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Language, Vol. 62, No. 1. (Mar., 1986), pp. 1-18.

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HAS THERE BEEN A 'CHOMSKYAN REVOLUTION' IN LINGUISTICS?

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While it was once uncontroversial to refer to a 'Chomskyan revolution' in linguistics, a number of commentators have recently questioned whether generativist theory truly represents a revolutionary departure from earlier approaches. This paper defends the view that a Chomskyan revolution has indeed occurred—a revolution which began with the publication of *Syntactic structures* (1957) and which has had profound effects, both intellectually for the study of language and sociologically for the field of linguistics. Paradoxically, however, the revolutionary success of generative grammarians has not resulted in their achieving ADMINISTRATIVE power in the field—a fact which is both documented and explained.*

1. It was once uncontroversial to refer to a 'Chomskyan revolution' in linguistics. Commentators took it for granted that the publication of *Syntactic structures* by Noam Chomsky in 1957 ushered in an intellectual and sociological revolution in the field—a revolution that deepened with the following decade's work by Chomsky and his associates. The term 'Chomskyan revolution' has appeared in the titles of articles (Searle 1972) and book chapters (Newmeyer 1980); and an historian of linguistics has written that the work of Chomsky 'fully meets [the philosopher Thomas] Kuhn's twin criteria for a paradigm [in science]' (Koerner 1976:709). Even Chomsky's professional opponents have acknowledged the revolutionary nature of his effect on linguistics. G. Sampson, who feels (1980:163) that 'the ascendancy of the Chomskyan school has been a very unfortunate development for the discipline of linguistics', nevertheless writes (130) that 'Chomsky is commonly said to have brought about a "revolution" in linguistics, and political metaphor is apt.' R. Longacre, an individual who has a quite different orientation to grammar from Chomsky's, writes (1979:247) that 'the field was profoundly shaken by him', and has identified the essence of the Chomskyan revolution (a term which he uses without surrounding quotes) as its commitment to the construction of an explanatory linguistic theory.

However, the idea that the field ever underwent a 'Chomskyan revolution' has been challenged in recent years, and the challenges appear to be on the increase. Koerner has now reconsidered his earlier position and believes (1983:152) that 'upon closer inspection, the term "revolution" does not properly apply to TGG.' The sociologist S. Murray, whose professional specialty is the informal groupings and networks within the field of linguistics (see Murray 1983), writes that 'Chomsky did not make a revolution with [*Syntactic structures*]' (1980:81); rather, he and his associates engineered a 'palace coup' (82) in the early 1960's. In this view, Murray echoes the opinions of R. Anttila,

* I would like to thank Joseph Emonds, James McCawley, Sol Saporta, William Bright, Keith Percival, David Lightfoot, Geoffrey Pullum, Noam Chomsky, Patricia Keating, and Richard Ogle for commenting on an earlier draft of this paper. It goes without saying that they bear no responsibility for errors of either fact or interpretation.

who, with 'an increasing number of linguists, ha[s] realized that this allegedly linguistic revolution was a social coup d'état' (1975:171); and of B. Gray, who finds no Chomskyan revolution, but only a situation in which, 'as all admit, transformationalists have succeeded in capturing the organs of power' (1976:49). Those who see the generativist ascendancy as little more than a successful power grab tend to regard generative grammar as essentially post-Bloomfieldian business as usual. As Koerner puts it (1983:152), 'TGG is basically post-Saussurean structuralism, characterized by excessive concern with "langue" ... to the detriment of "parole" ...' Others do believe that Chomskyan theory represents a fundamental break with its antecedents, but one which did not occur with the publication of *Syntactic structures*. Rather, it is felt that Chomsky departed from earlier traditions only in the early 1960's, when he and his associates began to campaign against the autonomous phoneme (for such a view, see Hill 1980:75)—or in the middle years of that decade, when he began explicitly to embrace a 'rationalist' philosophical basis for the theory (see Uhlenbeck 1975:106–8).

In this paper, I will defend the position that Chomskyan theory represents a revolutionary approach to the study of language, and one whose revolutionary content was present in explicit form in *Syntactic structures*. Moreover, I will argue that, sociologically as well as intellectually, the field has undergone a Chomskyan revolution. Paradoxically, however, the sociological transformation of the field has not been accompanied by a corresponding success on the part of generative grammarians in achieving institutional power.¹ I will demonstrate that—far from being comfortably seated on the throne after their successful 'palace coup'—generativists, as they compete for adherents with linguists of other persuasions, find themselves well outside the walls of the palace.

