The Chomskyan Revolution

2.1. OVERVIEW

Early in 1957, Noam Chomsky's *Syntactic Structures* was released by Mouton Publishers in The Hague. This book, which introduced the theory of transformational generative grammar, did not share the fate of most first books by unknown authors distributed by obscure publishers. Within weeks, a review by Robert B. Lees appeared in *Language*. Lees, who enjoyed a modest reputation in the field, left no doubt that the book would change linguistics. Such praise for a new approach to the study of language and such derogation of the contemporary paradigm had never been seen before in the pages of that journal. Lees (1957) wrote that Chomsky's book was "one of the first serious attempts on the part of a linguist to construct within the tradition of scientific theory-construction a comprehensive theory of language which may be understood in the same sense that a chemical, biological theory is ordinarily understood by experts in those fields" (p. 377).

Actually, the tone of the review as a whole made it clear that Lees regarded it as the only serious attempt, and a completely successful one at that. And C. F. Voegelin (1958), in another review, noted that even if *Syntactic Structures* managed to accomplish only part of its goals, "it will have accomplished a Copernican revolution" (p. 229). The distinguished British linguist, C. E. Bazell, after one reading of the book, remarked to a student that "linguistics will never be the same" and immediately initiated a continuing correspondence with Chomsky.

By the mid 1960s, after the full implications of transformational generative grammar had become evident, counterattacks by the score were directed against this "theory spawned by a generation of vipers" (Charles Hockett, quoted in Mehta 1971:175). But the challenge to the new theory was not successful. In the mid and late 1960s, American universities underwent their greatest expansion in history.

New linguistics departments sprang up in a dozen locations, and some existing ones literally quadrupled in size. The reputation of Chomsky's theory had grown to the point where, by and large, its partisans were sought to fill the new positions. In fact, the growth of linguistics outpaced that of most other fields, a testament to the intellectual appeal of the theory.

Even as early as 1964, transformational generative grammar was being described as having "established itself as the reference point for discussion of linguistic theory" (Hymes 1964:25). An obvious indicator of the rapid success of the theory is that, by the late 1960s, its advocates had stopped replying to criticism from linguists in the post-Bloomfieldian structuralist tradition. They had no further need to answer the Old Guard. Chomsky and others turned their attention to assaults on the fundamentals of the theory by psychologists and philosophers. But the increased stature of cognitive psychology and rationalist philosophy within their respective disciplines in recent years points to the impact that generativist theory has had outside of linguistics proper and to the indifferent success of the psychological and philosophical critique. The 1970s also saw the unprecedented internationalization of a linguistic theory; by the 1980s transformational generative grammar had at least as many practitioners outside the United States as inside.

In the 1980s there has been an undeniable fragmentation of the once-monolithic theory. Many alternative models of linguistic description have been proposed, some heralded as making as much of a break from mainstream transformational grammar as this theory made from the varieties of structural linguistics that were practiced in 1957. But, by and large, the debate has taken place well within the general framework of theoretical assumptions first articulated in *Syntactic Structures*. A truly alternative conception of grammar with general credibility or widespread support has yet to emerge.

2.2. *SYNTACTIC STRUCTURES*

2.2.1. The Revolutionary Import of *Syntactic Structures*

What made *Syntactic Structures* revolutionary was its conception of a grammar as a theory of a language, subject to the same constraints on construction and evaluation as any theory in the natural sciences.[1] Prior to 1957, it was widely thought, not just in linguistics but throughout the humanities and social sciences, that a formal, yet nonempiricist, theory of a human attribute was impossible. Chomsky showed

---

[1]Many European structuralists, Saussure among them, have explicitly regarded themselves as doing "scientific" linguistics, though it is fairly clear that they have not seen themselves as paralleling natural scientists in their goals and methodology. For many, the term "scientific" seems to have meant little more than "objective" or "nonprescriptive." Consider for example, the following passage from Martinet: "Une étude est dite scientifique lorsqu'elle se fonde sur l'observation des faits et s'abstient de proposer un choix parmi ces faits au nom de certains principes esthétiques ou moraux. 'Scientifique' s'oppose donc à 'prescriptif' " (Martinet 1960:9).

that such a theory was possible. Indeed, he devotes the central chapter of *Syntactic Structures*, "On the Goals of Linguistic Theory," to demonstrating the parallels between linguistic theory, as he conceived it, and what uncontroversially would be taken to be scientific theories. Still, *Syntactic Structures* would not have made a revolution simply by presenting a novel theory of the nature of grammar. The book had revolutionary consequences because it was not merely an exercise in spec-ulative philosophy of science. On the contrary, *Syntactic Structures* demonstrates the practical possibility of a nonempiricist theory of linguistic structure: half of the volume is devoted to the presentation and defense of a formal fragment of English grammar.

Chomsky's conception of a grammar as a theory of a language allowed him to derive the major insight of earlier theorizing about language: the *langue/parole* (later competence/performance) distinction. For Saussure, who conceived of lin-guistics as a branch of social psychology, the distinction was merely stipulated; surely, for him there was no necessary reason why *langue* should be "a well-defined object in the heterogeneous mass of speech facts" (Saussure 1959:14). One can easily imagine a social system of verbal exchange in which such a "well-defined object" is absent. For Chomsky, however, the distinction followed as a logical consequence of the assimilation of linguistics to the natural sciences. Just as physics seeks to specify precisely the class of physical processes and biology the class of biological processes, it follows that a task of linguistics is to provide "a precise specification of the class of formalized grammars" (Chomsky 1962a:534). Interestingly, Chomsky's empiricist antecendents in American structural lin-guistics, who were in principle incapable of postulating a sharp dichotomy on the basis of observationally graded data, were forced to negate the *langue–parole* distinction by regarding the former as no more than a set of habits deducible directly from speech behavior (Hockett 1952). Not surprisingly then, Hockett's major at-tempt to rebut Chomsky (1968) recognized that the question of whether the gram-mar of a language is a well-defined system was the central issue separating his view of language from Chomsky's.

The publication of *Syntactic Structures* represented a revolutionary event in the history of linguistics for a second reason: it placed syntactic relations at the center of *langue*. By focusing on syntax, Chomsky was able to lay the groundwork for an explanation of the most distinctive aspect of human language—its creativity. The revolutionary importance of the centrality of syntax cannot be overstated. Pho-nological and morphological systems are essentially closed finite ones; whatever their complexity or intrinsic interest, their study does not lead to an understanding of a speaker's capacity for linguistic novelty nor to an explanation of the infinitude of language. Yet earlier accounts had typically excluded syntax from *langue* al-together. For Saussure, most syntagmatic relations were consigned to *parole*, as they were for the linguists of the Prague School, who treated them from the point of view of "functional sentence perspective." Zellig Harris, it is true, had begun in the 1940s to undertake a formal analysis of intersentential syntactic relations, but his empiricist commitment to developing mechanical procedures for grammatical

analysis led him to miss the implications that the study of these relations had for an understanding of linguistic creativity.

Chomsky attacked the empiricist conception of linguistic theory held by the post-Bloomfieldians for imposing a condition on theory formation that prevented the development of an insightful account of language, namely the condition that the description must be mechanically extractable from the primary data. But such a condition, Chomsky argued, is not to be found in any science. In his view, the most a linguistic theory can be expected to provide is an evaluation procedure for grammars—a mechanical way of deciding between alternative grammars within a particular theory. And, as Chomsky pointed out, even an evaluation procedure is more than most sciences would hope to accomplish: "There are few areas of science in which one would seriously consider the possibility of developing a general, practical, mechanical method for choosing among several theories, each compatible with the available data" (p. 53).

Given the abandonment of the aim of literally "discovering" a grammar, there need be no concern about a description that mixes levels. Since a grammar would no longer result from a cookbooklike set of directions that tells the linguist to "find the phonemes first," the rules and the inventory of phonemes, morphemes, and other components might be arrived at "by intuition, guesswork, all sorts of partial methodological hints, reliance on past experience, etc." (p. 56). In the resultant description, if syntactic information, say, entered into the statement of a phonological rule, there would be no cause for concern: the question of the interdependence of levels is to be answered by empirical investigation rather than by methodological fiat.

While Chomsky's conception of the nature of grammatical theory was a revolutionary one, there are, needless to say, numerous respects in which *Syntactic Structures* retains crucial conceptions of its historical antecedents. Foremost among them is Saussure's great insight that at the heart of language lies a structured interrelationship of elements characterizable as an autonomous system. Such an insight is the essence of "structuralism" and, since it is assumed throughout *Syntactic Structures* and Chomsky's subsequent work, one can, with good reason, refer to Chomsky as a "structuralist."[2] Some commentators have pointed to this fact in order to dismiss the idea that there could have been a Chomskyan revolution. Since structuralism was well established years before the publication of *Syntactic Structures,* by what criteria, such individuals ask, could it be correct to refer to a "Chomskyan" revolution? George Lakoff, for example, concluded from Chomsky's commitment to structural analysis that early transformational grammar, rather than representing a revolutionary development, "was a natural outgrowth of

[2]The structuralism issue is confused by the fact that in the early 1960s Chomsky and his followers began to reserve the label "structuralist" just for those synchronic approaches in the Saussurean tradition that did not share their views on theory construction. The result is that now within linguistics, when one speaks of a "structuralist," it is normally understood that one is referring to a pre-Chomskyan or an anti-Chomskyan. Interestingly, commentators from outside the field have always labeled Chomsky a "structuralist," and we find his ideas discussed in most overviews of twentieth century structuralism (see, for example, Lane 1970:28–29; De George and De George 1972:xx).

American structural linguistics'' (Lakoff 1971a:267–268). The same point has been made more recently by Stephen Murray, who finds "the base of Chomsky's early work . . . in American structural linguistics, especially as developed by Zellig Harris'' (1980:76); by Dell Hymes and John Fought, who regard *Syntactic Structures* as showing "no evidence of basic revolutionary change'' (1981:241); and by Konrad Koerner, who feels that "TGG is basically post-Saussurean structuralism, characterized by excessive concern with 'langue' . . . to the detriment of 'parole' '' (Koerner 1983:152).

Chomsky's structuralism, however, no more disqualifies his theory from being revolutionary than does Einstein's Newton-like search for physical laws undermine the revolutionary nature of relativity theory. Saussure's victory was the victory of structuralism, just as Newton's victory was the victory of a lawful universe. We would no more expect the next revolution in linguistics to be an antistructuralist one than we would expect the next revolution in physics to return to divine intervention as an explanatory device. Chomsky's revolution was a revolution within structural linguistics, a revolution that profoundly altered our conceptions of the nature of linguistic structure and opened the way to an understanding of how its nature bears on the workings of the human mind.

When Charles Hockett wrote that "Chomsky's outlook . . . is so radically different from Bloomfield's and from my own that there is, at present, no available frame of reference external to both within which they can be compared'' (Hockett 1966:156), one presumes that he was (correctly) ignoring the fact that the approaches of the three linguists share the property of "structuralism.''

