Chapter II

A Reconsideration of Some Issues in Structural Linguistics

A few words of explanation are in order as to the raison d'être of this chapter. It may well be thought peculiar that it is held necessary to unearth controversies which occurred in structural linguistics some three decades after the fact. But because a pervasive mythology has sprung up concerning this pre-generativist or 'taxonomic'[1] period of linguistics, a mythology which continues to be perpetuated as if bearing anything more than a casual relation to actual figures, issues and events, another view is arguably warranted. This is primarily because the issues which arose in this period of structural theorizing have not lost their relevance to linguistic theory and its philosophical understanding, although it will require a bit of historical uncovering to recognize just what these issues are and to distinguish them from the caricatures by which they are currently recognized and even taught. Our overall concern with the validation and justification of grammars therefore will benefit from a retrospective 'clearing of the air' about issues such as the status of meaning in linguistic analysis and the doctrine of autonomy of linguistic form - the possibility of linguistic analysis without, in principle, relying on meaning, on the related issues of "mechanical discovery procedures", non-uniqueness of linguistic description and criteria of adequacy for grammars. Our treatment here is restricted to rectifying misconceptions and we must defer to later chapters a full presentation of the approach to validation of grammars we are concerned to demonstrate. But this reassessment of issues will set the stage for the appearance of Chomsky's Logical Structure of Linguistic Theory in June, 1955, a work which has determined, in broad outline, the

[1] Chomsky's term; see, e.g., Mehta (1971:65).

metatheoretical perspective of much of linguistic theory up until the present day. To assess correctly the point of departure of this work and its genuine innovations requires then that we reevaluate what has become the 'received view' of these issues in this period of American linguistics, a view (e.g., Newmeyer (1980: 20)) which holds that "The essence of Chomsky's revolution in linguistics was his gift to the field of a truly scientific perspective. ...he characterized a grammar simply as 'a theory of language,' and rejected the empiricist view of one as a mechanically constructable abbreviation of (sic) corpus." and that "A truly alternative theory with any credibility has yet to emerge". [1] We shall try to extricate the matters of substance in the examination of the scientific status of grammatical theories from a remarkably successful partisan historiography based more upon emulation of authority than upon any perceptible familiarity with the fundamental texts of the period. [2] Our brief examination of these historical topics cannot, in any sense, be considered an adequate treatment. Its sole purpose is to provide an initial pathway through the inaccuracies of existing accounts which have become standard, and as a spur to encourage further efforts in this direction.

[1] Lest it be thought these remarks are not representative, see the references cited throughout this chapter. It is, however, difficult to be even-handed with a work which issues such declarations as "The dominant intellectual force in the United States from the 1930's to the 1960's was empiricism" (3) and "On the basis of this idealization (i.e., "a linguistic theory is a formal model of a speaker's abstract linguistic competence" - TR), more has been learned about the nature of language in the last 25 years than in the previous 2500"(250). The extent of the problem of rectifying the historical account may be appreciated by perusing the favorable review of this work by D.J. Napoli (1981) in Language, still the most prestigious journal in American linguistics.

[2] On the legitimizing function of partisan disciplinary histories, see Graham (1983).

2.1 Bloomfield. Many misperceptions center around the central figure of Bloomfield; in particular, these regard his attack on "mentalism" and the nature of his views about meaning. We review these in turn.

2.1.1 Anti-mentalism. In his belief that "mechanism is the necessary form of scientific discourse" (1933 :vii) Bloomfield held that a genuine science of language including linguistics could ultimately only be based in a "materialistic (or better, mechanistic) theory" of psychology (33). As regards psychological theory, mechanism, as Bloomfield understood it, entailed that "human actions...are part of cause-and-effect sequences exactly like those we observe, say in the study of physics or chemistry". Elsewhere, Bloomfield considers the terms 'behaviorism', 'mechanism', operationalism' and 'physicalism' interchangably denominating what was a view of scientific method rather than a proposal demarcating the scope of science. [1] The target of these general remarks is mentalism, a theory which "supposes that the variability of human conduct is due to the interference of some non-physical factor, a spirit, or will or mind...that is present in every human being" (32), a characterization which shows that Bloomfield's concern is to confront linguistic doctrines which on analysis are seen to rely on or imply some form of vitalism or teleology.