2. What makes *Syntactic structures* revolutionary is 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.² Prior to 1957, it was

¹ Throughout this paper, the terms 'generative grammarian' and 'generativist' will be used to refer to any individual whose published work has been devoted largely to the elaboration or defense of the conception of language outlined in Chomsky's *Syntactic structures* and *Aspects of the theory of syntax*. That is, the terms will be applied to those who are committed to the program of characterizing the human linguistic capability in terms of grammar whose essential properties are universal and which consist of a set of formal, discrete, and interacting rules. By such a criterion, individuals who might agree with Chomsky's conception, yet have published little or nothing in generative grammar, will not be classed as 'generativists'. Likewise, the term as thus defined excludes those whose approach to grammar challenges the competence/performance dichotomy by abandoning the discrete rule, as in the 'fuzzy grammar' theory of Lakoff 1973 or in models embodying 'variable rules' (cf. Labov 1972a). Needless to say, people can be 'generativists' at one stage of their careers, but not at another.

² Many European structuralists, including Saussure, have explicitly regarded themselves as doing 'scientific' linguistics; but 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 'non-prescriptive'. Note the following passage from Martinet (1960:9): '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".'

widely regarded—not just in linguistics, but throughout the humanities and social sciences—that a formal yet non-empiricist theory of a human attribute was impossible. Chomsky showed that such a theory was possible. Indeed, the central chapter of *Syntactic structures*, 'On the goals of linguistic theory', is devoted 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 speculative philosophy of science. Rather, it demonstrated the PRACTICAL possibility of a non-empiricist theory of linguistic structure: half 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 distinction (later competence/performance). For Saussure, who conceived of linguistics 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' (1966:14). 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 followed that a task of linguistics would necessarily be to provide 'a precise specification of the class of formalized grammars' (Chomsky 1962:534). Interestingly, Chomsky's empiricist antecedents in American structural linguistics, 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 attempt (1968) to rebut Chomsky recognized that the question of whether the grammar 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. Phonological and morphological systems are essentially closed and finite; whatever their complexity or intrinsic interest, their study does not lead to an understanding of a speaker's capacity for linguistic novelty, or to an explanation of the infinitude of language. Yet earlier accounts had typically excluded syntax from langue altogether. 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'. Z. Harris, it is true, had begun in the late 1940's to undertake a formal analysis of intersentential syntactic relations (see Harris 1957); but his

empiricist commitment to developing mechanical procedures for grammatical analysis led him to overlook what the study of these relations implied for an understanding of linguistic creativity.

The fact that *Syntactic structures* was syntax-centered lay at the foundation of the INTERDISCIPLINARY revolution that it initiated. Consider its effect on psychology. Psychologists had certainly taken an interest in pre-Chomskyan structural linguistics; indeed, J. B. Carroll had written (1953:106): 'From linguistic theory we get the notion of a hierarchy of units ... It may be suggested that stretches of any kind of behavior may be organized in somewhat the same fashion.' Yet the approach to language to which Carroll referred, by granting primary position to phonology or morphology, offered little to an understanding of language processing or more general aspects of verbal behavior. As a consequence, the results of structural linguistics were completely ignored in Skinner's *Verbal behavior* (1957), and were given only limited attention in the major pre-Chomskyan survey of psycholinguistics, Osgood & Sebeok 1954. But shortly after Miller et al. 1960 had revealed to the community of psychologists the implications for the structure of human behavior latent in Chomsky's theory of syntax, the 'psycholinguistic revolution' (Greene 1972:11) was well under way.

The effect of *Syntactic structures* on philosophy was equally profound. Although the two major schools of mid-20th century philosophy—logical empiricism and ordinary language philosophy—were preoccupied with problems of language, they paid scant attention to structural linguistics. But Chomsky's syntax-centered approach, with its implications for limitless yet rule-governed creativity, had initiated a dialog among philosophers even before he had called attention to the 'Cartesian' properties of the theory (cf. Putnam 1961, Chomsky 1962, Bar-Hillel 1962, Scheffler 1963).

While Chomsky's conception of the nature of grammatical theory was revolutionary, there were, needless to say, numerous respects in which *Syntactic structures* retained 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'.³ 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, they ask, could it be correct to refer to a CHOMSKYAN revolution? Thus G. Lakoff concluded from Chomsky's commitment to structural analysis that early transformational gram-

³ The 'structuralism' issue is confused by the fact, that in the early 1960's, Chomsky and his followers began to reserve the label 'structuralist' for those synchronic approaches in the Saussurean tradition that did not share their views on theory construction. The result is that, when one now speaks of a 'structuralist' with linguistics, 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 20th century structuralism (cf. Lane 1970:28-9, De George & De George 1972:xx).

mar, rather than representing a revolutionary development, 'was a natural outgrowth of American structural linguistics' (1971:267–8). The same point has been made more recently by Murray (1980:76), who finds 'the base of Chomsky's early work ... in American structural linguistics, especially as developed by Zellig Harris'; by Hymes & Fought, who regard *Syntactic structures* as showing 'no evidence of basic revolutionary change' (1981:241); and, as we have already seen, by Koerner.

Chomsky's 'structuralism', however, no more disqualifies his theory from being revolutionary than Einstein's Newton-like search for physical laws undermines the revolutionary nature of relativity theory. Saussure's victory was that of structuralism, just as Newton's victory was that of a lawful universe. We would no more expect the next revolution in linguistics to be an anti-structuralist 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—one which 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 Hockett wrote (1966:156) 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', one presumes that he was (correctly) ignoring the fact that the approaches of the three linguists share the property of 'structuralism'.