2.2.2. Simplicity and the Evaluation of Grammars

To say that a theory provides an evaluation procedure for grammars obviously invites the question: By what criteria are grammars to be evaluated? Clearly, the nature of a successful linguistic description had to be made explicit. Chomsky termed the criteria of evaluation "external conditions of adequacy'' and in *Syntactic Structures* outlined four of them:

1. The sentences generated are acceptable to the native speaker.
2. Every case of a "constructional homonymity'' (the assignment of more than one structural description to a sentence) describes a real ambiguity; every case of ambiguity is represented by constructional homonymity.
3. Differential interpretations of superficially similar sentences are represented by different derivational histories. (His example involved the sentences *the picture was painted by a new technique* and *the picture was painted by a real artist.*)
4. Sentences understood in similar ways are represented in similar ways at one level of description.

But it is not sufficient that a grammatical analysis in isolation meet the external conditions of adequacy. In addition, it is necessary to posit "a CONDITION OF

GENERALITY on grammars; we require that the grammar of a given language be constructed in accord with a specific theory of linguistic structure in which such terms as 'phoneme' and 'phrase' are defined independently of any particular language" (p. 50). The "condition of generality" (Chomsky used this term only once) corresponds to what were later called "linguistic universals" and comprises those theoretical terms, notational conventions, etc., that interact in such a way as to allow the external conditions of adequacy to be met in the most linguistically revealing way. For example, since a grammar without transformational rules presumably could not meet the external conditions, the condition on generality demands the incorporation of such rules into the descriptive apparatus of the theory. But what of a case where there appeared to be two grammars, each meeting the external conditions? Here the evaluation procedure alluded to above would come into play. Chomsky hoped the condition of generality might be formulable so that, given two alternative descriptions, the shorter one (as measured in terms of absolute length, number of symbols, or some such criterion) would always be the one of maximum generality as well.

2.2.3. Three Models of Linguistic Description

Chomsky had two major goals in *Syntactic Structures*. First, he had the general goal of motivating linguistic theory and its formalization by means of generative grammars that were subject to certain conditions of adequacy. But also, he had the more narrow goal of demonstrating that only a specific type of generative grammar had the ability to meet these conditions, namely a generative grammar embodying transformational as well as phrase structure rules. The former were inspired by, and are essentially generative reinterpretations of, the identically named rules proposed by his teacher Zellig Harris in his attempt to apply the methods of structural linguistics to the analysis of discourse.

Chomsky was in the peculiar position of having to argue against two generative grammatical models—finite state grammars and phrase structure grammars—which had very few outspoken adherents. He had to do this because these models were the closest generative interpretations of the views of language current in the 1950s. Finite state grammars bore a close resemblance to (or were identical to, as in the case of Hockett 1955) the type of device promoted by communications theorists. The sorts of descriptions which phrase structure grammars provided were (for all practical purposes) identical to those that resulted from the post-Bloomfieldians' procedures. So Chomsky's demonstration of the inadequacy of these two models in Chapters 3, 4, and 5 of *Syntactic Structures* was directed to—and was most convincing to—those linguists who might have been won over to his general goal of constructing a formal nonempiricist linguistic theory but still clung to the generative analogs of earlier views of language. Clearly, a linguist who rejected the need for generative rules, external conditions of adequacy, etc., would not have been terribly impressed by Chomsky's demonstration of the superiority of transformational grammar over phrase structure grammar.

Chomsky contrasted the three models in terms of their "weak generative capacity" and their "strong generative capacity," to use terms that appeared a few years later. The former refers to their string-generating ability, the latter to their ability to assign structural descriptions to these strings. Since a grammar unable to generate all and only the sentences of a language is of no further empirical interest, the demonstration of the defect of a model in terms of weak generative capacity makes any discussion of its strong capacity unnecessary. Chomsky proved that finite state grammars could not even weakly generate the sentences of English in an argument taking the following form:

First Premise: No finite state grammar is capable of generating a language containing an infinite set of strings with nested dependencies while simultaneously excluding the infinite set of strings that contradict these dependencies.

Second Premise: A subpart of English is a language as described in the First Premise.

Conclusion: All and only the sentences of English cannot be generated by a finite state grammar.

Chomsky proved the first premise in his 1956 paper, "Three Models for the Description of Language." The second depends crucially on the assumption that sentences of the form *if* _____ *then* _____, *either* _____ *or*, _____ etc., can be embedded in each other without limit. Although it is probably the case that the degree of nesting is quite restricted in actual speech, speakers (with paper and pencil) can normally interpret highly complex nested constructions such as the following one cited in Chomsky and Miller (1963) (a dependent relation holds between like-subscripted elements):

Anyone$_1$ who feels that if$_2$ so-many$_3$ more$_4$ students$_5$ whom we$_6$ haven't$_6$ actually admitted are$_5$ sitting in on the course than$_4$ ones we have that$_3$ the room had to be changed, then$_2$ probably auditors will have to be excluded, is$_1$ likely to agree that the curriculum needs revision. (p. 286).

Given that there is no principled limit to the nesting possibilities, it follows that English (and, by extension, language in general) cannot be described in finite state terms. There was no need for Chomsky even to mention the more serious defects of finite state grammars in terms of their strong generative capacity.

Chomsky did not question in *Syntactic Structures* that phrase structure grammars are capable of weakly generating the sentences of English.[3] He rather argued that

[3]Chomsky later (1959a) proved that the language consisting of all and only the strings [XX] is not a context-free language where X varies over an infinite set of strings in an alphabet of two or more symbols. Postal (1964b) then demonstrated that Mohawk, an Iroquoian language, contains, as a subpart, an infinite set of sentences with this property. While his proof demonstrated only that Mohawk sentences could not be weakly generated by a context-free phrase structure grammar, he also argued that a phrase structure grammar containing context-sensitive rules would require at least thirty-six million symbols.

The question of the context-freeness of natural language has become a major theoretical issue in recent years (see Section 8.3).

they can do so only in a cumbersome fashion and, furthermore, do not come close to assigning the correct structural descriptions to the sentences generated. His examples of the defects of phrase structure grammars were illustrated simultaneously with the demonstration that grammars containing the more powerful transformational rules can handle the same phenomena in an elegant and revealing manner. By far his most persuasive demonstration involved the English verbal auxiliary system. While Chomsky did not attempt to state the phrase structure rules that would be involved in generating all of the possible combinations of auxiliary verbs in English (and excluding the impossible ones), it was generally accepted at that time that such rules would have to be enormously complex. The *Syntactic Structures* analysis, however, treated the superficially discontinuous auxiliary morphemes *have . . . en* and *be . . . ing* as unit constituents generated by the phrase structure rules and posited a simple transformational rule to permute the affixal and verbal elements into their surface positions, thus predicting the basic distribution of auxiliaries in simple declarative sentences. Moreover, Chomsky was able to show that the permutation rule, "the Auxiliary transformation" (later called "Affix Hopping" by other linguists), interacts with rules forming simple negatives and simple yes-no questions to specify neatly the exact locations where "supportive" *do* appears. The ingenuity of this analysis probably did more to win supporters for Chomsky than all of his metatheoretical statements about discovery and evaluation procedures and immediately led to some linguists' proposing generative-transformational analyses of particular phenomena despite a lack of enthusiasm for the foundations of the theory itself (see, for example, Gleason 1961:171–194 and the suspiciously Chomskyan analysis in Joos 1964:53ff.).

Chomsky's motivation of the Passive transformation was analogous to that of the Auxiliary transformation. He argued that the contextual restrictions holding between the passive morpheme, the passive *by*-phrase, and the transitive verb would be extraordinarily difficult to state by means of phrase structure rules. However, one transformational rule permuting the subject and the object and adding the morphemes that are characteristic of the passive construction (*be . . . en* and *by*) eliminates the need for any of these special restrictions.

Chomsky's arguments for transformational rules in *Syntactic Structures* were all simplicity arguments, that is, arguments appealing to weak generative capacity. They all involved showing that a grammar with phrase structure rules alone required great complexity, a complexity that could be avoided only by the positing of a transformational rule. Chomsky felt that analyses motivated strictly in terms of simplicity would invariably turn out to be the optimal ones in terms of strong generative capacity as well. For example, Chomsky did not posit a transformational analysis of the auxiliary system because of the inability of phrase structure grammar to generate discontinuous morphemes as the unitary elements that they intuitively are. He based his analysis on formal simplicity alone, opting for a simple phrase structure rule and a simple transformational rule over a large number of cumbersome phrase structure rules. However, as he demonstrated, the generation of these morphemes as constituents turned out to be an important by-product of the analysis,

which was motivated on purely formal grounds. Likewise, Chomsky was unwilling to point to the undeniable semantic correspondences between declaratives and interrogatives, affirmatives and negatives, actives and passives, etc., as evidence for setting up transformations relating them. He felt that to use such evidence would have led merely to a stipulation of the correspondences rather than to an explanation of them. Hence, the relevant transformations were motivated on the grounds of formal simplicity alone (for further discussion of this point see Section 2.2.4).

The transformational model of *Syntactic Structures* contained three levels: phrase structure, transformational structure, and morphophonemics. The rules of the phrase structure level generated a finite set of "underlying terminal strings," that is, strings with phrase structure interpretations. This set was finite because there were no recursive rules applying at the phrase structure level.[4] Most phrase structure rules were context free, such as Rule (2.1), which expands the Auxiliary node:

(2.1)   Aux $\rightarrow$ Tense (Modal) (*have* + *en*) (*be* + *en*)

However, it was necessary to posit context-sensitive phrase structure rules as well as to account for lexical subcategorization. The following rule (taken from Chomsky 1962b:138), which subcategorizes verbs with respect to their cooccurrences within the verb phrase, is an example:

$$(2.2)\ V \rightarrow \left\{ \begin{array}{l} \left\{ \begin{array}{l} V_s \\ become \end{array} \right\} \text{in env.} \underline{\hspace{2cm}} \text{Pred} \\ V_t \text{ in env.} \underline{\hspace{1cm}} \text{NP} \\ V_i \text{ in env.} \left\{ \begin{array}{l} \# \\ \text{Adv} \end{array} \right\} \end{array} \right\}$$

Phrase structure rules (ordinarily context free) then introduce the lexical items themselves:

(2.3) $V_s \rightarrow$ *feel, seem, . . .*

Transformational rules then map these underlying terminal strings into other strings, each derived string being assigned a new constituent structure. Chomsky had no term for the output of the transformational rules more technical than "a string of words." Transformations were of two fundamentally distinct types: singulary (simple) and generalized (double-based). Singulary transformations, such as Passive, the Auxiliary transformation, and the Negative transformation, applied within the simple phrase markers generated by the branching rules. Generalized

---

[4]Strictly speaking, this is not correct. Chomsky (p. 73) envisaged handling some very marginal recursive processes, such as the unlimited successive appearances of the modifier *very*, by phrase structure rules. Technically, then, the output of this component is an infinite set of terminal strings in the *Syntactic Structures* model.

transformations embedded into each other or joined with each other these derived phrase markers without limit, thereby capturing the recursive property of human language. Similarly transformations could apply after embedding, as well as before.

