

STRUCTURAL LINGUISTICS AND THE PHILOSOPHY OF SCIENCE

In 1957 American linguistics seemed to have reached a plateau of achievement and acceptance on which its practitioners could pause in retrospective pride. That year saw the publication, under the aegis of the American Council of Learned Societies, of a sampling of papers, edited by Martin Joos (*Readings in Linguistics*) and documenting, according to its subtitle, "The development of descriptive linguistics in America since 1925." In a period of about five years around that date H. A. Gleason, Jr., Charles F. Hockett and Archibald A. Hill published textbooks which summarized and extended current views on linguistics, while another leading linguist, Kenneth L. Pike, summed up his considerable experience in a preliminary version of a major work on Language.¹ In the same year there appeared a slim volume

¹ H. A. Gleason, Jr., *An Introduction to Descriptive Linguistics* (New York, 1955, a revised edition appeared in 1961); Charles F. Hockett, *A Course in Modern Linguistics* (New York, 1958); Archibald A. Hill, *Introduction to Linguistic Structures* (New York, 1958); Kenneth L. Pike, *Language in Relation to a Unified Theory of the Structure of Human Behavior*, I, II, III (Glendale, 1954, 1955, 1960).

which was to have a startling impact on linguistics. Its author, Noam Chomsky, was a student of Zellig S. Harris who himself had codified the methods of structural linguistics in 1951 and was one of the leading proponents of the assumptions and goals of American linguistics attacked so sharply by its new critic.² Truly a paradigm of the Hegelian myth of history.

The turmoil aroused by Chomsky has not subsided. Almost every article of faith or working assumption held by American linguists prior to 1957 has been called into question. The debate has been often bitter, always vigorous, sometimes irrelevant to the real issues. At the same time it has offered an extremely interesting illustration of the influence of ideas about the nature and methods of science on the progress of science itself. It is from this point of view that I would like to review some events of roughly the last decade of American linguistics.³

Let me begin by sketching the characteristics (with I hope only a slight element of caricature) of two views of science. One—which I may call Baconian—goes something like this. The purpose of science is to obtain secure knowledge about the world. The only sure basis for such knowledge is observation and experiment. The scientist collects a large body of statements about particular happenings in the world or the laboratory. Starting from these true statements about real events he proceeds by a method of induction to limited generalizations about classes of events. After verifying these cautious generalizations he proceeds to more general statements. A general statement is reliable to the extent that it is based on such inductive methods. Hence, it is of the utmost importance to give the evidence for any general statement. The theory—if we admit this term at all—which is

² Noam Chomsky, *Syntactic Structures* (in *Janua Linguarum*, IV, The Hague, 1957, additional bibliography in the second printing, 1962); see also the important review by Robert B. Lees in *Language*, 33.375-408 (1957). Harris's book *Methods in Structural Linguistics* (Chicago, 1951; reprinted as *Structural Linguistics*) will be mentioned below. Chomsky has always been careful to point out his debt to Harris.

³ Two recent papers dealing in part with the subject of this survey should be mentioned: Karl V. Teeter, "Descriptive Linguistics in America: Triviality vs. Irrelevance," *Word* 20.197-206 (1964); and Jerrold J. Katz, "Mentalism in Linguistics," *Language* 40.124-137 (1964).

based on the widest body of evidence and is thus most probably true is the one most worthy of acceptance. Any speculations, metaphysical or *a priori* statements about the world, are excluded from science. In Bacon's words (Aphorism civ of the *Novum Organum*):

But then, and then only, may we hope well of the sciences when in a just scale of ascent, and by successive steps not interrupted or broken, we rise from particulars to lesser axioms; and then to middle axioms, one above the other; and last of all to the most general...

The understanding must not therefore be supplied with wings, but rather hung with weights, to keep it from leaping and flying. Now this has never yet been done; when it is done, we may entertain better hopes of the sciences.⁴

The key notions in this conception of scientific method are "verification," "induction," "based on."