[1] (1939a :13): "...we can distinguish science from other phases of human activity by agreeing that science shall deal only with events that are accessible in their time and place to any and all observers (strict behaviorism)or only with events that are placed in co-ordinates of time and space (mechanism), or that science shall employ only such initial statements and predictions as lead to definite handling operations (operationalism), or only terms such as are derivable by rigid definition from a set of every day terms concerning physical happenings (physicalism). These several formulations, independently reached by different scientists, all lead to the same delimitation and this delimitation does not restrict the subject matter of science but rather characterizes its method."

Bloomfield's characterization of mentalism has been thought peculiar [1] from the standpoint of a "sophisticated" latter-day mentalism compatible with a causal account of linguistic behavior (Katz, 1964). Sophisticated mentalism refers ultimately to the causal role of neurological mechanisms of which actual linguistic communications are the observable effects. Although mentalist terminology cannot yet be given a ready neurological translation, the mentalist is not, by this fact, committed to any form of dualism or non-physical causation. As Katz admits, there is nothing incompatible with Bloomfield's proscriptions against mentalism in this (74). The genuine issue, Katz conjectures, is over the legitimacy of appeal to hypothetical constructs in fashioning statements of linguistic description, statements which, according to the latter-day mentalist, play a role in explaining various facts about linguistic behavior. We are to understand Bloomfield's opposition to mentalism as due to the observational inaccessibility of the "electrochemical events going on" inside "a brain mechanism" (77) coupled with Bloomfield's adherence to a strictly empiricist methodology which views "the rules and ordering restrictions of a linguistic description as simply convenient fictions" (83):

> It is perhaps because, from the behaviorist viewpoint, this observational inaccessibility of the neural mechanism represents the boundary of the subject matter of linguistics, that taxonomic linguists have denied that theoretical concepts in a linguistic theory can have psychological reality (77).

From this point of view, Bloomfield's objections reduce to an unwarranted delimitation of linguistics, of "excluding from linguistics, a priori and

[1] "Bloomfield criticized, not mentalism in the contemporary sense of this term, but a highly theologized conception..." (74)

arbitrarily, just what is most important for this science to do"(84). Provision of explanations in linguistics requires reference to "the mental events, capacities, and processes of speakers" which underlie "the facts of linguistic performance", i.e., linguistic behavior (75).[1]

Katz' criticisms raise a number of issues which will concern us throughout this work. But with respect to Bloomfield, several clarifications must be made. First, in attacking mentalism Bloomfield was not tilting with "occult entities" as Katz would have it. In particular, if examination is made of how the issue of mentalism is addressed throughout his writings, rather than relying exclusively on the summary remarks provided in his major work of 1933, a very different purpose can be inferred. By repeatedly raising the spectre of mentalism, Bloomfield admonished against an all-too-common careless and often unrecognized employment of psychologistic and teleological terminology, a usage of which he himself had not been sufficiently critical in his earlier book of 1914.[2] Among his targets of criticism in the many reviews of the 1920's and 1930's were not only linguists who gave traditional teleological explanations of the origins of speech forms [3] but also linguists of the stature of Sapir and Jespersen, the former for asking as to "the absolutely essential concepts in speech" [4], the latter for remarking that the loss of inflectional endings in English was due to their no longer being needed for the expression of meaning.[5] Bloomfield also took

[1] One may note in this early paper of Katz the assumption of a close parallel between a grammar of a language and a model of the speaker or hearer, an assumption which Chomsky has been careful to deny.

[2] Bloomfield, 1914 a. Cf. "Preface" to his(1933) and (1927)

[3] "Review of Havers" (1934)

[4] "Review of Sapir" (1922 a)

[5] "Review of Jespersen's LANGUAGE" (1922 b)

issue with such staunch empiricists as Carnap and Neurath, Bridgeman, and Pearson [1] for using terms like "concept" , "thing-word" and "idea". But he was, as seen above, in sympathy with the physicalism propounded by Carnap and Neurath, and welcomed their efforts to insure the meaningfulness of scientific statements.

