintain their interest in questions of language planning and language policy. Whenever we look at language beyond its role simply as a code for transmitting connotative information, we find much to fascinate the investigator and much for him to learn.

Conclusion. Just as it is difficult to know what a true academic discipline or field really is, it is difficult to determine exactly what it is that the term sociolinguistics implies. In this paper we have argued that there are several current emphases or strands involved in the subject. One involves the solution of linguistic problems, one involves social factors in language use, and one involves the solution of significant educational matters in this country and abroad. If this sort of interdisciplinary area is to survive and be vital, we will need to develop a tradition which combines fieldwork with abstract and formal analysis of linguistic data. If it is seriously concerned about the social and educational problems this world faces, it will need to develop ways of effecting a rapprochement between linguistics and other relevant fields. We must learn to generate an attitude that, no matter how absurd or wrong-headed other relevant disciplines may appear to be, we can continue to work in the area, offering the insights and knowledge which our field provides and accepting the clues of other fields toward the solution of problems of mutual concern. As Labov (n. d.) has recently observed, "The interdisciplinary training of linguists may in turn help them to expand their empirical techniques and formal modes of analysis through acquaintance with broader academic fields, as well as to deal with wider ranges of data. This may be an essential step if linguists want to be sure that their analyses of linguistic structures are not artifacts, but direct reflections of the language used by the societies around them." The current emphases of sociolinguistics seem to be at least headed in this direction.

NOTES

¹Fasold 1970 gives arguments for modifying Labov's notational system, but these arguments are not relevant here.

²In a more formal explication of the rule, C must be specified so as to include pause, since silence affects deletion the same way that consonants do. Ablaut verbs take the formative boundary + (the constraint) rather than the morpheme boundary # before the suffix.

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DEVELOPMENTS IN BIOLOGICAL LINGUISTICS

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Abstract. An attempt is made to summarize the principal aims of biological research on language without, however, surveying actual projects. The conceptual and theoretical underpinnings of present-day psychobiology of language are examined, and it is shown that the popular characterization of the biological approach as 'nativistic', 'anti-environmentalistic', or 'ahistoric' is based on ignorance of the field. In the area of neurology it appears that a search for 'activity patterns' and 'modes of functioning' is more promising than a search for 'anatomical innovations' as explanatory basis for man's language capacity.

When you are introduced at a cocktail party as Mr. Jones, a linguist, you know what the first question is going to be: "How many languages do you speak, Mr. Jones?" If you are introduced as a psychologist, the exclamation is: "Oh, are you going to psychoanalyze me?" When your field is the biology of language, the reaction is: "Hmm [pause], a nativist." The latter characterization does not signal any fascination or interest. There is, in fact, only one appellate that is worse--a philosopher. Dichotomies such as nativism versus empiricism, innate behavior versus learned behavior, nature versus nurture, etc., etc., are the bane of my existence. They are slogans born in ignorance. I would like to present some of the basic principles of a biological approach to the study of language, in order to show how misleading all of these mini-characterizations are.

Aims of a biology of language

The ultimate objective of my field of inquiry is to study the relationship between the capacity for language and the peculiarities of human brain function. This is a tall order, and it is obvious that there are many preliminary questions that must first be broached. For the time

being, we are compelled to use only indirect data, and thus to make inferences about the brain based on nothing but circumstantial evidence. There is no order of priority in what must be done first; nor can we say of our data that some are more basic than others. Thus, the following account of kinds of data must not be understood as being in a logical sequence.

The most popular problem pertaining to biology of language (though by no means the most important one) is whether or not animals have the capacity to acquire language. In a sophisticated comparison, one would like to know how a natural language would have to be transformed (possibly simplified) so that members of a given species could become conversant with it.

Another area of concern in the biology of language is the natural history of language development in children. What do children actually do when they begin to speak, and what are the variables that control their progress? Just what are the environmental prerequisites for language development--the types of input, quantitative parameters, feedback such as correction, etc.? What are the biological prerequisites, such as degree of physical maturation, both central and peripheral? And, finally, what are the specific structural or physiological phenomena that control this particular type of behavior?

It should be obvious that one cannot work in the area of biological foundations of language without at the same time studying its exact nature. Therefore, linguistic studies are of intense interest to my field. In such studies one assumes that the speaker has reached a biologically steady state, maturity; further, one studies individual problems in the hope that a few individuals are a satisfactory sample of an entire speech community. This appears to be common practice in linguistics; the practice is based on our faith that individuals, in fact, do not differ one from the other in certain linguistically important ways. This faith, which I also share, is one of the fundamental postulates of a biological theory of language (Lenneberg 1967: Chapter 9) and reflects a very interesting and very universal biological property--the preservation of uniformity of patterns in the presence of constantly varying details.

Closely related to this sort of interest is the study of language universals. It is only because of the conviction that some universals exist that we can speak at all about Language with a capital L. During the two decades that were dominated by the Boasian and Bloomfieldian ways of thinking, the study of universals was badly neglected, but even in those days linguists did not doubt that there were some general characteristics of language and that this type of behavior could be sharply distinguished from other types of human or animal behavior. Today, there is still a wide margin of controversy over the exact nature of language universals; I think that a biological approach may very well supply us with guidelines for their further discovery and description.

Another field of research pertinent to the biology of language is the study of language sparing and loss in the presence of alterations of brain structure and function. The traditional questions have been confined to the possible discovery of cortical areas that serve as some sort of 'center' for the elaboration of language. However, this is a rather narrow approach to the wide field of brain and behavior studies, and it is necessary to include other factors in our research. The role of age and differentiation of tissues is probably the most important. Also, some progress has been made in the study of brain processes relevant to language by observing linguistic changes caused by pharmacological agents and by other influences that can speed, retard, or modify chemical reactions in the brain; examples are temperature, such as fever or hypothermia, or periodic peripheral stimulation, either by auditory interference or by strobe lights, or direct periodic stimulation of exposed brain structures.

The problem of heuristics

The study of languages does not necessarily lead to an objective account of what Language--with a capital L--consists of. Although this question is frequently branded as philosophical (as if this were a disgrace), it must be faced squarely if we hope ever to make any progress in the biological study of language. In fact, the wrong answer to the question frequently leads to absurd hypotheses and conclusions. If language simply consisted of the capacity to memorize thousands of arbitrary pairings, then it would be appropriate to see a beginning or a primitive form of language in a rat's capacity to learn a small number of such pairings (buzzer means food; light means drink; etc.). I have dealt with this problem in a formal way and in greater detail elsewhere (Lenneberg 1971). The basic conclusions I have come to can be put quite simply. The capacity for language must be understood as a capacity for operating on certain stimuli in highly specific ways. Thus verbal intercourse (giving evidence of understanding what is being signalled, either by executing commands or by responding in terms of the same code) is comparable to 'formal problem-solving', or, more specifically, problem-solving with certain types of computations. Once more allow me to be superficial and say simply that I have shown elsewhere (Lenneberg 1971) that the simplest form of making computations is to determine the existence of given relationships. Language activity consists of constantly computing relationships and also of relating relationships. I am sure these statements are cryptic at present. Perhaps they will become clearer in the following examples. One might say in general, however, that the difficulty that faces us in language research is to determine what another organism, human or animal, actually knows. The question is akin to the old chestnut that confronts

the biologist who is working on visual perception in animals: how does one know what a cat really sees? The verbal analogue is, how do I know what the speaker really means? This is a question that can never be answered in every detail. In face to face contact among human beings, one copes with this situation in two ways. First, one assumes that there are basic universal similarities in capacity, function, and propensities among all human beings; this gives us a baseline (or universe) from which we may start guessing at the correct interpretation of what we hear or see in our fellow men. Second, one collects evidence toward a hypothesis, so to speak. One elaborates questions and elicits further sentences until it becomes clearer and clearer that the interlocutors have the same thing in mind. This is what I mean by asserting that verbal behavior is a continuous effort to solve problems.

Do apes have language?

It is easy to see how failure to give language an appropriate definition would get us into trouble. If we think of the arbitrary associative aspects of language as its most essential feature, then all animals have language to some degree. Somewhere in this proposition is hidden some truth, but it is trivial and certainly does not lead closer to an understanding of the nature of language. The same, of course, holds true if you simply say in general that language is problem-solving. Animals also can solve problems. What matters here is a specification of what particular problems are to be solved.

Let us begin with words for objects. A dog can learn to get his leash upon a signal, even a verbal one. But this seems to me to be only vaguely reminiscent of what is involved even in object naming. Imagine a nondescript cubic object in a doll-house. If we put little chairs around it and silverware and plates on it, it becomes a table. If we changed the furnishings, using the same nondescript object, it could become a chair simply by being placed next to a larger table. In yet another setting it might be a box, a block, a bench, a bed, etc. This illustrates the vitally important fact that even object words are the labels of relations, not of things. If we want to assess an ape's capacity for language, we should not measure it in terms of how many individual objects have been tagged by words, but whether the fundamental principle of naming, as it occurs in natural languages, is present and understood. This principle could be tested with ten words as easily as with one hundred. Size of vocabulary is clearly irrelevant to the question of similarity to natural language.

Recently, three chimpanzees have been given intensive language training in two different laboratories (Gardner and Gardner 1969; Premack 1970). The world awaited with bated breath the moment that the pupils would begin to combine words. At least two of them finally did,

and the joy was great among those of us who are convinced that chimpanzees are almost, though not quite, human beings. Yet some lingering doubts remain. First of all, 'combining' is a very loose notion. A machine that generates random numbers also combines symbols, and it does so based on a specific principle, namely the principle of random sampling. It is obvious that this sort of combination would be irrelevant to language. Would any and all other sorts of combination be relevant, though? Take the following combination: grandma grub lilac and she if hog. The principle here is alphabetization according to the last letter in the word.

Our problem is best illustrated by a sequence such as Mary, John (or any other pair of words whatsoever). This utterance assumes meaning to the extent that we impute a relationship that holds between the two items (or a relationship that holds between the set of two unordered items and some other set). In natural languages such relationships are always made fairly explicit, either by using a verbal signal (a morpheme or an operator, such as Lat. -que, Engl. and), or by using a convention according to which place and order of words implies a specific relationship (such as attribution in some Slavic languages corresponding to the use of our copula). It is true that there are certain relations that are explicitly marked in some languages but customarily omitted in others. However, it is possible to give expression even to those relationships that are not usually stressed in a particular language. The important point here is that there is a general realm of relationships from which all languages draw; other relationships, entirely conceivable by us also, are never encountered in the natural fabric of a given language. What is more, some relationships appear to be virtually universal and thus obligatory to human verbal communication. The most important is the relationship called predication. No language has ever been described in which sentences neither explicitly signal nor remotely imply a topic and a comment, or a topic that is further specified. Other relationships that appear universal are conjunction, equation, similarity, relations of space and time, and many others.

When our simian colleagues begin to speak, we are caught in the same dilemma described earlier: how do we know what is on their minds when they have produced two or three verbal symbols? How do we know what the relationships are between their words? As already mentioned, there is, in fact, no way of knowing, just as there is no assurance of what our infants have in mind when they are in the one-word or two-word stage of language production. In the case of children, we build up confidence that they know what is meant by language by our informal observations of their powers of comprehension. Because they are human beings, we are fairly safe in assuming that their way of dealing with the environment and also their motivations are not so different from those of any other human being. The situation, however, is very different in the case

of the apes. Because they are not human beings, we cannot make the latter assumption--in fact, our entire problem is to discover in what way these animals' cognition differs from ours. When the apes combine words, it is we who impute relations and thus meaning to their 'talk', but we have no assurance that this meaning is also theirs. There is only one way to investigate the problem; we must test their powers of comprehension. If we think that a certain combination of words is based by the animal on a given relationship, we could test this hypothesis by confronting the animal with a situation in which we would command him to do one of two things--once we would relate the words in one way, and once in another. An example would be the words block and table; we would study whether the animal could obey: Put the block on the table as against Put the block under the table; or Point to the block and the table as against Point to the block under the table, etc.

Several of my colleagues have interpreted the monkey research as evidence against a biological foundation of human language. This is actually due to a misapprehension of what is meant by biological foundations of language. Suppose the test that I have outlined above had been carried out satisfactorily on the animals, and it had been shown thus that these creatures did, in fact, have a primitive form of language. This would indicate merely that their cognitive capacities were reasonably closely related to those of man; this would hardly come as a surprise to anyone, even though no one had previously suspected that the relationship was so close that the animals could master the beginnings of a natural language. Would such a finding (yet to be demonstrated) undermine the notion of species-specificity of language? It would tell us that language capacity is not totally unique to man; on the other hand, that the natural history of language is rather different in normal man from that in normal chimpanzee has been amply demonstrated in the several attempts to develop language in chimpanzee infants simply by talking to them. Species-specificity by itself is actually not much of an issue. Throughout animal behavior we find examples of species-specificities as well as examples of behavioral homologues; neither finding may be used to argue for or against a biological approach to behavior.

The history of language and Kaspar-Hauser-Gedanken experiments

I am frequently asked whether I believe that a hundred children abandoned in their infancy on a deserted island in the South Seas would automatically develop language. This question is apparently prompted by an assumption that biology of language implies some naive 'nativistic' or anti-environmental hypothesis. Nothing of the sort is true. First of all, there is no reason whatever to believe that abandoned infants would develop language. The necessary input for language

development in an organism that has the capacity for language is language. This is no stranger than the assertion that the necessary input for the formation of social bonds (herds, flocks, mother-infant relationships, etc.) is social contact. Or that the necessary input for an organism that can synthesize proteins is proteins. Notice that none of these statements implies any theory on the origins or first development of the mechanisms under consideration. A biology of language is not identical with a theory on its origins. As a matter of fact, I believe that the question of the origin of language is as futile as the question of the origin of man or the origin of the capacity to synthesize proteins. In biology there are no clear-cut origins of anything. Whatever we are studying has gradually developed from something else. There are no sudden initiations or beginnings. This consideration also refutes the claim that a biological approach to language denies any evolutionary history of language capacity. Biology is probably the most historically-minded of the natural sciences. The idea that everything has its antecedents permeates every hypothesis and every theory. The capacity for language is no exception. And if we had the proper means, we would be able to show exactly what it is in primitive hominoid forms that eventually led to language as we know it today. The difficulty is that the necessary data for such an undertaking are lost. The remains are simply too poor to allow us a reasonable and trustworthy reconstruction of what happened in biological history. I have spoken in my book (1967) of a discontinuity theory of language evolution. Perhaps the word was ill-chosen, because it is often construed to mean historical discontinuity. It was meant to refer, on the contrary, to a synchronic discontinuity among present-day species. So far there is little evidence to show that the communication behavior of primates or any other taxon is a primitive form of language behavior. It is in this sense that I believe that baboon communication behavior is discontinuous with human natural language. It is a common mistake of non-biologists to think of modern living animals as constituting an evolutionary continuum.

A word on the nature-nurture controversy is also in place. Especially in the social sciences, nature tends to be equated with genes, nurture with environment. This, however, is a gross over-simplification. In the first place, genes correlate only to specific enzymes, which in turn, regulate certain biochemical reactions. Strictly speaking, there are no behavioral traits that one can observe that are directly due to the operation of specific genes. In some instances, such as the presence of pigments, the connection between the gene and the trait may be reasonably direct. In many other instances, however, the relation is extremely indirect, particularly when it comes to the control of behavior. Furthermore, the genetic background merely regulates what sort of use is to be made of environmental conditions. It determines propensities and capacities, given certain environmental conditions, and in most cases, the

genetic background is a necessary but insufficient prerequisite for the development of a given behavioral trait. Thus it is obvious that genetic factors do not stand in any opposition to environmental factors. To the contrary, what matters is the interaction between the two.

The concept of universals.

To talk about universals to linguists is to bring owls to Athens. Biology of Language draws heavily on the linguistic research of the last twenty years. At the same time, it is important for my field to reformulate the discoveries and to discuss them in terms that may sooner or later have some physiological reality. The ubiquitous presence of relations and relations of relations is important in this context. Relations imply operations and processes, and thus the study of relations forms the natural link between linguistics and brain research. But if this line of thinking is to be fruitful, it behooves us to specify the nature of the relations that are important--and importance here will be measured in terms of universality. Of course, much work needs to be done in this field; I can do no more than hint at some possible solutions. What is of interest to me in the area of phonology, for instance, is the fact that the speech sounds of all languages are parametrized; that is to say, there are always only a small number of degrees of freedom for how to produce language-relevant sounds. This is the same as what some linguists have called phonological features (an unfortunate term, because it seems to imply units rather than variables). Further, speech sounds must always be specified in terms of categories rather than the specific values of each parameter (phonemes). Moreover, these categories themselves can be defined only in relation to other categories, so that a phonemic system is always a relational fabric, one category being balanced against several others. This has physiological implications, in that the organism must perform a very complex set of computations in order to ascertain whether a specific sound belongs to a given relational category or not. We are close today to a reasonable mathematical model to serve as a theory for production and reception of speech sounds. Syntax and semantics may perhaps be approached in similar ways, although there is very little encouragement so far from the existing theories. Nevertheless, few of us doubt today that it is possible to characterize some aspects of, at least, syntax in terms of abstract algebraic relationships. To the extent that we succeed in this endeavor, we will have made more explicit the types of operations that universally underlie important aspects of language.

Relevance to brain research.

Our knowledge of brain physiology is almost entirely based on animal research. Animals, however, do not have language; therefore, this research can hardly tell us anything about the neurophysiological basis of language. On the other hand, the organism that speaks, man, is fairly immune to the performance of truly crucial physiological experiments. Human neurophysiology can only be inferred from the effect of disease and its treatment. Further, we have a way of describing man's behavior in rather different terms from those used for animal behavior. The symptomatology of neurological practice is permeated with concepts that are simply inapplicable to animals (hallucinations, thought disturbance, personality changes, alterations of consciousness, agnosia, apraxia, and aphasia), or concepts that do not seem to be quite the same when we use them in connection with animals (learning, concept formation, memory, recognition). These conceptual differences between the basic approach to brain research on animals and brain research on man make it awkward to do comparative psychobiology of language. This is best illustrated in our attempts to discover the neurological basis of man's capacity for language. Ideally, we should like to compare man's brain with that of other primates or mammals in order to be able to attribute any unique developments in our own brain to our unique capacity for language development. I have demonstrated elsewhere (Lenneberg 1967) that sheer brain size is irrelevant to this problem. A next possible step might be first, to identify the cerebral 'language centers' in man and then, to see whether lower forms have homologous structures. But new problems are in store for us now. The concept of language centers is based on the peculiar way we construct human neurophysiology, namely, by using clinical data as the primary input for theory. It is true that circumscribed lesions in certain areas of the brain cause fairly predictable types of language disturbances and that this enables us to draw a cortical map that indicates what kinds of consequences we may expect from regional damage. It is this sort of data that engenders the notion of 'centers'. When it comes to animal research, however, the idea of narrowly circumscribed brain centers for given species-specific behavior has been all but abandoned. One no longer searches for the 'dam-building center' in the beaver's cerebral cortex or for the 'herding-center' in a horse's brain, the ganglion responsible for a specific type of web spun by a spider species, etc. This is not because neurophysiologists believe that the brain is one homogeneous mass, but because it has become progressively clear that the regional functional differences that exist throughout the brain have to do with physiological controls and balances that have at once multiple somatic implications and an effect on behavior in a rather molar way. One may stop

respiration by a single lesion, but one cannot neatly abolish the nesting habit or the capacity for one specific sensory association by a strategically placed cerebral lesion. Lesions that abolish one aspect of behavior have immediately secondary and tertiary consequences.