Chomsky's debt to his predecessors encompassed, of course, more ideas than that of a systematically structured langue. Again, his critics have pointed to these ideas to bolster the charge that the field has not undergone a Chomskyan revolution. Thus Hymes & Fought (167) stress that generative grammars were not introduced to the field in *Syntactic structures*; and they note that, 'if by generative theory, one means explicit formal theory, it is a whopping error to identify the concept with Chomsky and his followers.' And Koerner (1983:159) emphasizes that Chomsky borrowed the idea of the transformational rule from his teacher Harris.

While Hymes & Fought and Koerner are correct, their observations are irrelevant to the question of whether *Syntactic structures* was revolutionary. No partisan of Chomskyan theory has ever suggested that the proposal of a generative grammar embodying transformational rules constituted, in 1957, a revolutionary break with past practice. Indeed, the idea that a grammar could be viewed as a set of instructions for generating the sentences of a language had been in the air for several years (Harris 1954, Hockett 1954).⁴ As far as formalism is concerned, Saussure had said in the 1890's that a linguistic description 'sera algébrique ou elle ne sera pas' (Godel 1957:49), though he never put his statement into practice in a synchronic analysis. Models have even been

⁴ Chomsky's 1949 undergraduate thesis and his 1951 master's thesis (published in 1979), which propose a generative account of Hebrew, antedate the Harris and Hockett papers by several years. Koerner 1984, apparently unaware of Chomsky's work, cites Harris and Hockett to bolster his claim that *Syntactic structures* was not revolutionary. In fact, Harris and Hockett present a far more rudimentary theory than that of Chomsky's 1949 thesis, in that the rule system to which they point does not assign a full structural description to every linguistic expression generated.

proposed that are faithful to the over-all Chomskyan research program, but which explore alternatives to generative grammars (cf. McCawley 1970). Chomsky's contribution to linguistics was not the proposal that generative grammars were suitable devices for representing human language; rather, it was the reinterpretation of generativity within a revolutionary conception of what a linguistic theory is a theory OF.

Correspondingly, transformational rules are not central to Chomskyan theory, nor have they ever been regarded as an innovation of the theory. Far from it: Chomsky has always (e.g. 1957:6) credited Harris with originating them. Such rules are simply one of a number of possible devices available to syntactic theory for the expression of formal generalizations. In any event, an improved system of formalism had little, if anything, to do with what made *Syntactic structures* revolutionary. Not surprisingly, then, frameworks for syntactic analysis have appeared since 1957 which are wholly Chomskyan in their basic world-view, but which reject outright the necessity for transformational rules (cf. Koster 1978, Gazdar 1981).

In their long discussion of research continuities in American linguistics, Hymes & Fought do in fact identify the central distinguishing feature of Chomsky's theory, though they fail to identify it as a revolutionary innovation. They write (180):

'Chomsky's true argument with the Bloomfieldians was with regard to the KIND of evaluation procedure, the kind of formal justification of a linguistic analysis, or linguistic theory, that should be followed. To the criterion of theoretically possible induction, he opposed the criterion of theoretically definable simplicity (generality).'

But Chomsky's 'true argument' dealt with nothing less than the very nature of linguistic theory. No issue is as important as the relevant criteria for theory evaluation, since a radically revised evaluation procedure entails a theory with a radically revised ontological basis. To abandon a procedure based on induction, and to adopt one based on generality, is to break from past practice at its most fundamental point; it requires one to cease thinking about a grammar as an operationally-derived synthesis of a corpus, and to begin regarding it as a theory of a language.

3. We may conclude that, INTELLECTUALLY, *Syntactic structures* had a revolutionary effect on the field of linguistics. Its effect was equally revolutionary in a SOCIOLOGICAL sense—though this question is complicated by the fact that historians of science have presented conflicting criteria for identifying successful revolutions. For example, Kuhn, in his influential *Structure of scientific revolutions* (1970), claims that a central criterion is the resultant uniformity of belief, within the scientific community, in the new 'paradigm'. If Kuhn is correct, then NO (scientific) revolutions have occurred in linguistics.⁵ As K. Percival

⁵ McCawley 1985 interprets Kuhn's 'universal assent' claim to demand only the APPEARANCE of consensus; i.e., normal science exists when adherents of the dominant approach do not bother to reply to critics of that approach. Even if McCawley is correct, however, I do not believe that an approach in linguistics has ever existed whose partisans have felt secure enough to ignore their critics. In particular, many of Chomsky's publications (esp. 1972, 1975) devote close to half their pages to rebuttals and replies to critics of generative grammar.

