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Topics in the Theory of Generative Grammar

MOUTON

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TOPICS IN THE THEORY OF GENERATIVE GRAMMAR

by

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M.I.T.

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ASSUMPTIONS AND GOALS

My original intention was to use these lectures to present some recent work on general linguistic theory and on the structure of English, within the general framework of transformational generative grammar. However, a sequence of recent publications has indicated that many points that I had hoped to take for granted are widely regarded as controversial, and has also indicated misunderstanding, on a rather substantial scale, of the general framework I had expected to presuppose – in particular, a misunderstanding as to which elements of this framework express substantive assumptions about the nature of language and are, therefore, matters of legitimate controversy and rational discussion, and which, on the other hand, relate only to questions of goals and interests and are therefore no more subject to debate than the question: is chemistry right or wrong? In the light of this, it seems advisable to change my original plan and to spend much more time on background assumptions and general questions of various sorts than I had at first intended. I still hope to be able to incorporate an exposition (much abbreviated) of some recent work, but I will lead up to it more slowly, in the following steps:

(1) discussion of general background assumptions and goals that underlie and motivate much of the work in generative grammar of the past decade;

(2) discussion of various objections to this general point of view that seem to me to be based on error, misunderstanding, or equivocation of one sort or another;

(3) presentation of a theory of generative grammar of a sort exemplified, for example, in N. Chomsky, *Syntactic structures* (The Hague, 1957), R. B. Lees, *The Grammar of English nominalizations*

(Bloomington, 1960), M. Halle, "Phonology in a generative grammar", *Word* 18.54-72 (1962), and J. Katz and J. Fodor, "The Structure of a semantic theory", *Lg.* 39.170-210 (1963);

(4) discussion of various real inadequacies that have been exposed in this position in work of the past half-dozen years; and

(5) sketch of a refined and improved version of this theory, designed to overcome these difficulties.

I will try to cover these points in the first three sections, concentrating largely on syntax. Section I will deal with the first point, section II with the second, and section III with the third, fourth and fifth.

In the final section I will discuss an approach to the study of sound structure that has been gradually evolving since Chomsky, Halle, and F. Lukoff, "On accent and juncture in English", *For Roman Jakobson*, eds. M. Halle, H. Lunt, and H. MacLean 65-80 (The Hague, 1956) and has been presented in various stages of development in publications of Halle's and mine (listed in the bibliography below) since then, and will, hopefully, soon emerge to full light of day in a book that is now in active preparation. In the course of this presentation, I will also discuss a few criticisms of this approach. The discussion of criticisms will be very brief, however, since Halle and I have discussed most of them, insofar as they are known to us, in considerable detail elsewhere.[1]

In general, this essay contains no new or original material. It is intended only as an informal guide to other books and papers,[2] in which questions touched on here are dealt with more throughly, and as an attempt to clarify issues that have been raised in critical discussion.

[1] In particular, see Chomsky, *Current issues in linguistic theory* 31, 105-7 (The Hague, 1964), which deals with criticisms in C. A. Ferguson's review of Halle, *The sound pattern of Russian* (The Hague, 1959); and in Chomsky and Halle, "Some controversial questions in phonological theory", *Journal of Linguistics* 1, 97-138 (1965), which deals with objections raised by F. W. Householder jr., "On some recent claims in phonological theory", *Journal of Linguistics* 1, 13-34 (1965).

[2] E.g.Katz and P. Postal, *An integrated theory of linguistic description* (Cambridge, Mass., 1964); Chomsky, *Current issues in linguistic theory*, and *Aspects of the theory of syntax* (Cambridge, Mass., 1965).

In the course of this paper I will also make a few remarks about historical backgrounds for the position that will be outlined.[3] Quite a few commentators have assumed that recent work in generative grammar is somehow an outgrowth of an interest in the use of computers for one or another purpose, or that it has some other engineering motivation, or that it perhaps constitutes some obscure branch of mathematics. This view is incomprehensible to me, and it is, in any event, entirely false. Much more perceptive are those critics who have described this work as in large measure a return to the concerns and often even the specific doctrines of traditional linguistic theory. This is true – apparently to an extent that many critics do not realize.[4] I differ from them only in regarding this observation not as a criticism, but rather as a definite merit of this work. That is, it seems to me that it is the modern study of language prior to the explicit study of generative grammar that is seriously defective in its failure to deal with traditional questions and, furthermore, to recognize the essential correctness of many of the traditional answers and the extent to which they provide a fruitful basis for current research.

A distinction must be made between what the speaker of a language knows implicitly (what we may call his *competence*) and

[3] This matter is discussed in more detail in Chomsky, *Current issues in linguistic theory*, § 1, in *Aspects of the theory of syntax*, Ch. 1, § 8, and in *Cartesian linguistics* (New York, 1966).

[4] To cite just one example, consider A. Reichling, "Principles and methods of syntax: cryptanalytical formalism", *Lingua* 10.1-17 (1961), who asserts that obviously I could not 'be said to sympathize with such a "mentalistic monster" as the "innere Sprachform"'. But in fact the work that he is discussing is quite explicitly and selfconsciously mentalistic (in the traditional, not the Bloomfieldian, sense of this word – that is, it is an attempt to construct a theory of mental processes), and it can, furthermore, be quite accurately described as an attempt to develop further the Humboldtian notion of 'form of language' and its implications for cognitive psychology, as will surely be evident to anyone familiar both with Humboldt and with recent work in generative grammar (for explicit discussion, see the references cited above).

I will not consider Reichling's criticisms of generative grammar here. The cited remark is just one illustration of his complete lack of comprehension of the goals, concerns, and specific content of the work that he was discussing, and his discussion is based on such gross misrepresentation of this work that comment is hardly called for.

what he does (his *performance*). A grammar, in the traditional view, is an account of competence. It describes and attempts to account for the ability of a speaker to understand an arbitrary sentence of his language and to produce an appropriate sentence on a given occasion. If it is a pedagogic grammar, it attempts to provide the student with this ability; if a linguistic grammar, it aims to discover and exhibit the mechanisms that make this achievement possible. The competence of the speaker-hearer can, ideally, be expressed as a system of rules that relate signals to semantic interpretations of these signals. The problem for the grammarian is to discover this system of rules; the problem for linguistic theory is to discover general properties of any system of rules that may serve as the basis for a human language, that is, to elaborate in detail what we may call, in traditional terms, the general *form of language* that underlies each particular realization, each particular natural language.

Performance provides evidence for the investigation of competence. At the same time, a primary interest in competence entails no disregard for the facts of performance and the problem of explaining these facts. On the contrary, it is difficult to see how performance can be seriously studied except on the basis of an explicit theory of the competence that underlies it, and, in fact, contributions to the understanding of performance have largely been by-products of the study of grammars that represent competence.[5]

Notice, incidentally, that a person is not generally aware of the rules that govern sentence-interpretation in the language that he knows; nor, in fact, is there any reason to suppose that the rules can be brought to consciousness. Furthermore, there is no reason to expect him to be fully aware even of the empirical consequences of these internalized rules – that is, of the way in which signals are assigned semantic interpretations by the rules of the language that

[5] For discussion, see G. A. Miller and Chomsky, "Finitary models of language users", *Handbook of mathematical psychology*, Vol. II, eds. R. D. Luce, R. Bush, and E Galanter (New York, 1963); Chomsky, *Aspects of the theory of syntax*, Ch. 1, § 2.

he knows (and, by definition, knows perfectly). On the difficulties of becoming aware of one's own linguistic intuitions, see the discussion in Chomsky, *Aspects of the theory of syntax*, Ch. 1, § 4. It is important to realize that there is no paradox in this; in fact, it is precisely what should be expected.

Current work in generative grammar has adopted this traditional framework of interests and concerns. It attempts to go beyond traditional grammar in a fundamental way, however. As has repeatedly been emphasized, traditional grammars make an essential appeal to the intelligence of the reader. They do not actually formulate the rules of the grammar, but rather give examples and hints that enable the intelligent reader to determine the grammar, in some way that is not at all understood. They do not provide an analysis of the 'faculté de langage' that makes this achievement possible. To carry the study of language beyond its traditional bounds, it is necessary to recognize this limitation and to develop means to transcend it. This is the fundamental problem to which all work in generative grammar has been addressed.

The most striking aspect of linguistic competence is what we may call the 'creativity of language', that is, the speaker's ability to produce new sentences, sentences that are immediately understood by other speakers although they bear no physical resemblance to sentences which are 'familiar'. The fundamental importance of this creative aspect of normal language use has been recognized since the seventeenth century at least, and it was at the core of Humboldtian general linguistics. Modern linguistics, however, is seriously at fault in its failure to come to grips with this central problem. In fact, even to speak of the hearer's 'familiarity with sentences' is an absurdity. Normal use of language involves the production and interpretation of sentences that are similar to sentences that have been heard before only in that they are generated by the rules of the same grammar, and thus the only sentences that can in any serious sense be called 'familiar' are clichés or fixed formulas of one sort or another. The extent to which this is true has been seriously underestimated even by those linguists (e.g. O. Jespersen) who have given some attention to the problem of

creativity. This is evident from the common description of language use as a matter of 'grammatical habit' [e.g. O. Jespersen, *Philosophy of grammar* (London, 1924)]. It is important to recognize that there is no sense of 'habit' known to psychology in which this characterization of language use is true (just as there is no notion of 'generalization' known to psychology or philosophy that entitles us to characterize the new sentences of ordinary linguistic usage as generalizations of previous performance). The familiarity of the reference to normal language use as a matter of 'habit' or as based on 'generalization' in some fundamental way must not blind one to the realization that these characterizations are simply untrue if terms are used in any technical or well-defined sense, and that they can be accepted only as metaphors – highly misleading metaphors, since they tend to lull the linguist into the entirely erroneous belief that the problem of accounting for the creative aspect of normal language use is not after all a very serious one.

Returning now to the central topic, a *generative grammar* (that is, an explicit grammar that makes no appeal to the reader's 'faculté de langage' but rather attempts to incorporate the mechanisms of this faculty) is a system of rules that relate signals to semantic interpretations of these signals. It is *descriptively adequate* to the extent that this pairing corresponds to the competence of the idealized speaker-hearer. The idealization is (in particular) that in the study of grammar we abstract away from the many other factors (e.g., memory limitations, distractions, changes of intention in the course of speaking, etc.) that interact with underlying competence to produce actual performance.

If a generative grammar is to pair signals with semantic interpretations, then the theory of generative grammar must provide a general, language-independent means for representing the signals and semantic interpretations that are interrelated by the grammars of particular languages. This fact has been recognized since the origins of linguistic theory, and traditional linguistics made various attempts to develop theories of universal phonetics and universal semantics that might meet this requirement. Without going into any detail, I think it would be widely agreed that the general

problem of universal phonetics is fairly well-understood (and has been, in fact, for several centuries), whereas the problems of universal semantics still remain veiled in their traditional obscurity. We have fairly reasonable techniques of phonetic representation that seem to approach adequacy for all known languages, though, of course, there is much to learn in this domain. In contrast, the immediate prospects for universal semantics seem much more dim, though surely this is no reason for the study to be neglected (quite the opposite conclusion should, obviously, be drawn). In fact, recent work of Katz, Fodor, and Postal, to which I return in the third section, seems to me to suggest new and interesting ways to reopen these traditional questions.

The fact that universal semantics is in a highly unsatisfactory state does not imply that we must abandon the program of constructing grammars that pair signals and semantic interpretations. For although there is little that one can say about the language-independent system of semantic representation, a great deal is known about conditions that semantic representations must meet, in particular cases. Let us then introduce the neutral technical notion of 'syntactic description', and take a syntactic description of a sentence to be an (abstract) object of some sort, associated with the sentence, that uniquely determines its semantic interpretation (the latter notion being left unspecified pending further insights into semantic theory)[6] as well as its phonetic form. A particular linguistic theory must specify the set of possible syntactic descriptions for sentences of a natural language. The extent to which these syntactic descriptions meet the conditions that we know must apply to semantic interpretations provides one measure of the success and sophistication of the grammatical theory in question. As the theory of generative grammar has progressed, the notion of syntactic description has been clarified and extended. I will discuss below some recent ideas on just what should constitute the syntactic

[6] Working in this framework then, we would regard a semantically ambiguous minimal element as constituting two distinct lexical entries; hence two syntactic descriptions might differ only in that they contain different members of a pair of homonymous morphemes.

description of a sentence, if the theory of generative grammar is to provide descriptively adequate grammars.

Notice that a syntactic description (henceforth, SD) may convey information about a sentence beyond its phonetic form and semantic interpretation. Thus we should expect a descriptively adequate grammar of English to express the fact that the expressions (1)-(3) are ranked in the order given in terms of 'degree of deviation' from English, quite apart from the question of how interpretations can be imposed on them [in the case of (2) and (3)]:

(1) the dog looks terrifying
(2) the dog looks barking
(3) the dog looks lamb

A generative grammar, then, must at least determine a pairing of signals with SD's; and a theory of generative grammar must provide a general characterization of the class of possible signals (a theory of phonetic representation) and the class of possible SD's. A grammar is descriptively adequate to the extent that it is factually correct in a variety of respects, in particular, to the extent that it pairs signals with SD's that do in fact meet empirically given conditions on the semantic interpretations that they support. For example, if a signal has two intrinsic semantic interpretations in a particular language [e.g., (4) or (5), in English], a grammar of this language will approach descriptive adequacy if it assigns two SD's to the sentence, and, beyond this, it will approach descriptive adequacy to the extent that these SD's succeed in expressing the basis for the ambiguity.

(4) they don't know how good meat tastes
(5) what disturbed John was being disregarded by everyone

In the case of (4), for example, a descriptively adequate grammar must not only assign two SD's to the sentence but must also do so in such a way that in one of these the grammatical relations of *good*, *meat*, and *taste* are as in 'meat tastes good', while in the other they are as in 'meat which is good tastes Adjective' (where the notion 'grammatical relation' is to be defined in a general way within the

linguistic theory in question), this being the basis for the alternative semantic interpretations that may be assigned to this sentence. Similarly, in the case of (5), it must assign to the pair *disregard-John* the same grammatical relation as in 'everyone disregards John', in one SD; whereas in the other it must assign this very same relation to the pair *disregard-what* (*disturbed John*), and must assign no semantically functional grammatical relation at all to *disregard-John*. On the other hand, in the case of (6) and (7) only one SD should be assigned by a descriptively adequately grammar. This SD should, in the case of (6), indicate that *John* is related to *incompetent* as it is in 'John is incompetent' and, that *John* is related to *regard* (*as incompetent*) as it is in 'everyone regards John as incompetent'. In the case of (7), the SD must indicate that *our* is related to *regard* (*as incompetent*) as *us* is related to *regard* (*as incompetent*) in 'everyone regards us as incompetent'.

(6) what disturbed John was being regarded as incompetent by everyone.

(7) what disturbed John was our being regarded as incompetent by everyone.

Similarly, in the case of (8), the grammar must assign four distinct SD's, each of which specifies the system of grammatical relations that underlies one of the distinct semantic interpretations of this sentence:

(8) the police were ordered to stop drinking after midnight.

Examples such as these should suffice to illustrate what is involved in the problem of constructing descriptively adequate generative grammars and developing a theory of grammar that analyzes and studies in full generality the concepts that appear in these particular grammars. It is quite evident from innumerable examples of this sort that the conditions on semantic interpretations are sufficiently clear and rich so that the problem of defining the notion 'syntactic description' and developing descriptively adequate grammars (relative to this notion of SD) can be made quite concrete, despite the fact that the notion 'semantic interpretation' itself still resists any

deep analysis. We return to some recent ideas on semantic interpretation of SD's in section III.

A grammar, once again, must pair signals and SD's. The SD assigned to a signal must determine the semantic interpretation of the signal, in some way which, in detail, remains unclear. Furthermore, each SD must uniquely determine the signal of which it is the SD, (uniquely, that is, up to free variation). Hence the SD must (i) determine a semantic interpretation and (ii) determine a phonetic representation. Let us define the 'deep structure of a sentence' as that aspect of the SD that determines its semantic interpretation, and the 'surface structure of a sentence' as that aspect of the SD that determines its phonetic form. A grammar, then, must consist of three components: a *syntactic component*, which generates SD's each of which consists of a surface structure and a deep structure; a *semantic component*, which assigns a semantic interpretation to a deep structure; a *phonological component*, which assigns a phonetic interpretation to a surface structure, Thus the grammar as a whole will associate phonetic representations and semantic interpretations, as required, this association being mediated by the syntactic component that generates deep and surface structures as elements of SD's.

The notions 'deep structure' and 'surface structure' are intended as explications of the Humboldtian notions 'inner form of a sentence' and 'outer form of a sentence' (the general notion 'form' is probably more properly to be related to the notion 'generative grammar' itself — cf. Chomsky, *Current issues in linguistic theory*, for discussion). The terminology is suggested by the usage familiar in contemporary analytic philosophy [cf., for example, Wittgenstein, *Philosophical investigations* 168 (Oxford, 1953)]. C. F. Hockett has also used these terms [*A course in modern linguistics*, Ch. 29 (New York, 1958)] in roughly the same sense.

There is good reason (see below, section IV) to suppose that the surface structure of a sentence is a labeled bracketing that segments it into its continuous constituents, categorizes these, segments the constituents into further categorized constituents, etc. Thus underlying (6), for example, is a surface structure that analyzes it into its

constituents (perhaps, 'what disturbed John', 'was', 'being regarded as incompetent by everyone'), assigning each of these to a certain category indicated by the labeling, then further segmenting each of these into its constituents (e.g., perhaps, 'what disturbed John' into 'what' and 'disturbed John'), each of these being assigned to a category indicated by the labeling, etc., until ultimate constituents are reached. Information of this sort is, in fact, necessary to determine the phonetic representation of this sentence. The labeled bracketing can be presented in a tree-diagram, or in other familiar notations.

It is clear, however, that the deep structure must be quite different from this surface structure. For one thing, the surface representation in no way expresses the grammatical relations that are, as we have just observed, crucial for semantic interpretation. Secondly, in the case of an ambiguous sentence such as, for example, (5), only a single surface structure may be assigned, but the deep structures must obviously differ. Such examples as these are sufficient to indicate that the deep structure underlying a sentence cannot be simply a labeled bracketing of it. Since there is good evidence that the surface structure should, in fact, simply be a labeled bracketing, we conclude that deep structures cannot be identified with surface structures. The inability of surface structure to indicate semantically significant grammatical relations (i.e., to serve as deep structure) is one fundamental fact that motivated the development of transformational generative grammar, in both its classical and modern varieties.

In summary, a full generative grammar must consist of a syntactic, semantic, and phonological component. The syntactic component generates SD's each of which contains a deep structure and a surface structure. The semantic component assigns a semantic interpretation to the deep structure and the phonological component assigns a phonetic interpretation to the surface structure. An ambiguous sentence has several SD's, differing in the deep structures that they contain (though the converse need not be true).

So far I have said little that is in any way controversial. This discussion has so far simply delimited a certain domain of interest

and a certain class of problems, and has suggested a natural framework for dealing with these problems. The only substantive comments (i.e., factual assertions) that I have so far made within this framework are that the surface structure is a labeled bracketing and that deep structures must in general be distinct from surface structures. The first of these assertions is well-supported (see below), and would probably be widely accepted. The second is surely much too obvious to require elaborate defense.