The second view of science may be labeled Keplerian (with perhaps a little less historical justification than exists for the term "Baconian"). Whereas the Baconian stresses caution and "sticking to facts" with a distrust of theory and hypotheses ("anticipations" in Bacon's terminology), the Keplerian emphasizes the creative nature of scientific discovery, the leap to general hypotheses—often mathematical in form—whose value is judged in terms of fruitfulness, simplicity and elegance. This attitude is well illustrated in an article by the physicist P.A.M. Dirac who tells how Erwin Schrödinger succeeded in discovering by pure cogitation his wave equation but abandoned it when it did not give results that agreed with experiment (because the phenomenon of electron spin was not known at the time). He published only a weaker and more approximate form which agreed with experimental results. Later when the spin was discovered a complete agreement with the earlier equation was obtained. Dirac concludes: "I think there is a moral to this story, namely, that it is more important to have beauty in one's equations than to have

⁴ Quoted from Edwin A. Burt, ed., *The English Philosophers from Bacon to Mill* (New York, 1939). The debate about scientific method has a long history; compare for instance the arguments between John Stuart Mill and William Whewell in the nineteenth century.

them fit experiment. If Schrödinger had been more confident of his work, he could have published a more accurate equation."⁵ The case for the Keplerian view has been argued vigorously by Karl R. Popper.⁶ Denying the existence of "inductive logic" as well as the notion that theories can be verified, Popper argues for the criterion of "falsifiability" in judging hypotheses. From this idea it is possible to show that simpler, more general, and more precise hypotheses are better since they exclude more consequences. As stated pointedly by Popper, the best hypotheses are those which are least probable. Of course, it must be possible to deduce some predictions about matters of fact or experiments with the help of the hypotheses, but even this condition is relative to the state of technology. The importance of Tycho Brahe's painstaking observations for Kepler's discovery of the elliptical motion of the planets is a case in point. What may have been metaphysics for Democritus has become scientific hypothesis for Dalton or Bohr (but Dalton is reputed to have been a very bad experimenter).

Let me now try to establish the contention that the prevailing assumptions of American linguistics prior to 1957 were essentially Baconian in character. Before proceeding, a number of qualifications and warnings must be given. First, there never has been sufficient unanimity among linguists either in America or elsewhere to justify blanket statements about all linguists at any one time. At best, the following will refer to what I judge to be some typical and influential representatives. Second, it is necessary to distinguish between actual practice in linguistic work and methodological or programmatic statements about this practice. I shall be concerned mainly with the latter and with the influence of such methodological positions on linguistic work; at the same time I am well aware that a great deal of work has been done which directly contradicts the Baconian tenets of the worker. Third, participation in any representative gathering of linguists

⁵ P.A.M. Dirac, "The Physicist's Picture of Nature," *Scientific American*, Vol. 208, No. 5 (May 1963), pp. 45-53. I am indebted to Gary Prideaux for pointing out the relevance of this article to the present discussion.

⁶ Karl R. Popper, *The Logic of Scientific Discovery* (New York, 1959, translated and expanded from *Logik der Forschung*, Vienna, 1935).

or a perusal of any of the leading linguistic journals will quickly dispel the notion that any consensus has been reached since 1957 on the goals or methods of linguistic research. My account will be deliberately over-simplified at first, but I shall add some corrective remarks.