has shown, no approach to linguistic analysis, past or present, has garnered universal acceptance; most relevantly (1976:289), 'Generative grammar does not command universal assent among linguists all over the world; it is not a conceptual framework shared by all the members of the profession.' The conclusion seems inescapable: the 'Chomskyan revolution', if there was one, was not a 'Kuhnian revolution'.⁶

Kuhn's theory of scientific revolutions has been subject to considerable scrutiny; and it seems fair to say that only a small number of philosophers of science accept it, even in broad outline. But of all the components of his theory, none is as vulnerable as the 'uniformity of belief' hypothesis. It is apparently the case that no scientific theory, even the most uncontroversially revolutionary ones, has ever generated universal assent. As the philosopher L. Laudan has pointed out (1977:137),

'we speak of the Darwinian revolution in nineteenth century biology, even though it is almost certainly the case that only a small fraction of working biologists in the last half of the nineteenth century were Darwinians. We speak of a Newtonian revolution in early eighteenth century physics, even though most natural philosophers in the period were not Newtonians.'

What sociological criterion, then, uniquely characterizes a scientific revolution? Laudan provides a compelling answer (137–8):

'... a scientific revolution occurs, not necessarily when all, or even a majority, of the scientific community accepts a new research tradition, but rather when a new research tradition comes along which generates enough interest (perhaps through a high initial rate of progress) that scientists in the relevant field feel, whatever their own research tradition commitments, that they have to come to terms with the budding research tradition. Newton created the stir he did because, once the *Principia* and the *Opticks* were published, almost every working physicist felt that he had to deal with the Newtonian view of the world. For many, this meant finding cogent arguments AGAINST the Newtonian system. But what was almost universally agreed was that Newton had developed a way of approaching natural phenomena which could not be ignored. Similarly, late nineteenth century biologists, whether fervent Darwinians or confirmed anti-evolutionists, found themselves having to debate the merits of Darwinism. To put the matter in a more general fashion, I am suggesting that A SCIENTIFIC REVOLUTION OCCURS WHEN A RESEARCH TRADITION, HITHERTO UNKNOWN TO, OR IGNORED BY, SCIENTISTS IN A GIVEN FIELD, REACHES A POINT OF DEVELOPMENT WHERE SCIENTISTS IN THE FIELD FEEL OBLIGED TO CONSIDER IT SERIOUSLY AS A CONTENDER FOR THE ALLEGIANCE OF THEMSELVES OR THEIR COLLEAGUES.' (emphasis in original)

If Laudan is correct, there can be no question that the field has undergone a 'Chomskyan revolution'. As Hymes noted (1964:25), the year before the pub-

⁶ The Chomskyan revolution failed to meet Kuhnian conditions in another respect. For Kuhn (74–5), a revolution is in part a response to 'crisis'—to a situation in which, in the 'typical' case, there is 'a profound failure in the normal problem-solving activity'. Yet far from being in a state of crisis, post-Bloomfieldian structuralism in 1957 was enjoying a period of unprecedented optimism, in which it was believed that the fundamental questions of linguistic analysis had all been solved (see Newmeyer 1980:1–3). It is rather puzzling, then, that so many commentators, generativist and non-generativist alike, have taken the Chomskyan revolution to exemplify Kuhn's conception of a scientific revolution (cf. Thorne 1965:74, Sklar 1968:213, Maclay 1971:163, Searle 1972:16, Katz & Bever 1976:11, Koerner 1976:709).

It should be pointed out that Percival maintains a critical stance with respect to Kuhn throughout his article, and is careful to avoid suggesting that the failure of the latter's criteria to apply to linguistics should lead to the conclusion that the transition from pre-Chomskyan to post-Chomskyan linguistics should not be characterized as 'revolutionary'.

lication of Chomsky's *Aspects*, 'it remains that transformational grammar has established itself as the reference point for discussion of linguistic theory ... it remains the case that it has been Chomsky who has effectively opened the American linguistic scene to its present free and fruitful discussion.' In other words, by 1964 it was Chomsky and his theory that formed the focal point for debates about how to carry out theoretical linguistics.

We know that there was a Chomskyan revolution—but not because every linguist in the world was at one time, or is now, a generative grammarian. There was a Chomskyan revolution because anyone who hopes to win general acceptance for a new theory of language is obliged to show how the theory is better than Chomsky's. Indeed, the perceived need to outdo Chomsky has led him to be the most attacked linguist in history. Presentations of alternative conceptions of grammar routinely devote entire chapters to debunking his views (cf. Givón 1979, Prideaux et al. 1980, Moore & Carling 1982). One can appreciate the depth of Chomsky's impact by noting that a large percentage of his opponents are Europeans; his ideas have been subjected to book-length critiques published in Britain (Robinson 1975), Holland (Uhlenbeck 1975), France (Hagège 1976), Germany (Weydt 1976), Sweden (Collinder 1970), and the Soviet Union (Akhmanova & Berezin 1980). Since only a small percentage of European linguists have ever been generative grammarians, the motivation for these attacks can only be that he is seen as the dominant figure in WORLD linguistics.