A destruction of Broca's area in man will, in many instances, cause a motor aphasia; it does not, however, abolish language capacities. A large parietal lobe lesion, on the other hand, may cause the patient to be unable to understand what he hears or reads, may cause his utterances to be incomprehensible (although words are well formed), but there will be, at the same time, other cognitive impairments. In general, then, 'language centers' are at best metaphors; and the precise function of the so-called primary sensory projection areas is also in need of further elucidation. In view of these considerations, man's capacity for language cannot be attributed simply to the elaboration of cortical centers, of association cortex, or to systems of fiber-connections between them. In fact, the very concepts invoked in this sort of explanation are imprecise. It is hard to demonstrate that there are structures in man's brain that are definitely lacking in a monkey brain; assertions of this kind are at least to some extent based on definitions. There are other reasons for repudiating a simple explanation of language capacity based on the identification of loci and connections. Cortical loci are not predetermined at birth for language function. Lesions in early infancy may cause a rather different cortical functional organization. Moreover, the physiological function of cortico-cortical fiber-connections is still unknown. Signals from one cortical area to another do not depend on tangential connections, but are clearly relayed through deep sub-cortical stations probably located in the mid-line. The claim that associative learning in animals must necessarily involve the limbic system, that man alone has direct cortical pathways, and that man's language capacity is due to these direct connections is seriously undermined by the fact that limbic lesions in man (particularly of the hippocampi) also abolish associative learning in man across the board (i.e. including all sense modalities) and make new language learning impossible. Finally, it is questionable that association between visual and auditory stimuli is the cornerstone for language development. Both congenitally blind and congenitally deaf children have the power of language.

This is not to say that it is hopeless to search for neurological correlates. Because we have characterized language as a set of computations, it becomes at once tempting to hypothesize that it is function and activities, rather than structures, that must be investigated. Some beginnings along these lines exist. First, it is possible to reexamine aphasic symptoms in terms of their temporal structure, in order to isolate temporal parameters (sequence, rhythm, rate, accelerations, etc.) that might be important to the underlying physiological processes.

Second, it will be useful to compare the ways in which language function is disturbed with the ways other cognitive functions become disordered in disease, including the capacity for calculating, as well as recognition and perception. I have recently demonstrated (Lenneberg 1970) how comparisons of this kind elucidate the neurophysiological basis of cognition in general and how they lead, in a rather natural way, to the assumption that it is activity patterns in the brain that should command our attention, rather than possible innovations of anatomy.

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NEW MODELS AND METHODS IN TEXT ANALYSIS

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Abstract. The systems model from General Systems Theory is proposed here as a model of text analysis. It describes texts as dynamic processes. These processes or texts are seen to consist of a complex network of linguistic components and relations. Differences in texts are derivable from their communicative functions. It is possible to make general statements about different types of texts based on their communicative functions.

With a few exceptions, problems arising from the dynamic and communicative aspects of texts have been dealt with only during the last decade. Recent work on the linguistic features of texts also indicates the growing interest of linguists in this field. The systems model proposed here aims at integrating such former approaches in a coherent general framework for the description of texts.

The model to be discussed in this paper is the General Systems Model and its application to texts. Some methods of analyzing texts which were developed mainly during the second half of the last decade will be reviewed with reference to the particular problems of text analysis.

The term 'text analysis' is intended to refer to the analysis of written and spoken linguistic units that are larger than sentences. The word 'text' will, for the time being, be used as it is used in everyday language; meaning reports, literary and commercial prose, but also spoken discourse.

The goal of text analysis will here be considered to be a linguistic description of those features which make a text a text, or, in other words, constitute its textuality. This goal is different from exploring the conditioning of sentence structure through their contexts, and also from investigating performative traits of language use as has recently been suggested by Fraser (1970).

The order of presentation will be : (1) an examination of some aspects of recent approaches to text or discourse analysis, and (2) a proposal that we use the Systems Model as a new model for text analysis.

The need for such a model seems to be indicated for a number of reasons:

1. It integrates various approaches to text analysis in a coherent conceptual framework.

2. It provides an alternative to those approaches in which texts are considered to be an extension of lower level grammatical units. In contrast to such approaches, this model implies that we cannot construct a continuous grammatical hierarchy in which a text is just the step beyond the sentence.

3. It provides the framework for a number of new questions.

The problems of text analysis stem partly from the ambiguity of the word 'text', partly from the qualitative differences between texts and other linguistic units.

Ever since Hjelmslev established the dichotomy between grammar and text, system and process, restricted and unrestricted, the meaning of 'text' has been related to that of parole or performance. And a fairly general feeling of linguists towards the study of language performance is exemplified by Chomsky's well-known statement that "there seems to be little reason to question the traditional view that investigation of performance will proceed only so far as understanding of underlying competence permits." (Chomsky 1964:10).

On the other hand, texts have been understood to be linguistic units comparable to other linguistic units such as sentences.

Though it is desirable to treat texts as units, the analogy between text and a unit of grammar seems questionable. Such an analogy makes it difficult to deal with such features as dynamic structure, extra-grammatical regularities and the relation between different types of text on the one hand and a general model of texts on the other.

The dynamic aspects of texts have alternatively been called 'communicative dynamism', 'progressive growth', 'thematic progression' (Daneš 1970). Daneš points out that our difficulties in dealing with these aspects "arise from the lack of an exact model of the dynamic structure of objects". A model that is capable of taking into account the 'communicative dynamism' of texts will have to include their communicative functions.

At this point the question arises: What is the relation between the communicative structure of a text and its grammatical structure? Though the communicative structure is somehow mapped onto the grammatical structure, there is certainly no one-to-one relation between the two, as has been pointed out by Halliday (1967). Therefore, the dynamic structure of a text cannot be deduced in any simple way from its grammatical structure, but will require a process model which might be called extra-grammatical.

Such an extra-grammatical model would have to account for a high degree of complexity. The highly interesting work that has been done--among others by the Czech linguists Danes (1967, 1970), Sgall (1970), Firbas (1964), as well as by Kirkwood (1969) and Halliday (1967, 1968, 1970) in Great Britain--on the dynamics of sentence structure, deals mostly with relations in the sentence or in pairs of sentences. These relations are explored in terms of 'rheme and theme', 'topic and comment', 'new and given'. The features 'new and given' have recently been interpreted by Chafe as specifications of semantic units (Chafe 1970). An application of this concept might prove very helpful to text analysis. The description of an entire text will, however, entail over and above features in sentences or pairs of sentences, a multiplicity of variables interacting with one another, and components and relations among them. This multiplicity of variables and relations is what is meant here by complexity.

Communicative dynamism and complexity make it difficult to describe texts in the same way as units of grammar. Yet they do have particular linguistic properties. These properties are indicated for example, by the speaker's behavior in distinguishing between texts on the one hand and a series of isolated sentences on the other, between complete and incomplete texts, and between different types of texts.

Attempts have been made, among others by Harweg (1968), to describe and define texts on the basis of such features as pronominalizations or by various other means of syntagmatic substitution. Such attempts are based on interesting, but in my opinion questionable, assumptions. The first assumption seems to be that a text can be described as the sum of its connected sentences, the second that in characterizing texts, predominance must be given to their syntactic features, and the third, that those sets of features which are characteristic of texts must necessarily cross sentence borders.

While syntagmatic substitutions do occur in texts, they do not necessarily constitute textuality. That texts do not have to contain sentences, connected through pronouns or lexical repetitions, and that sentences which are thus connected do not always form texts, can be shown empirically. An example that illustrates the latter point is the following:

Rubenstein played two sonatas. A play by Synge is called Deirdre of the Sorrows. Play and work alternate in this school. Playboy of the Western World is seldom staged.

That the sentences of a text do not have to be continuously connected, is shown by this text:

Bangkok is grown-up Disneyland with those crazy beautiful temples of pink, green, blue, and lavender. Buddhas are everywhere

and dazzling--one, 160 feet long, lies on his elegant side. One day we rose early to see the floating market. A tiny launch takes you miles inland to a lush and jungly place where everybody is trading bananas, baskets, orchids. Upper class Siamese women are dovelike and gentle. The lower class ones carry baskets or produce on their shoulders, row the boats, cook the rice and care for their children. En route to New York across the Pacific we stopped in Honolulu for a day.

There are even a considerable number of texts which do not contain sentences at all, as for instance:

Mysterious Red Rivers of the North
Obi Ubang African Montanas
of the Gulchy Pearly
Earth -
Lakes of Light - Old Seas -
Mississippi River, Chicago,
the Great Lakes -
The Small Rivers like Indiana,
the Big Ones
Like Amazon.

(Jack Kerouac, 242 Choruses,
8th Chorus)

The assumption that one can get from syntactic concatenation to text constitution seems theoretically to be based on a hidden notion that there is a hierarchy of units which comprises texts and sentences as objects and which, apart from their extension, can be basically described that way. This notion is most clearly typified by Harris in his Discourse Analysis. Harris applies the basic techniques of structural linguistics--namely substitution and segmentation--to texts in a rigorous attempt at extending grammatical analysis to just one other unit. It seems hardly surprising that this attempt exerted much more influence on early transformational grammar than on discourse analysis.

The deeper reason why it is impossible to integrate the analysis of textual units into grammatical analyses also is rooted in the qualitative difference between texts and other linguistic units. This difference is reinforced by the observation that a speaker's judgment as to the acceptability of a text is not directly dependent on a judgment as to its grammaticality. Many texts--if not a majority--are quite ungrammatical. Texts such as poetry and oral discourse or texts produced by non-native speakers are not necessarily any the less coherent.

The qualitative difference between text and grammar is further indicated by the fact that grammar deals with a homogeneous set of utterances, whereas a text may comprise highly heterogeneous utterances--such as utterances from various different languages. Finally a speaker's competence in judging texts is not merely an extension of his grammatical competence. This is evident by the fact that we do very little teaching or learning of text formation in foreign languages, but rather expect to be prepared for the understanding and producing of texts, once grammar and lexicon have been mastered.

These considerations support the view that those features that indicate textuality are not exclusively syntactic nor are they describable in grammatical terms. The need for an extra-grammatical model is thus emphasized.

A third problem which text analysis faces is that of employing a model having sufficient generality. It would seem desirable that a model of text analysis yield a description sufficiently general to encompass the features common to all types of texts, and sufficiently particular to show the characteristics of different types of texts. We may ask: which texts should be described--everyday prose, poetic texts, oral discourse? All texts, apart from the fact that they are either factual or poetic, or written or spoken, exhibit some common features such as communicative purpose and coherence. On the other hand there seem also to be specific relations between different types of texts which, for example, permit a prose text to be identified as a paraphrase of a poetic text, or a dialogue as a dramatization of a third person narrative.

So far one type of text has been given particular attention, namely poetic texts. But this study has--to my knowledge--never been considered as a study in text formation, but rather in poetics. This may be asserted, even though some of the contributions to the study of poetic language, like that of the Jacobson (1960:358) equivalence principle, actually interpret general features of poetic text formation. As these were not studies in text formation, no attempts were made at that time to link poetic text formation with the structure of other types of texts. In order to establish such a relation, a model of text analysis is required which is more general.

Dynamic process, extra-grammatical regularities, general properties of different types of texts--it is with reference to these aspects of texts that the Systems Model from General Systems Theory is proposed here. In the field of General Systems Theory, the term 'system' is used with a meaning quite different from that of de Saussure. The systems concept in General Systems Theory has not been developed with an eye on linguistics or for that matter any particular science, but has successfully been applied to various fields and interdisciplinary problems.

One very well known example of a system which has recently been brought to our attention is the ecological system.

System, in this framework, denotes any process which runs through a cycle of changing states. This cycle of changing states implies a dynamic concept because something is going on, or is happening through the completion of each cycle. What is going on is usually interpreted as a transmission of energy or information through the system. Each system constitutes a whole and each completed cycle has its beginning and its end. Each process is determined by the function it has to fulfill in a wider context.

By analogy to text analysis, the function which determines the textual process is its communicative function. It is this dependence of the textual process on the communicative function that makes it clear that texts can be interpreted as systems. They must, however, be considered to be units of communication, manifested in language. They cannot be units that are first definable through some linguistic properties and which over and above this, happen to have a communicative function. Texts are however different from the unrestricted process of language performance because each process constitutes a whole. In fact we can conclude that the dynamic structure of texts cannot be defined or described without reference to its communicative function.

What does the structure of these textual processes look like? If we view a text as a system, the whole text may be pictured as a network of interrelated components. It consists of a highly complex, but organized set of components and relations between components. If one of these components changes, however, the whole system changes, because many other components are affected by the change. In this network of interdependent components, individual components lose their autonomy. They function in this system not because of some inherent qualities, but because of their integration into the system.

Interaction relations exist not only among components, but also between the system and its components. The network of relations between components is defined as the structure of the system. This network fulfills certain functions for the system as a whole, but vice versa the network receives these functions only from the system as a whole. Very simply said, this means that the whole system must be considered as more than the sum of its parts.

The application of this concept to texts means that the structure of texts is viewed as a network of interdependent components, not merely as a hierarchy of lower level units or as a one-dimensional linear concatenation. Everyday language points into the same direction as the notion of a network when it characterizes texts by the term 'coherence'.

Not all expressions in a text are equally important for its coherence. Primary importance must be attributed to those whose occurrence and selection is directly dependent on the text as a whole. These we will

call text constituents. They are the linguistic features that constitute textuality without being directly determined by grammar. They are comparable to the nodes of a network.

More practically speaking, all types of recurrences which are conditioned by the text may be considered to be text constituents. For the time being we will set up a simple dichotomy of types of text constituents, though this will probably not be sufficient for future work. We will classify text constituents either by content (Class I), or by expression (Class II). Examples for Class I are pronouns, synonyms, 'metathemes', and ellipses.

- (1) I met Joan this morning. She was in a good mood.
- (2) One of the seniors came to see me. The student offered some interesting suggestions.
- (3) My car did not start, it was snowing heavily, I could not get a cab--all this prevented me from arriving in time.
- (4) The president will give a press conference. The conference is scheduled for nine-o'clock tonight.

One condition for the occurrence of text constituents of Class I seems to be identity of reference, as has been pointed out by others earlier. What is coded and transmitted in text constituents of this kind is information on designata. Included may be information on the linear order of components, namely whether these items have or have not been mentioned before. As text constituents of this kind depend on referential identities, we call them referential text constituents. They constitute factual coherence.

In contrast, constituents of type II are relatable because they are similar or identical forms of expression. Examples are:

- (1) blue took it my
far beyond far
and high beyond high
bluer took it your
but bluest took it our
away beyond where

(e. e. cummings, 'o by the by')

- (2) Am Rande des Märchens strickt die Nacht sich Rosen/ ...
Die Rosen schreiten auf Strassen aus Porzellan und stricken
sich aus dem Knäuel ihrer Jahre einen Stern um den andern./
Zwischen Sternen schläft eine Frucht/ ... Zwischen Früchten
schläft ein Stern. Manchmal lacht er leise im Schlaf wie eine
porzellanene Harfe.

(Hans Art, Rosen schreiten auf Strassen
aus Porzellan)

(3) Homme de constitution ordinaire, la chair n'était-elle pas un
fruit pendu dans le verger, ô journées enfantes! le corps un trésor
à prodiguer . . .

(Rimbaud, Jeunesse, cf. Kloepfer/Oomen 1970)

These text constituents do not usually indicate any linear ordering. As they depend on similarity on the level of expression, they will here be called expressive text constituents. Whereas the first kind (Type I) creates factual coherence, these (Type II) create coherence of expression.

As a consequence, an analysis of text structure that aims at taking both the dynamic and the linguistic features of texts into account, would have to examine the network of text constituents as to its complexity, type of constituents involved, their ordering and their function for the text as a whole. But before we look at these classes of text constituents in more detail, let us ask first how we can arrive at a general description of texts which explains the relation between the different types of text constituents.

If we argue that each system as a process is directed through its function, then by analogy we may assume that the selection and occurrence of text constituents is determined by the communicative function of the text. In fact, we expect to find examples of expressive text constituents in poetry, advertisements, nursery rhymes, charms and litanies. A common feature of these types of texts is that they address the recipient in a specific role--as a consumer, voter, as a participant in a game or rite. They aim generally at exerting a certain influence on the addressee.

Referential text constituents, on the other hand, will generally occur in factual prose.

Whereas the production of texts connected on the expression level only generally requires a special skill or special education which only a minority undergo, every normal speaker is competent to produce or understand referential texts.

There is, then, a contrast, on the one hand between a factual text and on the other a persuasive text, a referential class and an expressive class of text constituents, a referential and an expressive kind of coherence, general competence and a special secondary kind of competence. Contrast notwithstanding, the relations between these pairs can, however, be explained through their dependence on various communicative functions. There is, in other words, a relation between specific communicative functions, specific types of text constituents, specific kinds of coherence they create and specific types of texts.

It is possible to oversimplify the relation between types of texts and classes of text constituents. It is, for example, easily observable that different types of texts are not characterized by mutually exclusive

text constituents. Pronouns, articles, synonyms occur not only in factual texts, but also in poetry. On the other hand factual texts may be interspersed with rhymes, parallelisms, and rhetorical figures. Oversimplification in this case resulted because we disregarded the possibility that a text may contain more than one communicative function. Poetic texts normally thought to possess expressive coherence, may also have a referential function, while factual prose may exhibit traits of expressive coherence as well as its normal referential function. Texts have, in other words, multiple goals. In the framework of General Systems Theory they might be called complex systems because a complex system may have multiple goals, not all of which are simultaneously obtainable, and more than one point of equilibrium. "Often as the system approaches one equilibrium point, the system is pulled out of equilibrium on another. This return (i.e. to one point of equilibrium) is usually the result of compensating actions on the part of the system". (Litterer 1969:14). By equilibrium we imply a certain average performance of the whole system.

Analogously, texts with more than one function may be said to have more than one point of equilibrium. The textual system may in such a case be pulled out of its equilibrium with respect to one goal to just the extent to which it approaches a second. In a poetic text, for instance, an increasing lack of referential coherence may be compensated for by a very dense network of expressive text constituents. This is often true of modern poetry, where destruction of referential coherence has reached a degree that no longer permits a paraphrase of its contents. Multiple goal systems, to be viable, are generally centralized. A characteristic of centralized systems is that one sub-system or element plays a dominant role, which is also termed a leading part. By analogy, the concept of the leading part explains why different classes of texts can be characterized by different types of text constituents, though no class occurs anywhere exclusively. It is the leading part or the leading sub-system that establishes the character of any particular type of text. There is a relation, in other words, which exists between types of texts, dominant classes of text constituents, dominant functions and dominant forms of coherence. This relation means that different types of texts differ through different types of text formation. Their relation is not that of various sub-languages or dialects to each other, but of different processes. These different processes have, however, a unifying similar structure, namely that of a network of interrelated components. The different manifestations of this structure are conditioned by the specific communicative purposes of different texts. A general approach to text analysis can then not be based on any particular linguistic properties, but only on such abstract features as dynamism, interrelatedness and coherence. It is, however, just this abstract general notion of the structure of texts which would make the study of the relation between different types of texts possible.

If we must conclude that text constituents do not consist of a particular grammatical inventory of expressions, but rather constitute specific patterns of recurrence, determined by the function of the text, then it seems natural that the acceptability and coherence of texts is a fact independent of their grammaticality. It might be interesting to explore the degree to which irregular grammatical features--deviances in a text--can be interpreted now to be in fact regularities of the textual process. Related to this question is the problem as to how far diversity in a language is describable as diversity of textual processes.