> Carnap and Neurath agree, then, with the American students in saying the mentalistic phraseology, in so far as it is not nonsensical, is only a troublesome duplication of linguistic phraseology. (1936 :324)

The lesson which Bloomfield drew again and again was the general uselessness of mentalistic terminology and its likely contribution to obfuscation and confusion. The employment of this commonsense vernacular in scientific discourse, though perhaps only intended as a short cut via familiar modes of speech, in fact helped to create pseudoproblems due to a scarcely avoidable tendency to hypostatize entities corresponding to the mentalist terms. [2]

Secondly, for Bloomfield, it was a serious misconception to think that reference to biological mechanisms or psychological processes of the individual speaker - however hypothetically characterized - is required to account for (or "explain") language structure or the patterning of linguistic forms or any of the various aspects of linguistic behavior. The linguist's description of a language was a description of a system of distinctive linguistic forms - forms distinguished by an observable increment or difference of meaning as revealed in the speech habits or uses of language

[1] "Language or Ideas" (1936)

[2] "It is the belief of the present writer that the scientific description of the universe...requires none of the mentalistic terms, because the gaps which these terms are intended to bridge exist only so long as language is left out of account." (1939a:13) Cf. Wells (1962: 708): "What Bloomfield was unclearly driving at in his well-known attack on mentalism... was that propositions connecting a way of speaking with a way of thinking are not empirical but a priori propositions, unless there is some logically independent evidence for a given way of thinking other than the way of talking itself."

in a given speech community. For Bloomfield it was a kind of category mistake to appeal to genetic endowment or to developmental psychological or biological mechanisms as significantly involved in the determination of linguistic structure.[1] The psychology of interest to the linguist was not cognitive psychology but social psychology:[2]

> To the linguist who is interested in the implications of his method, no psychology can be acceptable which tries to explain on an individual basis phenomena which he knows to be historically conditioned by the social group. (1927a:174)
>
> Practice shows that descriptive study involves the full measure of scientific generalization and classification; only by a scientific process can one abstract from a series of actual speech utterances the socially-determined features and their systemic patterning. (1927a:179)
>
> A person's membership in a speech community is not merely something that is superadded to his existence as a biological unit. Human behavior is entirely permeated by social factors. With the possible exception of some physiological processes, the activities of a human individual cannot be classified or predicted on the sole basis of biological equipment, but depend very largely on the society in which he lives and upon his place in this society. (1942: 397)

The issue separating Bloomfield and Katz is not, therefore, the former's unwillingness to license hypothetical entities nor the latter's more up-to-date preoccupance with explanatory models and stress upon the role of theory in scientific practice. The genuine issue between them concerns

[1] Cf. Chomsky (1976: 164): "...we can explain some property of attained linguistic competence by showing that this property necessarily results from the interplay of the genetically-determined language faculty, specified by UG, and the person's (accidental) experience."

[2] The socially-determined character of language structure was a principal assumption of Bloomfield's early work of 1914 which was expressly based on the psychology of Wundt. E.g., "Such mental processes, then, as those involved in the utterance of speech cannot find their explanation in the individual, - he receives his speech habits from others, - but must be traced for explanation from individual to individual ad infinitum. They are products of the mental action not of a single person, but of a community of individuals. These products, - not only language but also myth, art, and custom, - are the data which make possible the second phase of psychology, social psychology, (German, Völkerpsychologie)." (1914a: 324)

differing perspectives about the nature of language structure - as to whether it is to be understood as primarily attributable to underlying biological causal processes. Bloomfield's antimentalism should thus be seen in conjunction with and as an aspect - admittedly polemical - of an unchanging belief that explanations of linguistic form be consistent with the fundamentally social character of language.

Adherence to mechanism meant, according to Bloomfield, to adopt the premise that a human organism is a causally-determined system just as the processes encountered in physics and chemistry, only of a very complex kind. Indeed, it is all but impossible to provide a complete causal account for even the simplest changes in the state of a human body. But in principle mechanism implies the truth of a deterministic theory of human behavior: it would be possible to predict human behavior with sufficient knowledge of all the relevant variables.