4. Thus Chomsky's ideas have continued to generate resistance; but when they were first presented in the late 1950's and early 1960's, the leaders of American structural linguistics did not attempt to prevent them from being heard.⁷ Quite the contrary: while Chomsky's first two generativist manuscripts were rejected by publishers, all the subsequent ones were accepted. Chomsky was, almost from the start, touted by the leaders of the field as the brightest and most original of the younger generation of linguists. Speaking invitations were extended to him even before he received his doctorate; and as early as 1962, he was granted the honor of being one of the five speakers in the plenary sessions of an International Congress of Linguists.⁸ Furthermore, Bernard Bloch, arguably the most influential American linguist of the period, concretely abetted Chomsky and his theory in a number of ways.⁹

Chomsky's undeniable ease at gaining a hearing has been proffered as an argument that, instead of a genuine 'Chomskyan revolution' taking place, the

⁷ However, Chomsky's opponents typically exaggerate his ease of acceptance by the field. Thus Murray writes that 'two publishers were interested in publishing [Chomsky's ms] *LSLT* [*Logical structure of linguistic theory*] in 1957' (1980:78), and that, in this period, Chomsky 'had many chances to move. Indiana tried to get him about 1957-8' (p. 82). Murray provides no documentary evidence for these claims; and so far as I can determine, none has ever existed.

⁸ Though two coincidences played a role in Chomsky's being named a speaker: Harris turning down his own invitation, and the holding of the meeting in Cambridge, Massachusetts (see Newmeyer 1980:51 for further discussion.)

⁹ See Newmeyer (1980:47-8) and Murray 1980. However, Hall (1981:182) implies that Bloch did NOT aid Chomsky.

field saw only a power grab by him and his supporters in the mid-1960's. Thus Murray 1980 questions whether Bloch and other prominent structuralists would have accorded Chomsky 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; however, it might be attributable to a misreading of those passages in Kuhn which discuss the transition of power from one paradigm to the next. Kuhn points out (150) that it is 'very often' the case that older workers in a scientific field do not accept revolutionary developments; to support this idea, he cites examples of the non-adoption of the theories of Newton, Priestley, Kelvin, and others by the establishments in their respective fields.¹⁰ But Kuhn never implies that the Old Guard attempt to SUPPRESS revolutionary new ideas, or that 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, within either linguistics or science in general. Kuhn's point that establishment figures do not adopt 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), plus a very few others, the leading structural linguists of the late 1950's did not become generative grammarians.

In any event, the published commentary on Chomsky's early work by prominent American structuralists leaves no doubt that they saw in it a fundamental challenge to their own established views of how to carry out linguistic research. For example, C. F. Voegelin recognized that the rejection of empiricist-based discovery procedures was at the heart of Chomsky's approach to both syntax and phonology, and he wrote (1958:229): 'if transform grammar also persuades linguists to relegate phonemics to a preliminary stage of analysis ... and to operate in final analysis ... exclusively with morphophonemics, it will have accomplished a Copernican revolution'.¹¹ Likewise, while M. Joos recognized

¹⁰ Kuhn writes elsewhere that 'some' scientists cling to older views (19), and points out that 'only a few of the older chemists' (134) rejected Dalton's chemical theory. The major study of the resistance of established scientists to new theories, Barber 1962, warns against exaggerating the commonness and importance of this resistance. Hymes & Fought (48) point out that many major pre-structuralist linguists commented quite favorably on the classic of early American structuralism, Bloomfield's *Language* (1933).

¹¹ I cited only the final clause of the Voegelin quotation in Newmeyer 1980, and was accused by Koerner 1983 of thus giving a distorted impression of Voegelin's intent. The full sentence shows clearly that Voegelin regarded the abandonment of the structuralist phoneme as revolutionary, though he was careful throughout his review to maintain a critical stance with respect to the desirability of such an abandonment. Koerner is correct that Voegelin saw transformations as a natural outgrowth of earlier work by Harris; however, Koerner's observation is irrelevant, given

the 'structuralist' core of generative grammar, he also saw that it differed fundamentally from other structuralist approaches; hence he identified Chomsky's theory as a 'heresy within the neo-Saussurean tradition rather than as a competitor to it' (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.

I have pointed out elsewhere (Newmeyer 1980:46-7) that C. F. Hockett, far from viewing generative grammar as a mere logical extension of his own and other work in the post-Bloomfieldian tradition, went so far as to characterize the publication of *Syntactic structures* as one of 'only four major breakthroughs' in the history of modern linguistics (1965:185). He wrote:

'Between Sir William [Jones'] 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' 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.'

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 discovery from evaluation procedures, and practical description from formal theory, and which required setting the formal criteria that a theory must meet. Hockett referred to the various components of non-empiricist theory collectively as the 'accountability hypothesis', and wrote (196): 'that it is a breakthrough I am certain.'¹²

that Voegelin's quote was directed to the implications of generativist theory for phonology, not syntax. In fact, Voegelin saw at least revolutionary POTENTIAL in generative syntax as well, suggesting (230) that transformations might lead to 'a palace revolution, perhaps, in contrast to the interdisciplinary revolutions plotted by David Bidney, *Six Copernican revolutions* ...' By 'palace revolution', Voegelin had in mind a rebellion within the structuralist tradition. The 'interdisciplinary revolution', needless to say, began with Chomsky's 1959 review of Skinner.