To go on from here to develop a substantive linguistic theory we must provide:

(9) (i) theories of phonetic and semantic representation
(ii) a general account of the notion 'syntactic description'
(iii) a specification of the class of potential generative grammars
(iv) a general account of how these grammars function, that is, how they generate SD's and assign to them phonetic and semantic interpretations, thus pairing phonetically represented signals with semantic interpretations.

Before going on to discuss these substantive questions, let us reassure ourselves about the uncontroversial character of what has preceded. Is there, in fact, anything in this account to which exception can be taken? Surely there is no conceivable question about the necessity for distinguishing competence from performance in the way suggested above. Having made this distinction, one may or may not choose to be interested in the general question of accounting for linguistic competence. If one chooses to concern himself with this question, he must immediately face the fact of 'creativity' and must therefore focus attention on the problem of constructing generative grammars. It is difficult to see how a full generative grammar can be regarded, ultimately, as anything other than a system of rules that relate signals to semantic interpretations; and, having set this goal, one is immediately faced with the problem of developing a rich enough notion of 'syntactic description' to support phonetic interpretation, on the one side, and semantic interpretation, on the other. The distinction between deep and surface structure emerges from even the most superficial examina-

tion of real linguistic material. Hence the conclusions outlined so far seem inescapable if the problem of studying linguistic competence is taken up. Notice that a substantive linguistic theory involves a specification of (9iv) as well as (9iii). For example, an essential part of the theory of phrase structure grammar is a particular specification of how categories and relations are determined for generated strings (see Chomsky, *Logical structure of linguistic theory*, Cambridge, 1955, chapter VI), and such a specification has been presupposed whenever this theory has been investigated. A change in this specification is as much a revision of the theory as a change in the specification of the class (9iii) of potential grammars. Failure to understand this leads to immediate absurdities. Thus if one thinks of the theory of 'phrase structure grammar' with the technique of interpretation (9iv) left free, one can easily prove that a phrase structure grammar of the language L assigns to sentences of L the structural descriptions assigned by some transformational grammar of L, etc. This point should be obvious without further discussion.

Suppose that one chooses not to study linguistic competence (and, concomitantly, linguistic performance within the framework of a theory of competence). One might, alternatively, choose to limit attention to performance, or to surface structures, or to sound patterns in isolation from syntactic structure, or to voiced fricatives, or to first halves of sentences. The only question that arises, if any of these proposals is adopted, is whether any interesting result is likely to be attainable under such arbitrary limitation of subject matter. In each of the cited cases it seems quite unlikely. It is, in general, unclear why anyone should insist on studying an isolated aspect of the general problem of grammatical description unless there is some reason to believe that this is not affected by the character of other aspects of grammar.[7]

[7] Perhaps this matter can be clarified by considering examples of the latter sort. Thus, for example, it is quite reasonable to study semantics in isolation from phonology or phonology in isolation from semantics, since, at the moment, there seems to be no non-trivial relation between the systems of phonological and semantic interpretation and no significant way in which semantic considerations can play a role in phonology or phonological considerations in semantics. Similarly, it seems quite reasonable to develop a theory of syntactic

I have been discussing so far only the question of descriptive adequacy of grammars and the problem of developing a linguistic theory that will provide the basis for the construction of descriptively adequate grammars. As has been repeatedly emphasized, however [see, e.g., Chomsky, *Syntactic structures*; "Explanatory models in linguistics", *Logic, methodology, and philosophy of science*, eds. E. Nagel, P. Suppes, and A. Tarski 528-50 (Stanford, 1962); *Current issues in linguistic theory*; and *Aspects of the theory of syntax*], the goals of linguistic theory can be set much higher than this; and, in fact, it is a prerequisite even for the study of descriptive adequacy that they be set higher than this. It is essential also to raise the question of 'explanatory adequacy' of linguistic theory. The nature of this question can be appreciated readily in terms of the problem of constructing a hypothetical language-acquisition device AD that can provide as 'output' a descriptively adequate grammar G for the language L on the basis of certain primary linguistic data from L as an input; that is, a device represented schematically as (10):

(10) primary linguistic data → [AD] → G

structure with no primitive notions of an essentially semantic nature, since, at the moment, there is no reason to assume that a priori semantic concepts play a role in determining the organization of the syntactic component of a grammar. On the other hand, it would be absurd to study semantics (and similarly, it seems to me, phonology) in isolation from syntax, since the syntactic interpretation of a sentence (similarly, its phonetic interpretation) depends in an essential way on its deep (respectively, surface) structure. And it would be absurd to develop general syntactic theory without assigning an absolutely crucial role to semantic considerations, since obviously the necessity to support semantic interpretation is one of the primary requirements that the structures generated by the syntactic component of a grammar must meet. For discussion of these points, see Chomsky (*Syntactic structures*; *Current issues in linguistic theory*), Lees, Review of Chomsky, *Syntactic structures*, *Lg.* 33.375-408 (1957), Katz and Postal (*An integrated theory of linguistic description*), and many other references.

Far too little care has been taken in the discussion of these questions in modern linguistics. As a result, there has been much confusion about them, and many dogmatic claims have been voiced and repeatedly echoed with no attempt to justify or support them by serious argument. The issues are important; while no answers to any of these questions can be given with any certainty, the tentative position that the linguist accepts may have an important influence on the character of the work that he does.

We naturally want the device AD to be language-independent – that is, capable of learning any human language and only these. We want it, in other words, to provide an implicit definition of the notion 'human language'. Were we able to develop the specifications for a language-acquisition device of this sort, we could realistically claim to be able to provide an explanation for the linguistic intuition – the tacit competence – of the speaker of a language. This explanation would be based on the assumption that the specifications of the device AD provide the basis for language-acquisition, primary linguistic data from some language providing the empirical conditions under which the development of a generative grammar takes place. The difficulties of developing an empirically adequate language-independent specification of AD are too obvious to require extended discussion; the vital importance of raising this problem and pursuing it intensively at every stage of linguistic investigation also seems to me entirely beyond the possibility of debate (cf. the references cited above for elaboration of this point).

To pursue the study of explanatory adequacy, we may proceed in two parallel ways. First, we must attempt to provide as narrow a specification of the aspects of linguistic theory listed in (9) as is compatible with the known diversity of languages – we must, in other words, develop as rich a hypothesis concerning linguistic universals as can be supported by available evidence. This specification can then be attributed to the system AD as an intrinsic property. Second, we may attempt to develop a general evaluation procedure, as an intrinsic property of AD, which will enable it to select a particular member of the class of grammars that meet the specifications (9) (or, conceivably, to select a small set of alternatives, though this abstract possibility is hardly worth discussing for the present) on the basis of the presented primary linguistic data. This procedure will then enable the device to select one of the a priori possible hypotheses – one of the permitted grammars – that is compatible with the empirically given data from a given language. Having selected such a hypothesis, it has 'mastered' the language described by this grammar, (and it thus knows a great deal beyond

what it has explicitly 'learned'). Given a linguistic theory that specifies (9) and an evaluation procedure, we can explain some aspect of the speaker's competence whenever we can show with some plausibility that this aspect of his competence is determined by the most highly valued grammar of the permitted sort that is compatible with data of the kind to which he has actually been exposed.

Notice that an evaluation procedure (simplicity measure, as it is often called in technical discussion) is itself an empirical hypothesis concerning universal properties of language; it is, in other words, a hypothesis, true or false, about the prerequisites for language-acquisition. To support or refute this hypothesis, we must consider evidence as to the factual relation between primary linguistic data and descriptively adequate grammars. We must ask whether the proposed evaluation procedure in fact can mediate this empirically given relation. An evaluation procedure, therefore, has much the status of a physical constant; in particular, it is impossible to support or reject a specific proposal on the basis of a priori argument.

Once again, it is important to recognize that there is nothing controversial in what has just been said. One may or may not choose to deal with the problem of explanatory adequacy. One who chooses to overlook this problem may (and, in my opinion, surely will) find that he has eliminated from consideration one of the most important sources of evidence bearing on the problems that remain (in particular, the problem of descriptive adequacy).[8] His situation,

[8] The reason for this is quite simple. Choice of a descriptively adequate grammar for the language L is always much underdetermined (for the linguist, that is) by data from L. Other relevant data can be adduced from study of descriptively adequate grammars of other languages, but only if the linguist has an explanatory theory of the sort just sketched. Such a theory can receive empirical support from its success in providing descriptively adequate grammars for other languages. Furthermore, it prescribes, in advance, the form of the grammar of L and the evaluation procedure that leads to the selection of this grammar, given data. In this way, it permits data from other languages to play a role in justifying the grammar selected as an empirical hypothesis concerning the speakers of L. This approach is quite natural. Following it, the linguist comes to a conclusion about the speakers of L on the basis of an independently supported assumption about the nature of language in general – an assumption,

then, may be quite analogous to that of the person who has decided to limit his attention to surface structures (to the exclusion of deep structures) or to first halves of sentences. He must show that the delimitation of interest leaves him with a viable subject. But, in any event, he surely has no basis for objecting to the attempt on the part of other linguists to study the general question of which he has (artificially, in my opinion) delimited one facet.

I hope that these remarks will be sufficient to show the complete pointlessness of much of the debate over the specific evaluation procedures (simplicity measures) that have been proposed as empirical hypotheses concerning the form of language in the course of work in generative grammar. To mention just one example, consider Householder's criticism (Householder, "On some recent claims in phonological theory") of several proposals of Halle's regarding an appropriate evaluation procedure for phonology. Halle presented a certain theory of phonological processes, including, as an essential part, a certain empirical hypothesis regarding a simplicity measure. A crucial aspect of this theory was its complete reliance on distinctive features in the formulation of phonological rules to the exclusion of any 'segmental' notation (e.g., phonemic notation) except as an informal expository device. His evaluation measure involved minimization of features in the lexicon and the phonological rules. In support of this theory he showed that a variety of facts can be explained on these assumptions. He also discussed alternative theories that use segmental notation along with or instead of feature notation and gave several arguments to show that under these assumptions it is difficult to see how any empirically valid evaluation measure can be formulated – in particular, he showed how various rather natural measures involving minimization or maximization fail on empirical grounds.

Householder makes no attempt to refute these arguments but

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that is, concerning the general 'faculté de langage' that makes language-acquisition possible. The general explanatory theory of language and the specific theory of a particular language that results from application of the general theory to data each has psychological content, the first as a hypothesis about innate mental structure, the second as a hypothesis about the tacit knowledge that emerges with exposure to appropriate experience.

simply objects to them because they fail to meet certain a priori conditions that he arbitrarily imposes on any notion of 'evaluation procedure', in particular, the requirement that such a procedure must favor grammars that use fewer symbols and that are easy for the linguist to read. Since the grammars that Halle proposes, with their consistent reliance on feature representation, require more symbols than grammars that use auxiliary symbols as abbreviations for feature sets, and since Halle's grammars are (Householder claims) not easy to read, he concludes that the theory on which they are based must be mistaken. But clearly a priori arguments of this sort have no bearing on an empirical hypothesis about the nature of language (i.e. about the structure of a general language-acquisition device of the sort described above). Consequently, Householder's critique has no relevance to any issue that Halle discusses. Unfortunately, much of the criticism of recent attempts to develop valid evaluation measures is based on similar presuppositions.

Notice, incidentally, that there is an interesting but poorly understood sense in which one can talk of the 'simplicity' or 'elegance' or 'naturalness' of a theory (of language, of the chemical bond, etc.), but this 'absolute' sense of simplicity has no clear relevance to the attempt to develop an evaluation measure (a simplicity measure) as a part of a theory of grammar. Such a theory is an empirical hypothesis, true or false, proposed to account for some domain of linguistic fact. The 'simplicity measure' that it contains is a constituent part of this empirical hypothesis. This distinction between 'simplicity' as an absolute notion of general epistemology and 'simplicity' as a part of a theory of grammar has been repeatedly emphasized; confusion regarding this point has, nevertheless, been quite widespread. Failure to make this distinction vitiates most of the criticism of evaluation procedures that has appeared in recent years.

2

DISCUSSION OF CRITICISMS

I have concluded chapter 1 of the outline presented in the introductory remarks to this essay and would now like to turn to chapter 2, namely, to various objections that have been raised against the position sketched above. I have tried to indicate why I think any such objections must be mistaken, by attempting to show that the position is really quite uncontroversial. Perhaps further clarification can be achieved through a more detailed examination of some of these objections.

Consider first the Reichling-Uhlenbeck criticisms.[1] Their view is, apparently, that the linguist must limit himself to what I called above 'surface structure', in fact, to certain restricted aspects of surface structure. They observe that a sentence is a linear sequence of elements which are grouped into units and then into larger units 'according to certain rules'.[2] The only clues to the relational

[1] Reichling, op. cit.; E. M. Uhlenbeck, "An appraisal of transformational theory", *Lingua* 12.1-18 (1963). I will concentrate on the latter, since, as noted above, Reichling's remarks are based on an account of 'generative grammar' that has little identifiable relation to any of the actual work in generative grammar. However, Uhlenbeck asserts that their views as to the nature of syntactic description are essentially the same, so perhaps nothing is lost by restricting the discussion largely to his paper.

[2] No examples of such rules are given. In his discussion of linguistic rules, Uhlenbeck limits himself to rules governing morphological processes (e.g. formation of plurals). He gives no examples of recursive rules, and therefore does not touch upon what for syntax is the central problem, namely, the problem of creativity mentioned above. We are therefore left to guess from his examples what kind of system of rules he has in mind. That he is in any position to deal with the problem of creativity is doubtful, given his view that the linguist must consider only 'regularities ... observed in speech', and must limit himself to the 'set of habits by which ... [native speakers] ... know how to proceed in speech'. But there is nc reason to believe that syntactic rules represent 'observable regularities of speech', beyond the simplest cases; and few aspects of the

structure of an utterance are intonation, arrangement, and phonetic signals (by 'arrangement', I presume they mean linear ordering and grouping of units into larger units). This grouping of units into larger units, these into larger units etc., defines the 'fundamental aspects of the utterance'. 'It is impossible to conceive of other types of syntagmatic indications.' One must avoid the making of distinctions that are not present (presumably, this means 'formally marked') in the linguistic data. Thus, for example, transitive and intransitive verbs cannot be distinguished in English (cf. Uhlenbeck, 17) – it is, therefore, impossible to distinguish 'John compelled' or 'Bill elapsed John' from 'John compelled Bill to leave' or 'a week elapsed', on any syntactic grounds, in their view. Restricting ourselves to the consideration of surface structure, such expressions as (11) can correctly be described as syntactically ambiguous, but not the expressions of (12) (Uhlenbeck, 9):

(11) (i) old men and women
(ii) they are flying planes

(12) (i) the shooting of the hunters
(ii) John was frightened by the new methods.

The ambiguity of the latter two must be explained as in some way based on 'extralinguistic data'.[3] Uhlenbeck does not elaborate on

normal formation and use of sentences appear to involve 'habits', in any reasonably well-defined sense of this notion. Consequently, the study of observable regularities and habit can hardly be expected to have much bearing on syntactic theory or syntactic description, or on the normal non-stereotyped use of language.

[3] Uhlenbeck is, incidentally, in error in assuming that in a transformational grammar (12i) would be derived from either the phrase 'shoot the hunters' or the phrase 'the hunters shoot' and that this is the proposed explanation for the ambiguity. Sentences are never derived from kernel sentences, as he asserts, but rather from the abstract structures that underlie kernel sentences (and all others). The difference is fundamental. What is claimed is not that the hearer first converts (12i) into one of two other expressions, and that (12i) is understood in these terms, but rather that (12i) is analyzed in terms of one of two abstract underlying systems of grammatical relations, one of which happens also to underlie 'the hunters shoot' and the other of which happens to underlie 'shoot the hunters'. That is, in one case the phrase (12i) is interpreted with the Subject-Verb relation holding of *hunters-shoot*, and, in the other, with the Object-Verb relation holding of this pair. One may choose to define 'linguistics' in such a way as to exclude this observation from its domain, but it is hard for me to

the difference between the two cases (11) and (12). From what he does say, we might suppose that he is willing to agree to the constructional homonymity of (11) because the alternative interpretations can be represented by a difference of bracketing (hence in surface structure); but in the case of (12i) and (12ii) he insists that there is only one syntactic structure and one system of grammatical relations because the alternative interpretations cannot be represented in surface structure (this, of course, being the reason for the choice of these examples in the exposition of transformational grammar that he is discussing). Uhlenbeck concludes that it is his conviction (and Reichling's) that the connections that can be established in the terms he allows, as well as the rules that express these connections will turn out to be very simple. Though he gives no examples of such rules, I do not doubt that his conviction will turn out to be correct. That is, if one limits oneself to surface structure (pure labeled bracketing) and to formally marked relations, then so little of the structure of sentences is expressible that what remains is, no doubt, likely to be quite simple.

In brief, Uhlenbeck proposes that syntactic analysis be restricted to surface structure. What he proposes is indistinguishable from the several varieties of taxonomic analysis (immediate constituent analysis) that have been developed, though with much greater clarity and detail, in the 'neo-Bloomfieldian' linguistics of the past several decades [for discussion of these, and their inadequacies, see Postal, *Constituent structures: a study of contemporary models of syntactic description* (Bloomington, Indiana, and The Hague, 1964), Chomsky, *Current issues in linguistic theory*, and many other references]. If he has something else in mind, it does not appear either from his examples or his exposition.[4] Similarly, Reichling wishes to

believe that Uhlenbeck means to deny the facts that are represented in these terms in a transformational grammar (but not, of course, represented in surface structure, in the technical sense that has been given to this term).

[4] His examples, however, deserve further discussion. Uhlenbeck differs from the tradition in that he analyzes 'Subject-Verb-Object' constructions as (Subject-Verb) (Object) – thus, for example, 'John hit Bill' has the immediate constituent analysis 'John hit – Bill'. No argument is offered for this analysis, which runs counter to all known syntactic, phonological and semantic considerations that are relevant to such examples. For discussion, see Lunt, ed. [*Proceedings of the*

restrict syntactic investigation to IC analysis and to formally marked relations between words, that is, to connections effected by concord, rection, categorization into units, or intonation (Reichling, 2). No other 'connections between meanings' are allowed. It therefore follows that the facts about the 'connections of meaning' in such examples as (4), (5), (6), (7), (8), (11), (12) are inexpressible within syntax, and presumably relegated to some (for the moment, non-existent) theory of 'extra-linguistic context'.

The reader of these papers will observe that the only criticism made of work in generative grammar (other than criticisms based on misstatement – cf., e.g. note 3) is that it does not remain within the limits set by the critics. The only argument for remaining within these limits is that they are the only ones conceivable. However, other more abstract representations of 'connections of meaning' are not only conceivable but have in fact been conceived and developed in considerable detail both in traditional grammar and in the modern work in transformational grammar that continues and extends traditional grammar. Thus the limitation to surface structure is quite arbitrary. There is no reason to accept it. There has been no indication that a viable domain of linguistic processes or linguistic structure is delimited by this arbitrary restriction. There is not the slightest reason why one should not investigate the mass of problems about interpretation of sentences that transcend these entirely arbitrary limits (e.g. the problems posed by the examples cited above).