> We could foretell a person's actions (for instance, whether a certain stimulus will lead him to speak, and, if so, the exact words he will utter), only if we knew the exact structure of his body at the moment, or, what comes to the same thing, if we knew the exact make-up of his organism at some early stage - say at birth or before - and then had a record of every change in that organism, including every stimulus that had ever affected the organism. (1933: 33)

Such a view has standing only as a guiding or regulative ideal which proscribes mentalist pseudo-explanations. The obvious practical impossibility of specifying the continuous history of a person from birth means that the investigator of language and linguistic behavior must resort to "indirect methods of approach" - observing the responses of individuals to carefully controlled stimuli, observing human responses in the mass or aggregate, observing conventional actions varying from speech community to speech community, and so on. (1933: 37-8)

2.1.2 "Exclusion of semantics". It is with respect to these "indirect methods of approach" to the study of language that Bloomfield's oft-cited 'definition' of meaning should be viewed:

> We have defined the meaning of a linguistic form as the situation in which the speaker utters it and the response it calls forth in the hearer. (1933 : 139)

This formulation, for all its behavioristic guise, should not, as is often done, be taken as definitive of Bloomfield's view of meaning. He himself used the term with sufficiently wider latitude extending far beyond the confines of "situation of utterance " and "response called forth" and was elsewhere more forthcoming in attempting to 'define' meaning, e.g.,

> The term 'meaning', which is used by all linguists, is necessarily inclusive, since it must embrace all aspects of semiosis that may be distinguished by a philosophical or logical analysis: relation, on various levels, of speech-forms to other speech-forms, relation of speech-forms to non-verbal situations (objects, events, etc.), and relations, again on various levels, to the persons who are participating in the act of communication. (1939 a : 18)

In fact, since a causal account of the occurrence of a particular utterance would require a completed scientific theory of nature, of which human organisms are considered a part, determining all the actual conditions of the occasion ("situation") of an utterance is not a possible goal of investigation.

> The situations which prompt people to utter speech, include every object and happening in the universe. In order to give a scientifically accurate definition of meaning for every form of a language, we should have to have a scientifically accurate knowledge of everything in the speaker's world. The actual extent of human knowledge is very small compared to this. (1933: 139)

In some instances of meaning it is, however, currently possible to be more precise than in others, as in those aspects of experience of which we have scientific knowledge. Here, Bloomfield again departs from speaking of

meaning in terms of "situation of utterance" and "response called forth" and invokes a notion of reference, as determined by scientific theory.

> We can define the meaning of a speech-form accurately when this meaning has to do with some matter of which we possess scientific knowledge. We can define the names of minerals, for example, in terms of chemistry and mineralology as when we say that the ordinary meaning of the English word salt is 'sodium chloride (NaCl)',...(ibid)

Some commentators (e.g., Julià, 1983:26) have accordingly taken Bloomfield as advocating "a restriction of 'meaning' to the traditional notion of 'reference'" but the example clearly shows that Bloomfield has here in mind a limiting case. It is certainly problematic as to how meaning specified in terms of reference is to be incorporated within the wider view of the meaning of a linguistic form cited above, and Bloomfield, to my knowledge, does not discuss the nature of the relation between the two. It may not be unwarranted to draw analogies with what have more recently been called "causal theories" of naming or reference. A similar account might link the particular chemical properties of sodium chloride with, say, physiological and chemical processes in human beings ('taste') and the social and cultural significance of these processes. Ultimately, the chemical properties of sodium chloride might be connected to the situation in which a speaker asks for the salt to be passed, in the standard example. Such an account can and must allow for a great deal of nuance and elaboration. For example, it is very probably part of the meaning of salt (i.e., sodium chloride) among many educated people in the contemporary United States that the taste for salt is to a large degree a habit which exhibits mild properties of addiction, that very little salt - which is naturally present in a balanced diet - is required for health, and that overuse of salt - an ingestion which in the past was considered 'normal' - has been linked to diseases such as hypertension, chronic high blood pressure,

and heart disease. Little, if any, of this 'meaning' of salt was available in Bloomfield's day, yet it perhaps gives an indication of how a socially acquired meaning, including the situations in which salt is uttered (for some speakers), is in part explicable by reference to results in physiological chemistry.