¹² Koerner (1983:162) claims that, far from characterizing the publication of *Syntactic structures* as one of the four linguistic breakthroughs (as seems quite explicit in the above quote), Hockett intended 'more the opposite'. Koerner's only support for such an idea is based on the following quotation, also from Hockett's article (1965:196):

'We are currently living in the period of what I believe is our fourth major breakthrough; it is therefore difficult to see the forest for the trees, and requires a measure of derethesis on

Even before the publication of Chomsky's *Aspects* (1965), commentators began to refer to his revolutionary effect on the field. This fact refutes the view (cf. §1, above) that it was only with the publication of this book that Chomsky was seen by contemporaries as departing from the mainstream post-Bloomfieldian tradition. Papers published in 1965 (and therefore presumably written before the appearance of *Aspects*) noted that 'it is a truism by now that the publication of *Syntactic structures* marks an epoch in the development of American linguistic thought' (Levin 1965:92); that 'a revolution of the kind Kuhn describes has recently taken place in linguistics—dating from the publication of Chomsky's *Syntactic structures* in 1957' (Thorne 1965:74); and the 'slim volume [i.e. *Syntactic structures*] was to have a startling impact on linguistics' (Bach 1965:111–12).

Along the same lines, overviews published after 1965 have typically pinpointed the Chomskyan revolution as an historical event marked by the appearance of *Syntactic structures*, not of *Aspects*. While we might expect an early dating from generative grammarians, non-generativists also consider 1957 to mark the turning point in the field. So T. Sebeok (1969:vii) describes 'the emergence, in the late 1950's, of the Chomskyan paradigm'; R. H. Robins writes (1971:33) that 'in 1957 the description and analysis of languages was thrown into exciting turmoil by the publication of Noam Chomsky's *Syntactic structures*'; and H. Parret notes (1974:27) that 'almost everybody considered the publication of *Syntactic structures* in 1957 as a revolution in contemporary linguistics.' The personal testimony of the psycholinguist H. Maclay might be colored in part by his sympathetic attitude toward generative grammar, but it captures a widely-held sentiment. According to Maclay (163), 'The extraordinary and traumatic impact of the publication of *Syntactic structures* by Noam Chomsky can hardly be appreciated by one who did not live through this upheaval.'

Even R. A. Hall Jr. could write (1969:192) that 'since 1957, especially, an extensive upheaval has resulted from the doctrines propounded by Noam Chomsky, which are in many respects diametrically opposed to those of earlier approaches.' According to Hall (226),

'in the United States, counter-attacks were slower in coming [than in Europe], primarily because many established scholars did not realize, AT THE OUTSET, the full implications of Chomskyan total "rejectionism" and anti-scientific positions, and expected his arguments to fall of their own weight, underestimating their appeal to newcomers untrained in linguistics and imbued with our culture's superstitions concerning language.' (emphasis added)

5. Chomsky's success in setting new ground rules for linguistic debate was matched by a corresponding success in drawing a host of new recruits into the

my part to say anything not wholly vague. Instead of a long list of names, I shall venture only the two of which I am sure; and since the two are rarely linked I shall carefully put them almost a sentence apart. I mean Noam Chomsky on the one hand and, on the other, Sydney M. Lamb. The order is intentional; Chomsky is unquestionably the prime mover.'

Why Koerner feels the above quote represents the 'opposite' view from the one cited in the main text (and why the former rather than the latter should be taken as representing Hockett's true views) is never revealed.

field. It is true that, in the 1960's, almost every area of American higher education expanded at a high rate; but the growth in linguistics (as measured by the number of doctoral degrees awarded) was three times the average rate of expansion for all fields. And in the early 1970's, when the number of first-year graduate students in physics declined by 41%, in English by 35%, and in history by 31%, linguistics actually saw an increase of 49% (Newmeyer 1980:52-4). Since the promise of future financial reward could hardly have motivated students to enter a linguistics program, it seems reasonable to attribute the accelerated growth of the profession after 1957 in considerable measure to the appeal of generative grammar.

One might assume then that generative grammarians would have been equally successful in securing for themselves a dominant ORGANIZATIONAL position within American linguistics. That is, given their other successes, it seems reasonable that it should be Chomsky and his associates who make the day-to-day decisions about who gets hired, what gets published, which grants get awarded etc. Indeed, as we have seen, Murray, Anttila, Gray, and others believe that such is their ONLY true achievement—one which they ruthlessly exploit to 'silence' critics lacking tenure at their universities (Maher 1980:6).