Ninth International Congress of Linguists 983 (The Hague, 1964]; Chomsky (*Aspects of the theory of syntax* p. 194); and Katz and Postal (*An integrated theory of linguistic description*).

Uhlenbeck apparently feels that his and Reichling's approach falls neither within the bounds of "American descriptivism" nor of "the traditional, more or less antediluvian approach of language description" (Uhlenbeck, 5). That their approach has little to do with traditional linguistics is quite clear, since it systematically avoids the traditionally central problems of deep structure, semantic interpretation, and creativity. But why he feels that it escapes the limitations of American descriptivism is not at all obvious. There is no aspect of his views, as described here, that is not strictly formulable in terms of the varieties of taxonomic linguistics developed by American descriptivists; and he does not suggest or indicate any respect in which his views diverge from these formulations.

Reichling-Uhlenbeck might be interpreted as making something more than a terminological proposal about the limitations of the term 'linguistics' in their remarks about the role of 'extra-linguistic information' in the interpretation of sentences – in particular, in the interpretation of the sentences for which the surface structure does not represent the semantically significant grammatical relations. But these remarks are based on a simple confusion. A sentence has an inherent grammatical structure; this structure provides it with a certain range of potential semantic interpretations. In particular, the rules of English grammar that constitute the competence of the native speaker, provide the sentences (4), (5), (8), (11), (12) with alternative syntactic descriptions, each expressing its network of semantically significant grammatical relations. In a particular situation, the hearer may use information that goes well beyond grammar to determine which of the potential interpretations was intended (or whether, perhaps, something was intended that goes beyond the explicit semantic content of the utterance that was actually used). Absolutely nothing of any significance is known about this use of extra-grammatical information in interpretation of sentences, beyond the fact that it exists and is an important characteristic of performance. If a person enters a room and produces, for example, sentence (12ii), we know that the content of his assertion is (roughly) either that John was frightened by the existence of the new methods or that new methods of frightening people were used to frighten John. If he produces (4), we know that the content of his assertion is either that they don't know how good the taste of meat is, or that they know little about the taste of good meat. The 'situational context' is the same in both cases, and does not affect the range of possible contents that these sentences may have, in accordance with the linguistic rules of English. The determination of what the speaker actually intended, of course, involves extra-grammatical considerations and other knowledge well beyond knowledge of language.[5] Surely this is quite obvious,

[5] Perhaps some of the confusion about this matter results from a failure to distinguish 'meaning' in the sense of 'linguistic meaning' from 'meaning' in the sense roughly of 'intention'. In the former sense we say that the meaning of (4)

and there is hardly much point in discussing it in further detail.

There is not a little irony in the fact that Reichling-Uhlenbeck seem to feel that they are somehow defending the study of meaning against the 'positivistic' attacks of 'cryptanalysts' and 'formalists'. The fact is quite the opposite. By arbitrarily limiting themselves to surface structure and to formally marked relations, they have simply excluded from linguistics, by fiat, just those aspects of grammatical structure that can lead to an account and explanation of semantic interpretation; and what they are opposing is, precisely, the attempt to develop linguistic theory and grammatical description to the point where it can deal with deep structures and the general problem of semantic interpretation.

A second critique of work in generative grammar is that presented in R. M. W. Dixon, *Linguistic science and logic* (The Hague, 1963). At first glance, this criticism seems to reject the framework described in Section I in a much more radical way than the Reichling-Uhlenbeck objections. Generative grammar follows the tradition in attempting to acount for competence. Dixon, on the other hand, insists that a grammar must simply deal with performance. It must restrict itself to regularities observed in a corpus.[6] So far, this is simply an arbitrary restriction of interest (motivated, apparently, by some curious terminological proposals about 'science' which

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or (12ii) is as just roughly described; in the latter, we may ask what someone meant by saying (4) or (12ii), or what he meant by slamming the door, rejecting the invitation, or any other act.

[6] And in the 'situational contexts' in which speech occurs. I will make no attempt to deal with this topic. Dixon has no more to say about context of situation than anyone else; that is, he has nothing of substance to say about it. I notice only one concrete example of reference to context of situation, namely on p. 101, where 'British Culture' is referred to as "the wider situation ... [within which] ... the lexical items *milk* and *white* will have situational correlation (one of the several factors demonstrating this will be a fairly high probability of their mutual collocation)". The high probability of the phrase 'white milk' in normal British English was a fact unknown to me before reading this. On the other hand, the high 'situational correlation' between whiteness and milk (though not *white* and *milk*) seems beyond dispute. Exactly what sense it makes to regard British Culture as 'a situation' is unclear, however. Perhaps citation of this example is sufficient to indicate why I will discuss this aspect of Dixon's proposals no further.

would exclude from science just about everything since, perhaps, Babylonian astronomy). But Dixon goes on to deny the existence of the most elementary and familiar aspects of competence. In particular, he asserts that people with no formal education "will certainly have no intuitive 'grammatical sense' and no "intuitive grammatical ideas" (p. 78), and he regards the failure to recognize this fact as one of the major flaws in generative grammar. Assuming now that words have their normal use, Dixon is apparently claiming that children will not distinguish in any way between 'look at the dog' and 'the at dog look'; that uneducated adults will note no distinction among (1)-(3), above; that the method by which they interpret the sentences (1), (4)-(8), (11)-(12) is precisely the same as the mechanism by which they would (with equal facility) interpret any arbitrary permutation of the words of these sentences, etc. Since "linguistic research amongst aborigines ... has confirmed that ... those that have not been exposed to the European tradition of grammatical teaching have no recognizable intuitions of 'grammaticalness'" [Dixon, "'A trend in semantics': rejoinder", *Linguistics* 4.17 (1964)], we may conclude, presumably, that aborigines are unable to distinguish between sentences and right-left inversions of sentences in their language and (by analogy) that English-speaking children or uneducated adults are in the same position.

Before attributing to Dixon any such absurd views, however, we must read a bit further. The quote from Dixon (1964) given above reads more fully as follows: "linguistic research amongst aborigines ... has confirmed that *although speakers have firm ideas concerning what is in their language*, those that have not been exposed to the European tradition of grammatical teaching have no recognizable intuitions of 'grammaticalness'" (italics mine). The italicized remark implies that Dixon's aborigines have firm intuitions about grammaticalness, in the only sense in which this term has, to my knowledge, ever been used. Therefore Dixon is not denying the obvious, as assumed in my comments above, but is rather proposing some new usage for the term 'grammatical' (a usage which he does not go on to explain, except to point out that in this usage, people have no intuitions of 'grammaticalness'). Further reading only confirms

this supposition. Thus he seems to imply (1963, 76-7) that the distinctions among (1), (2), (3) can be accounted for in terms of 'lexis and lexical patternings'.[7] Clearly there is little point in accounting for a distinction if it does not exist.

Evidently, then, Dixon's rejection of the notion of 'grammaticalness' on which all grammatical description, traditional, structuralist, or generative, is based,[8] is merely terminological and therefore of no

[7] Whatever this may be. Dixon points out (1963, p. 64) that theories of generative grammar have "no equivalent to our [i.e. his and Halliday's] lexis". But all that he says about lexis is that it somehow has to do with the probability of phrases or the probability with which an item will occur in certain contexts, no further specification of these notions being given. This 'theory of lexis' is too vague to discuss and, for reasons noted below, it is unlikely that it can be clarified in a way that may be of some linguistic significance.

Compounding the confusion is the fact that Dixon accepts the frequently expressed but quite incredible view that the relation of synonymy (or degree of synonymy) holds between two expressions if (or to the extent that) they have the same probability of occurrence in particular contexts (Dixon, 1963, p. 43-4). Taken literally, this implies that in the context 'my God, the baby has just fallen down the—', the two expressions 'stairs' and 'series of steps for passing from one level to another' must have the same probability of occurrence (courtesy of Webster's dictionary). Perhaps something else, is intended, but it is not easy to invent a coherent interpretation of this claim.

[8] This truism does not seem to be widely recognized. But obviously, if distinctions of grammaticalness are not assumed, there is nothing for a grammar to describe except 'regularities in a corpus'. Since no one has ever shown that anything at all can be said about 'regularities in a corpus' (on the syntactic level, at least), we may discard the latter possibility, noting also that anyone who has looked at a record of actual speech will be disinclined to pursue this purely 'naturalistic' study. In any event, all known grammatical descriptions are based on an assumed delimitation of grammatical and ungrammatical sentences, whatever terminology they may employ. For example, although A. A. Hill regards the distinction as a special and controversial feature of generative grammar [cf. Hill, "Grammaticality", *Word* 17.1-10 (1961)], he relies on it at every turn in his own descriptive work, for example, when he asserts that the expression *all the ten pretty young American children's twenty little old china dolls* "reaches the theoretical limit of complexity" [Hill, *Introduction to linguistic structures: From sound to sentence in English* 186 (New York, 1958)]. Similarly, in all descriptive syntax such a distinction is presupposed. It is true that the distinction becomes more crucial as one approaches explicitness in the formulation of grammatical rules, that is, as one approaches the construction of a generative grammar. This is only to say that empirical evidence is more relevant to the truth or falsity of hypotheses that have consequences than to the 'correctness' of sets of examples, which have no consequences.

In the same connection, it is important to be clear about the relevance of

importance. Let us now turn to what seems an equally radical objection to the position outlined above. Dixon appears to object to the assumption that a language is infinite (1963, 82f.). He asserts that this assumption is a fundamental error of a generative theory. On the contrary, from his point of view, "that of language *as it is*, every language is synchronically finite". Again, a superficial reading might lead one to think that Dixon is making an utterly fantastic proposal, namely, that a grammar should contain no recursions in its system of rules, and that the competence of the speaker must be represented as some vast list of sentences or category sequences, or something of this sort.[9] However, before considering this and other possible interpretations, we must, once again, read a bit further. Doing so, we find that Dixon is merely suggesting some new meaning for the word 'infinite'. This he asserts that "in the case of

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operational or behavioral tests to the grammatical-nongrammatical distinction (or scale, or set of scales). Certain experimental procedures have been proposed [cf., e.g. G. A. Miller and S. Isard, "Some perceptual consequences of linguistic rules", *Journal of Verbal Learning and Verbal Behavior* 2.217-28 (1963)] that appear to define a useful notion related quite closely to this distinction. On the other hand, there are obviously innumerable experimental procedures that will fail totally to characterize this distinction (for example, Hill has invented various tests of this sort, and has found – cf. Hill, "Grammaticality" – that they delimit no interesting sense of 'grammaticalness'). When an operational test is proposed, we may test it for significance by applying it. If it has no significance, it can be discarded. If it has significance, as indicated by the fact that it corresponds in some way to the notion it is intended to characterize, we may be able to rely on it to provide some evidence about unclear cases. But the intuitively given distinction is not called into question by the fact that some investigator is unable to develop a reasonable test, just as it would not be called into question by his inability to develop a theory – in this case, a generative grammar – that characterized the intended notion. Tests, as theories, are of interest in this connection only if they shed some light on tacit competence. There is, incidentally, no reason to take for granted that any simple, necessary and sufficient operational criterion can be invented for a theoretical notion like 'grammaticalness', although one would expect that some aspects of this notion can be clarified indirectly by operational tests (e.g., of the sort studied by Miller and Isard).

[9] It is a useful exercise to calculate the vastness of the lists that would be required. See G. A. Miller, E. Galanter, and K. H. Pribram, *Plans and the structure of behavior* (New York, 1960) and Miller and Chomsky ("Finitary models of language users") for some highly conservative estimates which, however, suffice to show the utter absurdity of pursuing any such approach to syntax.

sentences which each consist of a conjunction of clauses we are clearly unable to say that there is any definite number, N, such that no sentence contains more than N clauses". This quoted remark simply states that the language is infinite, in the only known technical sense of the word 'infinite'. Since Dixon goes on to deny that the language is infinite, we conclude that be must be using the term 'infinite' in some new and private sense.

What this sense may be is suggested by his discussion of the new notion 'synchronically finite', introduced in the quotation given above. He asserts that "to decide upon the size of a language at a particular time it is necessary not just to count the number of sentences allowable, but to sum the probabilities assigned to each allowable sentence". He goes on to point out that when we do this, we will find that this sum is a finite number, so that the language is 'synchronically finite'. Apparently, then, Dixon is suggesting the new term 'synchronically finite' which applies, trivially, to any set over which a probability measure is defined (since by definition, the sum of the probabilities is finite, in fact, unity) – in particular, which holds of any language if probability is somehow defined for the sentences of the language. Since he regards this as refuting the fallacious assumption that a language is infinite, he must be taking the term 'infinite' to mean 'not synchronically finite'. That is, a language is infinite just in case the sum of the probabilities assigned (somehow) to its sentences is infinite; by definition, then, a language is never infinite. What is unclear is simply why these vacuous concepts should be defined and why one should trouble to compute the 'size of a language' in the way he suggests, since we know in advance, by definition, that it will always be unity. Of course, this remark is based on the assumption that Dixon is using the term 'probability' in its normal sense. Some obscure remarks on p. 83 suggest that this assumption may be fallacious.

I see little point in carrying this discussion any further, except to make one final remark. Dixon speaks freely throughout about the 'probability of a sentence' as though this were an empirically meaningful notion. The few hints that he gives about the linguistic theory that he has in mind are based critically on the assumption

that some sense can be given to this notion. But this is not at all clear. We might take 'probability' to be an estimate of relative frequency, and some of Dixon's remarks suggest that he thinks of it in this way. This has the advantages of clarity and objectivity, and the compensating disadvantage that almost no 'normal' sentence can be shown empirically to have a probability distinct from zero. That is, as the size of a real corpus (e.g. the set of sentences in the New York Public Library, or the Congressional Record, or a person's total experience, etc.) grows, the relative frequency of any given sentence diminishes, presumably without limit. Furthermore, since most of the 'normal sentences' of daily life are uttered for the first time in the experience of the speaker-hearer (or in the history of the language, the distinction hardly being important at this level of frequency of occurrence), they will have had probability zero before this utterance was produced and the fact that they are produced and understood will be incomprehensible in terms of a 'probabilistic theory of grammar' – the reader who is suspicious of this remark may convince himself by searching for repetitions of sentences or for the occurrence of an arbitrarily selected 'normal sentence' in a real corpus. Dixon completely overlooks these obvious facts. Thus he asserts that the distinction between 'colorless green ideas sleep furiously' and some 'normal sentence' (say, 'revolutionary new ideas appear infrequently') is a matter of 'formal lexical meaning', i.e. frequency of occurrence: "That is to say: the collocation of 'colorless', 'green', 'ideas', 'sleep', and 'furiously' does not occur (or rather, has only a very small probability of occurring)" (Dixon, 1963, 75). That this sequence has a small probability of occurring is quite true (or was true, until a few years ago; actually, by now this sequence is one of the more frequent ones – i.e. it has occurred a handful of times – in the linguistic experience of some people, at least, without this sharp rise in frequency having affecting its status as a semi-grammatical sentence in the least, obviously). Precisely the same is true of 'revolutionary new ideas appear infrequently' and a host of others with an entirely different linguistic status. None of these sentences has a probability of occurrence detectably different from zero. The frequency of the

'collocations', in each case, is so ridiculously low that the attempt to account for fundamental linguistic distinctions in these terms is a complete absurdity. Similarly, there is no hope of distinguishing (1), (2), and (3) in terms of probability of occurrence, as Dixon seems to believe (1963, 76-7).

Putting aside Dixon's specific interpretation of 'probability of a sentence' as (apparently) literal frequency of occurrence of the sentence in an actual corpus, it is important to note that other probabilistic bases for grammatical or lexical properties seem equally out of the question. The problems are not overcome if we take probability to be something other than an estimate of relative frequency. The vastness of the set of sentences from which normal discourse draws will yield precisely the same conclusions; the probability of 'normal sentences' will not be significantly different from zero. Nor does it help to consider, rather, probability of sentence forms (if a sentence form is a sequence of categories, the categories containing elements that are 'mutually substitutable everywhere' in some fairly narrow sense – notice that if this is not a narrow sense, then the notion defined will not meet the demands placed on it). The numbers are still too vast for the notion to be taken seriously. Nor can we take the probability of a sentence to be an estimate based on the probability of its parts, in some way; such a decision would, as has repeatedly been observed, give a probability scale that has no useful relation to the intuitive scale of grammaticalness on which all linguistic description is based. Nor does it make any sense at all to talk about 'probability relative to a situation', since no one has ever given the slightest hint as to how 'situations' can be individuated or identified in any terms that have any use for this discussion. In fact, no sense has ever been given to the notion of 'probabilities or continuum type scales' (the fundamental importance of which Dixon repeatedly stresses) in the domain of grammatical description; and every concrete proposal that has been made has been shown to lead to absurdity. Perhaps the time has come for linguists who insist on the importance of such notions to face this simple fact.

In outlining the general framework for generative grammar at the

outset of these lectures, I distinguished between, on the one hand, those aspects of this framework that serve merely to define goals and research problems (pointing out that these are largely derived from the traditional study of language and mind), and, on the other hand, substantive assertions that go beyond delimitation of problems. Two substantive assertions were made in this account, namely:

(13) (I) the surface structure of a sentence is a proper bracketing of the linear, temporally given sequence of elements, with the paired brackets labeled by category names (that is, a labeled tree diagram, with such categories as Sentence, Noun Phrase, Verb Phrase, Noun and a small number of others serving as labels):

(II) the deep structure of a sentence is in general not identical to its surface structure, but is a much more abstract representation of grammatical relations and syntactic organization.

The criticisms of generative grammar that I have so far discussed do not bear on these substantive proposals; rather, I have suggested that they amount to no more than a proposal to limit 'linguistics' so as to exclude the mass of 'antediluvian' traditional questions,[10] for example, questions of competence, semantic interpretation, 'creativity', the nature of grammatical rules, etc. But no reasons have been offered for abandoning these topics, and no alternatives

[10] It is interesting to note that Dixon, like Uhlenbeck (see above, p. 15n.), regards traditional linguistics as essentially without value. But, like Uhlenbeck, he gives no reasons for this judgment, referring only to the fact that traditional grammars have been "long condemned by professional linguists" (Dixon, 1963, p. 78). Though true, this remark is hardly sufficient to prove the point. The fact is that traditional linguistics is little known today, and where there is discussion of it, this is often quite distorted and inaccurate (cf. the reference in Chomsky, *Current issues in linguistic theory*, 67, to some of Bloomfield's comments on traditional linguistic theory). Furthermore, much of the critique of traditional grammatical descriptions is little more than a reflection of the methodological limitations within which the critic has chosen to work. For some examples, see Chomsky (*Current issues in linguistic theory* 29-30, 108).

have been suggested that might lead to more fruitful study. Consequently, I think that these criticisms have no force.