Bloomfield was careful to stress that for most linguistic forms there was no precise definition of meaning available:

> ...but we have no precise way of defining words like love or hate, which concern situations that have not been accurately classified - and these latter are in the great majority. (1933 : 139)

Remarks such as these have often been taken as indicating an exclusion of meaning or semantics from linguistic investigations. But in fact it was a well-advised caution about defining meaning which guides Bloomfield's many references to meaning. And, in the appeal to science for what definitio- of meaning as could be made, Bloomfield comes very close to making the same point Quine expresses by holding that a principled distinction between language and theory is not available. [1] Bloomfield's reluctance to provide definitions of meaning and his hesitance in speaking in general terms of anything more definite than the commonplace that "the features of situation and action which are common to all utterances of a speech-form are the meaning of that speech-form" (which in its vagueness, Bloomfield admits, "sets off a great many problems"(1943 : 401)) have led to an almost incredible distortion of his views in the writings of Katz [2],

[1] Quine (1969c:308-11).

[2] E.g., Katz (1972 : xxii) who speaks of "the Bloomfieldian exclusion of semantics" and notes: "Since taxonomic grammars do no more than regroup and reclassify the speech sounds of utterances, there is no place for meaning in their description of the grammatical structure of utterances, and therefore, semantics must be thought of as outside grammar." On "taxonomic grammars", see the discussion below.

and Fodor [1] and others. In these writings there appears a classic employment of the old rhetorical device of characterizing guarded statements about a particular subject-matter as in principle declarations that nothing more can be said about the subject-matter. Pending changes in fashion or statements to the contrary or scrutiny of the relevant texts, the attributions enter the literature as historically sound.

It will be instructive to consider briefly just what role meaning and reference to meaning occupy in Bloomfield's principle work in order to see how far off the mark is the mythology of which the comments of Katz and Fodor are typical. In phonology, for example, Bloomfield stressed that meaning was necessary to establish phonemic identity, in particular, in determining whether two speech-forms were "the same" or "different".

> As long as we pay no attention to meaning, we cannot decide whether two utterances are "the same" or "different"...To recognize the distinctive features of a language, we must leave the ground of pure phonetics and act as though science had progressed far enough to identify all the situations and responses that make up the meaning of speech-forms. In the case of our own language, we trust to our everyday knowledge to tell us whether speech-forms are "the same" or "different"... (1933: 77)

> The study of significant speech-sounds is phonology... Phonology involves the consideration of meaning. The meaning of speech-forms could be scientifically defined only if all branches of science including especially, psychology and physiology, were close to perfection. Until that time, phonology and, with it, all the semantic phase of language study, rests upon an assumption, the fundamental assumption of linguistics; we must assume that in every speech community some utterances are alike in form and meaning...In order to recognize the distinctive features of forms in our own language, we need only determine which features of sound are "different" for purposes of communication.

[1] In objecting to a view which he attributes to Putnam, viz., that reference and meaning of words like 'water' are established only with respect to a prior scientific theory and not to psychological states of speakers of the language, Fodor (1980: 248) writes: "Bloomfield argues that, for all practical purposes, you can't do semantics. The reason that you can't is that to do semantics you have to be able to say, for example, what 'salt' refers to. But what 'salt' refers to is NaCl, and that's a bit of chemistry, not linguistics."