I find little evidence to support the idea that, organizationally, American linguistics is controlled by generative grammarians; in fact, their influence is disproportionately small.¹³ To begin with, there is ample opportunity to study with non-generativists for anyone who wishes to do so. The linguistics programs at many major universities are dominated by non-generativists, including those at California-Berkeley, Columbia, Florida, Georgetown, Harvard, Hawaii, Illinois-Chicago Circle, Michigan, Michigan State, New Mexico, Oregon, Pennsylvania, Rice, SUNY-Buffalo, and Yale. According to a recent survey sponsored by the National Research Council, the linguistics departments at California-Berkeley and Pennsylvania are among the ten best in the country. Even in the departments at Brown, CUNY, Cornell, Minnesota, Southern California, UCLA, and Wisconsin, where a majority of the faculty are generative grammarians, one is able to study with individuals who are hostile to the generativist approach.

Second, generative grammarians receive only a small minority of the grants allocated in the field. As public records show, in 1982, generativists received 7 out of 28 National Institutes of Health Grants in Linguistics, and only 11 out of 47 National Science Foundation Grants within their Linguistics Program.¹⁴ The Ford Foundation, incredibly, gives POLITICAL motivation for its refusal to fund generativist research: it objects to the fact that generativists 'have isolated [the field] from the world of non-linguistic events and concentrated on abstract and formal theories about the nature and structure of language' (Fox & Skolnick

¹³ I take it as uncontroversial that the question does not even arise in any other country.

¹⁴ Paul Chapin, the National Science Foundation Program Director for linguistics, has a doctorate in linguistics from MIT. However, his 1983 advisory panel contained only one generativist, namely Susumu Kuno (the other members were Melissa Bowerman, Michael Krauss, Peter McNeilage, Brian McWhinney, and Gillian Sankoff). And Kuno is better known for his functionalist alternatives to generative principles than for his contributions to generative grammar per se.

1975:6). The Foundation chooses to support those linguists who 'have come to view the relevance of their discipline as most importantly defined by its ability to contribute to an understanding of society'.

So far as I have been able to determine, the two major individual grant recipients in linguistics in the past fifteen years have been William Labov and Peter Ladefoged, neither of whose work can be regarded primarily as an elaboration or defense of Chomskyan theory.¹⁵ And the organizational unit within the field that has the greatest resources and personnel at its disposal appears to be the Summer Institute of Linguistics, few of whose members are generativists.

Much has been written about the military funding (direct or indirect) of generativist research in the 1960's, and there is no question that such funding played a role in the early success of the theory (cf. Newmeyer & Emonds 1971, Newmeyer 1985). But what is often forgotten is that, after the Mansfield Amendment to a late 1960's appropriations bill was passed, demanding demonstrable military relevance for all military spending, such funding came to a complete halt. Throughout the 1970's, when sociolinguists were drawing from the various agencies of the Department of Health, Education, and Welfare, and phoneticians from the National Science Foundation, the program at MIT had to content itself with rather non-lucrative National Institute of Mental Health training grants for its graduate students.

Third, one would be hard put to identify the Linguistic Society of America as a generativist-dominated organization. Only two of the annually elected presidents in the history of the society have been generativists, and partisans of Chomskyan theory make up a minority of its Executive Committee. Indeed, there is a feeling among many generativists that the LSA is an ANTI-generativist organization—or is, at best, irrelevant to their needs. As a consequence, many do not bother to join. An inspection of the December 1984 LSA *Bulletin* (no. 106) reveals that the following prominent generative grammarians were not members: Michael Brame, Joan Bresnan, Joseph Emonds, Robert Fiengo, Osvaldo Jaeggli, Lauri Karttunen, Edward Keenan, Charles Kisseberth, Edward Klima, Mark Liberman, Alan Prince, Paul Postal, and Edwin Williams.¹⁶

Likewise, *Language*, the journal published by the LSA, is not oriented toward generative grammar. While a majority of its editorial board members are generative grammarians, its Editor is not. If anything, generativist strength in the field is under-represented on the pages of *Language*; only about a third of its articles reveal such an orientation. Recent years have seen an article entitled 'On the failure of generative grammar' (Gross 1979); and a single issue (58:1,

¹⁵ While both Labov and Ladefoged have adopted conceptions from generative grammar in their work, they have also been highly critical of certain central generativist notions, in particular the competence/performance dichotomy (see Labov 1972b, Ladefoged 1985). In any event, little of their funded research is devoted to questions of grammatical analysis.

The major institutional grant recipient is the Center for Applied Linguistics, where virtually no generativist-oriented research takes place.

¹⁶ I have not checked with each of these individuals to find out their reason for not belonging to the LSA.