Many have wondered why Chomsky did not, from the very beginning, handle recursion by the device of recursive phrase structure rules—an approach both conceptually simpler and more constrained, in that base recursion limits the class of possible grammars.[5] The reason is that the interaction of the singulary and generalized transformations was a complete mystery at the time. The more conservative approach chosen by Chomsky, that of positing embedding transformations, left open the possibility that singulary transformations might apply in a matrix clause before another sentence was embedded into it. Certainly there was no evidence that such might not be the case. A multiclausal base structure would have made this possibility difficult, if not impossible, to state formally. I discuss this problem in some detail in the next chapter.

Chomsky further argued that at least some transformations had to be extrinsically ordered with respect to each other in the grammar. For example, the transformation that assigned number to the verb based on the number of the subject had to be specified to follow the passive transformation to avoid *the boy* (singular) *sees the girls* (plural) from being mapped into **the girls* (plural) *is seen by the boy* (singular).

Chomsky also drew a distinction between obligatory and optional transformations. The former had to apply whenever their structural description was met. The Auxiliary transformation and the *Do* transformation (which inserted the morpheme *do* before a stranded tense affix) are examples. The class of optional transformations was quite large: negatives, *wh-* and yes-no questions, and imperatives were all formed by optional rules. Hence, the terminal string *you-*Pres-*will-light-the-fire* underlay all of *you will light the fire, light the fire, you will not light the fire, will you light the fire? what will you light? who will light the fire?* etc.

Those sentences derived only by the application of obligatory transformations had a special name: "kernel strings." The kernel of the language corresponded exactly to the set of simple (i.e., uniclausal) declarative affirmatives.

The derivation of a sentence was complete after the string of words was converted into a string of phonemes by the morphophonemic rules. Each derivation in the pre-1965 model was graphically represented by its unique transformation marker. Such markers consisted of the specification of the phrase markers associated with each underlying terminal string and those singulary and generalized transformations that applied in the derivation, in the order in which they applied.

2.2.4. Grammar and Meaning

Superficially, the relationship between syntax and semantics seems quite straightforward in *Syntactic Structures* and can be captured by the following quote: "I think

---

[5]But see Bach (1977b) for a defense of the view that generalized transformations are in principle more constraining than base recursion.

that we are forced to conclude that grammar is autonomous and independent of meaning'' (p. 17). The independence of grammar and meaning is stressed so many times in that book that many commentators have assumed that Chomsky simply took over the position of Harris and Bloch, an assumption often going hand-in-hand with the implication that this demonstrates that he had not really broken completely from post-Bloomfieldian structuralism. But a careful reading of *Syntactic Structures* clearly falsifies this conclusion. First of all, the independence of grammar in no way followed from his methodology, as it did for the post-Bloomfieldians. Chomsky was clear that the question of the relation of grammar and meaning is an empirical one. He gave example after example to illustrate his position: Speakers have intuitions that cannot be expressed in semantic terms; neither phonemic distinctness nor morpheme identity is wholly semantic; notions like ''subject'' and ''object'' defy strict semantic characterization; etc. In fact, Chomsky used the apparent non-paraphrase relationship between sentences like *everyone in the room knows at least two languages* and *at least two languages are known by everyone in the room* as evidence that Passive (and transformations in general) cannot be defined strictly in terms of meaning. In other words, he was arguing that the assumption that syntax is semantically based is false and any theory built on this assumption must therefore be fundamentally deficient.

Second, an understanding of how Chomsky regarded the notion ''meaning'' at that time helps put many of his comments in a different light. While his theory of meaning was fairly eclectic (in footnote 10 on page 103 he seems to imply that much of meaning can be reduced to reference), he was then very much under the influence of the Oxford philosophers and their use theory of meaning. In fact, the words ''meaning'' and ''use'' are used almost interchangeably throughout *Syntactic Structures:*

> There is no aspect of linguistic study more subject to confusion and more in need of clear and careful formulation than that which deals with the points of connection between syntax and semantics. The real question that should be asked is: ''How are the syntactic devices available in a given language put to work in the actual use of this language?'' (p. 93)

In other words, many of Chomsky's arguments in *Syntactic Structures* for the autonomy of syntax were in reality arguments for (what he would call a few years later) the competence–performance dichotomy. Many aspects of meaning, in his view at the time, were part of performance.

Third, he regarded as theoretically significant a number of systematic connections between syntax and semantics. For example, he pointed out that the popular view that syntax is semantically-based, though empirically false, does contain a considerable element of truth. Yet, he went on to say, there is no possibility of explaining the considerable fit between syntax and semantics if one takes a semantically-based syntax as a starting point. He noted other systematic connections, as in the following passage in which deep structure interpretation was foreshadowed (note again the ''performance'' terminology): ''The general problem of analyzing the process of 'understanding' is thus reduced, in a sense, to the problem of explaining how kernel sentences are understood, these being considered the basic

'content elements' from which the usual, more complex sentences of real life are formed by transformational development'' (p. 92). Likewise, later claims about the relationship of T-rules and meaning were foreshadowed in statements like ''we find, however, that the transformations are, by and large, meaning-preserving'' (p. 123), a fact that would have never come to light if transformations ''had been investigated exclusively in terms of such notions as synonymity'' (p. 101).

Finally and most importantly, Chomsky proposed that grammars be evaluated on the basis of their ability to lead to insights about the meanings of sentences:

> We can judge formal theories in terms of their ability to explain and clarify a variety of facts about the way in which sentences are used and understood. In other words, we should like the syntactic framework of the language that is isolated and exhibited by the grammar to be able to support semantic description, and we shall naturally rate more highly a theory of formal structure that leads to grammars that meet this requirement more fully. (p. 102)

Recall that two of the external conditions of adequacy to be imposed on grammars were their ability to handle ambiguity and paraphrase—semantic notions par excellence. Chomsky was completely explicit about the direct syntactic capturing of ambiguity:

> If the grammar of a language is to provide insight into the way the language is understood, it must be true, in particular, that if a sentence is ambiguous (understood in more than one way), then this sentence is provided with alternative analyses by the grammar. In other words, if a certain sentence S is ambiguous, we can test the adequacy of a given linguistic theory by asking whether or not the simplest grammar constructible in terms of this theory for the language in question automatically provides distinct ways of generating the sentence S. (p. 123)

This quotation shows clearly that Chomsky felt that analyses motivated on purely formal grounds would have the property of capturing semantic ambiguity directly. We see in later chapters of this volume how the discovery that many T-rules motivated in this way do not have this property was to lead to radically different analyses of the same phenomena.

2.3. NOAM CHOMSKY

The Chomskyan revolution in linguistics was very much a revolution from the inside. Chomsky was immersed from childhood in an environment where language and its scientific study were constant topics of discussion. He was born in Philadelphia on December 7, 1928, and through his father, William, a noted Hebrew philologist, he developed an interest in language structure. At the age of 10, he was reading the proofs of his father's *David Kimhi's Hebrew Grammar*. Chomsky attributes his early interest in explaining linguistic phenomena, as opposed to simply describing them, to his childhood exposure to historical linguistics. In a period when leading theorists tended to look upon the desire for explanation as a sort of infantile aberration,[6] historians of language like his father, either ignorant of or

---

[6]In the words of the post-Bloomfieldian Martin Joos, ''Children want explanations, and there is a child in each of us; descriptivism makes a virtue of not pampering that child'' (Joos 1958:96).

indifferent to the contemporary "scientific" wisdom in the field, clung to a nine-teenth-century desire to explain why a particular distribution of forms existed at a particular point in time.

As an undergraduate at the University of Pennsylvania, Chomsky's main interest was Middle East politics. In fact, he had considered leaving his studies entirely "to live on a Kibbutz and work for Arab-Jewish cooperation" (quoted in Mehta 1971:186). In an effort to discourage him from going to Palestine, his parents introduced him to Zellig Harris, who taught at Pennsylvania and shared Chomsky's views on Zionism—and was able to function as a productive scholar at the same time. The ploy worked. Harris immediately took the 18-year-old Chomsky under his wing and gave him the proofs of his *Methods in Structural Linguistics* to examine—before Chomsky had even taken his first linguistics class! As he later remarked, "That's how I learned linguistics, by proofreading Harris' book—which was fine for me, I really learned the field" (Sklar 1968:215).

At Harris's suggestion, Chomsky began to work on a grammar of Hebrew. The combination of his thorough understanding of historical process, the striking un-suitability of the Hebrew language to description in post-Bloomfieldian terms, and the intangible factor of his own genius made him realize almost immediately that a revealing account of that language in terms of a taxonomic inventory of elements was hopeless:

> I started right off, without even asking any questions, working within the framework of generative grammar, which seemed to be the only conceivable thing, namely, trying to find a system of rules which would enable you to characterize all of the sentence structures of the language. I very quickly discovered that if you wanted to do this properly you had to have a long sequence of ordered rules. I also noticed right off that the logical order corresponded to some extent to the historical order which I knew of. I found that if you gave it the right kind of logical order then you could explain a lot of phenomena which otherwise seemed very inexplicable. I worked on this for a couple of years in really total isolation. (quoted in Sklar 1968:214)

"Total isolation" is no exaggeration. While Chomsky's Hebrew work developed into his 1949 undergraduate thesis and his 1951 M.A. thesis, "Morphophonemics of Modern Hebrew," there is no evidence that Harris, who "didn't pay any atten-tion to what anybody else was doing in linguistics or in anything else" (Chomsky, quoted in Mehta 1971:187), took it at all seriously. With the exception of Henry Hoenigswald, few linguists were then even willing to call what Chomsky was doing "linguistics," a feeling which reinforced the lingering doubts in his own mind about the fruitfulness of his approach.