It still, however, remains to consider objections to the substantive assertions (13I) and (13II). As mentioned above, (13I) has never been questioned and, in any event, is well-supported (see below). Let us assume it, then, for the purposes of present discussion, and turn briefly to (13II). This is an extremely important claim, and it is worthwhile to consider it with a bit more care.[11]

Given the assumption (13I) concerning surface structure, let us define 'taxonomic syntactic theory' as the view that such a representation exhausts the syntactic structure of an utterance, i.e. serves as the deep structure as well. There are, then, many varieties of taxonomic syntax: in particular, the few remarks that de Saussure offers concerning syntax indicate that he accepts this position; it was elaborated in various ways by American descriptivists in recent decades; it apparently subsumes the views of Reichling and Uhlenbeck (see above); it covers many models of language structure that have developed in the study of artificial languages and in computational linguistics; it includes the models of language-structure that have provided most of the substance of the mathematical study of language structure; etc. Taxonomic syntactic theory is what is rejected by assertion (13II). The theory of transformational generative grammar was developed as a specific alternative to taxonomic syntax, an alternative that incorporates assertion (13II). We return to it below, limiting our attention now to assertion (13II) itself, that is, to the question whether surface structure, as defined in (13I), expresses all grammatical structure that is relevant to semantic interpretation.

In many publications of recent years[12] the validation of assertion

[11] The reason why the claim is important is that it seems totally incompatible with the empiricist assumptions that have dominated the study of language and the study of cognitive processes in modern times. For discussion, see Chomsky, *Aspects of the theory of syntax*, ch. 1 § 8, and references given there; also, Katz, *Innate ideas* (forthcoming).

[12] E.g. Chomsky, "Three models for the description of language", *I.R.E. Transactions on Information Theory* IT-2/3.113-24 (1956); *Syntactic structures*; *Current issues in linguistic theory*; Katz and Postal, *An integrated theory of linguistic description*; Lees, Review of Chomsky, *Syntactic structures*; Postal,

(13II) has been attempted in the following way. First, an attempt was made to formulate a theory of generative grammar broad enough to comprehend all varieties of taxonomic linguistics, insofar as they are clear and express substantive hypotheses concerning the nature of language. This was one motivation for the development of the theory of phrase structure grammar (context-free or context-sensitive – I presuppose familiarity with this).[13] Second, an attempt was made to show that no theory formalizable within the framework of phrase structure grammar can succeed in giving the required notion of deep structure,[14] if these grammars are required to generate the actual sentences of the language. Consequently, taxonomic syntactic theory cannot be correct, the fundamental reason for this being that the deep structures that are required for semantic interpretation cannot be identified with the structures that are assigned to sentences by a phrase structure grammar that generates actual sentences directly.

Both steps of this argument seem to me to have been firmly established. In particular, every variety of syntactic theory that falls within the general range of taxonomic syntax seems to me formalizable (insofar as it is clear) within the framework of phrase structure grammar (in fact, with rare exceptions, its context-free variety),[15] whether these are formulated in terms of procedures of

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"On the limitations of context-free phrase structure description", *Quart. Progr. Rep. No.* 64 231-7 (Cambridge, Mass., 1961), *Constituent structure* (The Hague, 1964); "Limitations of phrase structure grammars", *Structure of language: readings in the philosophy of language*, eds. Katz and Fodor 131-51 (Englewood Cliffs, N.J., 1964).

[13] There is another and much more important empirical motivation for the theory of phrase structure grammar. Namely, it seems that although taxonomic syntax is inadequate (i.e. although assertion (13II) is true), nevertheless phrase structure grammar plays a fundamental role in a more adequate theory constructed in the light of (13II).

[14] Or, for that matter, the required notion of surface structure, cf., e.g. § 3 in Chomsky, "On the notion 'rule of grammar'", *Structure of language and its mathematical aspects. Proceedings of the 12th Symposium in Applied Mathematics*, ed. Roman Jakobson, 6-24 (Providence, 1961).

[15] An exception is the matter of discontinuous constituents, as has been noted in most studies of this topic since its inception. However, as has also been pointed out in these studies, this has little bearing on the argument concerning (13II), since the basic objections to taxonomic theory are not overcome by

analysis, as in many descriptivist studies, or in terms of conditions on any possible syntactic description of a sentence, as in the Reichling-Uhlenbeck view mentioned above and many others. Furthermore, the inadequacy of a categorially labeled bracketing of an actual sentence (a phrase structure tree) as a representation of deep structure seems to me to have been established beyond any doubt, by consideration of examples of the sort mentioned above, pp. 14-15, 26. See the cited references for further discussion. Consequently, it seems to me that assertion (13II) and the consequence that taxonomic syntax is fundamentally inadequate are among the best supported conclusions of linguistic theory, in its present state.

Both of the steps of the argument have been questioned, for example, in the Uhlenbeck paper discussed above. Thus Uhlenbeck seems to imply that his approach does not fall within the frameowrk of phrase structure grammar and, at the same time, he claims that linguistics should not study deep structure. I have tried to show that neither objection can be justified. I know of no similar objections that have any more substance than this.

There is one instance of what on the surface appears to be an objection to this two-step argument of a rather different sort, namely, a paper by G. H. Harman, "Generative grammars without transformation rules", *Lg.* 39.597-616 (1963). This is subtitled "A defense of phrase structure", and it appears to have been directed against step two of the argument, namely, against the claim that no theory formalizable within the theory of phrase structure

introducing devices of one sort or another to permit certain types of discontinuity.

It is important to be clear about just what is asserted here. What is asserted is that a phrase structure grammar that generates the actual linear, temporally given string of elements will not assign to this string the required deep structure (= surface structure, in this case). On the other hand, work in transformational grammar has always assumed that a good deal of what constitutes deep structure is provided by a phrase structure grammar with a much more abstract function, that is, one that generates only underlying structures, which are mapped into sentences by grammatical transformations. Recent work (discussed in the third section) suggests that in fact the full set of grammatical relations involved in deep structure can be characterized by a system which is a phrase structure grammar. This proposal must be clearly distinguished from the viewpoint of taxonomic syntax.

grammar can be empirically adequate. But a more careful study of this paper shows that it is entirely irrelevant to the whole issue.

Harman surveys several of the arguments against phrase structure grammar and accepts them as valid. However, he proposes that instead of regarding these as arguments against phrase structure grammar, one should restate them as arguments against using the term 'phrase structure grammar' in the way in which it has always been used. Since terminological points of this sort are clearly of no consequence in themselves, let us accept Harman's proposal (for the purpose of discussion of his paper), using the term 'restricted phrase structure grammar' in the sense of 'phrase structure grammar', as previously defined, and 'extended phrase structure grammar' for the theory he proposes (keeping in mind, however, that extended phrase structure grammar has no more connection with phrase structure grammar than antelopes have with ants). Having made this terminological change, we observe that the argument against taxonomic linguistics and in favor of assertion (13II) stands exactly as before, naturally, since no mere terminological change can affect the substance of this argument.

This observation is sufficient to allow us to dismiss this paper as a defense of taxonomic syntax, and as a critique of (13II). However, a more extended discussion of it will perhaps be useful, in that it offers a good opportunity to examine some issues that must be clearly understood if syntactic work is to proceed in any useful way.

Harman proposes that 'extended phrase structure grammar' is in the spirit of phrase structure grammar, and points out that there is no a priori necessity to use the term 'phrase structure grammar' as a designation for 'restricted phrase structure grammar'. The first point is unarguable. I know of no way of determining whether 'extended phrase structure grammar' is more in the spirit of phrase structure grammar than, for example, transformational grammar of the usual sort, or whether it is less so (arguments might be offered both ways); and, furthermore, I see no point in trying to establish this, one way or the other. The second point is correct. There is no a priori necessity to use the term 'phrase structure grammar' in the way in which it has always been used; any other term would, in

principle, have done as well. The significance of this observation and its role in the 'defense of phrase structure' are obscure. By similar logic, one can prove the baselessness of the charge that baboons cannot speak; for there is, after all, a creature rather like a baboon which can speak perfectly well, and there is no a priori necessity to use the term 'baboon' so as to exclude this creature.[16]

We observe once again, however, that the notion 'phrase structure grammar' in the familiar sense (i.e. 'restricted phrase structure grammar') is a well-motivated one, quite apart from terminology, having arisen not only in the formalization of taxonomic linguistics and in the theory of transformational grammar, but also, independently, in the study of the structure of artificial languages and in the study of mathematical models, where both context-free and context-sensitive phrase structure grammars occupy a central position.

Let us now consider 'extended phrase structure grammar' in some further detail. Such a system contains rules of the form $A \rightarrow X$ where A and X are not strings of category and terminal symbols, as in the theory of (restricted) phrase structure grammar, but strings of what we may call 'complex symbols' (A being a string of length one, and X possibly being null), where a complex symbol is a pair consisting of a category symbol and a set of indices. This is a modified form of a system proposed originally by G. H. Matthews seven or eight years ago, and elaborated in the Matthews-Yngve COMIT programming system. The rules are designed so that indices can be carried along in the course of a derivation. The indices can be used, in effect, to code many 'global operations' on strings (e.g. certain grammatical transformations) and to code context-restrictions of various sorts. The motivation for the development of the original system, it should be noted, was to simplify the coding of transformational processes.

Notice that an extended phrase structure grammar can be regarded as a (restricted) phrase structure grammar, namely, if we

[16] The analogy is not exact, however, An 'extended baboon' can indeed speak; there is little reason to suppose, however, that an 'extended phrase structure grammar' can describe syntactic structure.

take each complex symbol to represent a single category. Under this interpretation, what Harman has proposed is a new evaluation procedure for phrase structure grammar. Such a proposal is an empirical hypothesis about language. This hypothesis is immediately refuted, on grounds of descriptive inadequacy. Under this interpretation, if we consider the structures assigned to such sentences as 'John saw Bill', 'did John see Bill', 'look at Bill', etc., in the most highly valued grammar, we find that there is no category to which both occurrences of 'John' belong; there is no category to which all occurrences of 'Bill' belong; there is no category to which all five Nouns belong; etc. For such reasons as these neither the requirements on deep or surface structure begin to be met, and this interpretation must be rejected.

Therefore a prerequisite for the serious consideration of the theory of extended phrase structure grammar is that the condition (9iv) be met in some other way, that is, by some different proposal as to how a syntactic structure can be associated with a string generated by such a grammar, this structure in some way representing syntactically and semantically relevant information about the grammatical relations, sentence type, etc. But there is no suggestion in Harman's paper as to how this should be done,[17] so the matter remains quite open. What we have, then, is a theory of grammar which, though surely far richer than (restricted) phrase structure

[17] Strictly speaking, there are suggestions, but they are far too vague to fill the need. For example, on p. 608 a definition of 'transformational relation between sentences' is proposed which is so loose that it makes no distinction between the pair 'John saw Bill' – 'Bill was seen by John' and 'John saw Bill' – 'John was seen by Bill'. The crucial notions (e.g. grammatical relation) are not discussed at all. Thus there is no answer even suggested to the question of how to define the single grammatical relation that connects the italicized items in 'John *saw Bill*', 'John *reads books*', '*Bill* was *seen* by John', '*Bill* is easy for John to *find*', 'John ordered *Bill* to be *examined*', '*the proposal* is expected to be *brought* to the floor', 'the *book reads* easily', etc.; there is no indication as to how one can express the fact that *Bill* and *buy* are interconnected in the same way in 'they like to buy presents for Bill', 'Bill is easy to buy presents for', 'it is Bill that I like to buy presents for', 'presents were bought for Bill' etc.; there is no way suggested to express the grammatical relations that determine the semantic interpretation of the examples cited in section I, etc. In short, none of the questions that have motivated research in transformational syntax are faced.

grammar, is, for the moment, not well-defined in the only linguistically relevant sense. Cf. p. 19.

What possible reason, then, can there be for considering the theory of 'extended phrase structure grammar'? One suggestion that comes to mind is that it may, in some sense, be a less powerful theory than the theory of transformational grammar, even though it is clearly much richer in expressive power than the theory of (restricted) phrase structure grammar. If true, this would be an interesting curiosity; it would become an interesting observation for linguistics if the theory could be shown to approach descriptive adequacy in nontrivial respects. But it is not true. The theory of extended phrase structure grammar is incomparable in expressive power to the theory of grammatical transformations; in each, certain hypothetical grammatical processes can be formulated that are not formulable in the other.[18] Consider, for example, a hypothetical language with a grammatical process P that forms one class of sentences from another by, let us say, deleting a word belonging to the lexical category A if this occurrence of A is *immediately dominated* by the phrase category B. Within the framework of extended phrase structure grammar, this process is easily formulable (far more easily, for example, than the system of rules for generating passives of simple NVN sentences). It is only necessary to code P as an index of the initial symbol S, then formulating each rule $B \rightarrow \ldots A \ldots$ so that it adds a new index Q to the introduced occurrence of A (Q appearing nowhere else). We then give the rule deleting the category A when it contains the two indices P, Q. Such a process is, however, not formulable as a grammatical transformation; it would require a radical extension of transformational

[18] Obviously, we are interested in developing a linguistic theory the expressive power of which matches precisely the range of formal devices that actually function in natural language. If a theory is too weak in expressive power to accommodate such devices, or so strong in expressive power as to accommodate devices not to be found in natural language, this counts as a defect of the theory. As we shall see in the next section, earlier versions of the theory of transformational grammar exhibit both kinds of defect; and it would hardly be surprising to discover that the revised version that will be suggested there also suffers from defects of both kinds. This question is, of course, closely related to the question of explanatory adequacy touched on briefly above.

theory to permit use of quantifiers in structural descriptions of transformations far beyond anything permitted in this theory (apparently, the theory can in fact require that these structural descriptions be quantifier free); and, furthermore, it would violate the general conditions on deletion. Within the theory of transformattional grammar, as this has been explicitly formulated, there is no way to identify an occurrence of a category that is immediately dominated by a given category.

Similarly, consider a rule that deletes (or iterates, or adds a morpheme to) a word of the category A just in case this occurrence of A belongs simultaneously to two distinct phrases, one of which is the left-most category in some expansion and the other the right-most category. The indexing conventions of extended phrase structure grammar permit straightforward formulation of this process,[19] but it is unformulable as a grammatical transformation.

On the other hand, although certain grammatical transformations can be coded into the format of extended phrase structure grammar, others cannot. Consider, for example, the transformations that form (14) from the pair of abstract structures underlying 'John read the book' and 'Bill read the book', or that form (15i, ii) from an abstract structure that includes the phrase-marker underlying 'Bill Auxiliary leave':

(14) John read the book and so did Bill.

(15) (i) John persuaded Bill to leave.
(ii) Bill was persuaded to leave by John.

[19] To illustrate with the simplest example, consider a grammar with a single category S and a single word X, and with the rules S → SSS, S → X. In this language, then, the only grammatical information is that certain strings of X's belong to the category S. Suppose that we add to this grammar a grammatical process P that converts a certain occurrence of X to Y if X is dominated by an occurrence of S which is left-most in some expansion and also by an occurrence of S which is right-most in some expansion. Though unformulable as a grammatical transformation, this process is easily expressible in extended phrase structure grammar, for example, by allowing P to be an optional index of the initial symbol and indexing by L and R, respectively, the left-most and right-most occurrences of S in the recursive rule S → SSS. To express P, then, we simply add a rule converting X to Y when it has the indices L, R and P.

Obviously, the information provided by the deep structures underlying these sentences, as this is formulated by the familiar transformational analysis of them [e.g. Chomsky, *Syntactic Structures*; "A transformational approach to syntax", *Third* (1958) *Texas conference on problems of linguistic analysis in English*, ed. A. A. Hill 124-58 (Austin, 1962)] is essential to their semantic interpretation. Thus (14) implies that Bill read the book and it is evident that the semantic interpretation of (15) requires that *Bill* be specified as the Subject of *leave*. But the necessary information concerning (14) cannot be presented in terms of any structures assigned by an extended phrase structure grammar, and there is no indication of how the information required for (15) can be represented (it is not, as we shall note below, in Harman's derivation of these sentences, in contradiction to his claim (p. 610) that his coding of certain transformational grammars preserves grammatical information). But in any event, the processes which are directly formulable as grammatical transformations either cannot be stated at all, or cannot be formulated without very involved and elaborate mechanisms, within the framework of extended phrase structure grammar.

In short, the theory of extended phrase structure grammar is a rich system, incomparable with transformational grammar in expressive power, and (insofar as it is well-defined) not empirically adequate for natural language.

Harman's defense of phrase structure grammar is based on the claim that he has constructed a phrase structure grammar that generates exactly the set of sentences of a certain transformational grammar (namely, that presented in Chomsky, "A transformational approach to syntax"). The first part of this claim, as we have noted above, is based on nothing more than terminological equivocation. The second part is false. In fact, one of the central topics of the transformational grammar he was recoding is the system of Verb-Complement constructions illustrated in (15). In the transformational grammar, sentences such as (15) are based in part on an underlying structure 'Bill Auxiliary leave'. In particular, the constraints on Subject and Verb Phrase in sentences are carried over to the complement construction (15). Such strings as (16) are

excluded from direct generation and thus marked as deviant, as a direct consequence of the exclusion of (17):

(16) John { persuaded / ordered / expected } Bill to { accumulate / elapse / be numerous / be abundant / be parsed }

(17) Bill { accumulated / elapsed / is numerous / is abundant / is parsed }

(exclusion of the last being a consequence of the deviance of 'X parsed Bill'). But in Harman's recoding, no relation is expressed between the positions occupied by 'Bill' and 'leave' in such sentences as (15) – that is, the fact that in both (15i) and (15ii) 'Bill' functions as the subject of 'leave' [as well as being the object of 'persuade (to leave)', in this case] is not indicated. This failure of descriptive adequacy is reflected in the failure to express the deviant character of (16) by excluding these from direct generation, although the analogous property of (17) is correctly expressed by exclusion of these from direct generation by the grammar.

Whether the defect illustrated by these examples can be overcome by more elaborate mechanisms I have no idea, but the point is hardly worth pursuing. Suppose that it were indeed possible to recode a transformational grammar within the framework of extended phrase structure grammar in such a way as to meet the requirement of *weak generative equivalence* (that is, in such a way that the coded version generated the same set of strings, though not the same set of structures, as the original). Suppose, in fact, that it were possible to construct even a restricted phrase structure grammar that generates exactly the set of sentences of English or, in fact, any natural language. Exactly what linguistic significance would this demonstration have? The answer is: none at all. It would not in the least support the view that the theory of phrase structure

grammar (restricted or extended) is a correct theory of syntactic structure (just as the ability of a transformational grammar to *weakly generate* – that is, to generate the sentences of – a natural language would not, if a fact, demonstrate the adequacy of the transformational theory). Such a demonstration would simply support the conjecture in the earliest studies of transformational generative grammar (e.g. Chomsky, *Syntactic structures*, 34) that English and other languages may very well be within the weak generative capacity of phrase structure grammar. In fact, there is little reason to doubt that all natural languages can be weakly generated by context-sensitive phrase structure grammars, although it is now known (cf. Postal, "On the limitations of context-free phrase structure description", "Limitations of phrase structure grammar") that context-free grammars fail to meet even the condition of weak generative adequacy, and thus fail the test of descriptive adequacy in a particularly surprising way.[30] The point is that consideration of weak generative adequacy is only of interest if it provides *negative* evidence, with respect to a particular linguistic theory – if it shows, in other words, that the theory is so mismatched to language that not even the set of sentences can be correctly generated. But, as has been clear since the earliest studies of generative grammar, considerations of descriptive and explanatory adequacy are the only ones that can be used in *support* of a proposed grammar or theory of language. The fact that a grammar weakly generates a language is hardly of any interest. What is important is that it should do so in such a way as to assign to each sentence the correct deep and surface structure, and beyond that,

[30] In particular, Postal's conclusions apply to extended phrase structure grammar, since despite its richness of expressive power, it has the weak generative capacity of context-free grammar. To achieve weak generative adequacy, then, the theory must be extended in one way or another, perhaps by permitting rules to be restricted with respect to context. The important point, however, is that such an extension of weak generative capacity would not overcome the defects of descriptive inadequacy, just as the extension of context-free phrase structure grammar to context-sensitive phrase structure grammar, though it may very well overcome the defect of weak generative inadequacy, nevertheless does not eliminate the linguistically most significant defects of phrase structure grammar.

that it succeed in this task of *strong generation* in an internally motivated way.[21]

Returning to a concrete example, the fact that the extended phrase structure grammar that Harman presents fails in weak generative capacity by allowing (16) [while excluding (17)] is not in itself important. What is important is the underlying reason for this, namely, the failure of this grammar to express the grammatical relations that bind *Bill* in (15) to the Complement of the Verb, on the one hand (as its grammatical Subject), and to the full Verb phrase, on the other (as its Object). This is the important fact about such sentences as (15); considerations of weak generative capacity, in contrast, are of marginal linguistic interest unless they yield negative conclusions. Of course, questions of descriptive adequacy cannot even be raised in connection with a theory until at least the conditions (9) are met. Cf. p. 19.