Leonard Bloomfield himself may be taken as a point of departure. In his influential book *Language* (New York, 1933, p. 20) he wrote: "The only useful generalizations about language are inductive generalizations." It is clear from the context and from other writings of Bloomfield that he was attacking the tendency to ascribe certain properties to all languages, for example, the existence of the categories of Subject and Predicate. The basic assumption here is that every language must be described in terms of its own structure without a preconceived system into which the linguist attempts to fit it. It is usual in such contexts to refer to Latin or philosophy as bases for wrongly conceived attempts to describe new languages. In an extreme form we have statements to the effect that "languages [can] differ without limit and in unpredictable ways"⁷ or the denial of "language universals." In the terminology of the followers of Pike, there is a reflection of this idea in the distinction between "etic" and "emic" concepts or terms, where the former are universal or preconceived schemes, categories, or units, the latter those which truly reflect the internal structure of the language. In the words of Joos (Preface to *Readings in Linguistics*):

The abandonment of deduction in favor of induction has never been reversed. At first it left the science stripped of general doctrines about all languages. Favorable at the start, this state of opinion could be, and in many older workers actually was, maintained past its function and could become a hindrance to further development. Once a number of unprejudiced descriptions had resulted from it, induction could be applied to these new descriptions too, and general doctrines about all languages could emerge again. It should be clear that these would have a better claim to credence than those founded upon Latin or upon a philosophy.

⁷ Martin Joos, ed., *Readings in Linguistics*, Second Edition (New York, 1958), p. 96, cf. also 228, in both places Joos identifies this view with the "Boas tradition."

Let us consider the issue here from the point of view of Popper. On the one hand we have a statement of the following sort: (1) All languages are like Latin, i.e. given any x , if x is a language then x is like Latin, where the conclusion of the implication must be thought of as decomposable into a series of statements specifying the respects in which the similarity is asserted to hold (i.e. a translation of the predicate "like Latin"). On the other hand we have a statement of the form: (2) There are no predicates P of which it is true to say that for any x if x is a language then P is true of x . It is easy to see that from the point of view of the falsifiability criterion (1) is a better hypothesis than (2).⁸ To falsify (1) it is sufficient to make the predicate "like Latin" precise and then find one language which is not "like Latin." To falsify (2) it is necessary to give an exhaustive account of the properties of all languages past, present, and future and then show that there is no property P which appears in the list for all languages. In other words (2) is simply not falsifiable, i.e. is not even a hypothesis, and amounts to a denial of the possibility of a science of linguistics.

One reflex of this anti-universalism is the standard apology for the use of terms like 'noun' and 'verb' for grammatical categories in various grammars, or structural sketches of non-Indo-European languages. Another is the bewildering proliferation of new terminology in such treatises. Much more serious was the complete abandoning of the attempt to state hypotheses of the form (1). It is clearly not true that all languages are like Latin in all respects. On the other hand if the term "language" has any general meaning at all, it must be true that all languages *are* like Latin in some respects. Rather than throw out the baby of linguistics with the bath of Latin (or Aristotelian-logical or Indo-European) terminology, it would seem more appropriate to search for better hypotheses of the same general form.

In order to falsify the statement "all languages are like Latin," however, it is necessary to improve the statement in the direction of greater precision (as indicated above). In addition, it

⁸ At several points in his comments on papers in *Readings in Linguistics*, Joos seems to be subscribing to the criterion of falsifiability, e.g. "a scientific statement is a vulnerable statement" (p. 31).

is necessary to have some means of showing which of two descriptive statements for a language is correct, or more correct, or better. Otherwise, with sufficient *ad hoc* adjustments and distortions it would be possible to describe every language in Latin terms (with a sufficient number of "epicycles") and the hypothesis would be corroborated by every new description. This was the state of affairs to which Franz Boas, Bloomfield and their followers were reacting when they objected to statements like (1). Another path might also be followed in attempting to falsify such a general hypothesis. If we could state a general procedure of analysis, a set of descriptive techniques, which when applied to any language data would give us a correct description of the language (or set of equivalent correct descriptions), then it would be sufficient to apply these techniques to Latin and other languages. On the first mismatch between the description of a new language and the description of Latin, our hypothesis would be rejected. Now if a description of a language is in any sense a theory about the language, i.e. a set of hypotheses, this path is precisely the path of Baconian induction. Some writers were apparently aware of this difficulty and rejected the idea that a description is a theory, thinking of it instead as a "compact one-one representation of the stock of utterances in the corpus."⁹ Compare also Joos's strictures against "explanation" (as against "description") in the preface to *Readings in Linguistics*.