Another conclusion we might draw from the relative independence of text formation from grammar, is that we would now no longer have any reason to expect that texts should exclusively be described through features of the supposedly next-lower level, namely through syntactic properties. Rather it appears that elements of any grammatical level can function as text constituents.

Arguing that texts are not part of a grammatical hierarchy is not to say that the notion of hierarchy is not applicable to texts at all. In fact, we may think of a text as a system which contains sub-systems within sub-systems. It is just this hierarchic structuring which partly accounts for the complexity of texts. Such sub-systems are again texts or textual units. But it is evident that such a partitioning of texts into sub-texts cannot be equated with the segmentation of a text into sentences.

Each partial system can again be described with reference to the wider context in which it functions. The context in which the text as a whole functions is an extra-linguistic context. The fact that no system can be described without reference to a higher level system is the deeper reason for the impossibility of basing a description of texts solely on linguistic criteria. For that same reason there are no unambiguous boundaries of texts, but only relative boundaries, depending on the level in a hierarchy of sub-systems at which we look. It seems to me that this degree of relativity in determining the boundaries of a text agrees with the speaker's intuition who, under different aspects, may consider different sub-systems of texts as units.

We may assume that the functioning of the entire system depends primarily on its highest level sub-system and only to a secondary degree on the functioning of subordinated partial systems. All subordinated sub-systems are connected with the leading system through components they share. Applied to texts this would mean that the fulfillment of its communicative function depends first of all on the leading system or the text constituents that connect the text as a whole. For the reader or the hearer it is important to follow the leading system, to understand the overall coherence of a text or where a poem is concerned, the overall poetical structure. The more autonomous are these subsystems, the less disturbing is it to skip or forget subordinated

subsystems. On the other hand, registering all connections inside subordinate partial systems can never succeed in compensating for that information which is transmitted through the highest level system.

We assume, then, that texts form hierarchies, not linear structures. Grammatical relations between systems of text constituents and grammatical constructions may be viewed as part of this hierarchy, as relations between a grammatical sub-system and a system of text constituents. If we remember that such subordinated sub-systems play only a secondary role for the function of the text, it seems natural that texts may fulfill their communicative function though they may be ungrammatical, faulty or syntactically not well-formed.

In conclusion, it would seem that a clear distinction between the objects described through grammatical models and the objects of text analysis would benefit both grammatical and textual analysis. On the one hand we may expect that text analysis may throw light on such grammatical problems as deviation, or the ordering of syntactic units. On the other hand the analysis of texts, as sketched out here, will hopefully let us gain more insight into how language functions as a tool of communication. Whether its results are applied to stylistics, to information retrieval or to other studies of linguistic performance, its final aim is to examine language by examining the functions it serves.

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DISCUSSION

SESSION 3

Paul Friedrich, University of Chicago: (To Mrs. Oomen) I have a question. I wondered whether it wouldn't be more fruitful in many cases to approach texts from a more psychological or normative point of view than you seem to be doing, that is, from the point of view of the person composing the text. I can just give one quick personal example. There was a period of several years when I was writing some articles in cultural anthropology which contained very vivid content and really weren't very different from short histories and biographies. So, I decided to try to make it good writing. Because I was also linguist I developed a lot of principles of good writing. For example, one of these was that when you want a tight flow it often works best to have the subject matter at the end of one sentence match with the beginning of the next sentence. I have a lot of other principles of that sort. Now that's a horizontal linking relationship, and I think that it's linear concatenation in a way. I think one could very profitably find out what self-conscious writers do when they construct texts. That would be a beginning as against approaching the text as external data.

The other point I was going to mention is that in this and other talks there's often an alternation between the sentence and the text. From the point of view of the structure of sentences or intersentence relations, the text is not the most relevant unit. I would focus on something like the paragraph or whatever unit constitutes the immediate frame for linking sentences.

Prof. Ursula Oomen, Georgetown University: I think that the Systems Model suggested here does make possible a more psychologically oriented interpretation, in which you may take the speaker as well as the hearer into account.

As to the second question I agree that it is not sufficient to set up a dichotomy between texts and sentences. I have tried to point out that a text may include a number of hierarchically ordered communicative subsystems. These may sometimes be characterized as sentences

connected through a particular subclass of elements, which does not occur elsewhere in the text. Such subsystems may coincide with paragraphs. But I would suggest that we stay with the term 'communicative unit' or 'communicative subsystem', not only because this is the more inclusive term, but because the aspects of communication and coherence are more important to us than the aspect of organization into paragraphs.

Kenneth L. Pike, University of Michigan: (To Lenneberg) I would like to suggest, if he cares to comment, turning his point of view downside up because I think he has it downside up. That is, this last fall in Amsterdam with Sebeok in a conference, I was with a substantial number of the investigators of baboon communication, ape communication and the like. We put a week attempting to try to analyze linguistically their results with their films, discussions, etc. In the tagmemic point of view, which is supposed to be able to handle everything, we found it difficult to deal with anything like the analogue of phoneme, morpheme, word, tagmeme. Eventually, what happened was that we had to turn the whole thing the other way as primitive utterance-response. Then we were able to take the entire presentation of grooming ceremonies from the baboons, restate them in a way which was satisfactory to the researcher, starting with discourse as the primitive. Discourse being defined as an utterance and a reply. From this point of view then, it seems quite clear that what is necessary is to say that discourse is more primitive than, more basic than, of wider application than, less species specific than, language viewed as a set of sentences, which has always impressed me as being hopelessly inadequate. But, treating language as a set of utterance response discourses leaves room for immediate contact with all the data which we heard this fall on baboons, apes, and everything else. It defines communication as a social response, or utterance response you can mathematize with the ABC, the way I was doing with the other material this morning.

Eric Lenneberg, Cornell University: I am somewhat baffled as to how to react to this. The first thing that comes to mind, Ken, is a meeting I went to recently. The speaker was asked whether he was a Democrat or a Republican. He thought and thought, and then he said, "yes". I think I agree with you, but maybe not.

Walter A. Wolfram, Center for Applied Linguistics: Either Roger Shuy or Ralph Fasold could respond to this. I think it is important to distinguish between two general fields in sociolinguistic inquiry, namely, what might be called 'The sociology of language' and 'linguistic socio-linguistics'.

Now the sociology of language I think is the sort of research that Fishman has traditionally been doing in terms of bilingual situations-- who says what to whom under what circumstances and so forth. On the other hand, I think the sociolinguistics characteristic of Labov is quite different, namely, what can social factors tell me about linguistic theory? From this latter standpoint I think sociolinguistics is properly considered linguistics. For example, Labov's approach to sound change by considering variability and social factors and his approach to optionality in terms of hierarchical constraints on variability are attempts to solve linguistic problems. If we are going to be respected by the linguistic community, I think we're going to have to emphasize the contributions that sociolinguistics can make to solving linguistic problems. This is not to underestimate the importance of the sociology of language, of course, but simply to realize what aspects of sociolinguistics are most intimately involved in solving current linguistic issues.

Ralph Fasold, Georgetown University: I find it hard to disagree with the necessity for making a distinction along those lines. I kind of like to think in terms of some sort of a trichotomy. The first category is basically linguistics, but includes sociological inputs. This branch, like pure linguistics, would deal with a person's linguistic competence. Into this category I would put Labov. The second branch involves something which we might call communicative competence and involves what the individual knows about how to use his linguistic competence. Then finally a third thing which we may or may not want to call sociology of language which deals with sort of nationwide interaction between language and society; for example, the characteristics of multilingual nations as opposed to nonmultilingual nations, and what you want to do about language policy and language planning which do not involve individuals directly. That's the sort of a three-way kind of distinction I would be interested in trying out.

DIRECTIONS IN SOVIET PHONOLOGY

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In this paper I shall briefly report on two lines of Soviet phonological thinking; that of Šaumjan and Piotrovskij on the one hand, and that of Avanesov and Panov on the other.

I mention Šaumjan and Piotrovskij with reluctance and only because I cannot hope to avoid reporting about them.¹ The reason for my reluctance is that, to the degree that I understand Šaumjan's writings, they are vacuous; to the degree that they may not be vacuous, I fail to follow his line of reasoning.²

As a case in point, consider Šaumjan's (1962:17-18) definition of his hypothetico-deductive method: "The hypothetico-deductive method is a cyclical procedure, which begins and ends with the data. In this procedure there are four phases. (1) The data that need explaining are delimited. (2) Hypotheses elucidating the data are set forth. (3) On the basis of the hypotheses, deductions are made concerning new data beyond those that the hypotheses were originally designed to explain. (4) The examination of new data predicted by the hypotheses, and the testing of the plausibility of the hypotheses." Consider now a class in Introduction to Linguistics, presented with the following data: [pʰæt], [spɪt], [hæpi], [kʰæt], [stɪk] and asked to comment on the status of aspiration. If they were to formulate a rule '[voiceless stops] > [+aspirated] / #____, [-aspirated] elsewhere', they would have created a hypothesis requiring verification. The hypothesis would be borne out by new examples like [tʰæk] and [baeti], but contradicted by [čæt], and the rule would have to be reformulated.

If this sort of thing is all Šaumjan means, he is guilty of boring his peers with the obvious; if he means more, he either has failed to explain his views, or I have failed to understand them.

After Šaumjan's initial publication of his two-stage theory in 1960 and after his monograph on the same topic in 1962, there appeared yet another monograph in 1966, in part at least intended to show the

relevance of and applicability of Šaumjan's principles. Most of the articles produced in me a sort of double vision. An exception to this was the article by Piotrovskij and Podlužnyj, who demonstrated that what appears to be the Belorussian obstruent system of p, k, č, t, f, x, š, s, p', c, c', f', s' is in fact /p, k, k', t, f, h, š, θ, p', k', t', f', and θ'/. Reinterpretation of phonemic analyses, again, is old hat; one thinks of Americanists more or less automatically interpreting a /š/ as a /sy/ (unless [š] contrasts with [sy]). As a credit to Piotrovskij and Podlužnyj, I do believe that they are the first to fully state their principles of reinterpretation (they view the reinterpretation as going from 'physical' to 'conceptual' phonemes); furthermore, one can readily attach importance to what they are doing--for typological studies it makes a lot of difference whether one can view subsystems of the type 'p, t, t', 'k' and 'p, t, č, k' as isomorphic or not. Rephonemicization and its typological importance are pursued at length in Piotrovskij 1966. It is hard to see, however, that rephonemicization has any theoretical interest, beyond the commonplace that if one is going to do areal and typological studies, the phonological systems should be described in a fashion that makes them easy to compare and to contrast.

I am much happier to talk about the work of Panov (1967), which in its theoretical outlook is rather straightforward Avanesov (1956), the best known, at least abroad, of the so-called Moscow school of phonology (cf. Halle 1960). Let me take time to present two important notions of the school, without going into technical considerations--first that of the syntagmatic phoneme, the second of the paradigmatic phoneme. Neither notion can be equated with the notion of a phoneme that we learn in introductory textbooks. For one, the 'same sound, same phoneme' principle does not hold, and the bi-uniqueness requirement does definitely not apply in the case of the paradigmatic phoneme, and only in a modified way to the syntagmatic phoneme.

To illustrate the notion of a syntagmatic phoneme, I adduce the following examples (taken from Latvian):

A	B	C	
/mæ t u	mæ s tu	mæ z dams	'throw'
væ d u	væ s tu	væ z dams	'lead'
næ s u	næ s tu	næ z dams	'carry'
gâ z u	gâ s tu	gâ z dams	'tilt'
kâ p u	kâ p tu	kâ b dams	'climb'
glâ b u	glâ p tu	glâ b dams	'save'
nâ k u	nâ k tu	nâ g dams	'come'
aû g u	aû k tu	aû g dams/	'grow'

The obstruents in column A contrast as central vs. peripheral, within

central as stops vs. spirants, within peripheral as labial vs. velar, and as voiced vs. voiceless throughout.

p	t	k
b	d	g
	s	
	z	
Peripheral	Central	Peripheral

In column B, however, only two oppositions are possible--central vs. peripheral, and within peripheral, labial vs. velar. For a syntagmatic phoneme /s/ in position A (maximally distinctive or strong position), four features are distinctive:

s
+ obst
+ cent
+ cont
- voic

Only two features are distinctive for the [s] in column B:

[s]
+ obst
+ cent

with the spirantization and devoicing determined by the following dental stop.

If we examine column C, we discover, that although the sound that occurs there is [z], it has the same distinctive features as [s] in column B, i. e. + obst, + cent. Accordingly, it is a manifestation of the same phoneme as [s] in column B. Thus, there are 5 central syntagmatic obstruent phonemes in Latvian--four of them strong, namely /t/, /d/, /s/, and /z/, and one of them weak--/S/ with the allophones [s] and [z]. In syntagmatic transcription, columns B and C would read mæStu, mæSdams, væStu, væSdams, næStu, etc. The syntagmatic phonemes constitute a sort of a 'decoder's' alphabet--the information contained by either manifestation of /S/ is the same--either /t/, or /d/, or /s/, or /z/ could have been neutralized in that position.

The notion of paradigmatic phonemes likewise proceeds from the position of neutralization; here, however, attention is focused on what is neutralized. If we can say that (t) is neutralized as [s] or [z] (line 1, columns A, B, and C), then we can speak of a paradigmatic phoneme (t) with phonologically conditioned realizations [t], [s], [z], etc., and a

comparable paradigmatic phoneme (d) with phonologically conditioned realizations [d], [s], [z], etc. In paradigmatic transcription, line 1 would read (mætu, mættu, mætdams), line 2 would read (vædu, vædtu, væddams), etc.

Let me illustrate the above with a few English examples, first transcribed phonemically, then in a syntagmatic phonemic transcription, then in a paradigmatic phonemic transcription:

phonemic	/kæts, kæbz, pEnz, pEns/
syntagmatic phonemic	kætS, kæbS, pEnz, pEns
paradigmatic phonemic	kætz, kæbz, pEnz, pEns

Both transcriptions presuppose a somewhat more advanced level of analysis than that which we usually call phonemic; at the same time, it would be wrong to call either level 'morphological' or 'morphophonemic'.³

Panov's (Avanesov's) phonemics handle well several very difficult (albeit limited) areas of Russian phonology--such as assimilation, neutralization, and vowel reduction, all basic for understanding the rest of phonology (and morphology). The system is not without its problems, but the problems are of interest, and in fact point out new areas of research. By now my prejudice is clearly showing--I should like to require of any new theoretical proposition, before it deserves serious attention, that it should demonstrate how the theory handles at least some difficult problems well. I believe that the value of the approach by the Moscow school of phonology has been fully demonstrated, and that it should require serious attention by phonologists elsewhere.

I have bypassed and will not report on several other approaches precisely on this basis; e.g. I have read Revzin's proposals in detail; until we have some assurance that some difficult area can be effectively dealt with by using his principles, I would prefer not to participate in a discussion concerning them.⁴

NOTES

¹ Šaumjan first gained major attention by assuming the role of the devil's advocate in the staged discussion on phonology in the early 1950's, and found himself the target of various scholars who felt that they were working in best native tradition. For an extremely revealing account of the controversy, see Reformatskij, 35-46.

² I have not read Šaumjan and Soboleva; the work looks impressive and may well provide the substance that I have otherwise felt lacking.

³ It should not be surprising to learn that Panov came down hard on the side of the traditionalists in the recent debate on orthographic

reform (Panov 1964). Zinder and Bulatova provide good starting points for tracing the debate.

It is also curious to note that the Academy Grammar, hitherto a sterile compilation of facts, proposes to incorporate some of the notions of the Moscow school in its phonology section of its new edition (Švedova 1966).

⁴I have likewise not reported on quasi-theoretical articles, which, unfortunately, abound both in the USSR and in the West. Thus, Lekomceva shows that OCS has triads (p b v; k g x; etc.) which can be said to be in ternary opposition. I fail to see any purpose in her demonstration.

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THEORIES OF GRAMMAR IN THE SOVIET UNION

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The development of linguistics is not a function of the Gregorian calendar. Because of this, a discussion of linguistics in the 1960's cannot reasonably begin with some particular New Year's Day, cover exactly ten years, and stop. Even if such a procedure were ever acceptable in other cases, it is clear, in the case of Soviet linguistics, that neither 1960 nor 1961 would be a well chosen year with which to begin. For it happened that there was a period of great activity in Soviet linguistics which opened around 1955 and continued for approximately ten years. The latter half of the 1960's, while interesting and productive, was not so innovative or varied as the ten preceding years had been.

A discussion of Soviet linguistics in the 60's, then, must inevitably cover events of the 50's as well, and the activities of this extended period were a development of, and a reaction to, the events of the years which preceded them.

Soviet linguistics of the 60's, then, must be regarded in the light of the linguistics of the preceding periods. Let us begin by reviewing some of the trends which led up to the period under discussion.

In the early years of this century there were three major points of view in linguistic research in the Russian empire. One point of view was held by the Moscow school, a formalist group led by Fortunatov; the members of this school regarded linguistic form as the starting point of their investigations, and frequently applied statistical methods in their investigations. A second point of view was that of the Kazan'-Petrograd school under the leadership of Baudouin de Courtenay, whose work was very similar to, and finally merged with, that of de Saussure. The third point of view was that of various Russian mathematicians who investigated the amenability of language phenomena to mathematical treatment.

The later work of A. A. Shakhmatov and A. M. Peshkovsky represents a general fusion of the above trends. The following generation includes L. V. Shcherba, V. V. Vinogradov, and I. I. Meshchaninov. These last must all be regarded as exerting an influence in the period of the sixties; thus the traditions and influences of the older Russian grammarians have persisted to the present day and are still traceable in much of the contemporary work.

Beginning in the 20's, N. Ja. Marr tried, with his 'new doctrine of language', to interpret the content of grammatical categories on the basis of the social and class characteristics of the speakers; his aim was to formulate a type of linguistics which would be consistent with Marxist philosophy.

Later Stalin intervened in the linguistic discussions of the time in support of Marr; though Marr's views were not wholeheartedly accepted by the Soviet linguistic community, no one cared to continue discussion of the matter. Even though Stalin later came out against Marr, Soviet linguistics remained surprisingly free of controversy.

After Stalin died in 1953, constraint diminished, and linguists throughout the Soviet Union began extensive discussion of the interaction between linguistics and other sciences, as well as between linguistics and the official political outlook of the Soviet Union.

As a result, the linguistic discussions of the late 50's were particularly lively, and these discussions continued into the 60's. Almost all of the discussions centered their attention on criteria for developing grammatical systems conditioned primarily by intralinguistic factors, rather than by political and social factors, as previously.

Along with the development of a new diversity of views, two other factors profoundly influenced the development of Soviet linguistics during this period.

One was the fact that some two hundred widely differing languages are spoken within the Soviet Union; descriptive grammars of a number of these languages have been and are being written. Interest in foreign languages also grew rapidly; descriptive and pedagogical grammars of a number of widely-distributed languages came into being in consequence. As a result of this activity, many new phenomena which had not yet been accounted for by existing grammatical theories needed description. (This is especially the case with the Caucasian and Paleo-Asiatic languages.)

A second major factor was the growth of interest in computational linguistics, which began in 1956 with machine translation, expanded later to distributional analysis and other aspects of research, and has continued ever since through various vicissitudes.

During the years 1956-1960, then, there was active discussion of all phases of linguistics. The methods of structuralism were generally approved (though the underlying idealist outlook was roundly condemned

in a resolution passed in 1959). There was some quarrel with the principle of a strict separation of synchronic and diachronic linguistics. An Institute of Semiotics was founded, and a Committee for applied linguistics was established. Mathematical methods were applied to linguistics in order to help develop an overall theory of language.