Returning to the main theme, it seems to me that both assertions of (13) have been established beyond reasonable doubt, and that there are, furthermore, no known alternatives to transformational grammar that begin to meet conditions of descriptive or explanatory adequacy.[22]

[21] Cf. Chomsky, "Three models for the description of language", *Syntactic structures*, *Current issues in linguistic theory*, *Aspects of the theory of syntax*, and many other references, for discussion.

[22] I might mention at this point that the recent literature contains many references to purportedly equally effective or even more adequate theories. For example, Longacre [Review of Z. S. Harris, *String analysis of sentence structure* (The Hague, 1962), in *Lg.* 39.478 (1963)] asserts that the approaches of Harris, M. A. K. Halliday and K. L. Pike escape the limitations of phrase structure grammar and go well beyond immediate constituent analysis (presumably, he refers here to IC analysis of the type studied by Harris, B. Bloch, R. Wells and other in the '40's). This point, he claims, "is persistently missed by critics of 'phrase structure' grammars, who indiscriminately lump together immediate-constituent analysis and the more recent (and more satisfactory) approaches". Unfortunately, Longacre makes no attempt to provide any evidence in support of his claim; he does not, for example, attempt to show just how critics of phrase structure grammars have misinterpreted the theories he is advocating, in formulating them within the framework of phrase structure grammar, As to the advantages of these more recent theories over 'old-fashioned' IC analysis, Postal, in his comprehensive review of the subject (Postal, *Constituent Structure*) demonstrates quite the opposite. That is, he shows that the theories of Pike and

To summarize, a linguistic theory has a substantive character only insofar as it meets conditions (9i-iv) of p. 18. To the extent that these conditions are met, we can raise the question of descriptive adequacy. If, furthermore, an evaluation procedure of the sort discussed on p. 20f, is provided, we can raise the question of explanatory adequacy as well. The published literature gives no reason to suppose that descriptive or explanatory adequacy can be achieved outside of the framework of transformational generative grammar, or that this approach to the development of a substantive linguistic theory is misconceived in any serious way. This approach is quite traditional in its general character. It extends and elaborates ideas that have long been part of general linguistic theory, and that have been widely applied in practice. Thus it seems, fair to conclude that the successes it has so far achieved, and its resistance to serious criticism, provide some further vindication for the rich tradition of syntactic theory that has been largely displaced by modern structuralism.

Halliday, at least (along with many others), are if anything more defective than the IC theories worked out in the '40's. Of course, he was referring to material that is on the printed page. It is unfortunate that Longacre gives neither evidence nor documentation to show that something crucial was omitted in that account. Much more far-reaching claims are made by H. A. Gleason ["The organization of language: a stratificational view", *Report of the Fifteenth Annual (First International) Round Table Meeting on Linguistics and Language Studies*, ed. C.I.J.M. Stuart, *Monograph Series on Languages and Linguistics* 17 (1964)], who mentions the existence of a linguistic theory (called 'stratificational grammar', but apparently critically different from the only published version of this theory, namely, that which Postal, *Constituent Structure* showed to be a variant of context-free grammar insofar as it was well-defined) that not only escapes the limitations of phrase structure grammar, but is demonstrably superior in descriptive adequacy to transformational grammar as well. It is disappointing, then, to discover that Gleason presents no linguistic evidence at all in support of his many claims, and, even more so, that he makes no attempt to sketch even the general outlines of the approach to language for which he claims these many virtues, though he does list some of the terminology that he expects will find a place in it. Some of these terms are explained, namely, those that are merely new terms for familiar notions (e.g. 'phonon' for distinctive feature); the rest, so far as this paper indicates, are devoid of any content.

3

THE THEORY OF TRANSFORMATIONAL GENERATIVE GRAMMAR

Having now covered the first two parts of the outline given in the introductory section, I would like to turn, much more briefly, to parts three, four, and five. These are discussed in much more detail in Chomsky, *Aspects of the theory of syntax* and in the references cited there.

The earliest versions of transformational generative grammar made the following general assumptions concerning syntactic structure. The syntactic component of a grammar consists of two sorts of rules: rewriting rules and transformational rules. The rewriting rules constitute a phrase structure grammar (with, perhaps, a condition of linear ordering imposed). Each rule is, in other words, of the form $A \rightarrow X$ (with a possible restriction to the context Z—W), where A is a category symbol and X, Z, W are strings of category or terminal symbols. The strings generated by this system we may call *base strings* (an alternative term is *C-terminal strings*). In the course of generating a string, the system of rewriting rules (let us call this the *base component* of the syntax) assigns to it a phrase-marker which we can call a *base phrase-marker*, this being representable as a labeled bracketing or a tree diagram with categories labeling the nodes.

The transformational rules map phrase-markers into new, derived phrase-markers. Each transformational rule is defined by a *structural analysis* stating a condition on the class of phrase-markers to which it applies and specifying an analysis of the terminal string of this phrase-marker into successive parts. The specification of the transformation is completed by associating with this structural analysis a certain *elementary transformation* which is a formal operation on strings, of a certain narrow class. For details, see the

references cited above. By defining the 'product' of two phrase-markers as the new phrase-marker derived essentially by concatenation of the labeled bracketings,[1] we can apply what have been called *generalized* (or *double base, triplebase* etc.) transformations to a phrase-marker representing a sequence of phrase-markers, mapping such a product into a new phrase-marker by the same apparatus as is required in the singulary case. The transformations meet certain ordering conditions (I return to these below), which must be stated in a separate part of the grammar. These conditions include a specification of certain transformation as *obligatory* or obligatory relative to certain sequences of transformations. To generate a sentence, we select a sequence of (one or more) base phrase-markers and apply singulary and generalized transformations to them, observing the ordering and obligatoriness requirements, until the result is a single phrase-marker dominated by S (the *initial category*, representing 'sentence'). If we select a single base phrase-marker and apply only obligatory transformations, we call the resulting sentence a *kernel sentence* (a kernel sentence is not to be confused with the base string that underlies it as well as possibly many other more complex sentences).

We can represent the system of transformations that apply in the process of derivation as a transformation-marker (T-marker). To illustrate, consider the sentence

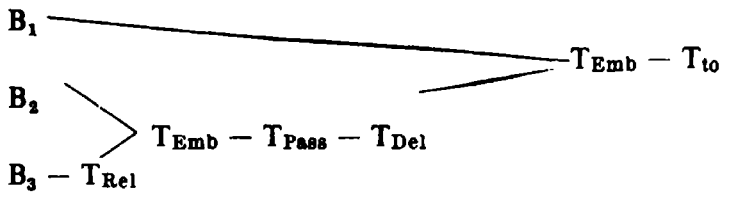
(18) I expected the man who quit work to be fired.

The transformational derivation of (18) might be represented by the T-Marker (19). In this representation, B_1, B_2 and B_3 are the three base phrasemarkers that underlie the (kernel) sentences (20i)-(20iii):[2]

[1] Precise definitions of the notions mentioned here are provided in Chomsky [*The logical structure of linguistic theory*. Unpublished manuscript, Microfilm M.I.T. Library (Cambridge, Mass., 1955)], and descriptions of varying degrees of informality appear throughout the literature. In particular, a phrase-marker is representable as a set of strings, and the 'product' of two phrase markers is then the complex product of the two sets (i.e. the set of all strings XY such that X is in the first set and Y in the second).

[2] Since I am presenting this merely as the basis for some revisions to be proposed below, I skip many details. In particular, I am completely overlooking the q on of how to describe the Auxiliary system, and I have also supposed, for

(19)



(20) (i) I expected it
(ii) someone fired the man
(iii) the man quit work

The interpretation of (19) is straightforward. It represents the fact that to form (18) we take the three base structures underlying (20i-iii), and proceed as follows. First, apply to B_3 the relative transformation T_{Rel} that converts it to 'wh (the man) quit work' (rather, to the abstract string that underlies this – cf. n. 2), with its derived phrase-marker. Call this new structure K_1. At this point, apply the generalized embedding transformation T_{Emb} to the pair of structures (B_2, K_1), deleting the occurrence of *the man* in the embedded sentence in the process, giving the string 'someone fired the man who quit work' with its derived phrase-marker K_2. To K_2, apply the passive transformation T_{Pass} to give 'the man who quit work was fired by someone', with the phrase-marker K_3. To this apply the deletion transformation T_{Del} to give 'the man who quit work was fired', with the derived phrase-marker K_4. Now

simplicity of exposition, that each of B_1-B_3 underlies a kernel sentence. Actually, this is not necessary, and in the transformational grammars presented in Chomsky (*The logical structure of linguistic theory; Syntactic structures*, "A transformational approach to syntax"), Lees, (*The grammar of English nominalizations*), and others, many of the base strings contain 'dummy symbols' [e.g., *Comp*, in the case of the analysis of such sentences as (15)] which are either deleted or filled in by sentence transforms in one way or another. Thus B_1 might have a dummy symbol as Object, B_3 might have an unspecified Subject, etc.

I am also assuming here a simpler analysis of the main (matrix) structure than was postulated in earlier work. The reasons for this go well beyond anything considered here. See P. Rosenbaum, (*A grammar of English Predicate Complement Constructions*. Unpublished Ph. D. dissertation, M.I.T. 1965) and, for further related discussion, Chomsky (*Aspects of the theory of syntax*, ch. 1, § 4).

Throughout the description of these structures, I cite sentences as examples, inaccurately, instead of the abstract strings that underlie them. It should be kept in mind that this is only an expository device.

apply to the pair of structures (B_1, K_4) the generalized embedding transformation T_{Emb}, giving 'I expected the man who quit work was fired' with the derived phrase-marker K_5. To K_5, apply the singulary transformation T_{to} giving the sentence (18) with its derived phrase-marker K_6.

I emphasize once again that only after all the transformations have been completed do we have an actual 'sentence' – that is, a string of elements that constitutes an 'output' of the syntactic component of the grammar and an 'input' to the phonological component.

Perhaps this example suffices to convey the content of the notion 'T-marker' (for further elaboration, see Chomsky, *The logical structure of linguistic theory*; Katz and Postal, *An integrated theory of linguistic description*). It should be clear, from this, how any transformational derivation can be represented as a T-marker which gives the full 'transformational history' of the derived sentence, including, in particular, a specification of the base phrase-markers from which it is derived. In Chomsky (*The logical structure of linguistic theory*) a general theory of linguistic levels is developed in an abstract and uniform way, with phrase structure and transformations each constituting a linguistic level. On each level, markers are constructed that represent a sentence. In particular, derived phrase-markers and T-markers fill this function on the phrase-structure and transformational levels, respectively. Each level is a system of representation in terms of certain primes (elementary atomic symbols of this level). On the level of phrase-structure, the primes are category and terminal symbols. On the level of transformations, the primes are base phrase markers and transformations. A marker is a string of primes or a set of such strings. Both phrase-markers and transformation-markers can be represented in this way. Levels are organized in a hierarchy, and we may think of the markers of each level as being mapped into the markers of the next lowest level and as representing the lowest level marker (that is, the phonetic representation which is the marker on the lowest, phonetic level – the primes of this level being sets of features), which is associated directly with an actual signal. We limit the discussion

here to the levels of phrase structure and transformational structure.

The general requirement on a syntactic theory is that it define the notions 'deep structure' and 'surface structure', representing the inputs to the semantic and phonological components of a grammar, respectively (see above), and state precisely how a syntactic description consisting of a deep and surface structure is generated by the syntactic rules. These requirements are met by the theory outlined above in the following way. The rewriting rules of the base component and the rules governing ordering and arrangement of transformations generate an infinite class of T-markers, in the manner just sketched. We take a T-marker to be the deep structure; we take the derived phrase-marker that is the final output of the operations represented in the T-marker to be the surface structure. Thus in the case of (18), the deep structure is the T-marker represented as (19), and the surface structure is what we designated as K_4. The phrase-marker K_4, then, must contain all information relevant to determination of the form of the signal corresponding to (18) (i.e., it is to be mapped into a phonetic representation of (18) by rules of the phonological component); the T-marker (19) is to contain all information relevant to the semantic interpretation of (18).

To complete the theory, we must add a description of the phonological and semantic components that interpret surface and deep structures, respectively. I will discuss the phonological component briefly in the fourth section, along lines suggested by R. Jakobson, G. Fant and M. Halle ([*Preliminaries to speech analysis* (Cambridge, Mass., 1952)]; Chomsky, Halle and Lukoff ("On accent and juncture in English"); Halle (*The sound pattern of Russian*, "Phonology in a generative grammar", "On the bases of phonology" (*Structure of language*, eds. Fodor and Katz 324-33); Chomsky ("Explanatory models in linguistics"); and other related publications. The theory of semantic interpretation is in a much less developed state, as noted above, although recent work of Katz, Fodor and Postal has been quite encouraging and, as we shall note directly, has had important consequences for the theory of syntax as well.

A theory of semantic interpretation based on the syntactic model

outlined above would have to provide first, a characterization of the notion 'semantic interpretation of a sentence', and second, a system of rules for assigning such an object to a deep structure, that is, a T-marker. Analogously a theory of phonetic interpretation must specify the notion 'phonetic interpretation of a sentence' – it must, in other words, specify a universal phonetic alphabet – and must provide a system of rules for assigning such an object to a surface structure, that is, the final derived phrase marker of a sentence. The notion 'semantic interpretation of a sentence' remains in a rather primitive state, for the moment. Several important steps have been taken towards the study of rules that assign semantic interpretations to deep structures, however.

First of all, it is evident that the grammatical relations among the elements of the string representing a sentence and the grammatical functions (i.e. Subject, Object, etc.) that these elements fulfill provide information that is fundamental for semantic interpretation Furthermore, it has been evident since the beginnings of recent work on transformational grammar that it is the grammatical relations and grammatical functions represented in the base phrase-markers underlying a sentence that are critical for its semantic interpretation (for example, it is not the 'grammatical subject' of the passive but rather its 'logical subject' that is the subject in the sense relevant to semantic interpretation). This is evident from consideration of the examples discussed throughout this paper. These examples were chosen primarily to illustrate this fact, as is characteristic of expository papers in transformational grammar. As emphasized above, it is examples of grammatical relations and functions that are obscured in the surface representation (the IC analysis) that provide the primary motivation for the rejection of all versions of taxonomic syntax, and for the development of the theory of transformational grammar.

To my knowledge, the first fairly explicit discussion of grammatical relations of the deep structure that are not represented in the actual physical form and organization of the sentence, and the first general discussion of the importance of these for semantic interpretation, is in the *Grammaire générale et raisonnée* of Port-Royal

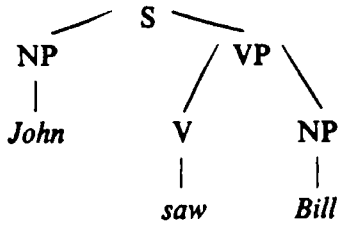
(1660). For some brief references, see Chomsky (*Current issues in linguistic theory*, § 1, and for some further discussion, Chomsky, *Cartesian linguistics*). In modern linguistics, the same insight was expressed by Harris, in somewhat different terms, in his early work on transformations,[3] and the point is also emphasized in Chomsky (*The logical structure of linguistic theory*; *Syntactic structures*), and in all subsequent work on transformational grammar.

To go beyond this observation, it is necessary to define grammatical relations and grammatical functions, and to show how the relations and functions of the base phrase-markers play a role in determining the semantic interpretation of the sentence that they underlie. A phrase structure grammar is, in fact, a very natural device for assigning a system of grammatical relations and functions to a generated string. These notions are represented directly in the phrase-marker assigned to a string generated by such rules, as has been frequently pointed out. Various ways of defining these notions are discussed in Chomsky (*The logical structure of linguistic theory*; *Current issues in linguistic theory*; *Aspects of the theory of syntax*) and Postal (*Constituent Structure*). For concreteness, consider a highly oversimplified phrase structure grammar with the rules (21):

(21)
$$\begin{aligned} S &\rightarrow NP\ VP \\ VP &\rightarrow V\ NP \\ NP &\rightarrow \textit{John, Bill} \\ V &\rightarrow \textit{saw} \end{aligned}$$

This grammar generates the string 'John saw Bill' with the phrase-marker (22):

(22)



[3] E.g. Harris, "Discourse analysis", *Lg.* 28.18-23 (1952); "Distributional structure", *Word* 10.146-62 (1954); "Co-occurrence and transformation in linguistic structure", *Lg.* 33.283-340 (1957).

To the grammatical rule A → XBY, we can associate the *grammatical function* [B, A]. Thus associated with the rules of (21) we have the grammatical functions [NP, S], [VP, S], [V, VP], [NP, VP]. We may give these the conventional names *Subject-of*, *Predicate-of*, *Main-Verb-of*, *Object-of*, respectively. Using the obvious definitions of these notions, we can say, then, that with respect to the phrase-marker (22), *John* is the Subject-of the sentence, *saw Bill* is the Predicate-of the sentence, *saw* is the Main-Verb-of the Verb Phrase, and *Bill* is the Object-of the Verb Phrase. We can go on to define grammatical relations (Subject-Verb, etc.) in terms of these and other notions and there are various ways in which one can attempt to formulate language-independent definitions for the central concepts (for details, see the cited references). The important point is that a phrase structure grammar need not be supplemented in any way for it to assign these properties to the strings it generates. Once we recognize the relational character of these notions, we see at once they are already assigned, in the appropriate way, with no further elaboration of the rules.

Notice that we might define the grammatical functions not in terms of the generating rules, but in terms of the Phrase-marker itself, in an obvious way. If we do this, we will have a more general notion of 'grammatical function' that will apply to derived phrase markers as well as to base phrase markers. I do not go into this here, since, in any event, it is only the functions in the base phrase-markers that are significant for semantic interpretation (but see Chomsky, *Aspects of the theory of syntax*, pp. 220, 221, for some discussion of the role of 'surface functions', so defined).