Thanks to the impression he made upon the philosopher Nelson Goodman, with whom he took philosophy courses as an M.A. student at Pennsylvania, Chomsky won a prestigious Junior Fellowship in the Society of Fellows at Harvard, where he worked from 1951 to 1955. Ironically, his project was to improve the techniques of structural linguistics; he even published one paper with this goal in mind (Chomsky 1953). But, little by little, his work in generative grammar became his central focus. Two individuals, the philosopher Yehoshua Bar-Hillel and the linguist Morris Hal-le, stand out above all others in their encouragement of Chomsky to pursue his ideas along these lines. It was Bar-Hillel who convinced him to put aside all hestitations

and postulate (as his intuitions had already told him was correct) something very much like the reconstructed historical forms at the abstract morphophonemic level. And it was Halle, whom Chomsky met in the fall of 1951, who, as a result of their constant discussions, was the most decisive factor in causing him to abandon any hope of a procedural approach to linguistic analysis. Here is Chomsky's own account of his moment of truth:

> By 1953, I came to the same conclusion [as Halle]: if the discovery procedures did not work, it was not because I had failed to formulate them correctly but because the entire approach was wrong. In retrospect I cannot understand why it took me so long to reach this conclusion—I remember exactly the moment when I finally felt convinced. On board ship in mid-Atlantic, aided by a bout of seasickness, on a rickety tub that was listing noticeably—it had been sunk by the Germans and was now making its first voyage after having been salvaged. It suddenly seemed that there was a good reason—the obvious reason—why several years of intense effort devoted to improving discovery procedures had come to naught, while the work I had been doing during the same period on generative grammars and explanatory theory, in almost complete isolation, seemed to be consistently yielding interesting results. (1979:131)

With post-Bloomfieldian structural linguistics now permanently in his past, Chomsky began writing *The Logical Structure of Linguistic Theory* (*LSLT*), his exposition of the goals, assumptions, and methodology of transformational generative grammar (one chapter of which earned him his Ph.D from Pennsylvania). A truly incredible work of the highest degree of creativity, *LSLT* completely shattered all contemporary conceptions of linguistic theory. This 900-page volume contains the initial proposals for the formalization and evaluation of grammars that would underlie all subsequent generative research. But the American linguistic community in 1955 was not impressed. *LSLT*'s rejection by MIT Press (the only publisher whom Chomsky felt might take the work seriously) came practically by return mail. Likewise, in the next two years his dissertation and an article on simplicity and explanation submitted to *Word* were turned down almost as rapidly. Since the only job offer he could muster at the end of the tenure of his fellowship was to teach Hebrew at Brandeis at a salary of $3500, he decided to stay at Harvard for another year.

But fortunately for Chomsky, Halle was teaching at MIT and was able to arrange for him to be hired in the Modern Language Department, with a joint appointment in the Research Laboratory of Electronics. (His responsibilities at first included teaching scientific French and German and some undergraduate linguistics, philosophy, and logic courses.) But he was able to find time to write up the notes to his introductory linguistics course, which Halle encouraged him to submit to Mouton Publishers (Mouton had just published Jakobson and Halle's *Fundamentals of Language*). By May of 1957, *Syntactic Structures* was off the presses.

As a result of Lees's review, Chomsky began to receive invitation after invitation to present his ideas, the most important of which was to the 1958 Third Texas Conference on Problems of Linguistic Analysis of English (see Chomsky 1962b). Here he scored his first important coup: he succeeded in winning over the prominent young structuralist Robert Stockwell, who soon became a vigorous campaigner for

the new model. While the other papers at this conference are not memorable and the content of Chomsky's is little more than an elaboration of some points in *Syntactic Structures,* the proceedings of the conference (Hill 1962) are nevertheless wonderful reading. The reason is that they faithfully transcribe the discussion sessions at the end of each paper. Here we can see linguistic history documented as nowhere else: Chomsky, the enfant terrible, taking on some of the giants of the field and making them look like rather confused students in a beginning linguistics course. The Chomskyan Revolution was now in full motion. I chart its progress in Section 2.5.

2.4. GENERATIVE PHONOLOGY

2.4.1. The Prague School

One structuralist school of language, the Prague School, had such an important influence on generative grammar that it seems appropriate to discuss it in this chapter. Broadly, the name describes the circle of scholars active in Prague and Vienna in the 1920s and 1930s. One leading member, Prince N. S. Trubetskoi, contributed *Grundzüge der Phonologie* (1939), the basic Prague School statement of phonology, the area in which it had the greatest impact on modern linguistic theory. Prague School phonology was brought to the United States in the 1940s and further elaborated by Trubetskoi's colleague Roman Jakobson.

The Prague School phonologists shared with their American counterparts the fundamental assumption of pre-Chomskyan structuralism that a linguistic description consists of an inventory of elements meeting the condition of biuniqueness. But in crucial respects, their theoretical outlook was diametrically opposed to that of the post-Bloomfieldians. Most importantly, they made it perfectly clear that their overall goal was explanation rather than taxonomy. Their requirement of biuniqueness, then, which guaranteed that a description would be a taxonomy, was regarded as an empirical hypothesis rather than (as it was to the Americans) an a priori assumption. For this reason they were not obsessed with developing the "correct" set of operational procedures by which an analysis might be obtained.[7] Furthermore, since they had no hesitation about imputing psychological reality to their linguistic descriptions, native-speaker judgments were not ruled out as admissible evidence. This enabled them to develop phonetic theory to a rather sophisticated degree. While the Bloomfieldians had always been suspicious of phonetics, given its essentially impressionistic basis, the Pragueans, not shrinking from impressionistic data, were able to make important cross-language generalizations about phonetic universals. Jakobson insightfully incorporated these results in the first major theoretical study

---

[7]On the other hand, the attraction of most of the leading Prague School members to phenomenology made them suspicious of formalism and dubious that the goals and methods of linguistic theory could be identified with those of the natural sciences.

of language acquisition, his remarkable *Kindersprache, Aphasie, und allgemeine Lautgesetze* (1941).

For Jakobson and other members of the Prague School, the phoneme was not simply a notational device but, rather, a complex phonological unit consisting of a set of distinctive features. This concept of the phoneme represented an important advance for several reasons. First, because it turned out that the phonemic systems of every language in the world are characterizable in terms of a small number of binary feature oppositions. Second, because features allow the formulation of generalizations impossible to state in other structuralist models. For example, the assimilation of nasals to following stops in English could be described by positing an abstract nasal ''archiphoneme'' before stops and a general rule which then filled in the redundant features. Third, features make possible the development of a mechanical evaluation procedure: since redundant features could be left unspecified in the basic feature matrix (which would be filled in by rules), the most highly valued analysis would be regarded as the one with the minimal number of feature specifications per phoneme.

A great amount of work took place in the 1950s to characterize precisely those features relevant for description of all human languages. The results were reported in two important books: Jakobson, Fant, and Halle's *Preliminaries to Speech Analysis* (1952) and Jakobson and Halle's *Fundamentals of Language* (1956). The universal inventory was reduced to 12 binary distinctive features, some defined in absolute acousitc terms (e.g., vocalic versus nonvocalic) and some in relative acoustic terms (e.g., grave versus acute).

From the above discussion it should be clear to anyone with the slightest familiarity with generative phonology how great a debt is owed to the Prague School phonologists. Roman Jakobson probably exerted a greater influence on transformational grammar than any other linguist. However, he himself was never to embrace without fundamental reservations the generativist view of linguistic theory. This was left to his student, Morris Halle, who began his collaboration with Chomsky in 1953. Together they developed the theory of generative phonology within a comprehensive theory of human language.

2.4.2. Morris Halle

Even before his collaboration with Chomsky began, Halle had built a reputation for himself in linguistics. The publication of *Preliminaries to Speech Analysis* (the research for which was carried out at the Acoustics Laboratory at MIT) made Halle a public figure in the field. Halle, who was born and educated in Latvia, emigrated to the United States in 1940. He studied engineering at the City College of New York before being drafted in 1943. After the war, he received a degree in linguistics from the University of Chicago. At the urging of Giuliano Bonfante (who was then at Chicago) he went to Columbia University to study with Roman Jakobson in 1948 and followed Jakobson to Harvard a year later. His Harvard Ph.D., which he

received in 1955, was awarded on the basis of his dissertation "The Russian Consonants: A Phonemic and Acoustical Study." In somewhat revised form, this later appeared as the second half of *The Sound Pattern of Russian* (1959).

Halle had worked on the MIT Research Laboratory of Electronics acoustics project while a student, and he was hired by that university's Modern Language Department (to teach German and Russian) in 1951. In addition to his scholarly activities, he was instrumental in initiating the Ph.D. program in linguistics at MIT, which he supervised from 1960 until he stepped down in 1977.

Halle combines in a rare fashion the qualities of productive scholar and organizer-administrator. While transformational generative grammar would no doubt have succeeded had he lacked one of these two attributes, it does not seem too farfetched to say that its history would have been very different had he lacked both.

2.4.3. Early Generative Phonology

Generative phonology, in essence, synthesized three contemporary trends in linguistics. First, it incorporated the unformalized insights about phonological processes that characterized the work of Edward Sapir and his students. Second, it drew from the post-Bloomfieldians the practice of explicit formalization of all rules. And, finally, it owed to the Prague School the overall explanatory goals of phonological theory as well as some specific insights (e.g., distinctive features).

Chomsky and Halle teamed with Fred Lukoff in 1956 to publish the first generative phonological analysis, "On Accent and Juncture in English." This paper, which proposed a retreatment of English suprasegmentals, hit the post-Bloomfieldians where they were weakest. In place of the four degrees of phonemic stress that previous treatments hypothesized, they were able to predict the full range of phonetic stress possibilities with only a simple phonemic accented–unaccented distinction. They achieved this economy by assuming a set of ordered rules sensitive to underlying junctures placed at certain morpheme boundaries. Since their analysis resulted in a nonbiunique relation between phonemics and phonetics and violated the prohibition against mixing levels, it was incompatible with post-Bloomfieldian methodology. But, they argued, four important benefits resulted from abandoning these methodological constraints: the constituent organization imposed to state the stress rules most simply coincided with that required for other levels of description; the binary phonemic feature of accent made special suprasegmental phonemes unnecessary; the simplicity and symmetry of the rules proposed contrasted markedly with the inelegant earlier account; and the rules predicted how native speakers could assign stress patterns to new utterances in a consistent and uniform manner.

The first major work of generative phonology was Halle's *The Sound Pattern of Russian* (1959). While his specific rules for Russian would be modified many times, the book is remembered primarily for its argument against the structuralist concept of the phoneme. This argument not only was regarded as the most compelling one for generative phonology at the time, but even reappeared in its broad

TABLE 2.1

Gloss	A	B	C
was he getting wet	{m'ok 1,i}	/m'ok 1,i/	[m'ok 1,i]
were he getting wet	{m'ok bi}	/m'og bi/	[m'og bɨ]
should one burn	{ž'eč 1,i}	/ž'eč 1,i/	[ž'eč 1,i]
were one to burn	{ž'eč bi}	/ž'eč bi/	[ž'eǯ bɨ]

structural outline 10 years later to be used against the level of syntactic deep structure (see Section 4.3.1). Hence, it is worth paraphrasing:

> Consider the phonetic representations (C in Table 2.1) and the morphophonemic representations (A in Table 2.1) of four Russian phrases. Since there are instances where the velar stops ([k] and [g]) are in contrast, the relationship between A and C violates biuniqueness. Structuralists would therefore have to set up an intermediate phonemic level (B) to insure a biunique relation holding between all levels of description. On the other hand, [č] and [ǯ] never contrast, and in structuralist grammar would have to be identical at phonemic level B. But consider the consequences of this. A grammar with level B has no choice but to break down the generalization that obstruents are voiced before voiced obstruents into two distinct statements: once as a morphophonemic (A to B) rule applying to {k}; once as an allophonic (B to C) rule applying to /č/. Only by abandoning biuniqueness, and with it level B, can we capture this generalization by one unitary rule statement.