Let us take a closer look at what I have called Baconian induction. Consider first the phrase "based on" as in such statements as "science must be based on observation and experiment." The geometrical metaphor is that of a pyramid. Connecting the structure to the solid earth of reality, or fact, is the foundation of observation sentences, i.e. purely existential and particular statements either about objects or happenings in the world or—in the more subjective or solipsist versions—individual sense-perceptions ("Otto sees green at time x , latitude y , longitude z , on planet w ..."). Such statements have been called protocol-sentences by those modern Baconians, the logical positivists. The next layer consists of Bacon's "lesser axioms" or limited generalizations "based on" this foundation and so on and on.

⁹ Harris, p. 366.

It is a comforting picture for the more acrophobic among us. But it is an extremely misleading picture, as I shall attempt to show. The foundation does not exist. To the extent that higher "floors" are "based on" lower ones, in any literal sense they are completely superfluous. A limited generalization is not a generalization at all.

Assuming for the moment that the foundation does exist, that there is a class of verified—i.e. indubitably true—observation sentences, in what sense could we say that a generalization is "based on" them? There are three possibilities: first, all the terms occurring in the generalization could be definable by primitive terms which were purely observational in nature (this is the view of "strong reductionism," if the primitive terms are presumed to be physical, it is the view of "physicalism"); second, the generalization could follow logically from the observation sentences; or third, both of these senses could obtain. The first view was applauded by Bloomfield, but even before he announced the confirmation for his views of science by the Vienna circle the thesis of strong reductionism had been modified by the positivists themselves. The reason is very simple. In the attempt to ban metaphysics from philosophy, they had also banned most of modern science, since even the commonest concepts of physical science, e.g., "magnetic," "soluble in water" and so on fail the test of strong reductionism.¹⁰

Consider now the second sense of "based on." This is similar to the sense in which we say that "Socrates is mortal" is based on the statements "Socrates is a man" and "all men are mortal." Now a generalization is a statement which includes the quantifier "all." But the only such statement that follows logically from one or more particular (existential) statements is a negative one. From the statement "there is a language which has the categories of noun and verb" (this is hardly an observation sentence, of course) I can conclude "it is not the case that all languages do not have the categories of noun and verb," but that is just another way of saying the same thing.

¹⁰ See especially Carl G. Hempel, *Fundamentals of Concept Formation in Empirical Science* (in *International Encyclopedia of Unified Science*, II, No. 7, Chicago, 1952). Bloomfield cites the assumptions of physicalism with approval in "Language or Ideas," *Language* 12.89-95 (1936).

Further, while I can conclude from the statements " x is a language" and "for all x , if x is a language, x has such and such a property" that " x has that property," to work in the other direction involves the logical fallacy of asserting the consequent. No matter how many men have been observed to die, it is a fallacy to *conclude* (in the strict sense of logic) that all men are mortal. All efforts to justify a principle of induction have failed. This is not surprising. It is just another way of saying that theories about the world are not absolute truths that can never be upset. The history of science shows this. On the other hand, if we mean by "limited" or "inductive" generalization a summation of several particular statements without any claims beyond them, then our generalization is completely superfluous (however convenient) since it is just a compact way of restating the particular cases. In this view, the entire pyramid collapses and we have only a rearrangement of the blocks of the foundation.

Students of modern American linguistics will be reminded by the Baconian pyramid of another architectural structure, the several-storied house of linguistic levels. Every serious linguistic theory operates with several "levels" of units, rules, and relations, for instance, a level of phonology or sound structure, and a level of syntax, morphemics or whatnot. However, some of these houses have been described in terms that sound suspiciously Baconian, and it has been assumed that there is a necessary progression in terms of the notion "based on" from a lowest level—the phonological—to the higher levels. This house was popular during the period in which "mixing of levels" was the cardinal sin of linguistics. To quote (from *An Outline of English Structure*, by George L. Trager and Henry Lee Smith, Jr.)¹¹:

The morphemic analysis should be based on the fullest possible phonological statement in order to be complete.... the analyst must at all times be aware of the level-differences, and the systematic presentation must always be made in terms of the logical sequence, in one linear order, with the levels carefully distinguished (pp. 54 f.).