The first attempt to develop a theory of semantic interpretation as an integral part of an explicit (i.e. generative) grammar is in Katz and Fodor ("The structure of a semantic theory"). This is the first study that goes beyond the assertion that the base phrase-markers underlying a sentence are, in some sense, the basic content elements that determine its semantic interpretation. Basing themselves on the account of syntactic structure outlined above, Katz and Fodor argue that the semantic component of a grammar should be a purely interpretive system of rules that maps a deep structure (a

T-marker) into a semantic interpretation, utilizing in the process three sorts of information: (i) intrinsic semantic features of lexical items; (ii) the grammatical functions defined by the base rules; (iii) the structure of the T-marker. The semantic component should have two sorts of 'projection rules'. The first type assign semantic interpretations ('readings') to categories of the base phrase-markers in terms of the readings previously assigned to the elements dominated by (belonging to) these categories, beginning with the intrinsic readings of the lexical items and using the grammatical functions defined by the configurations of the base phrase-markers to determine how the higher level readings are assigned; and, ultimately, assigning a reading to the dominant category S. The projection rules of the second type utilize the readings assigned in this way to base phrase-markers, and, in terms of the elements and configurations represented in the T-marker, determine the semantic interpretation of the full sentence. Not much is said about type two rules; as we shall see below, this is not a serious gap in their theory.

With this brief survey, we conclude part three of the outline of the introductory section, having now sketched a certain theory of generative grammar that in part overcomes the fundamental inability of taxonomic syntax to provide an adequate notion of deep structure.

Turning now to part four of the outline, I would like to consider some of the defects that have been exposed in the theory just sketched as it has been applied to linguistic material.

In Lees (*The grammar of English nominalizations*), it is shown that the negation transformation of Chomsky (*Syntactic structures*, "A transformational approach to syntax")[4] is incorrectly formulated. He shows that there are syntactic arguments in favor of an alternative formulation in which the negation element is not introduced by a transformation but is, rather, an optional element introduced by rewriting rules of the base, the transformation serving simply to place it in the correct position in the sentence. At about the same time, E. S. Klima pointed out that the same is true of the question

Publication delays account for the discrepancy in dates, here, and in several other places.

transformations of Chomsky (*Syntactic structures*, "A transformational approach to syntax"). There are syntactic arguments in favor of assuming an abstract 'question marker' as an element introduced by base rules, the question transformations then being conditional on the presence of this marker (i.e. obligatory when it appears in a string, and inapplicable otherwise). Further arguments in support of this view, and further elabortaion of it, are presented in Katz and Postal (*An integrated theory of linguistic description*). See now also Klima, "Negation in English", *Structure of language: Readings in the philosophy of language*, eds. Fodor and Katz, 246-323).

In Katz and Postal, it is further observed that the same is true of the imperative transformation of earlier work. In the light of this and other observations, Katz and Postal then conclude that all singulary transformations which affect meaning are conditional upon the presence of markers of this sort; in other words, the singulary transformations in themselves need not be referred to by the rules of the semantic component since whatever contribution they appear to make to the meaning of the sentence can be regarded as an intrinsic property of the marker that determines their applicability, and can therefore be handled in base structures by type 1 projection rules. It follows, then, that the function of type 2 projection rules is much more restricted than Katz and Fodor were forced to assume, since they need not take into account the presence of singulary transformations in a T-marker.

Turning then to generalized transformations, Katz and Postal carry out a detailed analysis of many examples described in earlier studies that seem to demonstrate a contribution of generalized transformations to the semantic interpretation of the generated sentence in some way that goes beyond mere 'amalgamation'. They argue (quite convincingly, it seems to me) that in each such case, there are syntactic grounds for regarding the description as in error; furthermore, that in each such case the only function of the generalized transformation is to embed a sentence transform in a position that is already specified in the underlying structure (let us say, by the presence of a dummy symbol).

Generalizing upon these various observations, they conclude that the only function of generalized transformations, so far as semantic interpretation is concerned, is to interrelate the semantic interpretations of the phrase-markers on which they operate; in other words, to insert the reading for the embedded phrase-marker in the position already marked (by a dummy element) in the phrase-marker in which it is inserted. Thus the only aspect of the T-marker that need be considered in semantic interpretation is the interrelation specified by the nodes where generalized transformations appear in the representation. Beyond this, transformations appear to play no role in semantic interpretation. Thus the function of type II rules is still further restricted.

This principle obviously simplifies very considerably the theory of the semantic component as this was presented in Katz and Fodor ("The structure of a semantic theory"). It is therefore important to observe that there is no question-begging in the Katz-Postal argument. That is, the justification for the principle is not that it simplifies semantic theory, but rather that in each case in which it was apparently violated, syntactic arguments can be produced to show that the analysis was in error on internal, syntactic grounds. In the light of this observation, it is reasonable to formulate the principle tentatively as a general property of grammar.

Furthermore, it seems that there are good reasons for regarding even the passive transformation as conditional upon the presence of an abstract marker in the underlying string (see Chomsky, *Aspects of the theory of syntax*, for a survey of syntactic arguments in support of this), rather than as optional, as assumed in earlier work. Consequently, it seems that all singulary transformations other than those that are 'purely stylistic' (cf. Chomsky, *Aspects of the theory of syntax*, pp. 221, 223, for some discussion of this distinction – discussion, incidentally, which is far from satisfactory, although it seems to me that a real and important distinction is involved) are conditional upon markers in base strings, whether or not these transformations effect semantic interpretation.

Independently of these developments, C. J. Fillmore pointed out that there are many restrictions on the organization of T-markers

beyond those that were assumed in earlier attempts to formulate a theory of transformational grammar [Fillmore, "The position of embedding transformations in a grammar", *Word* 19.208-31 (1963)]. What his observations come to is essentially this: there is no ordering among generalized transformations, although singulary transformations are ordered (apparently linearly); there are no singulary transformations that must apply to a matrix sentence before a constituent sentence is embedded in it by a generalized embedding transformation,[5] although there are many instances of singulary transformations that must apply to a matrix sentence after embedding of a constituent structure within it and to a constituent sentence before it is embedded; embedding should be regarded as substitution of a sentence transform for a 'dummy symbol' rather than as insertion of this transform in a categorially unspecified position. The last observation is further elaborated by Katz and Postal (*An integrated theory of linguistic description*), as noted above.

Returning now to the T-marker (19) used as an example above, we observe that it has just the properties that Fillmore outlines. That is, singulary transformations are applied to a matrix sentence only after embedding and the only ordering is among singularies. But the earlier theory of T-markers left open the possibility for ordering of a much more complex sort. It is therefore quite natural to generalize from these empirical observations, and to propose as a general condition on T-markers that they must always meet Fillmore's conditions and have the form illustrated in (19).

As just formulated, this principle appears to be quite ad hoc, but

[5] The terms 'matrix sentence' and 'constituent sentence' are due to Lees (*The grammar of English nominalizations*); the matrix sentence is the one into which a constituent sentence is inserted by a generalized transformation. The same notion appears in the analysis of transformational processes in *the Grammaire générale et raisonnée*, where the terms 'proposition essentielle' and 'proposition incidente' are used for 'matrix sentence' and 'constituent sentence', respectively. Actually 'matrix proposition' and 'constituent proposition' would, in any event, be preferable terms, since what is involved here is not an operation on sentences but rather on the abstract structures that underlie them and determine their semantic interpretation. This is the way in which these operations are interpreted, correctly, in the *Grammaire générale et raisonnée*.

there is another way of saying exactly the same thing that makes it seem entirely natural. Notice that if no singulary transformations apply to a matrix phrase-marker before embedding, and if, furthermore, all embedding involves the insertion of a constituent phrase-marker in a position marked by a dummy element in the matrix structure, then we can, in fact, dispense with generalized transformations entirely. Instead of introducing constituent phrase-markers by embedding transformations, we can permit the rewriting rules of the base to introduce the initial category symbol S, i.e. we can permit rewriting rules of the form A → ...S...

Wherever such a symbol is introduced, we can allow it to head a new base derivation. In short, we can apply the linearly ordered system of base rewriting rules in a cyclic fashion, returning to the beginning of the sequence each time we come upon a new occurrence of S introduced by a rewriting rule. Proceeding in this way, we construct what we can call a *generalized phrase-marker*.

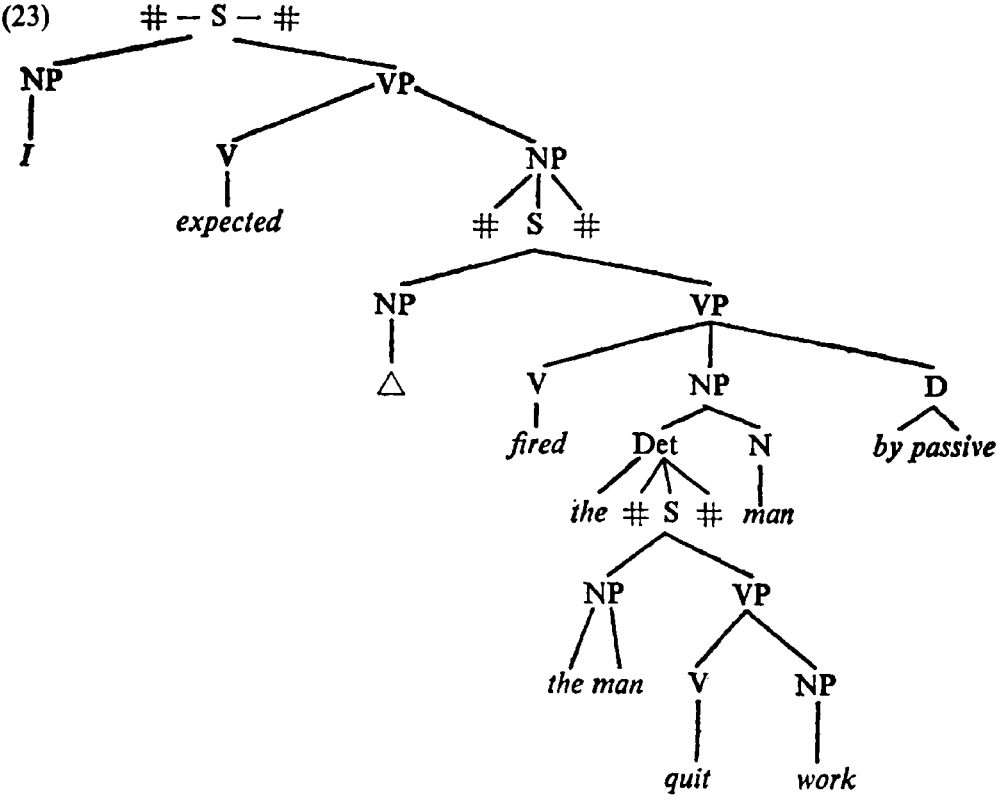
We now apply the linear sequence of singulary transformations in the following manner. First, apply the sequence to the most deeply embedded structure dominated by S in the generalized phrase-marker. Having completed the application of the rules to each such structure, reapply the sequence to the 'next-higher' structure dominated by S in the generalized phrase-marker. Continue in this way, until, finally, the sequence of transformations is applied to the structure dominated by the occurrence of S which initiated the first application of base rules, i.e., to the generalized phrase-marker as a whole. Notice that with this formulation, we have, in effect, established the particular formal properties of the T-marker (19) as general properties of any transformational derivation.

Let us now return to the example (18)-(20) in the light of these suggested revisions of the theory of transformational grammar. By the application of the rewriting rules of the base, we construct the generalized phrase-marker (23) (omitting all but the central configurations, and many details).

The transformations indicated in (19) now apply, obligatorily, in the following order. First, T_{Rel} applies to the most deeply embedded structure. We then turn to the next higher structure, i.e.

the one dominated by the occurrence of S in the fourth line of (23). At this point, an inversion rule (not indicated in (19), though in fact also needed in the earlier formulation) inverts the relative clause and the following N. Next we apply the passive transformation and the subsequent deletion of the unspecified subject, these operations now being obligatorily marked by the dummy elements *passive* and △ (standing for an unspecified category) in (23). Since no further transformational rules apply at this point, we turn to the next higher structure dominated by S – in this case, the full generalized phrase-marker. To this we apply T_{to}, as before, giving (18). The transformations indicated in the T-marker (19) are now obligatory and the structure of the T-marker (19) is fully

(23)



determined by (23) itself, given the general convention for cyclic application of transformations.

Notice now that all of the information relevant to the semantic interpretation of (18) is contained in the generalized phrase-marker (23) that underlies (18). Furthermore, the same will be true in all other cases, if the modifications suggested above are correct. By the principle suggested by Katz and Postal, the singulary transformations will not make an intrinsic contribution to meaning, and the generalized transformations will do so only insofar as they interrelate base phrase-markers. But we have now eliminated generalized transformations in favor of a recursive operation in the base. Consequently all information relevant to the operation of the interpretive semantic component should be contained in the generalized phrase-marker generated by base rules.

The advantages of this modification are obvious. It provides a more highly structured theory which is weaker in expressive power; in other words, it excludes in principle certain kinds of derivational pattern that were permitted by the earlier version of transformational theory, but never actually found. Since the primary goal of linguistic theory is to account for specific properties of particular languages in terms of hypotheses about language structure in general, any such strengthening of the constraints on a general theory is an important advance. Furthermore, there is good internal motivation for enriching the structure (and hence decreasing the expressive power) of transformational theory in this way, namely, in that this modification permits us to eliminate the notion of 'generalized transformation' (and with it, the notion 'T-marker') from the theory of syntax. Hence the theory is conceptually simpler. Finally, the theory of the semantic component can be simplified in that type two projection rules are no longer necessary at all.

Recapitulating, we are proposing that the syntactic component of a grammar consists of rewriting rules and transformational rules. The rewriting rules are permitted to introduce the initial symbol S. These rules apply in a linear sequence; if the initial symbol S appears in a derivation, then the sequence of rules reapplies to form

a subderivation dominated by this symbol, in the usual manner. The recursive property of the grammar (its 'creative aspect', to return to terminology used above) is restricted to the base component. In fact, the restriction may be still heavier than this, since recursion may be limited to introduction of the symbol S, that is, to introduction of 'propositional content'. This is not a necessary property of a phrase structure grammar.

The base rules, applying in the way just outlined, form generalized phrase-markers. The function of the transformational rules is to map generalized phrase-markers into derived phrase-markers. If the transformational rules map the generalized phrase-marker M_D into the final derived phrase-marker M_S of the sentence X, then M_D is the deep structure of X and M_S is its surface structure.

This approach to syntax formalizes, in one specific way, the view that the phonetic form of a sentence is determined by its actual labeled bracketing, whereas its semantic interpretation is determined by the intrinsic semantic properties of its lexical items and by a network of grammatical relations, not necessarily represented in the surface structure, that interconnect these items [cf. (13)]. The underlying grammatical relations are determined by the base rules. This abstract system of categories and relations is related to a labeled bracketing of the actual sentence by transformational rules and the interpretive rules of the phonological component. There is fairly good reason to suppose that the base rules are rather narrowly constrained both in terms of the symbols that may appear in them and in terms of the configurations of these symbols, but I will not go into this further question here (see Chomsky, *Aspects of the theory of syntax*, for some discussion). Insofar as information is presently available about syntactic structure, and about the relation of signals to semantic interpretations of these signals, this view seems compatible with it. It is worth mention that a view very much like this is expressed in the *Grammaire générale et raisonnée*, to which we have now had occasion to refer several times.

We might ask why a natural language should be constructed in this way; why, in particular, should it not identify deep and surface structures and thus dispense with the transformations that inter-

relate them. One would naturally attempt to find an answer to this question on perceptual grounds. For some speculations that seem to me worth pursuing further, see Miller and Chomsky ("Finitary models of language users", part II).

Observe that the base rules may form generalized phrase-markers that cannot be mapped by any sequence of transformations into a surface structure. For example, suppose that we had chosen the phrase 'the boy' instead of 'the man' in the most deeply embedded structure of (23). In this case, the generalized phrase-marker would evidently constitute the deep structure of no sentence; there is no sentence for which this structure provides the semantic interpretation. And in fact, the relative transformation would block when applying to this structure, because of the lack of identity between the Noun Phrases of the matrix and constituent sentences.[6] Hence not all generalized phrase-markers underlie sentences and thus count as deep structures. The deep structures are the generalized phrase-markers that are mapped into wellformed surface structures by transformational rules. Thus the transformations serve a 'filtering' function; in effect, they supply certain global constraints that a deep structure must meet, constraints that are, in fact, entirely unstatable within the framework of elementary rewriting rules that seem perfectly adequate for the generation of base structures with the grammatical functions and relations that they express. For further discussion of this property of transformations, see Chomsky (*Aspects of the theory of syntax*, Ch. 3).

In this way, we can construct a theory of grammatical transformations that is conceptually simpler than the earlier version, described above, but still apparently empirically adequate. In this modified formulation, the functions of the base rules and the transformational rules are more clearly expressed, as are also the notions of deep and surface structure. We have, correspondingly, a simplification of semantic theory.[7]

[6] What is involved here is a set of very general conventions on recoverability of deletion, in the transformational component of a grammar. For discussion, see Chomsky (*Current issues in linguistic theory; Aspects of the theory of syntax*); Katz and Postal (*An integrated theory of linguistic description*).

[7] Incidentally, only embedding transformations were considered here. It is

I began this section by presenting a certain theory of grammar in outline. I have now tried to show that this theory was too broad and rich in expressive power, and that a much more restricted version of it (which is, furthermore, conceptually well-motivated) will suffice to account for what empirical data is now available. I would now like to turn to an inadequacy in earlier theory of the opposite sort, that is, to a class of problems that show this theory to be too poor in expressive power, in a certain way.

Let us limit our attention now to the base component of the syntax. The theory outlined followed structuralist assumptions in supposing that the relation of lexical items to the categories to which they belong is fundamentally the same as the relation of phrases to the categories of which they are members. Formally speaking, it was assumed that a lexical item X is introduced by rewriting rules of the form $A \rightarrow X$, where A is a lexical category, exactly in the way that phrases are introduced.[8] However, difficulties in this view quickly emerged. Shortly after the publication of the earliest work in transformational generative grammar, it was pointed out by G. H. Matthews that whereas the categorization of phrases is typically hierarchic, and therefore within the bounds of phrase structure grammar, lexical categorization typically involves cross-classification, and therefore goes beyond these bounds. For example, a Noun may be either Proper or Common, and, independently of this, may be either Animate or Inanimate; a Verb may be Transitive or non-Transitive, and independently of this, may or may not take non-Animate Subjects; etc. This fact is unstatable within the framework of phrase structure grammar. Consequently, the theory of the base must be extended in some way so as to

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also necessary to show how various transformations that introduce coordinate structures (e.g. conjunction) can be developed within this framework. For some remarks on this question, see Chomsky (*Aspects of the theory of syntax*) and the references cited there.