In Chomsky and Halle (1960), the analysis of English stress was simplified still further. By incorporating syntactic categorical information into the stress rules, they were able to dispense with underlying phonemic accent entirely and, at the same time, eliminate the phonemic /i/ required in the 1956 paper. That year also saw the publication of Stockwell (1960), the first attempt to incorporate pitch into a generative description.

The major theoretical discussion of generative phonology prior to Chomsky and Halle's *The Sound Pattern of English* (1968) was Halle's "Phonology in Generative Grammar" (1962). Halle gave the first clear statement of the economy gained by formulating phonological rules in terms of distinctive features rather than in terms of indivisible phonemes. He pointed out that in the latter approach, despite the fact that Rule (2.4) represents a natural phonological process and (2.5) an unnatural one, the two rules would be equally complex:

(2.4)   /a/ → /æ/ in the env. — $\left\{ \begin{array}{c} /i/ \\ /e/ \\ /æ/ \end{array} \right\}$

(2.5)   /a/ → /æ/ in the env. — $\left\{ \begin{array}{c} /i/ \\ /p/ \\ /z/ \end{array} \right\}$

But under an analysis in which rules are stated in terms of features, Rule (2.4) would be vastly simpler to state than Rule (2.5)—a clear demonstration of how

feature theory can provide a definition of the intuitive notions "natural class" and "natural phonological process." He further argued (using an example based on Sanskrit vowel sandhi) that the ordering of rule statements would, at one and the same time, lead to both a minimization of feature specifications and an analysis that intuitively was the most insightful as well. While this was by no means the first example given of phonological rule ordering, it was the first concrete illustration of the theoretical interrelatedness of this concept with that of distinctive features and the simplicity criterion for evaluating alternative analyses.

Halle broke ground in three other important areas in this article. First, he explained how differences between dialects could be explained by hypothesizing that they contain the same set of rules applying in different orders. Second, he gave a broad overview of how generative phonology was suited to the description of language change. He suggested that rule addition characteristically takes place at the end of the grammar (or at the end of natural subdivisions in it). Hence, it is no accident that the synchronic order of rules characteristically mirrors their relative chronology, a point first observed by Bloomfield (1939b) in his study of Menomini. He went on to give what he claimed was an example of a case (from the history of English) where two phonemes that had merged later reappeared, the reemerging phonemes corresponding exactly to their historical antecendents. Since such a phenomenon would be utterly inexplicable under an account of phonology that assumed biuniqueness, Halle's example pointed to the need for an abstract level of representation not simply extractable from the superficial phonetic data.

Finally, Halle (1962) gave the first theoretical explanation of the diminished language learning ability of the adult:

> I propose to explain this as being due to deterioration or loss in the adult of the ability to construct optimal (simplest) grammars on the basis of a restricted corpus of examples. The language of the adult—and hence also the grammar that he has internalized—need not, however, remain static: it can and does, in fact, change. I conjecture that changes in later life are restricted to the addition of a few rules in the grammar and that the elimination of rules and hence a wholesale restructuring of his grammar is beyond the capabilities of the average adult. (p. 64)

While few of Halle's specific theoretical claims stand unaltered today, except in the most general terms, the importance of "Phonology in Generative Grammar" should not be underestimated. This article was the closest thing to a *"Syntactic Structures* of phonology"—the basic theoretical statement that would direct research in this area of linguistics for years.

2.5. WINNING THE REVOLUTION

2.5.1. Robert B. Lees

Any discussion of the revolutionary period of transformational generative grammar must begin with a portrait of Robert B. Lees. I have already noted the impact of his review of *Syntactic Structures*. But the debt that linguistic theory owes him far exceeds that one piece of writing. Lees's book *The Grammar of English Nomi-*

nalizations (1960) (see Section 3.2.1) was to linguistic analysis what *Syntactic Structures* was to linguistic theory. It meant that the opponents of the theory had the burden of responding to (and finding alternatives to) highly detailed analyses of many central syntactic phenomena in English. But most importantly of all, Lees was a campaigner. At every conference, at every forum, there was Lees—to champion the generative view of language in as articulate and methodical a manner as is humanly imaginable. Nobody who was around linguistics in the late 1950s and early 1960s can talk about that period without recalling Lees's colorful style and unyielding determination to win victories for the new theory, and without recalling how moved they were by his charismatic presence—whichever position in factional debate they held then or hold now.

Lees's undergraduate studies in chemical engineering were interrupted by World War II (in which he was trained as a meteorologist). The four postwar years in which he worked at the Argonne National Laboratories were influential to his future linguistic research. He was involved in a project whose goal was to develop a methodology for counting carbon isotopes—the same project that led to Willard Libby's discovery of carbon-14 dating. As a voracious reader in linguistics in his spare time, Lees was led to see in Morris Swadesh's observations about lexical loss in Salish a process which could be described by a familiar first-order rate-equation; as a result he worked out the mathematical equations involved in glottochronology (Lees 1953). While Lees soon saw the limitations of the method, the fame he gained through his work in glottochronology was indispensible in giving him the credibility he needed as "Chomsky's Huxley" several years later.

Disenchanted with chemistry, Lees entered the University of Chicago Linguistics Department in 1947; he received his M.A. in 1950. After editing a book on English for Turkish speakers and working at several other jobs, Lees in 1956 accepted Victor Yngve's invitation to come to MIT to work on his machine translation project. While the project held little interest for him (in fact, Yngve fired him in short order), it was there he began his collaboration with Chomsky, whose views he began to champion and develop creatively in his own right. *The Grammar of English Nominalizations* earned him his MIT Ph.D. in 1959 in the Department of Electrical Engineering (there was no Linguistics Department there at the time). After a brief stint at IBM, he accepted a position at the University of Illinois, where he built one of the leading linguistics programs in the United States. Since 1969, he has taught at Tel Aviv University, where he has also built a successful program.

2.5.2. The Ascendancy of Transformational Generative Grammar

The established leaders of American structuralism recognized from the outset that Chomsky's theory represented a profound challenge to contemporary ideas about how to carry on linguistic research. And they recognized as well that the essence of the challenge lay in Chomsky's ideas about how language might be studied scientifically. Charles Hockett, for example, in his presidential address to the Linguistic

Society of America in 1964 (published as Hockett 1965), went so far as to characterize the publication of *Syntactic Structures* as one of "only four major breakthroughs" in the history of modern linguistics (Hockett 1965:185). In Hockett's words:

> Between Sir William [Jones's] address and the present Thirty-Ninth Annual Meeting of the Linguistic Society of America there is a span of 178 years. Half of 178 is 89, a prime number. If we add that to 1786 [the date of Jones's address] we reach the year 1875, in which appeared Karl Verner's 'Eine Ausnahme der ersten Lautverschiebung'. Thereafter, two successive steps of 41 years each . . . bring us to the posthumous publication of Ferdinand de Saussure's *Cours de linguistique générale* and then to Noam Chomsky's *Syntactic Structures*.
>
> I have allowed myself this bit of numerology because I know none of you will take it seriously. But behind this persiflage there is a sober intent. Our fraternity has accomplished a great deal in the short span of 178 years; yet, in my opinion, there have been only four major breakthroughs. All else that we have done relates to these four in one way or another. (p. 185)

Hockett recognized that the major breakthrough of *Syntactic Structures* was its abandonment of empiricist constraints on theory formation and evaluation, which, as he noted, involved distinguishing between discovery and evaluation procedures and between practical description and formal theory and required setting the formal requirements that a theory must meet. Hockett referred the various components of nonempiricist theory collectively as the "accountability hypothesis" and wrote "that it is a breakthrough I am certain" (p. 196). Later in his address he confessed:

> I know how I would have reacted to [the abrasive style of the Junggrammatiker], because I know my reaction to the similar tone of Robert B. Lees's review, which appeared in 1957, of Chomsky's *Syntactic Structures*, and of the introductory remarks in his *Grammar of English Nominalizations*, published in 1960. We do not enjoy being told that we are fools. We can shrug off an imprecation from a religious fanatic, because it does not particularly worry us that every such nut is sure he holds the only key to salvation. But when a respected colleague holds our cherished opinions up to ridicule, there is always the sneaking suspicion that he may be right. (p. 187)

Similarly, while Martin Joos recognized the "structuralist" core of generative grammar, he also recognized that it differed from other structuralist approaches in a fundamental way. Hence, he identified Chomsky's theory as a "heresy within the neo-Saussurean tradition rather than as a competitor to it" (Joos 1961:17). Why was it heretical? Because it

> ignores . . . something which has been either taken for granted or circumvented for many years . . . this is the neo-Saussurean axiom which we may try to state in these words: 'Text signals its own structure.' From this tacit assumption there follows automatically the most troublesome rule of neo-Bloomfieldian methodology: the rule demanding 'separation of levels'. . . . But [the generativist] leaders are able to point out that NO OTHER SCIENCE HAS A PARALLEL RULE. (17–18, emphasis added)

In short, Joos recognized that the rejection of empiricist constraints on theory formation was at the heart of the Chomskyan movement and that Chomsky's thrust was to bring linguistics into accord with the natural sciences.

The post-Bloomfieldian establishment not only recognized the importance of Chomsky's theory, but concretely abetted it in a number of ways. For example, Archibald A. Hill provided Chomsky with an important forum at the Third Texas

Conference alluded to in Section 2.3. While many feel that Hill's primary intent in inviting Chomsky was to exorcise the demon of generative grammar, the theory nevertheless benefited immensely from the exposure the conference provided.

Bernard Bloch, arguably the most influential post-Bloomfieldian in the mid 1950s, went out of his way to help Chomsky. While Bloch never publicly endorsed the new theory, he did confide to at least two colleagues, "Chomsky really seems to be on the right track. If I were younger, I'd be on his bandwagon too."[8] Bloch's actions certainly bear out the attitude expressed in this quotation. As editor of *Language*, he unhesitatingly published the crucial Lees review of *Syntactic Structures*. And several years later, in an unprecedented action, he inserted an editorial comment in a published review in that journal. The review was Paul Postal's (1966) uncompromising attack on R. M. W. Dixon's *Linguistic Science and Logic* (1963a), a book filled with polemics against generative grammar. Bloch's comment added insult to injury by bolstering Postal's case against Dixon.[9] Bloch also was responsible for placing *The Morphophonemics of Modern Hebrew* and *The Logical Structure of Linguistic Theory* in the Yale University linguistics library as early as 1958.

To digress for a moment: Chomsky's ease at gaining a hearing has been proferred as an argument that instead of a genuine "Chomskyan revolution," the field saw only a power grab by him and his supporters in the mid 1960s. Stephen Murray (1980), for example, questions whether Bloch and other prominent structuralists would have accorded Chomsky easy access to the public organs of the field if they had seen his ideas as an intellectual threat to post-Bloomfieldianism or had regarded him as likely to form a sociological pole of attraction. Murray's case is built implicitly on the idea that no rational individual would willingly help to undermine his or her own dominant position; hence the field's leaders (being rational) must have viewed Chomsky's ideas as quite congenial to their own.