¹¹ *Studies in Linguistics, Occasional Papers* 3, Third Printing, (Washington, 1957).

Structural Linguistics

Or again:

The presentation of the structure of a language should begin, in theory, with a complete statement of the pertinent prelinguistic data... This should be followed by an account of the observed phonetic behavior, and then should come the analysis of the phonetic behavior into the phonemic structure, completing the phonology. The next step is to present the recurring entities—composed of one or more phonemes—that constitute the morpheme list, and go on to their analysis into the morphemic structure. (p. 8).

But why should the phonological level be accorded a logical, or evidential priority? Could one not say with equal justification "The phonological analysis should be based on the fullest possible morphemic statement in order to be complete?" The answer to this question shows, I believe, the extent to which Baconian assumptions underlay the methodological program of one trend of American linguistics. If "based on" is taken in either the definitional or implicational sense (or both) then *one* order must be maintained or else there will be a vicious circle. This seems to be the basic objection to "mixing of levels" (although as James Sledd pointed out at the First Texas Conference in 1956, the notion was so unclear that it was difficult to attack or defend—the participants in that conference were sure that level-mixing was reprehensible but no-one seemed to know what it was¹²). With this understanding of "based on" you cannot have both a "phonologically based syntax" and a "syntactically based phonology." But even if this is a proper interpretation of "based on" (and I do not think it is defensible, that is, I think there is no vicious circularity and that both alternatives are true), why should the phonological level have a logical priority? The answer is, I believe, that it was felt that the phonological level was closer to reality, more objective, more "physical" than the other levels. This attitude is reflected in statements about "hugging the phonetic

¹² James H. Sledd, "Notes on English Stress," *First Texas Conference on Problems of Linguistic Analysis in English* (Austin, Texas, 1962), pp. 33-44, also in succeeding discussion.

ground" (Hockett's manual¹³) or in Floyd Lounsbury's suggestion that one method of morphological analysis "deals with the segmentation of actual utterances rather than with constructs once removed from reality" or again in his characterization of phonemic forms as "actually occurring forms of the language."¹⁴ But it has been clear since Yuen-Ren Chao's paper in 1934 that there is no way to guarantee a unique phonemic analysis of a phonetic system (and all experience since Chao's paper has borne out his contention); while W. Freeman Twaddell's monograph "On Defining the Phoneme" (1935) showed very clearly that phonemes are completely hypothetical constructs (Twaddell called them "fictions," a term that can be objected to on philosophical grounds).¹⁵ If phonology is taken to be the foundation of grammar for physicalist reasons, this foundation is very shaky indeed. Again, this does not seem to be cause for undue alarm. Linguistics deals with cultural data. The data about the sounds of a language are just as cultural as are those about the sentences of a language or those about the meanings of these sentences. The hypotheses and constructs of phonology are neither more nor less secure than those of syntax.

Because I am dealing with implicit assumptions it is difficult to find clear statements of position which could be offered in support of my categorization of this phase of modern linguistics. It is also easy to oversimplify (as has been done on occasion by some of Chomsky's adherents). Perhaps the clearest statement of the position that linguistics must provide a set of procedures for arriving at correct, unbiased descriptions of language structure is to be found in Harris's *Methods in Structural Linguistics*, to which I have already alluded. Most of the time Harris talks as if the linguist could

¹³ Charles F. Hockett, *A Manual of Phonology*, Indiana University Publications in Anthropology and Linguistics, Memoir 11 (1955), p. 155, see also the section "Phonetic Realism" pp. 156-158.