[8] Notice that although this has been the view of all work in modern syntactic theory that has gone beyond mere elaboration of terminology, the incorrectness of this view became obvious only when it was formalized within the framework of an explicit theory of grammar. An essential reason for formalization and explicitness is, of course, that it immediately exposes inadequacies that may otherwise be far from evident.

provide an analysis of lexical categorization that is different in fundamental respects from the analysis in terms of rewriting rules that seems quite adequate above the level of lexical category. Similar observations were made independently by Stockwell, Anderson, Schachter and Bach, and various proposals have been made as to how to remedy this defect of the base component. The general problem is studied in some detail in Chomsky (*Aspects of the theory of syntax*, Ch. 2), where reference is also made to the earlier work just noted. I will sketch briefly the proposals offered there for modification of the theory of the base component.

Notice that the problem of lexical cross-classification is formally analogous to the problem of phonological classification. Thus phonological elements are also typically cross-classified with respect to the operation of various phonological rules. Certain rules apply to the category of Voiced segments; others to the category of Continuants; membership of a segment in one of these categories is independent of its membership in the other. This is, furthermore, the typical situation. This, in fact, is one major reason for the view that segments (e.g. phonemes or morphophonemes) have no independent linguistic status and are simply to be regarded as sets of features.

More generally, a lexical item can be represented phonologically as a certain set of features, indexed as to position. Thus the lexical item *bee* can be represented by the feature set [Consonantal$_1$, Voiced$_1$, non-Continuant$_1$, ..., Vocalic$_2$, non-Grave$_2$, ...] indicating that its first 'segment' is consonantal, voiced, a non-continuant, ..., and that its second 'segment' is vocalic, non-grave, ... Such a representation can be given in matrix form in an obvious and familiar way. It provides a perfectly satisfactory solution to the cross-classification problem on the phonological level (and furthermore relates very nicely to what seems to me to be for the present the most satisfactory theory of universal phonetics namely, Jakobson's theory of distinctive features – I will presuppose acquaintance with this, in the form recently given to it by Halle, for the remainder of this paper).

Observe also that the semantic analysis of lexical items also

apparently requires a kind of feature theory, and that these features typically cross-classify lexical entries. Thus Katz and Fodor ("The Structure of a semantic theory") and Katz and Postal (*An integrated theory of linguistic description*) are led to the conclusion, essentially, that a lexical entry in its semantic aspect should consist of a set of semantic features.

These observations suggest that the problem of syntactic cross-classification be dealt with in the same way, particularly, since it apparently involves only lexical items and not phrase types. Adopting this rather natural proposal, let us revise the theory of the base in the following way. The base consists of a system (presumably, a linear sequence) of rewriting rules which we may call its *categorial component*. Beyond this, it contains a *lexicon*. The lexicon is an unordered set of *lexical entries*. Each lexical entry is simply a set of speecified features. The features constituting the lexical entry may be phonological (e.g. [± Voiced$_n$], where n is an integer indicating position), semantic (e.g. [± Artifact]), or syntactic (e.g. [± Proper]). We limit our attention here to the syntactic features. The categorial component of the base generates no lexical items in strings (though it may introduce grammatical morphemes). As a first approximation, we may think of each lexical category A (e.g. Noun, Verb, etc.) as being involved only in rewriting rules of the form A $\rightarrow \triangle$, where $\triangle$ is a fixed dummy symbol. Thus the final strings generated by the categorial component (let us call these *pre-terminal strings*) are based on a 'vocabulary' (i.e. a set of primes – see above, p. 54) consisting of grammatical morphemes and the symbol $\triangle$. The latter will occupy the position in which items from the lexicon will be inserted, in a manner which we will describe directly. A pre-terminal string is converted to a *terminal string* by insertion of an appropriate lexical item in each position marked by $\triangle$.

Recall that the deep structures that determine semantic interpretation are generalized phrase-markers generated by the base component. As we noted above, it seems plausible to develop semantic theory in terms of projection rules that assign readings to successively higher nodes of the deep structure, basing this assign-

ment on the readings assigned to already interpreted lower nodes and the grammatical relations represented by the configuration in question. The grammatical relations and the order of application of the interpretive projection rules are determined completely by the categorial component of the base. The intrinsic semantic properties that provide the initial readings for this process of semantic interpretation (i.e. the readings of the lexical items that are the terminal elements of the generalized phrase-marker) are provided completely by the lexicon. Thus the two separate aspects of the semantic theory are mirrored in the subdivision of the base into a categorial and a lexical component.

The functioning of the categorial component is clear; let us, therefore, consider the lexicon in some further detail. The lexical entry for a certain item should contain all information about idiosyncratic features of this lexical item, features that cannot be predicted by general rule. Thus the fact that 'buy' begins with a Voiced non-Continuant, that it is a transitive Verb, that it has irregular inflections, that it involves transfer of ownership, etc., must all be represented by features of the lexical entry. Other properties (for example, that the initial non-Continuant is non-Aspirated) can be predicted by rule (in this case, a phonological rule). But there may be *redundancy rules* of various kinds that operate on phonological, semantic, and syntactic features, and that specify interrelations among features of the various types. Insofar as regularities concerning feature composition can be expressed by rule, the features in question can be extracted from the lexical entry (for discussion of redundancy rules, see Chomsky, *Aspects of the theory of syntax*, particularly Ch. 4, § 2.1). Normally, a lexical item will be idiosyncratic in many respects. Since these can now be specified in the lexical entry, they need no longer be represented in the rewriting rules. This leads to an enormous simplification of the base component, as will be evident to anyone who has ever attempted to construct a detailed grammatical description.

Let us now consider the rule that inserts lexical items in preterminal strings. Notice that this rule must take account of the structure of the phrase-marker in which the item is being inserted.

For example, when we say that a Verb is Transitive, we are asserting that it can appear in the position – NP in a Verb Phrase. Therefore the syntactic feature [+Transitive] must specify some aspect of the phrase-marker in which the item can be inserted. Let us call a feature of this sort a *contextual Feature.* In contrast, we will call such features of Nouns as [± Human] *non-contextual.* The degenerate case of a contextual feature is the feature [± Noun] itself, which indicates a minimal aspect of the phrase-marker, namely, the category dominating the occurrence of △ for which the item in question may be substituted. These degenerate contextual features, we may call *category features.* For the category features, the obvious notation is [± A], where A is a lexical category. By convention, then, we assert that an item with the category feature [+A] can only replace an occurrence of △ dominated by the category symbol A.

Consider now the problem of a proper notation for the other contextual features, e.g. transitivity. Clearly the best notation is simply an indication of the context in which the item can occur. Thus the feature [+Transitive] can be represented simply [+— NP]. Similarly, the fact that 'persuade' can be followed by a Noun Phrase and a following Prepositional Phrase (e.g. 'I persuaded John of the pointlessness of his actions') can be indicated by assigning the contextual feature [+— NP PP] to the lexical entry for 'persuade' (in fact, this is apparently the only contextual feature needed to specify the frame in which 'persuade' can appear, all other forms being derived by transformation – for discussion, see Chomsky, *Aspects of the theory of syntax*). Contextual features of this sort, which specify the frame in which an item can be substituted, we will call *strict subcategorization features.*

Alongside of strict subcategorization features, there are contextual features of a radically different sort that we will call *selectional features.* Whereas the strict subcategorization features specify categorial frames in which an item may appear, the selectional features of a lexical item X specify lexical features of the items with which X enters into grammatical relations. Thus the selectional features for 'frighten' will indicate that its Object must be specified

as [+Animate], the selectional features for 'elapse' will indicate that its Subject cannot be [+Human] (and for true descriptive adequacy, must obviously give a much narrower specification than this), etc. Similarly, the selectional features for 'abundant' must indicate that it can be predicated of 'harvest' but not 'boy', whereas the selectional features for 'clever' must contain the opposite specification. We can represent selectional features by a notation very much like that suggested above for strict subcategorization features.

Contextual features can be regarded as specifying certain substitution transformations. The context stated in the contextual feature specifies the condition that must be met by the phrase-marker to which the transformation in question applies and the manner in which this phrase-marker must be analyzed for the purposes of this transformation. Thus it defines the structural analysis of the transformation (see above, p. 51). The elementary transformation that completes the definition of the transformation states that the lexical item in question (i.e. the set of specified features that constitutes the lexical entry) substitutes for the occurrence of Δ that appears in the position indicated in the structural analysis.

It is clear from the examples cited that there are many restrictions on the form of the substitution transformations defined by contextual features. Thus the strict subcategorization features only involve 'local contexts' – i.e. contexts dominated by the phrase category that immediately dominates the lexical category for which the lexical item is substituted. On the other hand, selectional features refer only to 'heads' of grammatical related constructions. These restrictions can be made precise, and can be shown to lead to certain interesting consequences concerning the possible constraints that may appear in a grammar. For discussion, see again Chomsky (*Aspects of the theory of syntax*).

I have not discussed the problem of deviation from grammaticalness here. However, it is clear that whenever a grammatical rule exists, we may ask how a sentence is interpreted that deviates from this rule. It seems that sentences deviating from selectional rules are interpreted quite differently from those deviating from strict subcategorization rules. Deviation from selectional rules

gives such examples as 'colorless green ideas sleep furiously', 'sincerity admires John', etc.; deviation from strict subcategorization rules gives such examples as 'John persuaded to leave', 'John found sad', etc. Sentences of the former type are often interpreted as somehow metaphorical; sentences of the latter type, if interpretable at all, must be looked at in an entirely different way. Deviations from contextual rules involving category features (see above, p. 72) are still different in interpretive potential. Thus the various types of contextual feature are rather different in the conditions that they impose on sentence structures.

Notice incidentally that the ease with which sentences deviating from selectional rules can be interpreted is not simply a result of the fact that 'low-level' syntactic features such as [±Human] or [takes Animate Object] are involved. These features can participate in rules that are not at all violable in the way in which selectional rules may be (consider, for example, such expressions as 'the table who I scratched with a knife', 'who I saw was John', 'a very barking dog', etc.). There is much to say about this general problem; it is clear, however, that a nontrivial study of it demands a rich and detailed understanding of the various types of grammatical process.

We assumed, in this brief account of syntactic features, that the features of a Noun are inherent to it and that the features that selectionally relate Nouns to Verbs or Nouns to Adjectives appear as contextual (selectional) features of the Verbs and Adjectives. This was not an arbitrary decision; it can easily be justified on syntactic grounds. For discussion of this question, and many of the other topics mentioned briefly here, see Chomsky (*Aspects of theory of syntax*, Ch. 2).

With this, I conclude part 5 of the introductory outline. I have now briefly sketched two major respects in which the first modern attempts to formulate a theory of grammatical transformations were shown to be defective by later work. The first defect was one of excessive richness in expressive power. We have now been discussing a defect of the opposite kind, namely, an inability to express certain aspects of grammatical structure, and have suggested a way to modify the theory so as to overcome this. The theory of

transformational generative grammar that results from these modifications is conceptually quite simple, and is reasonably well-supported by what empirical evidence is presently available. Each component of the theory has a well-defined function; I see no way in which any of the postulated mechanisms can be eliminated without sacrifice of descriptive adequacy, and know of no justification for postulating a more complex structure and organization of the theory of the syntactic component than what has been sketched in outline here. For the present, then, this theory seems to me to constitute the most satisfactory hypothesis as to the form of the syntactic component of a grammar.

4

SOME PROBLEMS IN PHONOLOGY

I would like to conclude this paper with a few remarks about sound structure, more specifically, about the organization of the phonological component of a generative grammar.

The phonological component is a system of rules that relate a surface structure to the phonetic representation of a string. We have been assuming that a surface structure is a labeled bracketing of a sequence of minimal elements which we may call *formatives*. In the last section, we distinguished between two types of formatives – grammatical and lexical. Each lexical formative is a *complex symbol*, that is, a set of features. Among these are phonological features, which can be represented in matrix form with rows corresponding to features and columns to 'segments'. Thus if a formative contains the phonological feature $[\alpha F_n]$, where α is $+$ or $-$, F is a feature, and $_n$ is an integer, then the matrix will have the entry α in the n^{th} column in the row corresponding to the feature F. This matrix is essentially a classificatory device; it determines the phonological rules that will apply to the item in question. In effect, then, the phonological matrices represent a classification induced by the system of phonological rules. The rules of the phonological component apply to such matrices, adding entries or revising them, and perhaps adding or deleting columns. In the course of this operation, entries that are blank in the surface structure may be filled in, and we may think of all entries (which are initially either blank, or marked $+$ or $-$) as being replaced by integers indicating the position of the segment in question in the scale defined by the feature in question. The rules will also delete grammatical formatives or 'spell' them in terms of feature matrices. The final output of the system of phonological rules will be a *phonetic matrix* for

the sentence as a whole in which columns stand for successive segments (phones) and rows define phonetic distinctive features, regarded now as scales, the entry indicating where a segment falls along a scale. The character of such rules has been described elsewhere in detail.[1] I presuppose familiarity with this topic in the following remarks. In particular, it should be noted that the phonetic distinctive features are proposed as linguistic universals, which meet the requirement (9i) of section 1 for general linguistic theory. Cf. p. 18.

The input to the phonological component I will call a *phonological representation.*[2] The output produced by the phonological component I will call a *phonetic representation.* I will not attempt to investigate the relation of phonological to phonetic representations in any detail, but I would like to mention a few crucial issues that arise when this problem is considered.

Before continuing, I would like to remark that there can hardly be any question as to the linguistic significance of phonological and phonetic representation, in the sense defined above. A generative grammar that does not provide representations of these two types is unimaginable, in the present state of our knowledge. Furthermore, I do not believe that there is any serious controversy about this question.

The first question that arises, in connection with the phonological component of a grammar, is whether there is any other linguistically significant system of representation intermediate between phonological and phonetic. In particular, we may ask whether there is an intermediate level meeting the conditions that have been imposed

[1] E.g. Halle, *The sound pattern of Russian*, "Phonology in a generative grammar", "On the bases of phonology"; Chomsky and Miller, "Finitary models of language users"; Chomsky, *Current issues of linguistic theory.*

[2] Alternatively, we might restrict the term 'phonological representation' to the representation that we have at the point at which all grammatical formatives other than boundary symbols are eliminated in favor of matrices, so that what we have is a string of phonological matrices and boundary symbols (which, incidentally, also require feature analysis), with IC structure (i.e. labeled bracketing) marked. This is what is called "systematic phonemic representation" in Chomsky (*Current issues in linguistic theory*), where the topics now under discussion are elaborated in much more detail.

on the notion 'phonemic representation' in modern (taxonomic) linguistic theory. I will not state these here; for a detailed discussion, see Chomsky (*Current issues in linguistic theory*). This is a substantive question, and cannot be settled by terminological decision. It is a question of fact. Furthermore, it is clear that the burden of proof is on the linguist who believes that the answer is positive — that there is, in other words, a linguistically significant level of representation meeting the conditions on taxonomic phonemics and provided by the phonological rules of the grammar.

The claim that taxonomic phonemics exists as a part of linguistic structure seems to me without justification. There are various ways in which one might try to establish the existence of this level; none of them, so far as I can see, succeeds in establishing this conclusion. A detailed argument is presented in Chomsky (*Current issues in linguistic theory*). I will survey briefly some of the main points.

Certain versions of taxonomic phonemics are based on distributional analytic procedures of various sorts, the notion of 'complementary distribution' being central to these. But this notion is fundamentally defective. It permits analyses that are not acceptable to the taxonomic phonemicist (or to anyone else); it excludes the optimal taxonomic phonemic analysis in certain cases. Similarly, the other procedures of phonemic analysis fail to provide the intended results.

But there is another consideration that cuts much deeper than this. It might, after all, be shown that in some way the procedures can be improved to the point where they provide an analysis of the type postulated as essential by the taxonomic phonemicist, and exclude all other analyses. It is therefore necessary to shift our attention to the postulated analysis itself, and to ask whether an analysis of this sort can be provided by a system of phonological rules. This question was considered by Halle several years ago, and he produced an argument, which has since been repeated in the literature many times,[3] to show that if a taxonomic phonemic

[3] Cf. Lees (*The grammar of English nominalizations*); Halle (*The sound pattern of Russian*); Chomsky ("A transformational approach to syntax", *Current issues in linguistic theory*).

representation is provided by the phonological component, then important generalizations of obvious and unchallenged linguistic significance must be given up. It seems to me that this argument is unanswerable, but since its force has not been fully appreciated, it will perhaps be useful to present it schematically once again.

Reducing the argument to its essentials, suppose that we have a language with a phonological asymmetry, for example, a language with [t], [d], [č] and [j] phonetically, and with a phonological contrast between /t/ and /d/ but none between [č] and [j]. Thus there are, let us say, morphemes /Xt/, /Xd/, but no comparable pair or near-pair for č—j. Suppose that there is, furthermore, a general rule of voicing assimilation in the language. This rule can be stated as (24), using customary conventions:

(24) Consonant → [+Voiced] in the environment: —[+Voiced].

Thus the morpheme /Xt/ will appear as [Xd] in the context —[+Voiced] and as [Xt] in the context —[—Voiced]; and a morpheme /Yč/ will appear as [Yj] in the context —[+Voiced] and [Yč] in the context —[—Voiced]. The rule (24) converts phonological representations directly to phonetic representations in both cases. Let us suppose that the only occurrences of [j] are those produced by rule (24).

But observe that this grammar does not provide phonemic representations. For the taxonomic phonemicist, of any school, the lexical representations /Xt/, /Yč/ are not phonemic but 'morphophonemic' (what we have been calling 'phonological', following essentially Sapir's usage of terms). The morphophonemic, phonemic, and phonetic representations would be as given in table (25).

The first column gives the phonological (=morphophonemic)

(25)

phonological morphophonemic	phonemic	phonetic	in the environment
Xt	Xd	Xd	—[+Voiced]
	Xt	Xt	—[—Voiced]
Yč	Yč	Yj	—[+Voiced]
		Yč	—[—Voiced]

representation of the forms in the third column; the second gives their phonemic representations. The first column does not qualify as phonemic because it fails biuniqueness; the third does not qualify as phonemic because [č] and [j] belong to the same phoneme, under the circumstances we have described, in accordance with any phonemic theory that has ever been produced.

The grammar containing rule (24) thus converts phonological to phonetic representations without providing phonemic representations as a linguistic level. That is, if we mean by the phrase 'level of representation' a system of representations that appears at some well-defined point in the process of sentence-generation, then the grammar provides no level of phonemic representation (it is difficult to imagine what other sense might be given to this expression). To provide a level of phonemic representation, the grammar would have to replace (24) by the two rules (26), (27):

(26) t → [+Voiced] in the environment: —[+Voiced]
(27) č → [+Voiced] in the environment: —[+Voiced].

Rule (26) is now a 'morphophonemic rule' and rule (27) a 'phonemic rule'. The rule (24), which expresses the linguistic facts in the most general form, is now not expressed in the grammar.

Real examples of this sort are easy to find. They show that a level of phonemic representation can be included in a grammar only if certain generalizations are lost as now inexpressible. Since obviously the point of grammar construction is to provide general rules governing the organization of the phonetic facts, this observation constitutes a strong argument against the assumption that there exists a linguistic level of phonemic representation.