It is difficult to ascertain whether reasoning such as Murray's has its roots in anything other than a thoroughly dismal view of human nature, but I suspect that it might be attributable to a misreading of those passages in Kuhn (1970) that take on the question of the transition of power from one paradigm to the next. In his discussion of this question, Kuhn points out that it is very often the case that older workers in a scientific field do not accept revolutionary developments, and to support this idea he cites examples of the nonadoption of the theories of Newton, Priestly, Kelvin, and others by the establishment in their respective fields. But Kuhn never implies that the Old Guard attempt to suppress revolutionary new ideas, nor do they even fail to encourage such ideas (however much they may disagree with them). It is easy to fall prey to the romantic (and pessimistic) idea that in order to win a voice, a young innovator in a field must struggle heroically against the obstructionist establishment. But such a scenario does not correspond to reality,

---

[8]Personal communication, Donald Foss and Sol Saporta.

[9]Dixon was later to become a productive contributor to generative grammar (see, for example, Dixon 1970, 1972, 1977).

either within linguistics or within science in general. And Kuhn's point about establishment figures not adopting new theories themselves is borne out completely by the Chomskyan revolution. With the exception of Sol Saporta and Robert Stockwell, both of whom were quite young at the time, and only a very few others, the leading structural linguists of the late 1950s did not become generative grammarians.

Interestingly, most of the very earliest published attacks on the theory were by non-Americans (see Reichling 1961; Uhlenbeck 1963; Dixon 1963a, 1963b, 1964; Winter 1965; and the responses in Chomsky 1966b). This fact should not seem surprising: while Chomsky shared with the post-Bloomfieldians the commitment to building a formal scientific theory of language (albeit their conceptions of "science" differed drastically), many Europeans have traditionally taken the position that language, as a human institution, is not amenable to scientific study. Hence they saw in generative grammar a further move away from their humanistic concerns.

By the mid 1960s, however, the post-Bloomfieldians, as well as the Europeans, had joined in the polemical assault against generative grammar. There are several reasons why their hostile reaction to the new theory was delayed by half a decade. Most importantly, they were appalled by the openly rationalist underlying assumptions of Chomsky's *Aspects of the Theory of Syntax*, published in 1965 but presented in public lectures for several years prior to that date. Chapter One of this book, which points to Cartesian rationalism as an intellectual antecedent of generative grammar, so repelled Hockett that he felt compelled to revise the preface of his book *Language, Mathematics, and Linguistics* for the express purpose of condemning it. Hockett (1966) wrote: "This chapter is a *reductio ad incredible* of the mistakes we have been making in linguistics for the last thirty or forty years; my study of it, after the present essay was completed, was responsible for the radical change of view reported in this Preface" (p. 8). By 1971, Hockett had come to regard studies within the framework of generative grammar as "as worthless as horoscopes" (quoted in Mehta 1971:218).

Another reason that the post-Bloomfieldian rebuttal was delayed comes from the fact that the generativists turned their full attention to phonology only several years after the publication of *Syntactic Structures*. Since the post-Bloomfieldians never had much to say about syntax, they felt no immediate threat from a new approach to that aspect of grammar. But they reacted with horror to Halle's and Chomsky's assaults on their approach to phonology. Indeed, the debate over generative phonology continued to rage long after transformational syntax had won a significant degree of acceptance (see Chomsky 1964a; Chomsky and Halle's 1965 reply to Householder; and Postal's 1968 reply to Lamb). As Archibald Hill put it, "I could stay with the transformationalists pretty well, until they attacked my darling, the phoneme" (Hill 1980:75).

To a certain extent, the generativist leaders provoked the attack from the post-Bloomfieldians in the mid 1960s. While the latter frequently showed signs of genuine willingness to compromise, Chomsky and Halle felt that intellectual princi-

ples were at stake—principles that admitted no compromise. As a consequence, the post-Bloomfieldians had become so antagonized by that time that they felt they had no alternative but to launch a frontal assault on the new theory. Their antagonism was increased by the behavior of the partisans of generative grammar, particularly the younger ones. Halle and Chomsky had the policy of encouraging their students to integrate themselves into American linguistic life from the moment they entered MIT. It was not unusual in the early 1960s to see even first-year graduate students from that institution presenting and commenting on papers at meetings. But their combative spirit surely got out of hand at times, as even undergraduate advocates of the theory embarrassed their teachers by ruthlessly lighting into linguists old enough to be their grandparents.

A final explanation for the delay in the post-Bloomfieldian counterattack is impossible to document but has a good deal of plausibility nevertheless. By the mid 1960s, many post-Bloomfieldians must have felt that their own power in the field, both intellectual and political, was gradually slipping away. One can assume that their marshalling of their forces at that time was in part a response to what must have seemed to them to be a very alarming development.

One of the earliest criticisms of generative grammar, and one that has survived to the present day (see Hagège 1976; Hall 1977), is the supposed English-language orientation of the theoretical work within this model. American structuralists, whose roots are in the Boas tradition that gave priority to the description of indigenous languages, and European scholars have united to condemn what many have implied is at best poor linguistics, at worst a reflection of American chauvinism and arrogance. Yet even the earliest work was not as English-centered as many believe. For example, of the six faculty members in the MIT Linguistics Department in the late 1960s, four were known primarily for their work in languages other than English: Kenneth Hale for Amerindian and Australian; G. Hubert Matthews for Amerindian; Paul Kiparsky for general Indo-European; and Morris Halle for Russian. In addition, it will be recalled that Chomsky wrote a partial generative grammar of Hebrew before attacking English. And of the 28 doctoral dissertations written in linguistics at MIT in the 1960s, 17 (or 61%) dealt primarily with languages other than English, including those by Stephen Anderson (West Scandinavian), George Bedell (Japanese), Thomas Bever (Menomini), James Fidelholtz (Micmac), James Foley (Spanish), James Harris (Spanish), Richard Kayne (French), Paul Kiparsky (various languages), Sige-Yuki Kuroda (Japanese), Theodore Lightner (Russian), James McCawley (Japanese), Anthony Naro (Portuguese), David Perlmutter (various languages), Sanford Schane (French), Richard Stanley (Navajo), Nancy Woo (various languages), and Arnold Zwicky (Sanskrit).

It goes without saying, of course, that the majority of published syntactic analyses have dealt with English. This is an inevitable consequence of the American origins of the theory and the value placed on native-speaker judgments as data. But as more and more non-English speakers have adopted transformational generative grammar, the percentage of work involving English has steadily declined, and by

now generativist studies have appeared on literally hundreds of languages. It might also be pointed out that the first concerted attempt to train American Indians as professional linguists was undertaken at MIT (under the impetus of Kenneth Hale), where in recent years Navajo and Hopi speakers have received Ph.D's.

By the middle years of the 1960s, commentators were already speaking of Chomsky's revolutionary effect on the field (see, for example, Bach 1965:111–112; Levin 1965:92; Thorne 1965:74). How was this feat accomplished less than a decade after the publication of *Syntactic Structures?* While the early encouragement of the post-Bloomfieldians was an important factor, there were many others as well. From the beginning, Chomsky and Halle were able to attract some of the brightest young scholars in the United States to the new way of doing linguistics. Not only Lees, but also G. Hubert Matthews, Fred Lukoff, Edward Klima, Keith Percival, and John Viertel were part of Victor Yngve's machine translation project when they came into contact with generative grammar, a fact which exacerbated the theoretical differences which already existed between Chomsky and Yngve (see Yngve 1960, 1961; Miller and Chomsky 1963). Jerrold Katz and Jerry Fodor, philosophy students at Princeton, were won over and hired by MIT around 1960, as were Paul Postal and Jay Keyser from Yale. As Searle (1972) put it, "Chomsky did not convince the established leaders of the field but he did something more important, he convinced their graduate students" (p. 17). Many of the earliest transformationalists, such as Emmon Bach, Carlota Smith, Charles Fillmore, and Kenneth Hale, were students or recent Ph.D's who adopted the new theory despite the indifference or open hostility of their teachers.

The quality of the first two classes to enter MIT (in 1961 and 1962) was instrumental to the early success that the theory achieved. Not one individual who has failed to contribute to linguistic theory is found in its list, which includes Thomas Bever, James Foley, Bruce Fraser, Jeffrey Gruber, Paul Kiparsky, S.-Y. Kuroda, Terence Langendoen, Theodore Lightner, James McCawley, Barbara Hall Partee, Peter Rosenbaum, Sanford Schane, and Arnold Zwicky. None found difficulty in finding jobs upon graduation; during the 1960s, Illinois, California-San Diego, UCLA, Texas, Washington, and Ohio State joined MIT as universities in which transformational grammar predominated.

This decade was also a decade of rebellion, and the intellectual and political ferment going on in American universities at that time provided an ideal atmosphere for the intellectual movement sweeping linguistics, which was bent on overthrowing the rigid dogmas of American structuralism. Just as students began en masse to question the "common sense" political assumptions of their upbringing, which they felt were rationalizing an imperialist foreign policy and oppressive domestic policy by the American government, they began to question the "common sense" pseudoscientific assumptions of empiricism in linguistics. The appeal of nonobvious explanatory ideas in linguistics at this time was a reflection of the openness of students to such ideas in politics as well.

Far from substituting one dogma for another, as Hagège (1976) would have it, the

transformationalists encouraged—in fact, rewarded—students for questioning every assumption of linguistics, including those of transformational generative grammar itself. Paul Newman (1978) has.stressed:

> While there was a general consensus that "the other guys" were wrong and that the basic lin-guistic/philosophical tenets of generative grammar were essentially right, there was a singular ab-sence of dogma or rigidity. Everyone was encouraged to test the theory on a new language, to explore linguistic areas not yet treated, and to experiment with different kinds of formal devices—always with the freedom to modify or reject the then-standing TG theory as necessary. Of great importance in understanding the growth and spread of generative grammar is the fact that this freedom extended to students who, in key places, were permitted to follow their own lines of research, challenge the views of their teachers (generativist or not), take part in the continual discussion and debate, and otherwise participate actively in the creation and dissemination of this new theory. In the final analysis, generative grammar was a creative, liberating movement, which freed linguistics as a discipline and as a profession from the straitjacket of the post-Bloomfieldian period. Whatever the other factors involved, a great part of its success must be ascribed to this. (pp. 928–929)

The missionary zeal with which "the other guys" were attacked may have led some linguists, along with Wallace Chafe (1970), to be "repelled by the arrogance with which [the generativists'] ideas were propounded" (p. 2), but overall the effect was positive. Seeing the leaders of the field constantly on the defensive at every profes-sional meeting helped recruit younger linguists far more successfully and rapidly than would have been the case if the debate had been confined to the journals. Lees and Postal, in particular, became legends as a result of their uncompromising attacks on every post-Bloomfieldian paper at every meeting.