The Sixties

In the fall of 1960, Shaumjan, praising structural linguistics as the highest form of linguistic study, called for equal cultivation of four fields: descriptive linguistics, historical linguistics, historical comparative linguistics, and typological comparative linguistics.

In the General Assembly of 1963, N. I. Konrad again called for the encouragement of diversity in research. In discussing topics to which he felt that linguists could give their attention, he mentioned the theory of signs as applied to language, the interaction of intralinguistic and extralinguistic factors, the interrelationship of qualitative and quantitative procedures, and typology.

These General Assemblies of the early 60's set the stage for much innovation and led to much debate. In the main, the debate was between the traditionalists on the one hand, and the structuralists, or mathematical linguists, on the other. The traditionalists harked back to linguistic approaches which had been in vogue before the period of Marr's most extensive influence, to both Czarist and early Soviet times. The structuralists, however, were strongly influenced by the activities of various Americans and became increasingly interested in the attempts of American linguists to find suitable models for the description of language. Inasmuch as these models came more and more to be based on mathematical principles, this group of linguists came to be known in the Soviet Union as 'mathematical linguists'. Representatives of this school looked for an approach that would be a clear departure from previous linguistics, stressing the point that pre-structural linguistics did not meet the requirements of scientific rigor in theory or in method.

Then, by 1963, a split began to be apparent between an older, more extreme group of mathematical linguists, which included Shaumjan, Andreev, Kholodovich, and Apresyan, and a newer, more moderate group which included Belecky, Admoni and Makaev. Some of the non-traditional linguists, however, such as Akhmanova, avoided a definite association with either of these groups.

There are thus three chief groups of Soviet linguists during the 60's, the traditionalists, the extreme structuralists, and the more moderate structuralists. We will attempt to give some of the flavor of each of these three groups, devoting more time to the extreme structuralists,

because of the greater amount of innovation in their work. Even while we make this three-fold division, however, we wish to emphasize the fact that it is a simplification of a highly diverse situation.

The Traditionalists

The traditionalists in part carried on the work of the earlier periods, and in part reacted against the innovations of the structuralists. Vinogradov, Meshchaninov, Shcherba, and Smirnitsky continued their work on the description of the languages both inside and outside of the Soviet Union, using an approach which stemmed largely from de Saussure. They also continued the debate on the position of language in the sphere of social phenomena, criticizing the points of the Marrist period and deprecating that period's incorrect assessment of the relationship between thought and language.

R. A. Budagov restates in his writings the point of view that man can and should influence further development of his own language. He points out that a language is a creation of people exactly as any machine is and that people can reshape it if they wish to do so. The general tenor of his arguments inclines more toward the establishment of an academy rather than toward experimentation with artificial or international languages. He also discusses the interrelationship of automatic (subconscious) language behavior with conscious language behavior, especially as the latter applies to literary effort.

The traditionalists as a group tend to accept the structuralists' accusation that they are not scientific, and to counter it with the charge that the structuralists, for all of their claims, are equally unscientific in other directions, and, in particular, that most of their innovations are merely innovations in terminology, and not in substance.

The Extreme Structuralists

Of the extreme structuralists, we have chosen to mention those whose work is more directly concerned with syntactic questions. Many of these will be found to have done extensive research in computational linguistics.

In discussing the first attempts at machine translation of English into Russian, Kuznecov describes the model used in the following terms: "The most effective methodology proved to be that according to which the text is continually divided into bipartite groups in each of which one member is dependent on the other. Plainly, such a division is essentially a division into syntagmas (in Fortunatov's sense)." (1956) The similarity of this methodology to that of dependency grammar is obvious.

The orientation of this first experiment and of the others which succeeded it was entirely pragmatic, inspired by the intensely pragmatic approach adopted here at Georgetown in the Georgetown-IBM experiment of 1954.

Then the Soviet approach became much more theoretical, in part because of the developing interest in mathematical models, and in part because of 'machine hunger'--the inability to secure enough time on the computer to conduct practical experiments.

I. I. Revzin

Revzin is one of those who has worked intensively with computational linguistics. He takes the point of view that, while there are no purely deductive or inductive sciences, linguistics, though primarily inductive, has aspects which make deductive methods very attractive. In its deductive aspects, linguistics can be structured as logic or mathematics is structured, and can make use of models in the mathematical sense of that word. Primary terms can be selected, and all else defined in terms of these primaries. Primary assertions (axioms) can be formulated, and all other assertions should be proved from these axioms. The model is independent of the object to which it is applied, and it can be applied to varying objects, such as phonology, morphology, syntax, or semantics. Therefore one model can serve to describe all aspects of a language. However, a model is a fiction--a toy--until its interpretation has been elaborated by application to an object. The only good criterion of a model is how it operates in practice, and all models should be put to practical tests in computational systems such as machine translation.

Revzin draws the analogy between Shcherba's 'active and passive grammar' on the one hand and generative and receptive grammar on the other.

N. A. Andreev

Andreev, a mathematically oriented linguist, discusses Shcherba's criticism of de Saussure, in which it is pointed out that the distinction into langue, parole, and langage is insufficient. Shcherba had pointed out that there are basic distinctions among the 'language' itself, which is an abstract structure, and the 'speech act', which is the realization of certain structural possibilities of the language on a particular occasion (spoken language), and 'speech', which is a more formalized realization, usually intended for tradition (written, especially literary, form).

Andreev points out that language is not a list of units or a set of lists of units, though it is these in part. There is also a set of

relationships between the various units and the various lists and, moreover, a procedure for the development of these relationships, which procedure he likens to an algorithm--the algorithm of text synthesis.

To Shcherba's three divisions Andreev adds another--'speech probability'. He points out that in Russian, approximately fourteen percent of the roster of phonemes consists of vowels, but that in actual speech forty-two percent of the sounds occurring are vowels. Similarly, the language has a roster of six cases, all apparently equal, while speech uses the cases in by no means equal proportion so that the nominative and genitive cases are used several times more frequently than the instrumental and many times more than the dative. He feels that considerations of this sort require us to regard language as a quadruple system, rather than as the triple system of Shcherba, or as the, in Shcherba's interpretation, binary system of de Saussure.

S. K. Shaumjan

Shaumjan commends the advances over immediate constituent analysis made by Chomsky in his transformational analysis, but takes issue with the question as to whether the simpler analysis is necessarily the better analysis. While he feels that it is better in principle, he would like to make a distinction between descriptive simplicity and inductive simplicity. The former he feels is unimportant. The latter he equates with explanatory power, so that on the whole his criterion for judging the effectiveness of a model is only its explanatory power. He then proposes his own model--the Applicational Generative Model.

Shaumjan, in describing his applicational model, takes issue with other transformational models, and in particular that of Chomsky, pointing out that they all have the logical structure of so-called concatenative systems, in which the formal objects are conceived of as finite linear strings of symbols. Instead, the applicational model falls into the category of ob systems, in which the combinations of formal objects are conceived as entering into structures resembling a genealogical tree. The creation of the applicational model implies a reformulation of the task of generative grammars. The older idea is that the task of a generative grammar should be the direct generation of the sentences of a real language. However, a different task may be set. A generative grammar may be required to generate also an abstract ideal language, and the series of correlations which connect it with a natural language. In this case, the generative process must consist of a stage in which ideal linguistic objects must be generated, constituting a language which Shaumjan refers to as a genotype language, and of a second stage in which the objects of the genotype language must be transformed into the objects of a real language, or phenotype language, by means of correspondence rules. There is some question in

Shaumjan's mind as to how useful, or even interesting, other types of generative models might be, apart from practical applications such as machine translation. He feels that it will be at least no less interesting to develop generative models for generating ideal languages. He finds that this latter type of generative language is interesting, particularly in the area of semantics.

Shaumjan's applicational generative model consists of four hierarchically connected parts or submodels. These are the abstract generator, the word generator, the phrase generator, and the transformation field generator.

1. The abstract generator generates the genotype. Any linguistic unit, however complex it may be, may be decomposed into elementary semiotic objects analogous to the distinctive features in phonology. These are semions and episemions. Individual semions or combinations of semions constitute semion bundles. Episemions are semiotic classes or types to which semion bundles belong, so that any semion bundle can be said to represent a definite episemion. Both semions and episemions are therefore abstract analogues of grammatical categories.

Bundles of semions can be generated by the application of semions to other semions. There are four main schemes of semion bundle generation. These are the iteration scheme, the reduction scheme, the conversion scheme, and the connection scheme.

The iteration scheme applies the same operator-identifier to an operand any number of times; the application of an operator-identifier to the operand produces a structure which is directly equivalent to the operand. This process of generation may be continued to infinity, although, of course, the practical limitations of the language made such infinite generation improbable. (These are of the type of catenary structures.)

The reduction scheme applies a semion identifier to an operand which is also an identifier. The result is applied to a new operand which is also an identifier. Eventually an operand is reached which cannot serve as an identifier with respect to any other semion bundle. Such an operand is the nucleus of the reduction structure. (These are endocentric structures.)

The conversion scheme applies a conversion-operator representing one episemion to an operand representing another episemion, and achieves a combination which represents a different episemion from that represented by either the operator or the operand. (These are exocentric structures.)

The connection scheme generates semion bundles by means of connectors. The semion bundles joined by the connector must represent identical episemions, and the connector produces a coordinated construction. (These are coordinated structures.)

2. The word generator introduces the concept of an empty semion, which can only be an operand to which relators are applied, and which operates in such a way that it is the relator which determines which episemion is represented by the combination. A root is an empty semion. Elementary words are generated by applying one of a finite set of word relators to a root. Other words may be generated by applying these relators to the elementary words. In this way, the words of the lexicon (R-derivatives) are generated from roots in such a manner that it is at all times structurally clear which root is involved and by what process or series of processes the word was constituted. (This is derivational and inflectional morphology.)

3. The phrase generator takes the set of words (technically infinite) and by means of a finite set of relators, a connector or an adnector, generates both elementary phrases, which have the structure relator-plus-word, and more complex phrases in which more relators can occur, and in which connectors such as and, but, or or the adnector, which is the negative, can also occur. The order of phrase members is not fixed. (This is syntax.)

4. The generator of phrasal transform fields is not a strictly necessary part of the applicational generator model, which could generate every possible structure without this fourth component. It would not be possible, however, to show with any degree of simplicity or clarity that 'The boy wrote the letter' is in any sense related to 'The letter was written by the boy' if some transformational level were not introduced. The input for this level is the infinite set of phrase sequences generated on the previous level. With these there is used a finite set of phrasal transform field operators (phrasal T-operator). Each of these is a set of ordered relators which may be designated as phrasal T-suboperators, each of these being a permutation of n relators taken k at a time, with the exclusion of permutations which are mirror images of other permutations and also of permutations which contain adjacent relators in an order forbidden by the adjacency matrix. The adjacency matrix is constructed by listing all possible relators both across and down to form a grid each unit of which is thereupon filled in with a plus if the two items which intersect at that point are compatible and with a minus if they are not.

A graph is then constructed in which all of the R-derivatives from each member of the operand are listed across, and the various relators are listed down. Lines are then drawn from each point of intersection in the first column to those points of intersection in the second column permitted by the conditions of the adjacency matrix. Then those items in the second column are connected with those items in the third column to which they may stand adjacent. This process is continued until the last column is reached. All phrases which can be constructed from the graph in such a way that each phrase consists of one item

from each column is considered to be an operable phrase. Each operable phrase can now be treated as an operand and subjected to the same procedure of graphing. This procedure is continued until the generative process stops. Not only will all possible combinations be generated, but the inherent relationships will be recoverable so as to permit inter-transformations among such groups as

The brilliantly glowing embers
 The embers glow brilliantly
 The brilliance of the glow of the embers
 The embers' brilliant glow
 etc.

A. A. Kholodovich

Among those Soviet linguists who have devoted themselves to the interrelation between items, the work of Kholodovich is perhaps the best known.

Kholodovich assumes that a word may function as the nucleus of an environment in such a way that the nucleus determines both how many items shall occur in the environment, and what their specific nature shall be. The most obvious example of forms which determine environment is the form class of verbs.

Each verb can be said to have an optimal environment which may vary from a zero-position environment to a three-position environment, or, perhaps in some cases, a four-position environment. If all of the positions are filled in any particular case, the environment is optimal. If some positions remain unfilled, the environment is inadequate. If more than the optimal number of positions are filled, with certain restrictions, the environment is redundant.

An example of a zero-position verb in Russian is morozit 'it is freezing'. (The same point of view may be taken for the English verb, if we consider that English has different syntactic requirements from Russian, so that, in English, the presence of a subject independent of the verb is a necessity, and in such a case the subject is supplied by the pronoun it, which has only a syntactic function, but no extralinguistic reference.)

An example of a one-position verb is cvety zavjali 'the flowers faded'. Here, both in Russian and in English, a subject is required, subject being understood in the logical sense rather than the syntactic. It is usual in Russian, and obligatory in English, that a syntactic subject be present.

An example of a two-position verb is on ljubit rebenka 'he loves the child'.

An example of a three-position verb is on dal ej khleba 'he gave her some bread'.

Environments can be further distinguished as close or open. A close environment is one which is determined by the nucleus alone. An open environment is one which is determined basically by the nucleus, but where one or more of the items in the environment is itself a nucleus, and in turn requires that other environmental be introduced. An example is on vsadil nozh v derevo 'he stuck the knife into the tree', which is a three-position environment of the close type, whereas on vsadil nozh v nozhku 'he stuck the knife into the leg' is a three-position environment of the open type since nozhka 'leg' is in itself a nucleus with a one-position environment, which could be filled by such a form as stul 'chair' to give on vsadil nozh v nozhku stula 'he stuck the knife into the leg of the chair'. This last environment, then, is to be regarded as an example of an optimal open environment and not of a redundant environment, since redundancy is judged by the requirements of the nucleus, and not also in terms of any dependent nucleus.

Kholodovich then develops a procedure by which linguists can test for environment (particularly of verbs) so that linguists will achieve the best possible description of the environmental possibilities without omissions or vaguenesses.

1. Find a string of words containing a possible nucleus.
2. Identify the given string as having a redundant, an inadequate or an optimal environment.
3. Determine in each case whether the environment is open or close.
4. Classify the environment as to whether it is homogeneous or heterogeneous. A homogeneous environment is always filled by members of the same morphological class; a heterogeneous environment can be filled by members of different classes. If each item in the environment can be filled by members of only one morphological class, the item is a constant member of the environment; if not, it is a variable member of the environment. If all of the members of the environment are morphological classes, the environment is a one-level environment; if one or more item is a syntactic structure, such as a clause, the environment is a multi-level environment.
5. Decide to what semantic classes the items which can occupy each position in the environment can stand. Clearly, if different semantic classes are involved, there must be a separate classification for otherwise similar environments.
6. Determine whether each member of the environment has a different extralinguistic referent or not. In the two two-position environments on uvidel inzhenera 'he caught sight of the engineer' and on stal inzhenerom 'he became an engineer', the first has different referents for the two environmental positions, whereas the second has the same referent for its two environmental positions.

Yu. D. Apresyan

Apresyan interests himself in structural lexicology. Rejecting the use of semantic characteristics (*figurae* in Hjelmslev's terms) as being too restrictive, he discusses the possibility of a distributional analysis combined with a transformational analysis.

For his purposes he finds Chomsky's marked phrase (essentially anything which is syntactically acceptable) too broad, and Harris's marked phrase (essentially whatever is both syntactically and semantically acceptable) too narrow. His own marked phrase will be somewhat between these two, and the exact definition of it will depend upon the results of his analysis.

In his distributional approach he accepts all marked phrases which are oriented. Oriented phrases are finite strings of words, each of which is syntactically connected directly with one particular word, which is the nucleus. In one sentence there may be many oriented phrases overlapping as each lexical item in the sentence is taken as a nucleus. Items which occupy the same position in a same structure are to be directly compared. There are three types of position:

- (a) mandatory: those positions which must be filled (in 'He throws the ball', if throws is the nucleus, then the positions occupied by he and the ball are mandatory),
- (b) imaginary: those positions which do not exist at all (in 'He throws the ball', if he is the nucleus, the position preceding he is imaginary), and
- (c) optional: those positions which are neither mandatory nor imaginary (in 'he throws the ball against the wall', against the wall is optional).

By the reduction and expansion of oriented phrases and the comparison of those which have the same syntactic distribution, classes of lexemes can be evolved which are in direct semantic correlation with each other.

In his transformational approach Apresyan applies various transformations to each phrase. He proposes six types of transformation: passive, impersonal, prepositional, aspectual, derivational, and complement-adverbial; (these various transformations are clearly more suitable for a discussion of Russian than they would be for a discussion of English). If two phrases have the same distribution, and a transform is applied to each, if the resultants do not lose markedness, then the phrases are the same. If only one retains markedness, they are different.

There are thus three levels of analysis; the morphological is lowest and the two syntactic levels, namely the distributional and the transformational, are respectively higher. A differentiation of two lexical

items on a lower level automatically means their differentiation on a higher level. The non-differentiation of two lexemes on a lower level does not necessarily imply their non-differentiation on a higher level.

This process produces classes of equivalent lexemes the members of which can be usefully contrasted with each other.

Of the two approaches, the purely distributional approach does not differentiate sufficiently and it may provoke recourse to statistical counts, which are really not germane in language study. The transformational approach cannot be operated without the distributional. Therefore the best approach is what Apresyan calls the operational approach, in which both distributional and transformational operations are to be applied to the lexicon in an optimal fashion to be determined by practice. The operational approach leads to a hierarchic structuring of the lexicon in which it is possible to calculate mathematically the relative proximity of items to each other, and to select those items which are most closely proximate for contrastive discussion of their semantic content.

The Moderate Structuralists

As the 60's developed, a number of Soviet linguists came to adopt an eclectic position, in which the extreme points of view of the traditionalists on the one hand and of the mathematical linguists on the other blended and softened. It is not always easy to decide to assign a linguist to this modern group. We have chosen to mention the following.

Lomtev takes issue with the proponents of distributional analysis. While a great deal of work might be done in classifying language units and discovering their valences and various patterns of complementary distribution without any recourse to meaning, he feels that distributional analysis must always fall short of achieving its aims. Only the syntactic, or intralinguistic values can be discovered from this process. The meaning, or extralinguistic value, is not generated by the environment, and so is not susceptible of the type of analysis involved in distributional analysis. There is no doubt that distributional analysis has its uses, but a complete linguistic analysis cannot be achieved by this means.

Lomtev also takes issue with transformational analysis. He recognizes its importance, but he feels that too much has been claimed for it, and that there are areas of language analysis which might be more efficiently handled in other ways. He particularly points out that transformational analysis does not have the formal tools for identifying the valences of syntactic units. In particular, while transformational analysis can explain that a particular combination is ambiguous because it can be derived by two different transformation processes from two

different underlying structures, it does not have the means of resolving the ambiguity in any particular case.

Lomtev deprecates the fact that it is now unfashionable to correlate Marxism and linguistics. He feels that many of the so-called new approaches are really old-fashioned and reactionary whereas Marxist principles are the only truly progressive principles.

E. A. Makaev

Makaev, like many other Soviet linguists, takes de Saussure as his starting point, and develops a picture of language in which three components must be regarded. The 'system' consists of the list of units and their interrelationships, which allows them to interact. The 'standard' is a list of rules as to what types of interaction are permitted, and what types prohibited; this is, in effect, the systemzwang of earlier linguists. The 'usage' is the realization of the system in actual speech.