March 1982) contains an article by R. Langacker presenting an alternative conception of grammar from Chomsky's, a review article by H. Schiffman that derides generative grammar for hypothesizing the 'ideal speaker/hearer' and categorial rules, and a laudatory review by J. Jaeger & R. Van Valin of a book (Prideaux et al. 1980) which concludes that Chomsky's approach is disconfirmed by psycholinguistic experimentation.¹⁷

Wherever we look, we fail to find confirmation of the claim that generativists rule the field. Some journals have a generativist bias, some a non- or anti-generativist bias. Some conferences are based on generativist themes, some are not. The 1983 Linguistic Institute was oriented toward generative grammar;¹⁸ the 1985 Institute was not. And so on. One simply cannot avoid the conclusion that an open market for linguistic ideas exists in the United States; no single theory, framework, or orientation comes close to being in an administrative position to prevent the others from being heard.

It is interesting to speculate on why the generativists' intellectual achievements and public visibility are not matched by an accompanying organizational dominance. Several come to mind. Partly, the state of affairs is a simple result of the inevitable time lag between a scientific theory's being recognized as revolutionary and its institutionalization in academia. For example, Einstein had won the Nobel Prize and his name was a household word years before relativity theory was part of the core curriculum in all major physics departments, and before its advocates held the leading positions in the various professional societies of the field. Time lag is also the obvious explanation for the fact that only Morris Halle and Victoria Fromkin, of all generativists, have served as LSA president—few others have practiced linguistics long enough to achieve the degree of distinction to merit this honorary office.

Another reason has to do with Chomsky's own perception of the field. To quote him from an interview (1982:42-3),

'As I look back over my own relation to the field, at every point it has been completely isolated, or almost completely isolated. I do not see that the situation is very different now ... But I cannot think of any time when the kind of work that I was doing was of any interest to any more than a very tiny fraction of people in the field.'

Whether Chomsky is right or wrong (and he most certainly is wrong),¹⁹ the EFFECT of his perception of the field has been to discourage many of those under his influence from involving themselves in its bureaucratic infrastructure. This fact no doubt explains why so few generativists have served on LSA

¹⁷ I do not wish to imply that the Editor of *Language*, William Bright, is anything but scrupulously fair in his handling of submissions to the journal. I know from personal experience that he is a model of impartiality.

¹⁸ Though Koerner regrettably errs when he writes (1983:165) that I 'was appointed to teach the History of Linguistics at the Linguistic Institute held in summer 1983 at the University of California, Los Angeles'.

¹⁹ Chomsky feels that 'if the kind of linguistics [he is] interested in survives in the United States, it may very likely be in [cognitive science programs] rather than in linguistics departments' (1982:8; emphasis added).

committees, and it may even be at the root of the under-representation of generative grammar in *Language*. Generativist submissions are not rejected at a disproportionately high rate; there are simply not that many of them. Chomsky's alienation from the field has kept him from submitting a paper to that journal for almost 20 years; and, it seems, many of his colleagues have followed his example.

The lack of generativist hegemony also stems in part from the nature and scope of the field of linguistics. There are aspects of the field that the Chomskyan revolution has not touched, and is unlikely ever to. Consider, for example, such sociolinguistic topics as the language situation in Belgium, the characterization of turn-taking behavior in conversation, and the extent to which the prescriptively-sanctioned use of masculine 3rd person pronouns reinforces the social position of women. One's approach to these issues seems wholly unrelated to the correct form of the theory of grammar. The same point can be made for many of the topics taken up in experimental phonetics, pragmatics, lexical semantics, and other traditional areas of linguistic study. Since many sub-areas of linguistics complement grammatical theory rather than challenge it, it is not surprising that the success of the Chomskyan revolution has left intact the organizational power of their practitioners.

Finally, the diversity that exists in the field today is in part a result of the extraordinary complexity of its subject matter. While some linguistic phenomena lie uncontroversially outside the realm of grammatical analysis, a host of others might be amenable to grammatical treatment—or might not be. As a consequence, a multitude of approaches to language has developed, each mustering its share of supporters and each presenting its own particular account of the same phenomena. Chomsky himself has put forward a 'modular' conception of language, in which complex linguistic phenomena are attributed to the interaction of many different systems, of which formal grammar is only one (1965:3–4, 1981:1–6). But other popular points of view have ranged from the idea that grammatical systems are merely artifacts of principles of communication and cognition, with no independent existence at all (cf. Givón) to the opposite extreme—namely the idea that even such information as the speaker's culture, social standing, and attitudes should be incorporated into the grammar (Lakoff 1974). As suggested above, advocates of such positions have invariably defined (and defended) their positions with respect to Chomsky's; but they have had no difficulty in creating institutional poles of attraction in opposition to his. Indeed, the relatively decentralized and open nature of American academia, in which there is no 'Ministry of Education' to dispense all funds and appointments, has actually encouraged the development of a multitude of competing schools of thought in the field.

To conclude: despite the fact that generative grammar is not predominant institutionally, abundant evidence exists that there has been a successful 'Chomskyan revolution'. This revolution began with the publication of *Syntactic structures* in 1957 and has had profound effects—both intellectually, for the study of language, and sociologically, for the field of linguistics.

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[Received 9 March 1985;
revision received 17 May 1985;
accepted 28 May 1985.]