¹⁴ In Joos, *op. cit.*, pp. 381 and 380.

¹⁵ Yuen-Ren Chao, "The Non-Uniqueness of Phonemic Solutions of Phonetic Systems," *Bulletin of the Institute of History and Philology Academia Sinica*, IV, Part 4, 363-397. Both this paper and Twaddell's monograph (in part) are reprinted in Joos' *Readings in Linguistics*.

do without generalizations at all, in other words, as if the purpose of linguistic analysis were merely to rearrange the original data: "The overall purpose of work in descriptive linguistics," he writes, "is to obtain a compact one-one representation of the stock of utterances in the corpus" (p. 366). In line with my remarks about a schedule of procedures which would yield comparable statements about the structure of different languages (and hence allow us to falsify the statement that all languages are like Latin), we may note the following: "...the data, when arranged according to these procedures, will show different structures for different languages. Furthermore, various languages described in terms of these procedures can be more readily compared for structural differences, since any differences between their descriptions will not be due to differences in method used by the linguists, but to differences in how the language data responded to identical methods of arrangement" (p. 3). But Harris acknowledges that the linguist may also wish to make predictions ("synthesize utterances," pp. 365 f.) and his operations at one point presuppose statements which necessarily go beyond any finite corpus since the form of the statement allows for projection of an infinite set of sentences.¹⁶

It is, of course, impossible to prove that a set of procedures for discovering "correct" grammatical descriptions (or theories) cannot be given—and I am assuming that a grammatical description to be interesting must be a theory in a precise sense, which makes predictions beyond a limited corpus (otherwise any set of data will be a trivial grammar). I think it can be reasonably argued, however, that this is a rather unreasonable expectation. Before doing so we must first look briefly at the form of a grammar as a predictive theory. One way of characterizing Chomsky's contribution is to say that he did just this, namely, forced attention back to the form of grammars, to the explication of the notion of grammatical rule, and to the properties of grammars which would be necessary if grammars were to make the predictions about languages that would allow them to be tested in the broadest and most

¹⁶ Because of recursive formulas like $AN = N$ (p. 265).

explicit way. Stated succinctly, Chomsky turned linguistics away from the question: "What rigorous procedures must we follow in order to guarantee that we find the true properties of individual languages and of language in general?" and toward the question: "What are the properties that grammatical theories must have in order for them to make testable predictions about individual languages and what is the least set of such properties which will allow us to make theories for individual languages and hence allow falsifiable predictions about the structures of all languages?" (The revolution had something both of the Copernican and the Kantian.)

A grammar in Chomsky's sense is structurally very similar to a formalized mathematical theory. In fact, with the exception of the transformational rules which are the characteristic contribution of Chomsky (and this is probably only a temporary exception) a grammar can be given the precise form of a so-called Post system (when stated as a formal system) or, equivalently, as a particular type of abstract machine (something presumably more restricted than a Turing machine, although just how much more restricted and in what way is not clear at present). A formalized mathematical theory consists of a set of axioms (postulates, primitive propositions), explicit rules for deriving further terms and propositions from the original set, and an open-ended set of such derived propositions, called theorems. Similarly a grammar consists of a primitive set of formal objects (strings), a set of rules for deriving further strings from the primitive set, and a (metatheoretical) set of rules telling how this explicit procedure is to be applied, how various further statements about the derived (or generated) strings are to be derived, and so on. Naturally, such a system is vastly more complicated than a logical or mathematical theory, and for equally understandable reasons no such theory for any language is anywhere near complete. I shall not go into the question of the various parts of such a theory (e.g. the difference between the syntactic part and the phonological part or the different levels necessary in the syntax or the questions of a semantic annex to the grammar which is just now beginning to get interesting). But in a sense which is not too strained, the

sentences of the language being described are quite equivalent to the theorems of a mathematical system.