Makaev persists in seeing language as a series of levels, which he gives as phonological, phonomorphological, morphemic, syntagmatic, lexical, and meta-semiotic or stylistic. He agrees with de Saussure that there are bilateral or Janus-like units, but only in the lower levels. The higher levels have units of greater architectonic complexity. It follows, therefore, that a model, or any approach to description, which is valid and operable on one level is not necessarily so on other levels, so that a series of linguistic theories might be necessary in order to describe the language competently.

V. G. Admoni

Admoni is a theorist, a practician and a historian. His theory includes the following points. Both synthetic and analytic procedures can and should be equally developed; each structure can be regarded either as a unit or as a composite. Thus grammar is a unified hierarchic structure, and the old ideas of unilinearity of the speech chain (which arose primarily from interpretations of de Saussure's work) must give way to a view of language as a multi-linear formation of considerable complexity. He removes the phonology and the lexicon from this hierarchical structure, but, since he retains derivation as an area of grammar, a certain amount of lexicology remains.

His practical work has concentrated largely on German; he has produced a German grammar which conforms to his theoretical position; it is without a section on phonology.

His history of Soviet linguistics is of considerable interest.

Other Aspects

Since the focus of this paper is on syntactic models in particular, it is possible to mention only a few other aspects of Soviet linguistics in passing.

Approaches to lexicology and to the determination of parts of speech have centered around Fortunatov's dictum that the criteria must not intersect, if indeed more than one criterion is to be used. Steblin-Kamensky takes the point of view that multiple criteria are necessary if any degree of accuracy is to be achieved. This implies that a description of a language does not meet the requirements of a logical system. Word formation in various languages is discussed in the works of Smirnitsky, Zhirmunsky, Akhmanova, and Jarceva.

Computational linguistics continues to flourish. The development in the Soviet Union has been parallel to that in the United States in that it began with a thoroughly practical attack upon the problem, then shifted to theoretical consideration of the problems involved, and then sought for a means of combining the theoretical and the practical. This third stage was largely the result of difficulty in obtaining time on a computer in the Soviet Union, whereas, in the United States, it has been the result of the diminishing availability of funds to support research. The ALPAC Report of 1966, with its deleterious effect on machine translation research in this country, had only a brief effect in the Soviet Union, and is now roundly criticized for its shortsightedness by such writers as Mel'chuk, Kulagina, and Rosentsveig. Bel'skaya's English-to-Russian translation system may be cited as a sample of Soviet achievements in this area, but there are numerous linguists in this field, including Dr. Akhmanova whose paper will shortly follow.

As the decade moves to its close, there is greater difficulty in evaluating developments because of the shortening perspective in which we must regard them; an editorial by Mel'nichuk in a recent issue of *Voprosy Yazykoznanija*, in which he discusses system and structure is of interest in this respect.

Even more difficult, of course, is to turn and see what the future holds. If the present trends continue, it seems probable that Soviet linguists of the 70's will continue their work in diverse areas and without espousing extreme points of view; they will not stress the theoretical at the expense of the practical, but will continue to build test models rather than game models.

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SOVIET LEXICOLOGY IN THE SIXTIES

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The sixties, if compared with the fifties, show an obvious progress in Soviet lexicological studies. Until Stalin's death, in 1953, the Soviet scholarly output was at a virtual standstill. It took approximately another three years before Soviet scholarship could feel sufficiently liberated in order to produce valuable works. This brings us up to 1956-7. Some specialized periodicals, dictionaries, and collections began to appear in those years.

One of the peculiarities of the Soviet practice which begins to manifest itself toward the middle of the sixties is a profusion of collections of articles, usually grouped around a theme, often with very similar titles, which are bound to be easily confused. In normal conditions the articles forming such collections would have appeared in specialized periodicals. In the Soviet Union, on the contrary, a specialized periodical devoted to lexicology, *Leksikografičeskij sbornik*, ceased to appear in 1963 in order to be replaced by various non-periodical collections, with various titles and various contents, as it was explained in a prefatory note to the collection *Sovremennaja russkaja leksikologija* (1966). This new policy is by no means to facilitate the researcher's work.

The hey-day of such collections was the period between 1963 and 1966. These years seem also to be the culminating period in lexicological research in the U. S. S. R. After 1966 the wave of collections subsided and the scholarly output was considerably slowed down.

During that period two books were published on Russian lexicology--its subject, aims and methods--by Šanskij (1964) and Kalinin (1966). Both are of a popular type and highly traditional, especially the second.

In the survey which will follow, only the most important publications will be discussed, almost exclusively those published in a book-form. The very numerous articles had to be disregarded for lack of space. With a few exceptions the survey will concentrate on works published in

Moscow and Leningrad, the provincial publications being difficult to obtain and generally on a scholarly level inferior to that of the capitals. The discussed publications deal principally, but not exclusively, with the Russian language.

Dictionaries, except etymological and dialectal, are excluded from this survey, as are studies of derivation and phraseology. Publications of onomastics have not been discussed either.

Etymological research seems to have been popular in the sixties.

The awkward situation with etymological dictionaries of Russian is best illustrated by the fact that the only complete and up to date dictionary was published in German and outside the U. S. S. R. by Vasmer (1953-8). This dictionary is now being published in Russian translation by Trubačev at a rather slow speed: out of the four projected volumes only two have appeared in 1964 and 1967. A few additions of the translator are marked by square brackets, but the less numerous omissions go unmarked. Vasmer's work deserves high praise, although it should not be forgotten that Vasmer was mainly interested in establishing the etymology of a word and much less in studying the word's history.

Parallel to this translation another etymological dictionary is being published under the editorship of Šanskij, on a much larger scale, but showing the same desperately slow speed. So far, between 1963 and 1968, three parts of Volume I have been published, each of them bound and with separate pagination. They cover the first three letters of the Russian alphabet.

The same author, with Ivanov and Šanskaja had produced in 1961 a short etymological dictionary, with a minimum of explanations. Another short etymological dictionary, by Cyganenko, has just appeared. It is obviously intended for a wider audience, is unoriginal, covers less words, but gives more detailed historical information. The purely linguistic derivations, especially those involving Common Slavic, are not always reliable.

As may be seen all these works are limited to the Russian area. Etymological dictionaries of the Ukrainian and Belorussian languages seem to be in preparation, but so far nothing has been published.

An excellent etymological work by Trubačev (1966) on the Slavic handicraft terminology should be mentioned here.

There are two non-periodical series devoted to etymological studies and edited by the two promoters of etymological research in the U. S. S. R. One, Etimologičeskie issledovanija (1960 sqq.), is, beginning with its third volume, edited by Šanskij and somehow connected with his etymological dictionary; so far six volumes have been published. The other, Etimologija (1963 sqq.), is edited by Trubačev and is not limited to the Russian language. So far five volumes have been published. Since etymological studies are popular in the U. S. S. R., etymologies frequently appear in periodicals and collections.

The long awaited etymological dictionary of the Komi language by Lytkin and Guljaev (1970) has been finally published. To a large extent it covers the whole East Finnic area.

Before passing to works devoted to Russian lexicology proper, three books, which deal with Slavic lexicology in general or with separate Slavic languages, should be mentioned. They are: Ivanov's and Toporov's (1965) monograph on the nomenclature of the Slavic paganism, which is analysed with the help of unnecessarily complicated terminology and symbols; L'vov's (1966) excellent work on Old Church Slavonic lexical variants of fifty-two concepts; a collection of eight studies on various Slavic languages: Slavjanskaja leksikografija i leksikologija (1966).

The basic vocabulary of the modern standard Russian is a mixed one, since it includes both national Russian and Church Slavonic elements. The main problem of Russian lexicology is the relation between these two groups of elements, and this problem is rather of a historical nature. There is a small group of words which are of an undoubtedly Russian origin, and a slightly more numerous group of words of an undoubtedly Church Slavonic origin. Between these two 'marked' groups lies a much more numerous mass of 'unmarked' words which are common to both Russian and Church Slavonic. Their attribution to the one or the other of the marked groups entirely depends on the conception of the nature and origin of modern standard Russian.

The origin of modern standard Russian is not yet entirely clear. It may be, historically, either Russianized Church Slavonic or Slavonized Russian. In the first case the 'unmarked' group should be termed as basically Church Slavonic, in the second case--as basically Russian. The possibility of such a double approach accounts for the unusual oscillation in evaluating the percentage of Church Slavonic elements in the standard language. For different scholars these elements oscillate between 6 and 66 per cent (Vinogradov, 1961:6-7). With the actual tendency in the U.S.S.R. to emphasize the national side of any cultural development, it is not surprising that the importance of the Church Slavonic vocabulary should be toned down. In the already quoted Šanskij's work (1964:69) the genuine Russian vocabulary of the standard language is said to amount to more than 90 per cent.

The result of such an approach is that Church Slavonic is only too often simply termed Old Russian, and in many, but by no means all, lexical studies dealing with the pre-eighteenth-century language the two languages--Church Slavonic and Russian--are kept apart. Moreover, the main interest is devoted to the vocabulary of the old non-literary, administrative, language which is genuinely Russian. There is still room not only for a comprehensive study of the vocabulary of the old literary, i.e. Church Slavonic, language, but even of individual Church Slavonic texts. If Church Slavonic vocabulary is treated, it is

usually in those of its features which may be easily opposed to parallel Russian features, as for example pleophony and similar developments. The problem of 'unmarked' words, common to Russian and Church Slavonic, has never been discussed in the sixties.

Among works dealing mainly, though not exclusively, with Church Slavonic as literary language in ancient Russia should be mentioned Kovtun's (1963) monograph on Russian medieval lexicography, and a collection of eight studies Pamjatniki drevnerusskoj pis'mennosti (1968).

Several collections of articles include various studies of both Church Slavonic and Russian elements of the vocabulary: Iz istorii slov i slovarej (1963); Issledovaniya po istoričeskoi leksikologii drevnerusskogo jazyka (1964); Leksikologija i slovoobrazovanie drevnerusskogo jazyka (1966); Russkaja istoričeskaja leksikologija (1968); Issledovaniya po slovoobrazovaniju i leksikologii drevnerusskogo jazyka (1969).

A few monographs deal with the vocabulary of the Old Russian, non-literary, language, namely those by Bricyn (1965), Poroxova (1969), and Kotkov (1970).

The second half of the seventeenth and the eighteenth century was the period of the merging of Church Slavonic and Russian into the henceforth unified standard language. This process has attracted the curiosity of Soviet scholars which resulted in a number of successful lexical studies. Three books should be singled out: an analysis of the vocabulary of a translation from Polish of the end of the seventeenth century, by Isserlin (1961), and two excellent monographs on the scientific terminology of the first third of the eighteenth century, by Kutina (1964, 1966). The following three collections cover the whole of the eighteenth century: Materialy i issledovaniya po leksike russkogo literaturnogo jazyka XVIII veka (1965); Processy formirovaniya leksiki russkogo literaturnogo jazyka (ot Kantemira do Karamzina) (1966); Russkaja literaturnaja reč' v XVIII veke (1968).

The end of the eighteenth century and the beginning of the nineteenth century form the transition to modern standard Russian. The literary vocabulary of this period is covered in Levin's (1964) substantial monograph, while Veselitskij (1964) treats the development of the abstract vocabulary in the first third of the nineteenth century. The collection Obrazovanie novoj stilistiki russkogo jazyka v puškinskuju epoxu (1964) supplements both works. In most of the works devoted to the literary vocabulary of the eighteenth and the beginning of the nineteenth century the Church Slavonic elements are treated almost exclusively from the stylistic point of view, against the background of the standard language which is tacitly supposed to be of purely Russian origin.

The quiet and slow progress of the well established standard vocabulary in the nineteenth century seems to have tempted Soviet scholars much less than the development of the previous, rather stormy period.

It was, however, thoroughly and competently explored in an impressive monograph by Sorokin (1965).

The remainder of the lexicological studies is devoted to different aspects of contemporary vocabulary. A general, and not very successful, description of the contemporary vocabulary, with a socio-linguistic bias, may be found in a collective work under the editorship of Panov (1968), which forms the first part of a four-volume description of contemporary Russian. As in previous sections a number of collections include some interesting studies on the contemporary vocabulary: Razvitiye grammatiki i leksiki sovremennoj russkoj jazyka (1964); Razvitiye leksički sovremennoj russkoj jazyka (1965); Sovremennaja russkaja leksikologija (1966); Očerki po sinonimike sovremennoj russkoj literaturnoj jazyka (1966); Leksičeskaja sinonimija (1967); Issledovaniya po estetike slova i stilistike xudožestvennoj literatury (1964).

A few individual works may be added to these collections. A good monograph on verbs with a borrowed stem by Avilova (1967); a study of the word-variants in Russian by Rogožnikova (1966); as well as two booklets of a rather inferior quality, on Russian semantics, by Šmelev (1964), and on Russian synonyms, by Palevskaja (1964).

With the general trend to emphasize the national character of the standard vocabulary it is not surprising that a special interest should be devoted to this vocabulary's most popular and most autochthonous strata--the dialectal words. Since 1965 Filin is editing a Russian dialectal dictionary. It will include all dialectal dictionaries and other printed material so far published as well as the available manuscript sources preserved in various archives. The concentration of all the data hitherto dispersed in hundreds of works will naturally immensely facilitate the research in the field of dialectal lexicology. The list of printed sources (I:18-99) is so far the fullest bibliography of the subject. The dictionary's progress is much faster and more regular than that of the etymological dictionaries, so that six volumes have already been published covering the words from A to Gon.

The publication of this all-embracing dictionary did not stop the more limited regional dictionaries from appearing, as, for example, those of the regions of the Baltic, the Middle Urals, and the Middle Ob River. The Dictionary of the Pskov region differs from all the others since it is both contemporary and historical. So far only the first volume has appeared, from A to bibiška. A volume containing material for a dialectal dictionary of the Belorussian Poles'e region (1968) should also be mentioned.

As for other branches of lexicology, various collections of studies of dialectal vocabulary have been published of which three at least deserve to be mentioned: Slovo v narodnyx govorax russkogo severa (1962); Leksika russkix narodnyx govorov (1966); Slovo v russkix

narodnyx govorax (1968). Each of these books contains twelve studies. A reliable study by Merkulova (1967) of the popular nomenclature of herbs, mushrooms and berries should not be omitted.

The sixties have also brought a surprise: the professional secret languages, which were thought to have been swallowed up by the general slang, are still alive, so that Bondaletov (1965) and Alekseev (1965) were able to devote four studies to them. Bondaletov (1968) has also collected words of these secret languages from older dialectal dictionaries.

Studies of various slangs, abandoned since the thirties, have made a timid come-back in an article by Skvorcov (1964) on the student slang.

On the whole, in the sixties, lexicology seems to have fared much better than other sections of Soviet linguistics. This is certainly due to the fact that lexicological works were solidly built on precise and minute observations of concrete linguistic material and not on preconceived general theories however fashionable they might appear. It is significant that Apresjan's early attempt to inaugurate a method of structuralist lexicology should have remained totally impracticable.

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PROBLEMS IN SOCIOLINGUISTICS IN THE SOVIET UNION

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It could sound like a paradox if one suggested that the linguistics of a socialist society 'discovered' sociolinguistics in the 1960's. But this is exactly what happened in the Soviet Union. In spite of all earlier Soviet interest in the relationship between language and society--to mention only such names as I. A. Baudouin de Courtenay, L. I. Ščerba, E. D. Polivanov--something called sociolingvistika, social'naja lingvistika began to exist as a recognized and separate discipline in the Soviet Union only during the last decade. How much this evolution reflects the dialectics of the development of scientific thought in the Soviet Union, how much of the credit for it may be given to a general reaction to structuralism in linguistics at home, and how much it is due to the stimuli coming from the West remains to be determined by a historian. The fact is that the sociological aspect of language once again gained relevance in Soviet linguistics only after the official recognition, early in the sixties, of the role of the social sciences in the building of communism, which of course came after the dethroning of both the Marrian 'new theory of language', and Stalin's cult of personality.¹ Only then did the language and society theme become the topic of the day.²

1. What exactly the terms as sociolingvistika, sociologičeskaja lingvistika, social'naja lingvistika, sociologija jazyka, lingvističeskaja sociologija, sociologo-lingvističeskoe, lingvo-sociologičeskoe issledovanie mean in the Soviet Union today³ is by and large pretty close to what has been known here under the general label of 'sociology of language'. However, there have recently been some attempts made in the Soviet Union to arrive at a more precise and perhaps more rigorous definition of this concept. A senior philologist, V. M. Žirmunskij,* who--among other things--had been working in one aspect of sociolinguistics--social dialectology--as early as the 1930's, and who has

*V. M. Žirmunskij died in January 1971. At the time of the delivery of this paper its author did not know of his death.

recently become a leading figure in the organization of the language and society research at the Leningrad Linguistic Institute of the Academy of Sciences, uses the term in a way rather close to one of our understandings of this concept. In general, Žirmunskij's social'-naja lingvistika covers both levels of language study, synchrony and diachrony; it treats "the problems of social differentiation of language in a class society at one point in time of its evolution," and "the problems of the process of the social evolution of a language and its history as a socially differentiated phenomenon" (Voprosy social'noj lingvistiki 1968, 14). Having been himself mostly concerned with the problems of social dialects in the past and their growth into national languages (his major contributions in this area include National Language and Social dialects, 1936; History of the German Language, 1939, 1948, 1956, 1965; German Dialectology, 1956), it is clear that his interest lies in philologically oriented historical social dialectology. Of other promising topics Žirmunskij specifies as productive the following: the theoretical and practical problems of the relation between a national language and its local dialects, the problems of the formation of literary languages, and again, the theoretical and practical study of concrete tasks of Soviet internal language policy (Voprosy social'noj lingvistiki 1968, 20).

For Ju. D. Dešeriev, a relatively younger scholar, whose work is rooted entirely in the post-Stalin sociologically oriented period, socio-logical investigation seems to be more relevant on the level of application and language planning. One aspect of this level concerns general theoretical problems of sociolinguistics, (language as a social phenomenon and its place and role in social evolution; the methodology of sociolinguistic research; social factors in the functioning and evolution of language; social differentiation of languages; the problem of the evolution of language; the relation between functional and structural evolution of languages, etc.). Another aspect concerns the sociolinguistic problems and evolutionary possibilities in contemporary Soviet society (regularity of evolution of literary languages of the peoples of the U. S. S. R.; the relation between the social functions of Russian and other languages in the Soviet Union; the role of intranational language--i.e. Russian--in Soviet society⁴). Another sphere of sociolinguistics is the problems of the functioning and evolution of contemporary foreign languages (of Africa, Asia, and Latin America). Still another is the sociolinguistic problems of contemporary humanity as a whole in the light of the Marx-Lenin theory of social evolution. Still another is linguistic futurology, i.e. the fate of contemporary multilingualism and the problems of the language of the future (Zakonomernosti razvitiya 1966, 387-388).