> ...even a perfected knowledge of acoustics will not, by itself, give us the phonetic structure of a language. We shall always have to know which of the gross acoustic features are, by virtue of meanings, "the same", and which "different" for the speakers. The only guide to this is the speaker's situation and the hearer's response. Any description which fails to discriminate the distinctive features from the non-distinctive, can tell us little or nothing about the structure of a language. (128, added emphasis)

> It is important to remember the practical phonetics and phonology presuppose a knowledge of meanings: without this knowledge we could not ascertain the phonemic features. (137-8)

So far from excluding semantics from grammar (outside of phonology), Bloomfield explicitly subsumed grammar under the more general heading of semantics:

> When the phonology of a language has been established, there remains the task of telling what meanings are attached to the several phonetic forms. This phase of description is semantics. It is ordinarily divided into two parts, grammar and lexicon. (138) [1]

The significance as well as the possibility of a descriptive linguistics rested upon the unverifiable assumption that each linguistic form had a definite meaning, a meaning different from that of any other linguistic form in the same language. Linguistic description is simply description of distinctive linguistic forms, forms which, on grounds of meaning, can be distinguished as "different" from other forms. Description without regard to meaning makes no sense at all.

> We assume that each linguistic form has a constant and definite meaning, different from the meaning of any other linguistic form in the same language. We have seen that this assumption cannot be verified,...In the rough, however, our assumption is justified by the mere fact that speakers co-operate in a very refined way by means of language-signals. In describing a language, we are

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> primarily concerned with the working of this co-operation at any one time in any one community, and not with its success and failures or with its changes in the course of history. Accordingly, the descriptive phase of linguistics consists in a somewhat rigid analysis of speech-forms, on the assumption that these speech-forms have constant and definite meanings. (158)
>
> In language, forms cannot be separated from their meanings. It would be uninteresting and perhaps not very profitable to study the mere sound of a language without any consideration of meaning... In studying a language, we can single out the relevant features of sound only if we know the meaning. This appears plainly when one confronts an unfamiliar language...It is only the differences of meaning which decide that most of the inevitable variations of sound are irrelevant and only certain ones play a part in communication. In short, the significant sequences of sound (the phonemes) of a language are, of course, those which involve a difference of meaning. (1943 : 401-2)

Far from advocating the construction or feasibility of grammars through the exclusion of semantics, it is, to the contrary, textually accurate to say that Bloomfield throughout his career based grammar and descriptive linguistics in general on semantics. But semantics, pace the difficulties involved in trying to state meanings, was understood as differential meaning, the distinctive meaning increment or contribution of each linguistic form. The relation of form to meaning should not, therefore, be considered as a relation of assigned correspondence, such as might be given in an explicitly formulated semantical metalanguage. Form is only linguistically identifiable insofar as it can be determined to have meaning, i.e., to be distinctive.[1] Crucially, this latter condition does not require that the linguist be able to state or define meaning, a project Bloomfield saw as fraught with a priorist and ad hoc assumptions, assumptions which ~~are~~ become no less problematic when one terms ~~pronounced by terming~~ this project the construction of a semantic theory. It suffices for the linguist to determine whether speakers of the language 'recognize' a form as distinctive, whether it is "the same" or "different" from some previously identified form. Otherwise put, meaning is required to determine

[1] Another way of putting this is to say that language has no external metalanguage to designate its elements; see Chapter 5 §1 below.

which forms are repetitions. Precisely this problem - that of determining repetition - and the assumed nature of form and meaning relations which make this problem significant, establishes the theoretical, not merely methodological importance of operational tests like Harris' paired utterance test [1] and of the requirement that grammatical transformations be paraphrastic or information preserving (in a sense specified below in Chapters 5 and 6).

2.2 'Autonomy of form'. Bloomfield's views on the relation of form and meaning are, as Hockett has recognized (1968:19), the very opposite of a doctrine of autonomy of linguistic form, of a 'formal syntax' and an 'interpretive semantics'. Yet Chomsky, and many writers following Chomsky, have claimed that the doctrine of autonomy of form was a fundamental tenet of structural linguistics. Given Bloomfield's central position in structural linguistics, and seemingly consistent with his alleged antipathy to meaning, the autonomy doctrine is often attributed to him, as well as to figures such as Harris and Bloch. For example,

> Structural analysis studies language as an abstraction. The outcome is a conception of the subject matter as a self-contained, independent system of forms that can presumably be described without explicit appeal to speakers, listeners, and their common environments. (Julià, 1983: 40-1)

> (Harris) accepted Bloomfield's general position on the definition of meaning and the relations between form and meaning but denied that meaning could be used as anything more than an heuristic device in exact linguistic methodology...The independence and methodological priority of form over meaning is clearly affirmed. This assumption, that form is independent, may be regarded as one of the central conceptions of modern linguistic theory, and, ...it continues to be vigorously defended by such scholars as Chomsky...(Maclay, 1971 : 163)

> Chomsky inherits and maintains from his structuralist upbringing the conviction that syntax can and should be studied independently of semantics; that form is to be characterized independently of meaning. (Searle, 1972 : 15)

[1] Harris (1951a: 32). See the discussion in Chapter 3 below.