Consider now the problem of the linguist getting off his canoe on Pago-Pago. He has to invent or discover a formal system to account for the theorems which he hears from the lips of the curious natives around him. It is a great deal to expect that there will be a mechanical procedure to do this job for him, especially when we consider the various limitations known to exist about problems connected with the much more definite and precise deductive systems of mathematics and logic.

For instance, suppose we are given a set of axioms and an explicit set of rules for constructing proofs (derivations). There is no mechanical (effective) procedure for finding interesting theorems and proving them on the basis of the axioms. Or again, suppose we are given a set of axioms and rules of proof together with a supposed theorem which is alleged to follow from the axioms. There is no mechanical procedure for finding a proof (if it exists) or showing that one does not exist. Surely, it is unreasonable to expect a mechanical procedure for finding a set of axioms which will yield a given set of theorems (sentences) in a language for which even the basic symbols have to be discovered. (Incidentally, research in what can fairly be called mathematical linguistics has shown that there are parallel undecidable problems for whole classes of grammars considered abstractly as formal theories, e.g. whether two grammars are equivalent in the sense of describing the same language).

A crucial notion in the procedural theories of the sort exemplified by Harris's book is that of "distribution." The differences between those views and Chomsky's can be neatly epitomized as follows. Whereas the procedural theories took distribution as something given, as a primitive notion that could be used in defining the units and classes of a grammatical description, the generative approach takes distribution as that which must emerge as the end-result of a grammatical theory. Whereas distribution had been taken to be a comfortable notion to be used in stating rigorous techniques that could be applied in principle (although never in practice) to arrive at comparable

and unbiased descriptions, in the Chomskyan view distribution turns out to be precisely the problem.

I said before that almost every assumption of pre-Chomskyan linguistics had been called into question. I have dwelt at some length on what I feel to be the basic difference in the views of science of the several "schools" (if this unfortunate term must be used). The situation illustrates nicely the difference between what F.S.C. Northrop called the "natural-history" stage of science and the stage of "deductively formulated theory"¹⁷ (the somewhat invidious term "taxonomic" linguistics has been used in recent years to characterize the first type of theory). I cannot use these differences in philosophic outlook as evidence for the superiority of the newer approaches. That would be to put the philosophical cart before the scientific horse (although I think the recent developments add considerable weight to the view of those historians of science like Koyré who stress the importance of philosophical presuppositions in science). Rather it seems to me that recent advances in linguistics tend to show the correctness of what I have called the Keplerian view of science.

Perhaps the most important result of the recent trends and the one which will have the greatest effect in the long run has been an incredible sharpening of discussion about the foundations of our science. Chomsky's first major work—which is still unpublished—was a tremendous tome entitled *The Logical Structure of Linguistic Theory* [dittographed, n.d.o.p. but it was completed in 1955]. It is an attempt at what philosophers call the "rational reconstruction" of the whole field of structural linguistics and contains either in germ or in detail almost all of Chomsky's later work. In this work such notions as "grammar," "rule," "linguistic level," "transformation," "IC-structure," ("phrase-structure"), "simplicity," "structural description," and the like are explicated at a depth and a degree of precision and explicitness unmatched before. A good deal of modern linguistic discussion has turned on essentially philosophical questions (about the nature of science, "reality," scientific

¹⁷ F. S. C. Northrop, *The Logic of the Sciences and the Humanities* (New York, 1947).

evidence, etc.). But most such discussion was so unclearly formulated that it was impossible to decide just what was being said. As Chomsky wrote (preface to *Syntactic Structures*): "Obscure and intuition-bound notions can neither lead to absurd conclusions nor provide new and correct ones, and hence they fail to be useful in two important respects."