In the Soviet Union, the methodological positions of Western socio-linguists are, of course, considered incompatible with "the materialistic

concept of the social character of language and its role in social evolution (Zakonomernosti razvitiya 1966, 387), although the range of general problems seems to be the same. The concept of sociolinguistics as a branch of linguistics based on a premise that language as well as society represent structures, and that the sociolinguist's task is to show "the systematic covariance of both... and perhaps even a causal relationship between them,"⁵ has been in principle foreign to Soviet scholarship. In spite of deep respect for American sociolinguistics,⁶ precisely this concept has been under attack by several Soviet scholars. Quite recently, at the close of the 1960's, for instance, A. D. Švejcer, author of a forthcoming book on American sociolinguistics,⁷ took issue with the structural approach in sociolinguistics. As far as we can gather from a report on his paper on the theoretical positions of American sociolinguists, he claimed that in principle Marxist sociolinguistics cannot but reject every search for isomorphism of language and society, or even of language and culture.⁸ Less aprioristic and more plausible argumentation, however, uses the experience of Soviet sociolinguistics and claims that there indeed exists but only an indirect correlation between the total configuration of a language situation at a given time and place, and the social structure. This would certainly not support W. Bright's theorem (M. M. Guxman, N. N. Semenjuk in Norma i social'naja differenciacija 1969, 10). But interestingly, the sociologically oriented Soviet sociolinguistics is nevertheless toying with an analogous notion of another kind of isomorphism between the social functions which languages perform in society, and the internal structures of these languages, between the evolution of the social functions of languages and the evolution of their linguistic structure. To these problems Ju. D. Dešeriev devoted several chapters of his book on Regularities of Evolution and Interaction of Languages in the Soviet Society (Moscow 1966).

A general impression one gets from the relatively extensive body of Soviet production in sociolinguistics these days, however,--and this in spite of all the conscious attempt to produce a specifically Soviet variety of sociolinguistics--is that the terms sociolingvistika, social' naja lingvistika, lingvističeskaja sociologija, are still used interchangeably, simply as synonymous labels for general 'language and society' problems, many of which are concerned with language planning and the problems of language policy. The predominant concern with these latter aspects, the massive and intensive organizational and publishing activity which promotes it, and not the least, the commitment to the ideological and theoretical position of Soviet Marxism, make Soviet sociolinguistic endeavours rather unique or at least quite dissimilar to the way sociolinguistic problems are treated in this country.

2. Such as it is, Soviet sociolinguistics is entirely a child of the 1960's. The following brief chronology of events, spanning about ten years, may serve to highlight the stages of its growth.

1960. The appearance of the first major Soviet sociolinguistic study containing a series of generalizations on the evolution and functions of national and literary languages: Voprosy formirovaniya i razvitiya nacional'nyx jazykov, ed. by M. M. Guxman.

1961. The decision of the 22nd Congress of the Communist Party to recognize the role of the social sciences and linguistics in Soviet policy. Formulation of the problem: regularity of the evolution of national languages in connection with the development of socialist nations. --Establishment of a Special Section of the Linguistic Institute of the Academy of Sciences, the Scientific Council on the Complex Problem of the Regularity of the Evolution of National Languages in Connection with the Development of Socialist Nations, to coordinate the work on these problems.

1962. Completed prospectus of a major project proposed by V. V. Vinogradov and S. I. Ožegov in 1958: Russian Language and Soviet Society, aiming at a sociolinguistic analysis of contemporary standard Russian (Russkij jazyk i sovetskoe obščestvo). Recommendation of the Academy of Sciences of the U. S. S. R. to direct attention of Soviet linguists primarily to the investigation of language and society problems. --In November 1962, an All-Union Conference on the problems of the development of literary languages of the peoples of the U. S. S. R. in Alma-Ata (Kazak S. S. R.), organized by the Scientific Council on the Complex Problem of the Regularity of the Evolution of National Languages. In the plenary session of this Congress the main themes of Soviet sociolinguistic investigation were outlined: Russian language and Soviet society, the role of Russian as the carrier of culture and the means of communication between members of various nationalities; the study of the regularity of the creation of a common lexicographic stock for the languages of the peoples of the U. S. S. R.; the study of the social functions of Russian and its influence on the development of national languages; investigation of the results of the influence of Russian on the languages of the peoples of the U. S. S. R.; the study of the interrelationship of the social function between the national literary languages and the intranational means of communication in the U. S. S. R.

1963. The General Meeting of the Academy of Sciences of the U. S. S. R. discusses the role of the social sciences in the building of communism and reiterates the necessity and importance of Soviet linguists' interest in the problems of language and society.

1964. Kazan All-Union Conference on 'The Interaction and Mutual Cultivation of Languages of the Nations of the U. S. S. R.', organized by the Scientific Council on Development of National Languages of the Peoples of the U. S. S. R., and by the Linguistic Institute of the Academy of Sciences. The papers of this Conference were published in 1969.

1966. Publication of a book by Ju. D. Dešeriev: Regularities of the Evolution and Interaction of Languages in Soviet Society-- seeking to provide a theory of functional evolution of national languages in the Soviet Union.--Widespread interest in the problems of language and society topics was indicated by a number of conferences in different Academy of Sciences' Linguistic Institutes.--The proceedings of two 1966 conferences were published: a Moscow conference: Language and Society 1968 (Moscow 1968), ed. by F. P. Filin, and a Saratov conference: Language and Society 1967 (Saratov 1967), ed. by L. I. Barannikova.

1968. Publication of a four volume monograph Russian Language and Soviet Society, A Sociological Linguistic Investigation, prepared by some 30 authors who worked with data from the questionnaires returned by several thousand field workers. Individual volumes deal with the lexicon of contemporary standard Russian (prepared by S. I. Ožegov), word-formation (by M. V. Panov), morphology and syntax (by Panov), and phonetics (by Panov).

1969. Publication of a number of collective works. One: Problems of Social Linguistics (Leningrad 1969), ed. by A. V. Desnickaja, V. M. Žirmunskij, and L. S. Kovtun. The volume presents the results of two years of work of a language and society research team of the Leningrad division of the Academy of Sciences' Linguistic Institute, and contains 19 papers organized around the following focal points: Linguistic Interaction, Formation of Linguistic Unities, Territorial and Social Variants of Languages.--Two: Collective work: Interaction and Mutual Cultivation of Languages of Soviet Peoples, (Moscow 1969), ed. by A. Baskakov, and prepared by a similar research team of the Kazan branch of the Academy of Sciences' Linguistic Institute.--In the same year in Ašxabad (Turkmen S. S. R.) the

Scientific Council on Development of National Languages of the Peoples of the U.S.S.R. organized an All-Union Conference on Bilingualism and Multilingualism, dedicated mainly to the problems of the interaction of national languages in the Soviet Union.

1970. Publication of a second volume of Language and Society 1970, a collective work prepared by a research team at Saratov University, and of a collective monograph on general linguistics (Obščee jazykoznanie, ed. by B. A. Serebrennikov, Moscow 1970), prepared by the General Linguistics Sector of the Linguistic Institute of the Academy of Sciences of the U.S.S.R. The latter represents a conscious effort to incorporate the topics and problems of modern sociolinguistics in the frame of general linguistics as taught in university programs.⁹

One of the most interesting aspects of the evolution of Soviet sociolinguistics is its dynamic growth. In view of the rather strong socio-logical interest of Russian scholarship in pre-Revolutionary Russia, and in particular in view of an early sociolinguistic stage in post-Revolutionary time,² the return to the older native traditions was only a matter of time and opportune conditions. Such a revival was possible after the death of Stalin. Interestingly, this same event also triggered an intensified growth of Soviet linguistic structuralism. Thus, both directions in linguistics, which in the West appear in a logical sequence, separate and in opposition, in the Soviet Union turn up almost simultaneously with much less conflict. The polemics on the treatment of the external and internal linguistic factors in the study of language which was begun at the General Meeting of the Department of Literature and Linguistics at the Academy of Sciences in 1963, and was carried on for several years in Voprosy jazykoznanija, died out soon after the first major conferences on the problems of sociolinguistics.¹⁰ While this kind of discussion is still going on in some provincial centers of the U.S.S.R., the new discipline very soon received recognition by the leading linguists. The first prominent Soviet scholar who welcomed sociolinguistics as "a most beneficial trend in the evolution of modern linguistics" was O. S. Axmanova in her review of W. Labov's 'The Social Stratification of English in New York City'. She wrote in 1967:

It is hardly possible to doubt that linguistics as an empirical science must investigate all the phenomena of the functioning of language which influence its structure. Yet, too long a domination of formalistic schools in linguistics led to a situation where--incredible as it may sound--the basic problems of linguistics have been studied by scholars who are collectively and without sufficient precision called 'sociolinguists', and

linguistics in the very sense of this word, became a separate scholarly discipline called 'sociolinguistics'. Traditional structural linguistics found itself absolutely unable to give an answer to the basic questions of its scholarship...¹¹

3. In the following survey I tried to review some aspects of socio-linguistic research in the Soviet Union during the 1960's. Our survey is based on the 'Selected Bibliography of Soviet Sociolinguistics, 1960-1970', which appears in the Appendix to this paper, and is limited to works which were printed in Russian language. The discussion of topics which follows, however, is governed by limitations of time and space. Sometimes I will be not able to give more than a listing of titles, while a number of topics will be ignored altogether in our discussion. Thus, I am not reviewing the general informative papers on sociolinguistics outside the Soviet Union, the discussions of the problems of the individual non-Russian languages, the discussions of the relations between the languages of the U.S.S.R., and most papers covering the problems of language policy and language planning in the Soviet Union. The aspects which I do discuss, however, cover the following four areas: (1) sociolinguistic topics of the language and society complex; (2) bilingualism; (3) discussion of sociolinguistic problems of languages outside the Soviet Union; and (4) the recent Soviet sociolinguistic theory of the relation between the antinomies of the grammatical structures of languages, and external social factors.

1. Topics of the Language and Society Problems

The best Soviet sociolinguistic studies have centered on the problems of social dialectology. Here V. M. Žirmunskij's older contributions to dialectology gave a sound basis for the sociolinguistic study of Russian and German dialects, and the basic programmatic papers in this field are, of course, his. In 1964, he once again summarized his views on 'The Problems of Social Dialectology' (Izvestija AN SSSR, Serija literatury i jazyka, XXIII) and twice incorporated them in papers with broader topics: 'The Problems of the Social Differentiation of Languages' (Jazyk i obščestvo 1968), and in 'Marxism and Social Linguistics' (Voprosy social'noj lingvistiki 1969).

Quite a few studies in Russian social dialectology came from L. I. Barannikova, some of them theoretical: 'On the Delimitation of Language and Dialect' (Jazyk i obščestvo 1968); 'On the Problems of Social and Structural Variability of Dialects' (Voprosy social'noj lingvistiki 1969); 'On the Dialectalization of Languages' (Jazyk i obščestvo 1967); 'On Some Characteristics of the Evolution of Dialects on the Territories of Late (Russian) Settlements' (Jazyk i obščestvo 1967); on the 'Social Historical Conditionality of the Place of Colloquial Speech in the

Standard Language' (Voprosy jazykoznanija 1970); and finally a monograph, The Russian Dialects in the Soviet Period (Saratov 1967).

Another prolific social dialectologist is V. D. Bondaretov. His dissertation on The Professional Languages of Russian Artisans and Merchants (Leningrad 1966) was written under Žirmunskij. It generated a number of papers, the most important one on 'The Social-Economic Preconditions for the Disappearance of Professional Languages in Russian Towns' (Voprosy social'noj lingvistiki 1969).

Another language and society problem with a tradition rooted in the 1920's is the investigation of the speech of the urban communities in the Soviet Union. B. A. Larin's papers "On the Linguistic Investigation of Towns" (1928) and "On the Linguistic Characteristics of Towns" (1928) prompted a new interest in the study of Russian colloquial speech in Soviet cities. The sociological foundation of this research was discussed in 1966 in Gorki at a conference on the "Problems of the Theory and Practice of Linguistic Description of Colloquial Language". In a shorter paper, L. S. Kovtun reports on the Moscow, Leningrad, Gorki, Saratov, Perm' and Voronež centers for the study of the evolution of urban language (Voprosy social'noj lingvistiki 1968). In Voprosy jazykoznanija, 1963, V. Motš discussed 'The Problem of Relationship between the Spoken and Written Language', and M. M. Makovskij 'The Problem of the Interaction of Areal Variants of Slang and Their Relation to the Standard Language'.

Outside these two areas, we have a number of good papers dealing with such sociolinguistic topics as: 'Linguistic Interaction between Dialects and Languages with Different Structures', by A. A. Darbeeva (Jazyk i obščestvo 1968), 'On the Ethnic Function of Languages on the Basis of Marxist Ethnolinguistics', with a discussion of recent Soviet concepts of 'rodnoj' and 'vtoroj rodnoj jazyk', by A. G. Agaev (Jazyk i obščestvo 1968), 'On the Social Functions of Language and Its Functional Equivalents', by A. A. Leont'ev (Jazyk i obščestvo 1968), 'On the Problem of the Relation between the Spontaneous and Planned Processes in the Evolution of Languages', by K. I. Baxman (Voprosy jazykoznanija 1965).

Also interesting are two papers on the sociolinguistic aspect of computer languages. N. D. Andreev, in a paper 'On Language in a Society Using Cybernetic Machines' (Voprosy social'noj lingvistiki 1969), discusses a potential sociolinguistic role for the intermediary language, and the possibilities for improving the intermediary language and giving it a sound-dimension, which would make it a tool of direct communication. Such a qualitative leap, says Andreev, would have a vast impact on convergency of languages, for the time being on the level of their scientific-technical sublanguages, but later also for the creation of a language for international communication.

R. G. Piotrovskij in his paper 'On the External and Internal Linguistic Problems of the Processing of Text in the System: Man - Computer - Man' (Voprosy social'noj lingvistiki 1969), also discusses the sociolinguistic implications of the use of computer language. The use of bazovoj jazyk 'interaction language' says Piotrovskij, will influence the evolution and functioning of natural languages. The influence will be seen in its more rigid normalization, in reduction of homonyms and polisemy, and in partial elimination of synonyms.

Also interesting is a paper of G. A. Menovščikov 'On Some Social Aspects of Language Evolution' (Voprosy social'noj lingvistiki 1969). There is little or no correspondence between the evolution of languages and the evolution of socio-economic structure of societies, claims Menovščikov. Changes in social conditions of a given society may influence languages, but what influences them more, are different forms of language interference. Periods of prolonged bilingualism may cause interpenetration on all levels of linguistic structures: from lexicon to grammatical structure. To support the last claim, Menovščikov introduces some data from the language of Aleuts of Island Medno. To this problem of the interference in the grammatical structure of languages, Menovščikov dedicated a number of papers; the most important appeared in Voprosy jazykoznanija, 1964, and in Izvestija Sibirskogo otdelenija AN SSSR, 1965.

2. Problems of Bilingualism and Language Contact

Soviet sociolinguistics seems to have produced little original work on the theoretical and general problems of bilingualism and language contact. In spite of older pre-Revolutionary achievements in this area, the study of bilingualism in the post-Stalin period revived relatively late. It seems that the first discussion of this topic in the sixties was a paper by V. Ju. Rozencvejg; it appeared in Voprosy jazykoznanija in 1963, in the year when the two volumes of I. A. Baudouin de Courtenay's Selected Papers (Izbrannye trudy po obščemu jazykoznaniju, I-II, Moscow, 1963) directed attention to his long forgotten studies on linguistic interference and his notion of 'mixed languages'.¹² The Rozencvejg's paper 'On Linguistic Contacts' (Voprosy jazykoznanija, 1963) once again pointed to the earlier 'good traditions' of Russian linguistics in this field, citing I. A. Baudouin de Courtenay, L. V. Ščerbà, E. D. Polivanov, and N. S. Trubetzkoy's interest in bilingualism and language contact phenomena. It also gave publicity to some Western classics in language in contact problems, such as U. Weinreich, E. Haugen, L. Tesnière. We may stress here that the publication of one volume of selected Papers in General Linguistics of E. D. Polivanov five years later (Stat'i po obščemu jazykoznaniju, Moscow, 1968) had a similar positive effect on promoting interest in serious sociolinguistic investigation in the U. S. S. R.

There have been published, of course, a number of papers dealing with the problems of specific bilingual situations, language contacts, and linguistic interferences of the languages inside the Soviet Union. A synopsis of concrete data in this area, separately for different layers of linguistic systems of the languages with older and those with recent literary tradition, including bibliographical data, is given in Ju. D. Dešeriev's book Regularities of the Evolution and Interaction of Languages in Soviet Society (Moscow 1966). As to the individual papers, it is very difficult to judge their value and practical contribution to the field. Just two examples. One: E. M. Vereščagin wrote a paper 'On the Problem of the Borrowing of Phonemes' (Jazyk i obščestvo 1968), where he convincingly disproves the widespread thesis of many Soviet linguists that some typical Russian phonemes entered the phonemic systems of some non-Russian languages in the Soviet Union. Such an argument seems to be interesting more for the Soviet scene than for us. Two: M. A. Borodina wrote a good though apparently second-hand report on the dialect of the Swiss canton Graubünden ('The Influence of Foreign Linguistic Systems on the Evolution of Languages', Voprosy social'noj lingvistiki 1969), where she proposes a 'new' type of linguistic contact, characteristic of this dialect wedged between the Raeto-Romance and Swiss German dialects. She calls it 'introstrat' or 'instrat'. How exactly an instrat differs from the other forms of contact, however, is nowhere defined. The references are made to the interferences on all levels of linguistic structure, including grammatical, and Menovščikov's discoveries in the language of Aleuts from Medno Island dominate her argumentation.

More than 60 papers were presented at the All-Union Conference on Bilingualism and Multilingualism in Ašxabad (Turkmen S. S. R.) in 1969, which was indeed a major event for bilingualism studies in the Soviet Union. The papers were concerned mostly with Soviet problems of language contact. The topic of the Conference was: Contemporary Evolution and Interaction of National Languages in the Republics of the U. S. S. R. Two official papers dominated the plenary sessions: Ju. D. Dešeriev and I. F. Protčenko spoke on 'The Basic Problems in the Study of Bilingualism'; F. P. Filin on 'Contemporary Social Evolution and the Problems of Bilingualism'. The latter argued that bilingualism is an organic part of the social functions of language, and that the forms of its existence reflect the evolution of society. He proposed a division into stages of bilingualism which might well illustrate the Soviet approach to this problem; it does not go beyond the standard periods of the Marx-Lenin theory of the evolution of human societies.

A number of studies on general themes were also of some interest. O. S. Axmanova read a paper on 'The Dichotomy Language-Dialect in the Light of the Problems of Contemporary Bilingualism'; L. I. Baranikova 'On the Essence of Interference and Specificity of Its Main

Manifestation'; V. N. Jarceva 'On Structural-Semantic Calquing in Bilingual Situations'. The Section on Sociolinguistics of the Conference discussed a collective paper on 'The Character of the Bilingualism of the Soviet-Iranian Nationalities'. The majority of other contributions were dedicated to concrete bilingual and multilingual situations of the Soviet nationalities. At the end of the program, F. P. Filin addressed the conference. The recommendations of the plenary session stressed a need for further study of Soviet bilingualism and in particular of the processes of the evolution of the 'nacional'no-russkoe dvujazyčie' 'National-Russian bilingualism'.

3. Problems of the Languages Outside the Soviet Union

Interest in the sociolinguistic problems of the languages outside the Soviet Union has produced a rather copious literature. Basic programmatic papers are few; most of the individual studies are dedicated to concrete aspects of individual languages, far fewer to theoretical problems; their value is rather uneven.

Two areas of special attention, however, clearly stand out. One: A concern with providing historical and sociological substantiation for abstract theoretical discussions on the formation and evolution of national and literary languages which went on during the 1950's. Two: A definite effort to extend abroad the theoretical and practical achievements of the Soviet sociolinguistic experience at home, and to promote an active participation of Soviet linguists in the language planning policy of new Asian and African nations. This survey is concerned mostly with the first topic; for the second one I limit my observations to the recent endeavours in this direction of the Leningrad language and society research team.