Postal's fame derived in large part from his book *Constituent Structure*, pub-lished in 1964. He attempted to show that each of the models of grammatical description in competition with the transformational generative model was equiv-alent in weak generative power to phrase structure grammar and was therefore inadequate as a model of human language. This involved reinterpreting the goals of the formulators of these models as being identical to those of the transforma-tionalists, opening Postal to charges of gross distortion and rewriting history (for interesting commentary on this point, see Thorne 1965). Needless to say, Postal's approach had the effect of strengthening the resolution of established non-Chomskyan linguists to resist the new model. But their students were profoundly impressed; no single publication was more instrumental in drawing students into the transformationalist camp.

There were other factors that contributed to the theory's rapid success. First, the field in the late 1950s was very small. For example, the Linguistic Society of America in 1957 had only about 1300 members in the entire world, the vast majority of whom would not have identified themselves as linguists. Simultaneous sessions were not instituted at its meetings until 1968. This meant that a new idea could be disseminated rapidly to the entire profession.

Second, by sheer coincidence, the Ninth International Congress of Linguists was held in Cambridge, Massachusetts in 1962, with Halle and William Locke (who was Chairman of the Modern Languages Department at MIT) on the Local Arrange-ments Committee. After Zellig Harris turned down his invitation to present one of

the five major papers at the plenary session (the others were by Kurylowicz, Ben-veniste, Martinet, and Andreyev), there was no trouble in replacing him with Chomsky. Chomsky's paper, "The Logical Basis of Linguistic Theory" (1964b), thus reached an international audience, giving him the appearance of being the principal spokesperson for linguistics in the United States. There was no question in the mind of anybody at MIT that transformational generative grammar would prove a lasting success after that point.

Third, transformational grammar was blessed from the beginning with extraor-dinarily gifted teachers, writers, and explicators:

> [A] number of the early generativists were extremely good teachers. The outsider—outraged by the belligerent polemics of generative lectures and writings—could not know that, in the classroom, a Halle at MIT or a Stockwell at UCLA functioned as a sympathetic and dedicated teacher, prepared to spend long hours explaining the intricacies and nuances of the new model. Students were attracted to generative grammar because, among other reasons, their teachers made it intellectually challenging and exciting. (Newman 1978:928)

Two pieces of pedagogical writing greatly advanced the theory in this period: Emmon Bach's *An Introduction to Transformational Grammars* (1964a), which made it accessible and interpretable to beginning students, and Paul Postal's "Un-derlying and Superficial Linguistic Structure," published in the *Harvard Educa-tional Review* in 1964, which acquainted educators and psychologists with its basic goals.

And finally, there was enough money available in the late 1960s in America for university expansion that young transformationalists did not have to contend with Old Guard-dominated departments before or after finding employment. It did not matter that Hockett and Hall were at Cornell, Trager and Smith at Buffalo, or Harris and Hiż at Pennsylvania. New departments could always be founded to serve as academic bases for generative grammar from the very beginning. MIT was particu-larly favored in this respect. Chomsky (1979) has commented:

> We were able to develop our program at MIT because, in a sense, MIT was outside the American university system. There were no large departments of humanities or the related social sciences at MIT. Consequently, we could build up a linguistics department without coming up against problems of rivalry and academic bureaucracy. Here we were really part of the Research Laboratory of Electronics. That permitted us to develop a program very different from any other and quite indepen-dent. (p. 134)

The affiliation with the Research Laboratory of Electronics arose as a result of the classification of linguistics at MIT as a "communication science," thus placing it under the purview of the laboratory. This affiliation guaranteed that vast sums of money (largely military in origin) would trickle down into the department, enabling the kind of support for a linguistics program that no other university could hope to match.[10]

---

[10]Newmeyer and Emonds (1971) and Newmeyer (in press) discuss at length the funding of linguistic research in the United States. The point is made that while the source of funding may be irrelevant to the ultimate correctness of a theory, it is by no means irrelevant to a (partial) explanation of one's accep-tance. It is tempting to speculate on the speed with which generative grammar would have won *general*

TABLE 2.2
Linguistic Society of American Membership, 1950–1984

December	Active LSA membership	December	Active LSA membership
1950	829	1968	4166
1951	822	1969	4231
1952	914	1970	4383
1953	978	1971	4723
1954	1022	1972	4263
1955	1090	1973	4258
1956	1178	1974	4148
1957	1354	1975	4279
1958	1501	1976	4112
1959	1633	1977	4108
1960	1768	1978	4258
1961	1951	1979	4268
1962	2180	1980	4303
1963	2602	1981	4267
1964	2918	1982	4248
1965	3263	1983	4222
1966	3495	1984	4168
1967	3814		

Source: LSA Bulletins.

2.6. THE GROWTH OF THE FIELD

The period of the Chomskyan Revolution was one of unprecedented growth in the field of linguistics in the United States. Measured by every imaginable statistic, the discipline grew by enormous proportions throughout the 1960s. Tables 2.2 through 2.6 provide documentation of this fact: LSA membership grew (Table 2.2); the number of departments and programs increased (Table 2.3); more institutions offered degrees (Table 2.4); enrollments increased (Table 2.5); and more degrees were conferred (Table 2.6).[11]

Table 2.2 shows that since around 1971 growth of LSA membership has leveled off and has actually declined. This has gone hand-in-hand with an employment picture aptly described by Levy, Carroll, and Hood (1976:14) as "bleak." It is my impression that reduced funding to higher education rather than disenchantment with linguistics is primarily responsible for this, and that the field has suffered less than others oriented toward basic scholarly research. By way of confirmation, it is worth pointing out that between 1970 and 1975, while the number of first-year

---

acceptance had Chomsky and Halle's students had to contend with today's more austere conditions, in which not just military, but most other sources of funding, have been curtailed, and new positions are a rarity.

[11]Tables 2.3–2.6 and 2.8–2.12 report United States' statistics only.

TABLE 2.3
Organization of Linguistics Departments and Programs, 1963–1984

Organizational arrangement	1963	1966	1969–1970	1971–1972	1984
Department of linguistics	13	23	31	42	51
Department of linguistics and languages (or other subject)	4	8	10	12	14
Interdepartmental program or committee	14	25	52	78	73
Linguistics courses offered in other departments	43	29	30	39	n.a.
Languages	(7)	(4)	(4)	(2)	
English	(14)	(14)	(17)	(24)	
Anthropology	(4)	(7)	(5)	(4)	
Other	(18)	(4)	(4)	(9)	
Interdepartmental courses only	3	1	11	2	n.a.
Total	77	86	134	173	n.a.

Source: Levy *et al.* (1976:114) and *Directory of Programs in Linguistics in the United States and Canada.*

graduate students in physics declined by 41%, in English by 35%, and in history by 31%, linguistics actually saw an increase of 49%.

Of course it is well known that the 1960s saw the expansion of almost every area of American higher education. But the growth rate of linguistics was considerably above the average, suggesting that it is to a large extent the appeal of transformational generative grammar rather than economic growth alone to which this expansion must be attributed. For example, in 1956–1957, 16 doctorates were awarded in linguistics out of a total of 8752 in all fields in American universities—that is .18%. But by the years 1972–1973, the percentage had almost tripled to .51% (177 out of 34,790).

TABLE 2.4
Number of Institutions Offering Each Linguistics Degree, 1963–1984

Degree offered	1963	1966	1969–1970	1971–1972	1974–1975	1984
Ph.D. in linguistics	25	29	39	45	45	47
Ph.D.: linguistics concentration	9	16	17	13	19	28
MA/MS in linguistics	26	38	49	66	70	86
MA/MS: linguistics concentration	9	27	34	37	33	36
BA/BS in linguistics	16	22	40	49	66	106
BA/BS: linguistics concentration	4	16	27	35	46	57

Source: Levy *et al.* (1976:116); *Guide to Programs in Linguistics: 1974–1975; Directory of Programs in Linguistics in the United States and Canada.*

TABLE 2.5
Enrollment for Advanced Degrees in Linguistics,
1960–1975

Year	Enrollment	Increase	Percentage increase
1960	407		
1961	558	151	37.1
1962	739	181	32.4
1963	882	143	19.4
1964	1083	201	22.8
1965	1298	215	19.9
1966	1482	184	14.2
1967	1567	85	5.7
1968	1740	173	11.0
1969	1846	106	6.1
1970	1884	38	2.1
1971	2043	159	8.4
1972	2220	177	8.7
1973	2294	74	3.3
1974	2316	22	1.0
1975	2597	281	12.1

Source: Levy et al. (1976:119); U.S. Department
of Health, Education, and Welfare, Enrollment for
Advanced Degrees.

It is much more difficult of course to document the growth of transformational generative grammar within the field of linguistics. Many articles and presented papers reflect diverse (and sometimes contradictory) influences and assumptions, while individuals and departments often resist such categorizations as "transformationalist" or "stratificationalist." Table 2.7 represents my estimation of the percentage of papers at winter LSA meetings from 1961–1966 that either presuppose or defend transformational generative grammar. After about 1966 such attempts at quantification become quite impossible, given the questioning of certain theoretical fundamentals by many generativists (see Chapters 4 and 5) and the concomitant adoption by nongenerativists of many of the assumptions of the dominant theory. Yet even so, William Bright, editor of *Language,* could remark in the March 1975 *LSA Bulletin:* "It is clear that the overwhelming majority of papers submitted [to *Language*], and of those published, take for granted certain principles of generative grammar" (p. 12).

It is an unfortunate fact that women and racial minorities have not fared much better in linguistics (whether pre- or post-generativist) than in other disciplines. As Table 2.8 illustrates, female students in linguistics have consistently tended to drop out of the field at a greater rate than male students. Linguistics B.A.'s have always gone to a higher percentage of women than M.A.'s, and M.A.'s to a higher

TABLE 2.6
Degrees Conferred in Linguistics, 1955–1981, by
Level

Year	Bachelor's	Master's	Ph.D
1955–1956	38	41	18
1956–1957	25	31	16
1957–1958	20	73	30
1958–1959	31	72	21
1959–1960	57	70	26
1960–1961	41	90	31
1961–1962	64	105	33
1962–1963	54	103	38
1963–1964	57	114	48
1964–1965	67	173	60
1965–1966	113	229	84
1966–1967	132	232	70
1967–1968	126	340	97
1968–1969	192	343	90
1969–1970	220	338	109
1970–1971	254	352	150
1971–1972	296	373	139
1972–1973	443	452	177
1973–1974	431	455	145
1974–1975	434	506	166
1975–1976	534	523	151
1976–1977	564	552	174
1977–1978	596	522	159
1978–1979	585	502	143
1979–1980	552	521	162
1980–1981	551	498	166

Source: U.S. Department of Health, Education,
and Welfare, *Earned Degrees Conferred.*

percentage than Ph.D.'s, suggesting a channeling of women out of the field. How-
ever, this situation seems to be improving faster in linguistics than elsewhere. At the
Ph.D. level, women are closing the gap on men more rapidly in linguistics than in
academia as a whole.