To my knowledge, the only defense that has been offered for taxonomic phonemics against this argument is due to Sydney Lamb, in a paper delivered before the Linguistic Society of America in December, 1963. Lamb attempted to refute Halle's argument against taxonomic phonemics in the following way. Let us take the symbol h to be a 'devoicing element', so that t is represented dh and č is represented jh. Let us now construct a grammar for the example described above, using this notation. The morpheme /Xt/ will be

represented /Xdh/, the morpheme /Yč/ will be represented /Yjh/, in morphophonemic representation. We now give just the single rule (28):

(28) h → Ø in the environment: —[+Voiced].[4]

The rule (28) converts the representations /Xdh/ and /Yjh/ to [Xd] and [Yj], respectively, before Voiced elements, and leaves them in the form [Xdh] = [Xt] and [Yjh] = [Yč] everywhere else. Thus this grammar produces the correct phonetic forms, and does so without losing the generalization, since the full set of phonetic facts is summarized in the single rule (28). Thus Halle's argument is refuted, since it is not necessary to replace the generalization (28) by two special cases (26), (27), when the grammar is formulated this way.

But it is evident that this attempt to defend taxonomic phonemics against Halle's criticism is simply an exact restatement of this criticism in a new notation. Instead of writing features in columns, as Halle does, Lamb writes them sequentially; instead of using the feature [Voicing], Lamb marks the unvoiced case with h, and leaves the voiced case with no explicit indication.[5] But the crucial point is that Lamb's notational reformulation of Halle's argument gives a grammar in which there are no phonemic representations just as Halle's optimal grammar provides no phonemic representations, and for the same reason; namely, the generalization is incompatible with the presence of a phonemic level. There is nothing corresponding to the middle column of (25) in the Halle-Lamb analysis, and the only point at issue is the status of this column. Thus Lamb

[4] Technically, the environment should be stated as follows: '—CZ, where C is any consonant and Z is any segment other than h, or is a boundary symbol.'

[5] Lamb's conventions actually seem to provide a restricted form of feature theory in which a feature cannot be left unspecified, and in which direct reference can be made in the rules to only one of the two possible values of a binary feature. [-Voice] corresponds to presence of h, [+ Voice] to its absence. Why the single feature Voicing (in Lamb's Russian example, actually Voicing and Palatalization) should be singled out for this special treatment is unclear; an analysis of Russian sound structure that went beyond the one example would show that other features must also be extracted in this way. Such a restricted form of feature theory can be studied on its own merits, quite apart from any consideration of taxonomic phonemics, if it is presented in a clear enough form.

has simply presented a notational variant to Halle's argument against taxonomic phonemics.

From this and many other similar considerations, one must conclude that there is no internal linguistic justification for phonemics in its modern (post-Sapir) sense. The only way to demonstrate the linguistic significance of this concept, then, is by an argument based on some other grounds, e.g. perceptual or methodological grounds. However, no such approach seems feasible (for discussion, see Chomsky, *Current issues in linguistic theory*). For the time being, then, there is no reason to assume that the phonemic level is anything other than an artifact.

Taxonomic phonemics developed from the assumption that sound structure should be studied either in complete isolation from syntax, or, in the case of approaches such as those of Pike and Harris, in partial isolation, with consideration of syntactic structure only insofar as this can be introduced in accordance with certain specified analytical procedures. Unless the arguments that have been directed against taxonomic phonemics can be met, one can only conclude that these assumptions are incorrect. So far as I know, there has never been any attempt to offer any justification for them beyond the observation that they lead to a theory that is consistent and reasonably well-defined. These conditions are surely necessary, but hardly sufficient to guarantee linguistic significance.

Let us now return to the question of how the rules of the phonological component apply to surface structures to determine, ultimately, the phonetic form of the utterance that they represent. It was proposed several years ago (Chomsky, Halle, Lukoff, "On accent and juncture in English") that one part of the phonology may be a sequence of transformational rules that apply to surface structures in the following way: first, they apply in a prescribed sequence to the expressions within innermost brackets, erasing these at the termination of the sequence; then, they reapply in exactly the same way; etc., until the maximum domain of phonological processes is reached. These rules are 'transformational' in the technical sense of linguistic theory, in that they take into account the phrase

structure of the string to which they apply, and not just its linear form as a sequence of symbols. These transformational rules thus in effect determine the phonetic form of larger units on the basis of the ideal phonetic form of the smaller units that constitute them. Notice that in their manner of operation they are quite analogous to the projection rules of the semantic component. Once the transformational cycle has completed its operation, surface structure is completely erased (up to the maximal domain of phonological processes).

In support of this hypothesis, we showed how the multilevel stress contours of American English that had been observed by many phoneticians and phonemicists, involving at least five perceptual levels of stress, could be accounted for by postulation of only a single Accented-Unaccented distinction and a few simple rules operating in a transformational cycle (this is summarized and extended in various other publications, e.g. Chomsky, "Explanatory models in linguistics"; Chomsky and Miller, "Introduction to the formal analysis of natural language"). Since that time, several studies have shown that other complex phonetic data can be explained on the same assumption (see note 6, p. 14, of Chomsky, *Current issues in linguistic theory*, for references).

In the light of what information is now available, it seems reasonable to maintain that insofar as syntactic structure plays a role in determining phonetic form, it is the surface structure in the sense described in previous sections that provides the relevant syntactic information, and (except for pre-cyclic rules that involve only lexical category – i.e. syntactic features, in the sense of the preceding section), the rules that determine the phonetic form in this way apply in a transformational cycle, as just indicated. This seems a very natural assumption, as well as one that is, for the present, well supported empirically by its ability to provide an explanation for what is in any other terms merely a collection of data.

Our theory of the transformational cycle, as presented in Chomsky, Halle, Lukoff ("On accent and juncture in English"), was discussed at length in the Second University of Texas Conference in 1957, first in a critical paper by A. H. Marckwardt and then in an

extended discussion [all of which appears in Hill, ed., *Second* (1957) *Texas Conference on problems of linguistic analysis in English* (Austin, 1962)]. The participants felt that the theory was completely demolished by this discussion (cf. e.g. Hill, p. 95, Twaddell, p. 104). Therefore, it is worthwhile to take up the criticisms that were presented point by point.

Marckwardt's primary objection is that we were postulating new, complex items (namely, the surface structure of the utterance)[6] in order to account for the phonetic facts. Thus the Trager-Smith phonemic (we would call them 'phonetic') representations utilize four levels of 'phonemic' stress of the five or more that must appear in accurate perceptual representations, leaving the others to be predicted by rule. Although our transcription reduced this to a single 'phonemic' distinction, it did this at the cost of introducing the entire surface structure of the utterance, and therefore was not really more economical at all.

This objection is a very curious one. Obviously, there would be no point in eliminating a set of stresses from a transcription by introducing a new set of arbitrary distinctions from which the stresses can be deduced. But this is not what we proposed. In fact, we showed that no new distinctions are needed at all to account for the stress distinctions represented in, for example, Trager-Smith transcriptions. In fact, the data presented in these transcriptions can be accounted for by assuming a single Accent distinction and relying, beyond this, only on surface structure and on the general theoretical principle of the transformational cycle. But surface structure is not some new, arbitrary construction invented for the purpose of accounting for stress. It is present in a grammatical description quite apart from any consideration of phonetic form.

[6] In this paper, we did not use labelled bracketing to represent surface structure, but rather a system of abstract 'junctures', indexed by integers to define the hierarchic structure. No lexical categories and only two phrase categories were considered in that analysis, namely Noun and 'everything else'; corresponding to these were the junctures - and =, respectively. Thus, for example, in this notation the full labeled bracketing;
[NP[Det[N *John*]N *'s*]Det [N[N[A *black*]A [N *board*]N]N [N[V *erase*]V er]N]N]NP
would be represented; *John's* $=_2$ *black* $-_1$ *board* $-_2$ *eraser*.

Thus if one wanted to generate, let us say, conventional orthographic representations instead of phonetic ones, in a full grammar, he would arrive at exactly the conclusions regarding the derived phrase markers of strings of words as we assumed in our attempt to account for the phonetic form. The surface structure is provided by the syntax; it is not based on phonetic considerations.

This criticism shows that Marckwardt, as well as the other discussants, completely missed the entire point of our paper. Our purpose was to show how it is possible to go beyond mere listing of data by providing an explanation for this data on the basis of a general hypothesis about linguistic structure (the theory of the transformational cycle) and other, independently established facts about the language (namely, the surface structure of utterances). There is no question here of comparing two phonetic transcriptions in terms of their relative complexity. The distinction is rather between the development of a transcription to record data (e.g. the Trager-Smith system, which we adopted in our paper), and the attempt to account for data that is recorded in this way.[7] Marckwardt's first argument thus simply reduces to an objection to any attempt to account for certain data by bringing to bear other facts and general theoretical principles – that is, it amounts to nothing more than an insistence that one should not go beyond mere recording of data in linguistic description.

A second argument presented in the discussion is that junctures had been proven (in Joos' paper in the same conference) to be phonetically detectable, whereas we had argued that the delimitation of phrases given in surface structure is not represented by phonetically detectable junctures. There is surely no one who would be willing to claim today that the IC structure of an utterance is

[7] A system for recording data is itself a 'theory' in a certain weak sense, insofar as it embodies a hypothesis about the system of distinctions (and the fineness of distinction) that must be represented to account for some mass of data. Thus a universal phonetic alphabet makes such an assumption about language in general, just as, for example, the Trager-Smith phonemic analysis makes the assumption that for all dialects of English, the actual phonetic form can be determined by rules of some sort from a representation containing no more than four stress levels, four pitch levels, etc.

indicated by phonetically detectable junctures, so I will omit any further discussion of this point.

A third objection to our paper was that by assuming a surface structure which is not merely a projection of the phonetics (e.g. of phonetically detectable junctures), we had given up the hope of using taxonomic discovery procedures (i.e. procedures of segmentation and classification of various sorts) to establish syntactic structure. This we cheerfully admit, taking for granted that the discussion in the intervening years has shown conclusively that the attempt to discover syntactic structure in this way is hopeless (and, furthermore, entirely unmotivated).[8]

A fourth objection is that we invent syntactic analyses arbitrarily so as to produce the correct phonetic results. To support this, Marckwardt cites our example 'excess profits tax', which we had assumed to have the meaning 'tax on excess profits', and had analyzed accordingly, showing that this analysis (with the phrase 'excess profits' embedded within the Noun 'excess profits tax') accounts for the stress contour. This syntactic analysis of the expression 'excess profits tax' Marckwardt sees no reason to accept. Since no other examples are given, I pass over this objection in silence.

The fifth objection is that our analysis is not complete. Thus there are many constructions that we did not account for in our explanatory scheme, whereas, in contrast, the Trager-Smith system of transcription can, presumably, provide a representation for any utterance that is likely to be produced. It is quite true that our attempt to explain the facts of English was far from complete; at the same time, we quite agree that it is possible to construct a system of phonetic transcription that will be complete. But it is entirely senseless to compare the 'completeness' of an explanatory theory with the 'completeness' of a scheme for recording data. It is, for example, no criticism of physical theory to point out that its

[8] To support his criticism of our paper for its abandoning of the use of discovery procedures, Marckwardt offers only the following quotation from Trager and Smith: "The application of this [i.e. the representation of an utterance in terms of stress levels, phonetically detectable junctures, etc.] to the problem of determining immediate consituents is obvious."

'coverage' is far less than that of a system for recording meter readings. Within linguistics, obviously, the discrepancy between what can be recorded and what can be explained in any serious way is enormously greater than in physics. This sort of objection in principle to attempts to explain phenomena in terms of other facts and postulated theoretical principles can only have the effect of guaranteeing that linguistics will remain indefinitely at the stage of data-collection and data-arrangement.

The final objection presented in Marckwardt's critique is based on two assertions in our paper, namely, that surface structure is not in general represented by physically defined junctures, and that the studies of stress by Trager-Smith and others are purely impressionistic. Marckwardt does not question either assertion, and surely neither can possibly be questioned. But he concludes that it is pointless to attempt to explain impressionistically recorded stress contours in terms of IC analysis that is not marked by phonetic juncture. This conclusion is apparently based on his belief that if surface structure is not represented by phonetic junctures then it is hardly better than a figment of the imagination; more generally, that linguistics should not concern itself with phenomena that have only vague physical concomitants (90). This is a fantastic proposal, going far beyond the limitations on linguistic research proposed by Reichling, Uhlenbeck, Dixon, etc. (see Section 2). There is not the slightest reason to suppose that phonetics provides the only evidence in support of conclusions about syntactic structure (or, for that matter, that it provides any significant evidence). Neither the traditional nor the modern study of syntax has accepted the restriction to phonetically marked aspects of utterance (except for a brief moment when it was hoped, apparently vainly, that 'phonological syntax' might yield some useful conclusions).

As to the fact that representation of stress is purely impressionistic and not, so far as is known, determinable by physical measurement, this supports the conclusion of our paper and remains as an embarrassing difficulty for those who insist, for some reason, on limiting grammar-construction to what they can discover on the basis of phonetic observation alone. From our point of view, there

would be nothing at all surprising about the discovery that the stress levels heard by the careful phonetician have no physical basis at all.[9] Since in any event, the phonetic contour is largely an automatic reflection of the syntactic structure, it follows that anyone who understands an utterance and thus, in particular, has determined its surface structure, should be able to predict the phonetic contour by rules that constitute part of his linguistic competence. He will, then, 'hear' what these rules predict, as long as this is not in too violent disagreement with the physical facts.[10] However, the fact that stress judgements may not have a purely physical basis seems quite impossible to reconcile with a belief in the potentialities of 'phonological syntax', or, for that matter, with a belief in the usefulness of the study of sound in isolation from syntactic structure.

I am unable to extract any other specific points that merit discussion from the remainder of the recorded proceedings of the Texas conference. In summary, I find nothing in Marckwardt's paper or in the discussions that suggests any error or defect either in our proposals concerning rules of stress assignment in English or

[9] I do not assert that this strong statement is true, but only that we would not find such a conclusion incompatible with what we have found and presented. In fact, it seems not at all unlikely that something like this may be true – that is, that what the phonetician 'hears' is largely a reflection of what he knows rather than just of what is present in the physical signal itself. Such a conclusion would hardly surprise either psychologists or acoustic phoneticians, and it is entirely in accord with what little is known in linguistics about the nature of phonetic representation.

[10] The point is worth pursuing. Our theory of English stress is compatible with the assumption that the only 'functional' distinction is a single differentiation of relative stress – for example, the differentiation that distinguishes 'black bird' from 'blackbird'. We showed that this single differentiation (which is itself predictable in many syntactic contexts, though not, obviously, in isolation) is sufficient to give a mass of well-attested stress contours, with many levels, given the facts of syntactic organization and the principle of the transformational cycle. Consequently, we would expect that these many-leveled contours would be heard, whether physically present or not, by the phonetician who, as native speaker, has internalized the more-less differentiation that is functional and who understands the utterances he is transcribing. The question of the acoustic reality of these well-attested contours (which we do not challenge, as perceptual facts) deserves much closer investigation. For evidence bearing on this question, see Lieberman. "On the acoustic basis of the perception of intonation by linguists", *Word* 21.40-54 (1965).

in the principle that we suggested to account for the data that had been recorded. Insofar as they are not based on confusion or misreading, the criticisms agreed on by all participants simply reduce to the observation that within the narrow bounds that they impose on linguistic research, there is little possibility of studying the principles that determine phonetic form and no point in doing this, where it is possible. We agree with this observation, and feel that it can be generalized well beyond the domain of fact that was discussed in this conference. This, in fact, seems to us the primary reason for rejecting the approach to the study of language accepted by the participants in this conference. I think that a careful study of the proceedings of this conference should prove quite rewarding in the light that it sheds on the limitations of one particularly influential form of modern taxonomic linguistics.

There is, however one very real criticism of our paper. As we have since discovered, it did not go far enough in eliminating stress from phonological transcription. In fact, more recent work shows that even the Accented-Unaccented distinction is so marginal in English that it hardly makes any sense to regard Stress or Accent as a phonologically functional category (i.e. as constituting a row which is filled by +'s and −'s in lexical matrices), except for a class of examples comparable, essentially, to strong verbs or irregular plurals. Furthermore, we were in error in assuming that the reduced vowel [ɨ] must be represented in phonological matrices. Its occurrence, too, can largely be predicted by extension of the transformational cycle, along lines described in Halle and Chomsky ("The morphophonemics of English", *Quart. Progr. Rep. No. 58* 275-81, Cambridge, Mass.; Chomsky ("Explanatory models in linguistics"), Chomsky and Miller ("Introduction to the formal analysis of natural languages").

Apart from the iterating rules of the transformational cycle, there are also many nontransformational phonological rules that apply only at one stage of the process of derivation. We can express this fact by incorporating these rules into the transformational cycle, limiting their application to the level of word boundary. These nontransformational rules effect modifications of phono-

logical matrices that are not determined by surface (IC) structure.

There is a great deal to be said about the nature of both transformational and nontransformational phonological rules. In particular, it is important to observe that the phonological matrix postulated as an underlying form may undergo significant modification in the course of derivation, and in fact, it is not unusual for the postulated underlying form to appear in none of the actual phonetic realizations. Furthermore, it has been widely observed that the underlying forms are extremely resistant to historical change, whereas late phonetic rules are much less resistant to modification. Thus it is commonly found that the underlying forms for a language are barely different from those of much earlier stages of this language, and that dialects that differ greatly in phonetic realization and in surface organization of the phonetic data may be quite similar or identical in the phonological representations assigned to formatives and sentences. Recent work in English supports these general conclusions strongly; it shows, in fact, that phonological representation is for the most part rather like conventional orthography, for the dialects that have so far been studied.

SUMMARY

In this essay I have been discussing topics in linguistic theory from a point of view which is in most respects quite traditional, but which has been given new life and scope in recent work. I have also tried to show that this traditional view must be adopted, in its essentials, if linguistic research is to progress and to provide understanding of significant questions. There are value judgments here, of course; I have tried, here and in the references mentioned previously, to justify those that underlie the work I have been reviewing.

This work has been based on the assumption that competence must be distinguished from performance if either is to be seriously studied. It has, beyond this, attempted to provide an explanatory theory of competence, and to use this as a basis for constructing an account of performance. The theory of competence is mentalistic, naturally, in that it can at the present stage of knowledge draw no evidence from and make no direct contribution towards the study of the mechanisms that may realize the mental structures that form the subject matter for this theory, or that carry out the mental processes that it studies. Thus the theory of competence (i.e. the theory of grammar) deals with abstract structures, postulated to account for and explain linguistic data.

Certain aspects of the theory of grammar seem reasonably well-established today. The abstract character of underlying (deep) structure in both syntax and phonology is hardly open to question, and there are interesting general conclusions that can be drawn from this fact (see p. 38, n. 11). The role of grammatical transformations in syntax and phonology seems hardly disputable, in the light of present information, and the role of distinctive features in syntax and phonology also seems to be firmly established. There

is also little doubt that the rules relating abstract underlying structures to surface forms, in syntax and phonology, are ordered either linearly or cyclically in many or perhaps all parts of the grammar.

Nevertheless, it goes without saying that any theory of grammar that can be formulated today must be highly tentative. Many questions remain totally open, many partially so. In general, the empirical assumptions about the form of language that can currently be formulated will undoubtedly be refined and improved, and, no doubt revised in essential ways as new critical evidence accumulates and deeper theoretical insights are achieved. Changes in linguistic theory are inevitable in coming years. In short, linguistics is a living subject.

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