The search for a theory and definition of such language categories as jazyk narodnosti, approximately: 'the unstandardized native language of a speech community', i. e. our 'vernacular'; nacional'nyj jazyk, approximately: 'the standardized form of a native language'; literaturnyj jazyk, i. e. 'literary language', territorial'nyj dialekt, i. e. 'territorial dialect', which reflects a specific taxonomic problem of Soviet sociolinguistics, dominates the first sociolinguistic publication of the 1960's: the collection of papers on The Problems of the Formation and Evolution of National Languages, ed. by M. M. Guxman (Moscow 1960).

The titles of individual contributions indicate, perhaps, at best the goal of the volume--to provide a number of case-studies on the evolution of national and literary languages in different types of sociological situations. Thus, for example, N. I. Konrad has a paper 'On the Literary Languages of China and Japan', A. S. Garibjan 'On the Armenian National Literary Language', A. A. Mironov on 'The Dialect

Basis of the Literary Norm of the Netherlandic National Language', V. N. Jarceva 'On the Change of the Dialect Basis of the English National Literary Language', V. V. Rešetov 'On the Uzbek National Language', G. V. Stepanov 'On National Languages in the Countries of Latin America', V. M. Belkin on 'The Problem of the Literary Language and Dialects in Arabic Countries', T. B. Alisova on the 'Peculiarities of the Development of Norms in the Italian Literary Language', M. S. Guryčeva on the 'Initial Stage in the Evolution of the French Literary Language', A. V. Desnickaja a chapter 'From the History of Evolution of the Albanian National Language', M. M. Guxman on 'The Development of the Literary Norm of the German National Language', A. A. Jurdašev on the 'Problems of Formation of Common Norms of the Bashkir National Language'.

A general view and some theoretical remarks were given in Guxman's study on 'Some General Regularities of the Formation and Evolution of National Languages'. He discusses the problems of dialect bases, of mutual influences between dialects and written literary traditions, the relation of written and spoken varieties of national language, and the extent of the application of literary language and dialect in the period of formation of national language. The basic difference between 'jazyk narodnosti' and 'nacional'nyj jazyk' he sees not in structural features, but rather in the qualitative change of correlations of different forms of language existence, in the change of the character of the functions of a language in different social settings.

The extent of interests and topics in the field studied by the Leningrad language and society research team, under the direction of V. M. Žirmunskij, is shown by the titles of the papers which were published in the collection Problems of Social Linguistics (Voprosy social'noj lingvistiki 1969).

There are two main questions which attracted the Leningrad socio-linguists: the problems of territorial and social differentiation of languages, and the problems of dialect integration in developing literary standards.

Among the topics of the first question we mention G. V. Stepanov's study on 'The Social-Geographic Differentiation of American Spanish on the Level of National Variants' (Voprosy social'noj lingvistiki 1969), and N. N. Katagoščina's paper on 'Historical Prerequisites for the Development of the French Literary Language' (Voprosy social'noj lingvistiki 1969). Again and again the problem of ancient Greek dialects has fascinated Soviet linguists. Two books on this topic have been announced for 1966; I could not find out whether they were in fact published. There appeared, however, one paper on this topic, by I. M. Tronskij, 'On the Dialect Structure of the Greek Language in Early Ancient Society' (Voprosy social'noj lingvistiki 1969).

The following titles discuss the problems of language integration: 'On the Characteristics of Baxasa Indonesian as the Administrative Language of the Republic of Indonesia' (Voprosy social'noj lingvistiki 1969) by Ju. V. Maretin; 'On Hindi and Urdu, Their Origin, Evolution, and Interaction', by P. A. Barannikov (Voprosy social'noj lingvistiki 1969); 'On the French Language in Canada', by E. A. Referovskaja (Voprosy social'noj lingvistiki 1969), 'On Some Ethnolinguistic Problems of Africa', by D. A. Ol'derogge (Voprosy social'noj lingvistiki 1969). On The Problems of the Study of the Language Situation and the Language Question in Asian and North African Countries--speaks also a collective study, just recently published (Moscow 1970).

4. The Theory of the Relation between Linguistic Antinomies and External Social Factors

The work on the project 'Russian Language and Soviet Society' on which Russian linguistics toiled through most of the 1960's (1958-1968), was to yield a model sociolinguistic treatment, of capital importance for the post-Stalin teaching of Marxist linguistics. It indeed produced a theory of mutual dependence between the internal stimuli of linguistic evolution and the external factors of social evolution; but what seems to be more important, it gave us an outstanding description of linguistic tendencies of contemporary standard Russian.

The theory itself is outlined in the introductory essay in the first volume of the monograph Russian Language and Soviet Society, A Sociological Linguistic Investigation (Moscow 1968), under the title 'Principles of Sociological Investigation of the Russian Language in the Soviet Period'. This study was written collectively by seven linguists, the central part of the theory (Chapters 1-3, 5-6) was contributed by I. P. Mučnik and M. V. Panov.

The theory underlies the description of linguistic changes in contemporary standard Russian during the fifty years of Soviet rule. Its postulates are supposedly derived from the data and are fully illustrated in the individual parts of the monograph, which deal with lexicon, word-formation, morphology, syntax, and phonetics. I shall not discuss this factual linguistic data to which the theory is applied; our main interest is the theory itself.

Linguistically,¹⁴ the theory proceeds from Sapir's well-known view of language history as 'a movement in a current of its own making' with at least three major drifts which have operated in languages through the centuries: a tendency toward emptying the case differences, a tendency to exploit the position of words in sentences as important 'grammatical method', and a tendency toward the invariable word.¹⁵ Russian and the rest of Slavic languages, as A. Meillet pointed out, may well show some of these tendencies. The theory claims that of

these--the replacement of inflection by other means is exhibited in contemporary standard Russian.

Ideologically, the theory proceeds from the basic Marx-Lenin law of dialectics--law of the unity of opposites. Everything, language is no exception, is in a continual process of change, of becoming and ceasing to be. All things contain contradictory aspects, whose tension or conflict is the driving force of this change.

The contradictory aspects of language are found in inconsistencies between apparently reasonable principles of its evolution. Competition between these opposites, the struggle of opposites, explains the evolutionary drift of language. Every single concrete solution of every set of opposites generates new conflicts, new contradictions, and consequently, their final solution is impossible. The opposites are a constant stimulus of the inner evolution of language.

The theory operates with five such contradictions, using for them the Prague school term--linguistic antinomies.¹⁶ They are:

- (1) Antinomy between sender and receiver, or incompatibility of the speaker's tendency to simplify the message with the hearer's tendency to simplify the perception.
- (2) Antinomy between the use and the possibilities of language system, or the conflict between the tendencies to limit the use of linguistic combinations and to exploit all possible combinations which a language can provide.
- (3) Antinomy between code and text, involving economy and simplicity of speech, or the conflict between a tendency to reduce the code and increase the text, or vice versa.
- (4) Antinomies contained in the asymmetric character of the linguistic sign, or a conflict between the signifier and the signified of the linguistic sign (with the signifier tending to acquire new meanings, and the signified--new means of expression).
- (5) Antinomy of informative and expressive functions of languages, or a contradiction between regularity and uniformity vs. unconformity and originality in language.

These five antinomies, representing the internal stimuli of language evolution, as the theory goes, cannot be abstracted from continuously changing social conditions in societies. As far as they are determined by the essence of language as the most important tool of communication, they are responsive to social factors, though their relation to social evolution is not always absolutely clear (e.g. the code-text

antinomy). On the Soviet scene their combined action is producing profound changes in the language. One of the important tendencies of the lexicon of contemporary standard Russian is, for example, an inclination toward analyticity, evident also in other layers of the language structure; in word-formation--a tendency toward agglutination; in morphology--again toward analyticity; in phonetics--toward simplification of the vowel system and complication of the consonant system of contemporary Russian.

On the question of how the internal linguistic antinomies and external social factors interact and propel the evolution of languages, the theory gives the following answer: One: Sudden profound changes in social structure expose the latent antinomies to external tensions and force them to resolve their inconsistencies. Two: Changes in social structures may accelerate the evolution of linguistic processes; in both cases, external social actions do not cause linguistic changes, but only help to achieve the resolution of their antinomies. Three: In some instances, social factors may hinder the evolution of one or another tendency in the evolution of languages; both latter actions, consisting of stimulating and obstructing influence of social factors, are most typical in the evolution of languages.

Now these three types of processes of interaction are claimed to have universal validity. They are able to bring and they do bring quantitative and qualitative changes in the evolution of languages everywhere. They act in socialist and non-socialist societies. In contemporary standard Russian, all three processes are active, though the latter two appear to be more characteristic for its evolution.

Conclusion

In summing up this survey, I would like to stress one thought: During the 1960's Soviet linguistics witnessed the reemergence of a vigorous interest in sociological problems of language. Even if the primary impetus for the new direction came from the political sphere, some concrete achievements in the investigation of language and society topics in the Soviet language situation paved the way for the scholarly recognition of the new orientation inside the Soviet Union. We may expect, and we certainly should like to hope, that in the coming decade Soviet sociolinguistics will rise to the level of international competence.

NOTES

¹Cf. the programmatic paper of the Academician L. F. Il'ičev, Naučnaja osnova rukovodstva razvitiem obščestva. Nekotorye problemy razvitiya obščestvennyx nauk, Moscow, 1962; and: Stroitel'stvo

kommunizma i obščestvennye nauki. Materialy sessii Obščego sobranija AN SSSR 19–20 oktyabrya 1962 goda. Moscow, 1962.

²During the twenties and the thirties, a number of Russian linguists worked on several aspects of the language and society problems, in particular on the topics of social differentiation of language and of social dialectology. As early as in 1920, the Leningrad Institute of Linguistics had a Kabinet social'noj dialektologii; it was directed by V. M. Žirmunskij. From 1930 on, inside this Cabinet a research team, headed by B. A. Larin, investigated the language of Russian urban centers. This phase of Russian social linguistics is represented by the publications of K. N. Deržavin, A. M. Ivanov, L. P. Jakubinskij, N. M. Karinskij, B. A. Larin, M. V. Sergievskij, V. M. Žirmunskij, and others. Retrospectively, Žirmunskij gives the following evaluation of the early endeavours of his generation with sociolinguistic problems: "While these works rested on the methodological level of the time, they nevertheless raised the basic questions of the Marxist linguistics about language as a social phenomenon." Cf. 'Problemy social'noj dialektologii', Izvestija AN SSSR, Serija literatury i jazyka (Vol. XXIII, 2:105). Already in the second half of the 1930's, socio-linguistic topics completely disappeared. For a representative selection of the titles characterizing this early phase of Russian sociolinguistics, see the 'Selected Bibliography of Soviet Sociolinguistics, 1920's and 1930's', which appears in the Appendix to this paper.

³While all these terms have been used without discrimination, only lately the labels sociolingvistika (noun), sociolingvičeskij (adjective) appear to be generally accepted. V. M. Žirmunskij has been using consistently social'naja lingvistika, Ju. D. Dešeriev sociolingvistika, but lingvo-sociologičeskij in the adjectival form. A. S. Axmanova has sociolingvistika and sociologičeskaja lingvistika; cf. her Slovar' lingvičeskix terminov (2nd ed., Moscow, 1962).

⁴For Russian mežnacional'nyj we use English intranational; for meždunarodnyj, international.

⁵W. Bright, 'The Dimensions of Sociolinguistics', in: Sociolinguistics, Proceedings of the U. C. L. A. Sociolinguistics Conference, 1964. Ed. by W. Bright. The Hague, 1966:11.

⁶The most quoted works of American sociolinguistics during the 1960's were: Sociolinguistics (ed. W. Bright; The Hague, 1966); Horizons of Anthropology (ed. S. Tax; Chicago, 1964); Language in Culture and Society, A Reader in Linguistics and Anthropology (ed. D. H. Humes; New York, 1964); E. Haugen, Bilingualism in America (New York, 1956); U. Weinreich, Languages in Contact (New York, 1953). The most quoted individual authors, representing American sociolinguistics, are: U. Weinreich, W. Bright, E. Sapir, B. Whorf, W. Labov, H. Hoijer, Ch. A. Ferguson, D. H. Humes, J. Gumperz.

⁷Švejcer, A. D., Voprosy sociologii jazyka v sovremennoj amerikanskoj lingvistike. Announced by Sovetskije knigi, 1 (1971), No. 164, to be published in Leningrad in the 4th quarter of 1971.

8A. D. Švejcer read his paper at the Annual Meeting of the AN SSSR Scientific Council on the Problems of the Soviet Linguistic Theory, in Moscow, on 19 January 1970. Cf. Voprosy jazykoznanija, 1970, 3: 158-159.

⁹The central chapters of this textbook (604 pages) are dedicated to such problems as: 'Languages as a Historical Phenomenon' (Chapter 3, pp. 197-313), 'Language as a Social Phenomenon' (Chapter 6, pp. 419-451), 'Territorial and Social Differentiation of Language' (Chapter 7, pp. 452-501), all three written by B. A. Serebrennikov himself, a section on the 'Literary Language' (Chapter 8, pp. 502-548), written by M. M. Guxman, and a section on the 'Linguistic Norm' (Chapter 9, pp. 549-596), written by N. N. Semenjuk. The treatment of the problems in the monograph is based on the latest Western sources on the topics, and is documented with rather good bibliographic surveys of domestic and foreign literature.

¹⁰Cf., for instance, O. S. Axmanova and V. Z. Panfilov's article on 'The External and Internal Linguistic Factors in the Functioning and Evolution of Language' (Voprosy jazykoznanija, 1963, 4:45-57), and I. I. Revzin's article on the 'Structural Linguistics and the Unity of Language Study' (Voprosy jazykoznanija, 1965, 3:44-59). The polemics reached its climax in a much broader formulation of the problem, raised by V. I. Abaev in his attack on the 'Linguistic Modernism as Dehumanisation of Linguistic Science' (Voprosy jazykoznanija, 1965, 3:22-43) and the reaction the Abaev's paper provoked among Soviet linguists. Cf.: P. S. Kuznetsov, 'Once more on Humanism and De-humanisation' (Voprosy jazykoznanija, 1966, 4:66-74), and Ju. V. Roždenstvenskij's 'A Survey of Materials Concerning V. I. Abaev's Article "Linguistic Modernism as Dehumanisation of Linguistic Science" (Articles and Notes Received at the Editorial Office)', in Voprosy jazykoznanija, 1966, 4:75-79.

¹¹Voprosy jazykoznanija, 1967, 6:141.

¹²I. A. Baudouin de Courtenay, 'O smešanom xaraktere vsekh jazykov', (1901). Cf. his Izbrannye trudy po obščemu jazykoznaniju, I (Moscow, 1963), 362-372.

¹³Voprosy jazykoznanija, 1970, 2:142-146.

¹⁴The theoretical works underlying the discussion of the 'Principles of Sociological Investigation of the Russian Language in the Soviet Period', include: Hertzler, J. O., A Sociology of Language (New York, 1965); Jakobson, R. O., 'Zur Struktur des russischen Verbums', Charisteria V. Mathesio Oblata (Prague, 1938), 74-83; Karcevskij, S., 'Sur le dualisme assymétrique du signe linguistique', Travaux du Cercle linguistique de Prague, I (Prague, 1929). Lewis, M. M.,

Language and Society (London, 1947); Sapir, E., Language (New York, 1921); Whorf, B., 'The Relation of Habitual Thought and Behavior to Language', in: Language, Culture, and Personality (ed. L. Spier; Menasha, Wisconsin, 1941); Saussure, F. de, Cours de linguistique générale (Paris, 1949).--About the extent of the influence of the ideas of the Prague Linguistic Circle during the 1960's speak the publication of an anthology of the main representatives of the Prague school (Pražskij lingvistickij kružok, Sbornik statej. Ed. N. A. Kondrašov, Moscow, 1967); and the echo to the Problemy marxistické jazykovědy, (Prague, 1962); cf. its reviews in Voprosy jazykoznanija, 1963, 4:140-144; and in Izvestija AN SSSR, Serija literatury i jazyka, Vol. XXII (Moscow, 1963), 6:546-552.

15 Cf. Sapir, E., Language, pp. 150, 169.

16 The concept of the antinomies in grammatical structure was introduced by R. O. Jakobson in his 'Zur Struktur des russischen Verbums', in: Charisteria V. Mathesio Oblata (Prague, 1932), pp. 83-84.

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LINGUISTICS '70: A RETROSPECT

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Or is it "Où en est la linguistique Sovietique?"--as I see it? Ambitious anyway. Nevertheless...¹

1. Phonetics, Phonology, and Morphonology. There have been at least two attempts on phonology as the science of semiological relevance of speech sounds: (1) for it to be ousted by morphonology (in a different metalinguistic garb), i. e. 'Paradigma-phonemics' of the neo-Moscow school, 'systematic phonemics' of generative grammar, etc., (2) for the semiologically relevant differences of global speech sounds to be viewed mainly in terms of 'distinctive features', 'cues', 'minimal distinctor', etc. It is wonderful to think of the amount of research done and how it has helped to understand so much more thoroughly the nature of phonemes as 'Lautgebilde', of morphonology as the study of phonological oppositions within morphemes and at morpheme-boundaries to penetrate the 'paradox of levels'--morphological identity so often based on phonological non-identity or difference as in, e. g. Russ. ruka-ručnoj, drug - družit, or the morphonological processes which effect the identification on the morphological level of long strings of sounds,² as in the case of Russ. <o> = [ø], [o], [ö], [a], [bl'], [ɛ], [u].

2. Morphology. It is firmly established as the science of the system of morphological oppositions, proper to a given language, i. e. the system of its grammatical categories ('dimensions of categorisation', 'generic categories', etc.).³ In spite of the enormous amount of work done, however, the relationship between the facts of a given language, the proposed categorisations and the methods of investigation have not been properly explained. Does morphology consist merely in performing a certain set of operations over an 'ensemble' of inflectional forms which are 'given'--to be used as testing-ground for the application of the binary-distinctive-feature method or some of the newer approaches to the 'classification and synthesis of paradigms'? Or does one proceed

from a set of logical premises (Kolmogorov's mathematical theory of cases, for example) and refuse altogether to be bothered with linguistic facts?⁴

What does one normally do when dealing with natural human languages? First, direct observation of speech-events, from which 'the language' is extracted like metal from ore, provided, of course, the scholar knows the language and is familiar with a certain heuristic methodology: minute investigation of syntactic functions and word combinations in which the inflectional forms occur, together with the requisite historical and comparative-philological information. The still unresolved difficulties are: (1) Where does one draw the line between the compulsory grammatical distinctions and the *ad libitum* lexical ones? (2) Is it possible to tell 'analytical grammatical forms' from periphrastic collocations?⁵ (3) Does one still go on talking of 'morphological oppositions' when the number of grammatical contrasts is more than two? For instance, by the opposition of how many categorial forms ('specific categories') is the category of tense constituted in modern English?⁶ or the category of mood?

Derivation and composition form the 'lexical' part of morphology. Although we possess vast amounts of descriptive information, neatly arranged into types, patterns, etc.,⁷ there is still plenty of controversy because the difference between morphemic (morfemnyj or rather pomorfemnyj) and derivational (slovoobrazovatel'nyj) analysis, between členimost' and proizvodnost' is not always clearly understood.⁸

3. Lexicology. I spoke of 'attempts' on phonology; with lexicology it is a continuing onslaught! To imagine people speaking of arbitrary 'semantic languages' (to consist, e.g. of '32 names of elementary predicates, 54 names of elementary objects, 10 classifiers', etc.⁹ to serve as a metalanguage for the description of Russian)--and not only speaking but actually sparing no time or energy to twist and turn the facts of a well-developed literary language in vain attempts to make them conform to a preconceived 'semantic theory'. The problems which have not been seriously considered in those hasty and aprioristic schemata may be summarized as follows:

(1) The 'actual' semantics of a word in the object language and the 'number' of distinctive semantic features to be included in the meta-language if the latter is to be used for an adequate lexicographic definition of the former.