A kind of corollary of this attempt to construct a formalized theory of linguistics has been the increased use of the language of mathematics and modern logic for linguistic model-building. It is idle to speculate about whether or not mathematical linguistics (more precisely, algebraic linguistics) would have developed if Chomsky had not come along at the time he did. It is a fact, however, that a considerable body of results in an abstract and mathematical theory of grammars has developed in the last ten years and that Chomsky's work has played a crucial role in this development.¹⁸

The relation between this work and the work of empirical linguistics is the same as that between any purely mathematical study—say geometry or game theory—and a corresponding empirical discipline—say physics (physical geometry) or economics. The central problem of linguistics can be framed in the question: What is the structure of a natural language considered as a formal system? Algebraic linguistics attempts to study such systems in the abstract. It asks the questions: What kinds of possible grammatical systems are there? What are their properties in terms of generative power and in their ability to assign structural descriptions to the "languages" that they describe or generate? This study, which developed originally from purely linguistic sources, has been tied in (as I indicated before) with two branches of mathematics that have been pursued with considerable vigor in the last decades: proof theory (including recursive function theory) and the study of abstract automata. Most recently there have been connections made to modern algebra. The main result so far has been the establishment of a hierarchy of grammatical systems and

¹⁸ A good survey of this work is Noam Chomsky, "Formal Properties of Grammars," Chapter 12 in R. Duncan Luce, Robert R. Bush, and Eugene Galanter, eds., *Handbook of Mathematical Psychology*, II (New York and London, 1963).

their corresponding "languages" stretching from the so-called regular languages which are describable by finite state grammars (strictly finite automata) up to those systems which are of the most powerful kind conceivable (recursively enumerable sets of strings with corresponding grammars that are equivalent to Turing machines). The basic nature of the hierarchy is supported by the fact that a number of superficially different systems have turned out to be equivalent to one or another of the grammatical systems already placed on this scale of generality. The problem of linguistics, from this point of view, is to discover which abstract system matches most closely the types of systems needed to describe natural languages.

This mathematization of some parts of linguistics is to be welcomed. It has, of course, brought about considerable problems of communication. Fortunately, the parts of mathematics and modern logical theory that are needed to follow these developments are fairly elementary and limited (as yet), even though it is easy to find professional mathematicians who are not familiar with them. Courses in those parts of mathematics that are necessary, and in the abstract theory of grammars have been tried out at a number of institutions, and several people are working on elementary expositions of the basic ideas in order to alleviate this communication problem.

Revolutions always seem to bring about a reassessment of history, whether intellectual or political. Such a re-evaluation of the history of linguistics has been a prominent—and I think a healthy—part of the recent discussions. If statements and hypotheses are evaluated on the basis of how they were discovered, it is too easy to dismiss the efforts of earlier generations. There was a time when a reviewer could reject a book on linguistics merely by stating that the analysis was not "based on" formal criteria or because the author used psychological or "mentalistic" terminology. This time has passed. Recent years have seen the rehabilitation of the important works of men like Otto Jespersen or Wilhelm von Humboldt: and a work like the *Grammaire générale et raisonnée* of the grammarians of Port-Royal, Arnauld and Lancelot, which was held up to ridicule not too many years ago as an example of the much maligned attempts at "universal grammar," is cited

by Chomsky in his recent lectures at Indiana University as embodying the basic notions of the newest formulations of transformational grammar.¹⁹

Linguists are notoriously cantankerous creatures. When the polemical smoke has drifted away and a sober evaluation of the recent decade becomes possible, I think it will show that what seems now to be a series of reversals and abrupt changes, has been in fact a fairly steady progression. While it seems important now to stress the differences between the newer approaches and the immediately preceding ones (with a kind of "grandfather" law operating to forge a link to the last-but-one generation), a calmer look will show that the present advance guard was after all continuing the work of its mentors. But then, no doubt, we will all be under attack from some new and totally unexpected quarter, and the "existentialist trans-mogrificationist" school at the Sorbonne or the new Technical University of Goose Bay will be wondering why we do not give up the trivial to accept the obvious. I hope so, because controversy is the breath of science and when we all agree it will be only because our science is dead.

¹⁹ "Topics in the Theory of Generative Grammars," to appear in T. A. Sebeok, ed., *Current Trends in Linguistics*, III.