As far as academic hiring is concerned, conditions have been improving for
women over the past decade, to the point where they are being hired roughly in the
proportion that they are receiving Ph.D.'s.[12] Nevertheless, as Table 2.9 illustrates,
in 1982 women were still far outnumbered by men in linguistics positions at Ameri-
can universities and were heavily concentrated in the lower (nontenured) rungs of

[12]In a study commissioned by the Linguistic Society of America, Price (1983) finds that women are
more poorly represented at the 12 leading linguistics departments in the United States, as ranked in a
survey carried out by the National Research Council (Jones, Lindzey, and Coggeshell 1982), than they
are at other departments.

TABLE 2.7
Papers at Winter LSA Meetings Presupposing
or Defending Transformational Generative
Grammar, 1961–1966

Year	Number of papers	Number of generativist papers	Percentage of generativist papers
1961	31	4	13
1962	33	2	6
1963	45	8	18
1964	52	16	31
1965	37	14	38
1966	40	22	55

TABLE 2.8
Percentage of Degrees Awarded to Women, 1955–1981, by Level

Year	Linguistics			Ph.D.'s awarded to women, all fields
	Bachelor's	Master's	Ph.D.	
1955–1956	45	39	6	10
1956–1957	56	55	6	11
1957–1958	60	38	27	11
1958–1959	55	43	33	11
1959–1960	28	30	12	11
1960–1961	44	38	23	11
1961–1962	36	35	19	11
1962–1963	35	45	26	11
1963–1964	35	39	21	11
1964–1965	45	36	15	11
1965–1966	43	39	33	12
1966–1967	56	39	30	12
1967–1968	62	40	21	13
1968–1969	61	46	24	13
1969–1970	62	52	21	13
1970–1971	67	51	25	14
1971–1972	63	53	24	16
1972–1973	65	50	29	18
1973–1974	73	55	42	19
1974–1975	70	55	36	21
1975–1976	69	60	48	23
1976–1977	71	59	36	24
1977–1978	68	60	39	26
1978–1979	70	59	48	28
1979–1980	76	60	43	30
1980–1981	74	62	41	31

Source: U.S. Department of Health, Education, and Welfare, *Earned Degrees Conferred.*

TABLE 2.9
Composition by Sex and Rank of Linguistics Faculties, 1964–1982

Year	Number of programs surveyed	Below assistant professor			Assistant professor			Associate professor			Full professor		
		Male	Female	Percentage female	Male	Female	Percentage female	Male	Female	Percentage female	Male	Female	Percentage female
1964–1965	27	36	5	12	89	6	6	59	5	8	130	4	3
1974–1975	45	28	23	45	116	43	27	103	22	18	215	18	8
1978–1979	40	7	6	46	64	49	43	106	24	18	196	16	8
1982	123	26	27	51	143	103	42	270	73	21	356	43	11

Source: 1964–1965: *University Resources in the United States for Linguistics and Teacher Training in English as a Foreign Language: 1965; 1974–1975: Guide to Programs in Linguistics: 1974–1975; 1978–1979: LSA Bulletins. Nos. 80 and 94.*

TABLE 2.10
Percentage of Unemployment and Under-
employment (Combined) in Linguistics,
by Sex, 1973

Degree level	Men	Women
Ph.D.	5.5	16.4
Ph.D. candidate	8.0	31.3
Master's	30.4	21.1
Overall	7.8	19.2

Source: Levy *et al.* (1976:249).

the academic ladder. As tenure is becoming increasingly difficult to obtain, there is a real danger that the gains that women have made will be reversed in the coming years. A 1973 survey (see Table 2.10) indicated as well that unemployment and underemployment were considerably more acute among women in linguistics than among men. I suspect (but can provide no documentation) that things were worse in the mid 1980s.

The most overt forms of discrimination are, for obvious reasons, impossible to document. Perhaps the best example of antifemale (and anti-Asian) bias among linguists, and one that seemingly disallows any alternative conclusions, is reflected in an admission in the March 1974 *LSA Bulletin* that "the number of females and Orientals participating in the [LSA] meetings increased significantly when the abstracts were read masked, and they have been so read ever since" (Friedrich 1974:15). By the same token, the first year that Chicago Linguistic Society abstracts were read anonymously, the percentage of women presenting papers rose from 25 to 34.

A 1972 survey (Table 2.11) shows an insignificant number of minority-group faculty members in linguistics. While I know of no more recent tabulations of

TABLE 2.11
Minority Group Members as Percentage of Linguistics Faculty, by Rank, 1972

Minority group	Rank					
	Instructor/ lecturer	Assistant professor	Associate professor	Full professor	Total	Total number
Black	—	2.3%	1.5%	.5%	1.4%	10
Spanish speaking	4.6%	1.5	4.4	2.7	2.9	21
Asian	2.3	5.0	4.4	3.1	4.1	30
American Indian	—	—	.5	—	.1	1
Total	7.0	8.9	10.8	6.3	8.5	
Total number	3	23	22	14		62

Source: Levy *et al.* (1976:229).

TABLE 2.12
Ph.D.'s in Linguistics Awarded to Minority Group Members 1974–1983

	Total number	Blacks	Asian-Americans	Hispanics	American Indians
1974	145	1	2	4	2
1975	166	4	0	0	1
1976	151	2	1	0	0
1977	190	4	7	4	0
1978	175	3	4	3	2
1979	155	2	9	6	1
1980	182	9	6	6	1
1981	176	2	5	3	0
1982	191	3	8	5	0
1983	164	4	5	3	1

Source: National Research Council, Commission on Human Resources, *Summary Report, Doctorate Recipients from United States Universities.*

minority hiring, the percentage of minorities receiving Ph.D.'s in linguistics continues to be tiny (see Table 2.12). It seems to be the case that most black linguists in the United States are involved in work that might be considered "race-related": pidgins and creoles, African languages, and Black English.

2.7. THE EARLY IMPACT OF GENERATIVE GRAMMAR ON OTHER FIELDS[13]

The great appeal of *Syntactic Structures* cannot be explained exclusively by the fact that it presented a convincing theory of grammatical description. After all, the correct form of the theory of grammar is a topic of little interest to the nonspecialist. No, the theory presented in that book captured the imagination of scholars and pedagogues in numerous fields because it seemed likely to promote solutions to long-standing problems in every area in which language plays a role.

Chomsky himself did not bring up the question of the psychological implications of transformational generative grammar in either *LSLT* or *Syntactic Structures;* as he wrote later, it would have been "too audacious" for him to have done so (Chomsky 1975a:35). But Lees in his review did not shrink from this. He closed the review with a frontal attack on inductivist learning theory, arguing that there could be no alternative but to conclude that the grammar the linguist constructed was "in the head" of the speaker. But if that were the case, then how could these highly abstract principles possibly be learned inductively? "It would seem," he wrote, "that our notions of human learning are due for some considerable sophistication" (1957:408).

[13]For a more detailed discussion of much of the material in this section, see Newmeyer (1983).

It was Chomsky's 1959 review of B. F. Skinner's *Verbal Behavior* that drove home the fact that his theory of language was more than a neat manipulation of arcane symbols—it was a psychological model of an aspect of human knowledge. Chomsky's review represents, even after the passage of 20 years, the basic refutation of behaviorist psychology. The review takes in turn each basic construct of behaviorism and demonstrates that either it leads to false predictions or is simply devoid of content:

> a critical account of his book must show that . . . with a literal reading (where the terms of the descriptive system have something like the technical meanings given in Skinner's definitions) the book covers almost no aspect of linguistic behavior, and that with a metaphoric reading, it is no more scientific than the traditional approaches to this subject matter, and rarely as clear and careful. (Chomsky 1959b:31)

How then is verbal behavior to be explained? While its complexities defy any simplistic treatment, Chomsky wrote that

> the actual observed ability of a speaker to distinguish sentences from nonsentences, detect ambiguities, etc., apparently forces us to the conclusion that this grammar is of an extremely complex and abstract character, and that the young child has succeeded in carrying out what from the formal point of view, at least, seems to be a remarkable type of theory construction. (p. 57)

Chomsky went on to argue that this ability indicates that rather than being born "blank slates," children have a genetic predisposition to structure the acquisition of linguistic knowledge in a highly specific way: "The fact that all normal children acquire essentially comparable grammars of great complexity with remarkable rapidity suggests that human beings are somehow specially designed to do this, with data-handling or 'hypothesis-formulating' ability of unknown character and complexity" (p. 57).

Since Chomsky's review was published in a linguistics journal, its immediate impact on the field of psychology was minor. However, it did attract the attention of George A. Miller, Eugene Galanter, and Karl Pribram, three researchers at the forefront of the young discipline of cognitive psychology, who immediately realized the relevance of Chomsky's work to their interests. Miller, Galanter, and Pribram made extensive reference to generative grammar in their ensuing book *Plans and the Structure of Behavior* (1960). They saw in Chomsky's approach to syntax a model example of their claim that behavior must be organized simultaneously at several levels of complexity and require a complex planning device to coordinate the interplay between the various levels. As a result of their book (and the Skinner review, which by the mid 1960s had become well known among psychologists), Chomsky had come to be regarded as a leading figure in American psychology. As Judith Greene put it: "Chomsky's theory of generative transformational grammar was the first to force psychologists to reconsider their whole approach to the study of language behavior, and so heralded the psycholinguistic 'revolution'" (1972:15).[14]

[14]Maclay (1973) is an interesting discussion of the attitudes of psycholinguists before, during, and after the Chomskyan revolution.

Language teachers as well found transformational generative grammar to be relevant to their concerns. Disillusioned with behaviorist-inspired teaching methods like the audiolingual method and programmed instruction, many welcomed Chomsky's theory, whose emphasis on the creative aspect of language and its freedom from stimulus control seemed to encourage a more active role for the learner. By 1965, many agreed with Owen Thomas that "transformational grammar has significant application to the teaching of all languages, including English, at all grade levels and to both native and nonnative speakers" (1965:1). In the late 1960s, while generativist concepts had only rarely filtered down into the classroom itself, the journals of applied linguistics routinely discussed the application of the theory for some pedagogical purpose (for an historical overview of this period of second language research, see Newmeyer and Weinberger, forthcoming).

Finally, Chomsky's early work had an impact on philosophy, particularly the philosophy of science, even before the publication of his *Cartesian Linguistics* in 1966. While the success of generative grammar benefited from the retreat of empiricist philosophy, it helped contribute to that retreat as well. Indeed, Israel Scheffler's book *The Anatomy of Inquiry* (1963), a classic in the philosophy of science, cited Chomsky's results in *Syntactic Structures* to bolster his case for a nonempiricist analysis of scientific investigation. He pointed out that since Chomsky demonstrated the need to define such theoretical notions as "noun" and "morpheme" independently of particular languages, so philosophers should concern themselves with the general nature of scientific laws, rather than take an atomistic empiricist approach.