(2) The relationship between a word 'as such' and the word combinations (*slovosočetanija*) in which it occurs (descriptions of word-combinations and 'valencies' in place of a painstaking analysis of the actual semantic content of the lexeme in question distort the picture).

(3) The number of well-documented occurrences required. Every lexicographer knows that the meaning of a word can be correctly defined only by careful analysis and subsequent synthesis of masses of

of excerpts. It is therefore idle to imagine that even a very good speaker can regard himself as 'his own informant'. What looks obvious and simple within an elementary sentence, especially if 'generated' for the special purpose in hand, when checked against the sum total of the actual uses in the language will often prove to be downright wrong.¹⁰

4. Syntax. The 'properly formatted sentence'. Two main lines of approach: first, the paradigmatics of the simple sentence, next, its expansion, and finally the complex and compound sentences. Or the other way around: begin with complex sentences, i. e. those constructions in human languages which are naturally logical and then work gradually down--as far as one can go in one's quest for unity of syntax and logic.¹² The student of syntax is thus no longer expected to square the circle, i. e. to categorize within the same syntactic taxonomy every imaginable kind of utterance, provided it can be cut off from the rest of the flow of speech by a double-cross juncture.

5. Style. The 'functional' styles, of course, with growing interest for the language of science.¹³

Let us now turn to the three basic general-linguistic oppositions, of the importance of which we have been growing increasingly conscious:

I. Oral form of language vs. written form

(1) The written form of language is no longer regarded as a kind of 'imperfect quasi transcription, hopelessly lagging behind its spoken counterpart'.¹⁴ Phonemics is essentially a 'technique for reducing languages to writing'.

It does not follow that the oral form is unimportant. Close and unbiased studies of what actually happens, especially when one departs from full styles of speech have been opening new vistas. We are almost ready for comprehensive 'phonologies of languages in their oral form' based on 'cues' and 'minimal distinctors', followed by a revolutionized morphonology.¹⁶

(2) The morphology of the oral form is incomparably simpler. Thus for instance, the opposition xodil - xodila is actually reduced in speech to variation of an [l - l:] type. Or the endings -yj, -aja, -yje so clearly distinguishable in the written form, how are they distinguished in the oral one, if at all?¹⁷

(3) One is never quite sure about words. Still, doesn't one write, e.g. delete, but say 'take out'?

(4) With the properly formatted sentences firmly established, it is comparatively easy to describe all the departures from syntactic well-formedness in oral speech--all the different broken-off bits¹⁸ which make sense in contexts of situation.

II. Synchrony vs. diachrony

Everywhere the recovery from the infantile disorder of rigorously static linguistics. Not only is comparative philology and, more widely, historical linguistics, increasingly regarded as entitled to a 'position of leadership among the primary linguistic disciplines',¹⁹ it is also much more generally recognised that human languages are permanently in a state of flux; that they are open, not well-defined systems and cannot, therefore, be properly understood unless we know what happened before and what are the main underlying tendencies of the current stage.

III. Linguistics vs. interlinguistics

Most significant of all is the growing emancipation of linguistics as the science of 'natural human languages', with their histories, literatures, ethnologies, etc. 'Interlinguistics' is a robust, young science of great promise. It seeks ways and means of finding the 'optimum' (optimalizacija), of rationalizing communication--internationally, between man and machine, extraterrestrially in the future? The prospects and methods of interlinguistics were so breathtaking, they appealed so much, particularly to the young, that for a time linguistics proper was swamped by the wealth of new projects and revolutionary approaches. Above all it was so fashionable, so chic to brandish algorithms and formulae, to insist that there is no basic difference between linguistics and, e.g. physics, that all modern sciences must use the same mathematical methodology, etc. It is interesting to note, that in the USSR the dominance of interlinguistics was (and still is, because metalinguistic expressions are slow to reflect the changes of attitude) reflected in the acceptation of the term 'prikladnaja lingvistika'. Although, properly speaking it should mean exactly the same as 'applied linguistics', in the USA it was used to denote the 'applications' of interlinguistic theory to such 'practical' problems as machine translation, automatic information retrieval, the principles of mathematical mediator languages, etc. Linguistics is empirical and 'social', its method genetic. Interlinguistics is axiomatic; its method hypothetical-deductive.²⁰

NOTES

¹I wonder if 'retrospects' should be epic and personal--something like the eminently readable, lucid and convincing 'Introduction' to W. L. Chafe (1970)?

²It is the 'metalinguistic garbs' that prevent (or at least hamper) mutual understanding. This is made abundantly clear by Reformatskij (1970:3-120). A simple algorithm for translating the three metalinguistic

systems into one another is all that really matters, i.e. the phonological metadialects of the Avanesov-Smirnitsky*, the 'Moscow' and the 'Leningrad' ways of talking about phonology and morphonology in their interrelation. An improved version of what I had tried to do about it in Akhmanova 1966:8 ff. is at present with Mouton and Company.

³(Akádemická) Grammatika sovremennoj russkoj jazyka (1970:317 ff.). I do not mean to say, of course, that different authors are not forever trying to "... modify the store of selectional, lexical, derivational, and inflectional units already posited..." (cf. Chafe 1970: 347).

⁴As in, for instance, Z. M. Volockaja et al. (1964) or V. A. Uspenskij (1957).

⁵Smirnitskij was quite sure it is and showed how it can be done (Smirnitskij 1956).

⁶'A grammatical category is constituted by the opposition of no less than two categorial forms' (to avoid confusion I shall repeat that when we say 'grammatical category' we mean what, e.g. Martin Joos calls 'dimensions of categorisation' and Whorf, 'generic categories'. Our 'categorial forms', then, are just 'categories' and 'specific categories', respectively.)

If we know what an analytical form is and if more than two categorial forms can, theoretically speaking, be "opposed" to constitute a grammatical (morphological) category, what do we decide about the categorial form of 'future' (cf., e.g. R. A. Close 1970). Incidentally, in Chafe (1970) there is a 'present' and a 'past', but no 'future'!

⁷Even more neatly and exhaustively than heretofore in Grammatika... (1970).

⁸The historic Smirnitskij-Vinokur controversy (see A. I. Smirnitskij, Leksikologija anglijskogo jazyka, Moscow 1958, pp. 58 ff.) was clearly due to failure to distinguish these clearly enough. Nor has the difficulty been overcome in a recent passionate altercation in Izvestija ANSSR, Otdelenije literatury i jazyka.

One more passing remark: there is no doubt whatsoever now that certain facets of natural human languages do lend themselves to investigation by methods I have proposed to generalize under 'logiko-linguistics' (logikolingvistika). The part of language that lends itself most readily to the 'logical' approach is the complex sentence; also, probably, the 'invariant' patterns of 'slovoobrazovanije'. Although I never tire in my criticisms of sweeping pronouncements and invocations of our 'hypothetical-deductivists', I found P. A. Soboleva's doctoral thesis ('Applikativnaja grammatika i modelirovanije slovoobrazovanija') worthwhile. Derivational patterns, e.g. for Russian nouns

*I explained Smirnitsky's priority in "Über die Grundbegriffe der Phonologie", Zeitschrift für Phonetik u. allgemeine Sprachwissenschaft, 1958, N1.

in -enije are sometimes unbelievably productive, especially in some less sophisticated registers, such as technical nomenclature or 'hyper-urbanistic' style. (I thought, incidentally, that the latter could perhaps also be described as a variety of 'hypergrammaticality' (Akhmanova 1970).

⁹I was quoting from Švedova's brilliant article (Švedova 1970): I agree with her wholeheartedly: merely in terms of number Apresjan's proposed metalanguage is sorely inadequate! As I was typing these notes 'Voprosy Jazykoznanija' No. 1, 1971 came with Apresjan's disgraceful riposte. What a shame thus to avail oneself of freedom of discussion! Švedova had raised and conclusively presented important methodological problems, has criticised the Principles, the basic premises of A's approach. By what amounts to a sleight of hand, Apresjan turned it all into petty quibbles about the particular meaning of this or that word or acceptability of this or that word combination.

¹⁰This, of course, is basically the problem of 'language', la langue, the -eme. Something that some people 'know', while others do not, though they may be using it in all kinds of current communication. I shall add, that among the different paradoxes of our infinitely complex subject the following is, perhaps, the most universal: the better one knows the language (and the greater his understanding of the subject) the less he trusts himself, while the linguistic ignoramus is usually so sure of himself as to consider his own 'performances' the peak of reliability.

¹¹See Grammatika... (1970:577 ff.).

¹²See note 8. Also Padučeva 1964.

¹³It would be interesting to compare in detail V. V. Vinogradov's division of functional style with the acceptations of the English term 'registers'.

¹⁴Josef Vachek (1965:959).

¹⁵There is hardly any need in indicating the source.

¹⁶Generally speaking the two aspects or forms are hardly ever properly balanced in the thinking of any one particular linguist.

¹⁷With foreigners in mind the two morphologies should be described and presented separately. Everybody knows that it is comparatively easy to learn to recognise inflexions, to tell them from each other in a text, but very difficult to reproduce them correctly in one's own speech. If a Russian normally says, especially in weaker syntactic positions, something that sounds like bel' before, e.g. kost'um, bluzka, platje, etc., why should a foreigner be made to painstakingly 'vygovarivat' all the theoretical distinctions--and be branded 'foreigner' for his pains!

¹⁸That is 'Vyskazyvanija, ne vosproizvod'aščije strukturnyx sxem predloženija i ne javl'ajuščijes'a ix regul'arnymi realizacijami', Grammatika... p. 574.

¹⁹Szemerényi 1970.

²⁰I have got it in English now in Akhmanova 1970.

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DISCUSSION

SESSION 4

Michael Zarechnak, Georgetown University: Perhaps I can ask, as a member of the panel, the first question directed to Prof. Zeps. When he started, as you know, he surprised me that he had changed his title. He wasn't sure that he could discover models in Soviet phonology and yet, I am acquainted with Revzin's book which was published in English in this country, Models in Language, and a phonological section is contained in that. How come you did not discuss it?

Valdis J. Zeps, University of Wisconsin: In fact, I started out with Revzin and hence that somewhat mistaken title. Revzin in fact does have a fair section on phonology and it's not at all bad. The reason why I omitted it is because of the prejudice that I stated at the very end, namely, that I thought that a system should show that it can do something well.

Now the sort of thing that I'm talking about is, let's say, Revzin tries to apply logic and mathematics and set theory to linguistics. Let me give a concrete example. Let's suppose I shall now define a phonemic set as consisting of subsets which are, for this purpose, pairs distinguished by the same opposition (let's say, obstruents distinguished by voicing: p, b, t, d, and so on), plus those isolates which are members of the same larger subclasses, in this case obstruents. Such isolates could be the Russian [c] and [č], which have no voiced opposites. Thus, we'll have subsets and isolates, all of them part of a phonemic set as just defined. Revzin then presents a number of theorems and one of the theorems states that there cannot be a set of isolates. This, of course, is very true. Since a set has to involve at least some subsets, and since every subset has to contain at least one pair, then there cannot be a set of isolates as just defined. What has bothered me throughout, is that although I have been able to follow him, I thought quite clearly, I could never quite see where it all lead. I suppose if in five years it will lead someplace, I will eat humble pie and apologize, but right now I just don't see where he's going.

Zarechnak: I would like to make one sort of a statement and it might be of interest to those linguists who are not connected at all with the Slavic field. I'm thinking of Prof. Pike, Prof. Lamb and Prof. Chafe among others. Quite often when I read their papers and interpret them in my class in semantics, I find again and again the following statement: It doesn't matter for my purposes whether I use logical terms precisely or not in order to discuss the predical calculus of the first order. Usually this is an introductory statement to valence of verbs. They say here we have zero place verb, two-place verb, three-place verb. I'm particularly thinking of Fillmore in this case when he says there can be many more places around the nucleus. Now it is my experience that there are no more than four which are mandatory. I would like to hear at least one case from the audience where a verb, as a nucleus, could have more than four mandatory fillers. I ask this question because it would be good for me to be corrected for the record if I am wrong on that. Because this assumption that fillers could be unlimited in mandatory terms, reminds me of Chomsky's statement that a sentence could be one million words long. In experimental research this isn't the case. By mandatory fillers I mean those fillers which are directly connected with the nucleus. This was the statement and I would be pleased if there were any comments on it.

James R. Holbrook, Georgetown University: First I have a comment. In addition to the language barrier for those of us who do not know Russian, we are confronted with another not-so-obvious difficulty, namely, that the definition of the theoretical framework and the respective roles of certain linguistic subfields in the Soviet Union are not the same as here in the United States. This has been alluded to both here today and in Weinreich's article on lexicology in Sebeok Current Trends in Linguistics, Vol. I. Prof. Akhmanova's comments on the different interpretations of Soviet and American applies linguistics also corroborate this.

My question is not directly connected with my comment. Could we obtain from today's panel advice on where to turn in order to avail ourselves of Soviet findings for our particular linguistic subfields in English?

Zeps: I'm not sure that we could do anything quickly. I mean there are books and the Soviet books are being reviewed in journals. If your question is to some other point, I really must have missed it. But, they're not being ignored.

Holbrook: There are many linguists here today, and throughout the country who would be most eager to explore work that's being done in

the Soviet Union in fields which they are involved in here in the United States, but they cannot in a relatively short time learn Russian well enough to acquire this knowledge in the original. Also, there is no great amount of Soviet linguistic work being translated, or even reviewed, although of course this is improving. So for those of us who are left here today with, perhaps, appreciation of the work which is being done in the Soviet Union and for those of us who are very interested to see what is being done in our particular field, I wonder if there is any recommendation, any advice on where we might turn. I don't expect a bibliography but, rather, advice as to what we might do when we return to our respective universities to inquire as to the accessibility of any of this information in English or in another language which we can work in. It is a problem, I think, which all of us who do not speak Russian feel and we cannot really appreciate the results of today's panel without being able to explore this further.

Rado Lencek, Columbia University: I would have perhaps a partial answer to one aspect of this question. As far as I know there is one Soviet linguist who very intensely worked on English. This is the late A. I. Smirnitsky. Unfortunately his works were published only in Russian. His book on The Morphology of English is an extremely illuminating work throwing light at the same time on Russian morphology and on some potential problems in English morphology. If one can handle Russian, one can use this work with great benefit. It would deserve to be translated, I think. Then there was one of his books as well on the History of English and one on the English Lexicography it seems to me. All three are outstanding works. What I would like to point out with this remark is that there have been very serious contributions made in the Soviet Union to this particular area which are little known in this country. They can be used as a reference to their interests in our problems.

Werner Winter, University of Kiel. I would like to address a question to Prof. Unbegaun. Prof. Unbegaun, you gave us a review of shall we say the results in various fields of Soviet dictionary making. You probably, under the pressure of time, did not mention the field of phraseological dictionaries, which of course is a fairly important one. I was wondering whether in this particular field you think that there are substantial contributions on the part of Soviet scholars, not only as far as the output, that is, as far as dictionaries made are concerned, but also as with respect to the art of dictionary making. For, since I think this is one of the specialities in the field of Soviet dictionary work. I wonder whether you deem the results to be such that others could learn from their practices and their underlying theories.

Boris Unbegaun, New York University: Well, I don't think that the phraseological dictionary is a strong side of Soviet lexicology. There is one large Russian phraseological dictionary which has appeared in 1967 under the editorship of A. I. Molotkov. In my opinion it is unsatisfactory because in a phraseological dictionary we would expect to find not only the meaning of phraseological expressions, but also an explanation of their origin. Russian is full of loan-translations from French. For example, all the phraseological expressions with the verb иметь are loan-translations corresponding to French expressions with avoir, as avoir l'habitude, and so on. They are all listed in Molotkov's dictionary, but nothing is said of their being translated from French. Much better work has been done in Leningrad by Professor A. M. Babkin. He has published several works on the subject, and his last book on Russian Phraseology, its Development and its Sources (1970) is especially important. He has also produced a two-volume dictionary of foreign quotations in Russian literature, which is a very useful reference work. There is also a good article by Vinogradov on the 'Basic Types of Russian phraseological units'. There is still room for a comprehensive phraseological dictionary.

I have deliberately excluded phraseology and phraseological dictionaries from my paper, which, perhaps, was a mistake. I have not included either the dictionaries, Russian-Russian dictionaries, and others.

Kenneth Pike, University of Michigan: A question of Prof. Macdonald: The work of Šaumjan I found tremendously intriguing because of its high abstractness--and just sheer pleasure in being the most abstract known to me. But there's a potential enormous contribution to me from him, if he carries his materials far enough in the next decade. Since, however, I read only his English materials, I'm curious to know if you can tell me if it's on the horizon in the Russian publications. That is, one of the things I felt the need of for a long time in the work of Hjemslev (which he never gave us) was an exhausting of all possible logical relations of which mathematicians are specialists which would be mappable against all possible clause relations, sentence relations, paragraph relations--so that when we go into a strange language we would have, in advance, a mathematically exhausted taxonomy of all conceivable logical relations which might be reflected in some such language. Is this on the horizon? If so, it would be a tremendous help to all of us.

R. Ross Macdonald, Georgetown University: I must confess to ignorance as to whether it's on the horizon or not. The system I described is already several years old and I should imagine if nothing to advance it further has appeared, that it is not too high on the horizon.

George Lenches, Georgetown University: I am not a Slavicist myself but since I am very much interested in Uralic linguistics and since I'm a native speaker of Hungarian, I couldn't resist the temptation to ask somebody on the panel as to what research Soviet linguists have been doing in the field of Ugric languages, in the Finno-Ugric branch, that is the Ob-Ugric, namely Vogul and Ostyak. I don't know which professor would be best qualified to answer that question, so I leave the choice up to whoever wishes to respond.

Zeps. I suppose I am, which is not saying much. There is a great deal of Finno-Ugric studies done in the Soviet Union. There is a journal Sovetskoe Finno-ugrovedenie which is published in Tallinn, appropriately, and the Karelian branch of the Academy publishes monographs regularly. As far as I know, this also includes Ob-Ugric stuff. I could be mistaken. But they are rather well bibliographed, so there should be no difficulty in getting at them.

Paul Larudee, Georgetown University: I think all of us have gained a lot of insights into the developments of Soviet linguistics in the Soviet Union and I have the impression, although I may be wrong, that not everyone keeps up to date with such developments. I have the impression that there is not as much communication between linguistics here in the States and linguistics in the Soviet Union as there might or should be, and that more communication between developments there and here, which often seem to parallel each other, would bring forth many fruitful results.

I thought Dr. Macdonald's paper was a very fine description of Soviet linguistics but it contained little comparison between trends there and trends in the U. S. For this reason, I feel that it might be interesting for some members of the panel to compare a few of the more similar developments of the two countries, and to suggest how the two might influence each other.

Macdonald: Well, one of the reasons of course why there is no comparison in my paper was simply that there wasn't enough time. But, even if there had been time, I think I would have been a little wary about giving it in some cases, because it is all too easy as you read a description of some other system to say 'ah, yes, that's exactly like something that you're already familiar with'. This is frequently an oversimplification. It is not necessarily exactly like something that you're already familiar with. I would like to make a really careful and detailed investigation of the two systems before I presume to compare them. I think the chief advantage of this kind of thing is to see how other people approach the subject, and to gain insights from that. We're

all blind men around the same elephant, of course, and the elephant is about all we have in common I guess, but we can strive for a greater deal of commonality.