> He (Chomsky) followed the structuralists in maintaining that phonology and syntax should be described as a purely formal system without reference to meaning or semantics. (Bornstein, 1976:178)

> Superficially, the relationship between syntax and semantics seems quite straightforward in Syntactic Structures and can be captured by the following quote: "I think that we are forced to conclude that grammar is autonomous and independent of meaning...(p.17)" The independence of grammar and meaning is stressed so many times in that book that many commentators have assumed that he simply took over the position of Harris and Bloch, an assumption often going hand-in-hand with the implication that this demonstrates that he had not really broken completely from structuralism. (Newmeyer, 1980: 31)

> I am going to side with those (like Harris and Chomsky) who maintain that the grammatical structure of a language can be specified without first having settled any semantic questions. (Alston, 1962: 712)

As we shall see in Chapter 3,from its inception generative grammar has promoted a doctrine of formalism, that the theory of linguistic form is characterizable independently of questions of meaning. [1] Chomsky apparently saw - mistakenly we shall argue - in the procedures of distributional analysis a strong commitment to a principle of this kind, a commitment which could be represented as advocating the development of "mechanical discovery procedures" for grammars (see §2.4 below). He has in fact directly traced the pedigree of the doctrine of autonomy of form to structural linguistics and his opinions on this topic have usually been taken as authoritative.

> A central idea of much of structural linguistics was that the formal devices of language should be studied independently of their use. The earliest work in transformational-generative grammar took over a version of this thesis, as a working hypothesis. I think it has been a fruitful hypothesis. It seems that grammars contain a substructure of perfectly formal rules operating on phrase-markers in narrowly circumscribed ways. Not only are these rules independent of meaning or sound in their function, but it may also be that the choice of these devices by the language learner (i.e., the choice of grammar on the basis of data) may be independent, to a significant extent, of conditions of meaning and use. If we could specify the

[1] See Chomsky (1975b) for a comparison of "varying degrees of strength" of a "thesis of autonomy of formal grammar".

> extent precisely, the working hypothesis would become a true empirical hypothesis. Such an effort may be premature. It does, however, seem noteworthy that the extensive studies of meaning and use that have been undertaken in recent years have not... given any serious indication that questions of meaning and use are involved in the functioning or choice of grammars in ways beyond those considered in the earliest speculations about these matters, say in Chomsky (1957). (Chomsky, 1969 : 198-9)

We shall examine these "earliest speculations" in Chapter 3. For our present purposes, we note that the thesis of the autonomy of form, here attributed to structural linguistics, has in large measure determined the conception of a grammar as a system of "components" - "syntactic", "semantic", "phonological", etc. - and of the relations between these (i.e., mappings among the various levels of "representation"). Indeed, more recently, the componential view of grammars has been seen as only an instance of a wider ranging theory of "mind" as "modular" in structure, incorporating "perceptual knowledge" and "conceptual knowledge" "subcomponents" interacting in various ways with each other and with the grammatical module (Lightfoot (1982: Chapt. 3); Fodor (1983)). Close parallels with the theory of formal languages and formal systems have not been overlooked and the subsequent conception of a grammar has been widely taken to be definitive:

> A basic assumption in the view introduced by Chomsky is that an essential part of any human language is an abstract formal system, largely unconscious (sic), which specifies the internal structure of a sentence at various levels of analysis and defines classes of grammatical constructions. To an extent to be determined, this abstract formal system, characterizable by recursive sets of rules is independent of sound, meaning, and use. One of Chomsky's main contributions to linguistics as a science is that he offered a means to characterize the abstract formal properties of a sentence in a <u>syntactic structure</u> which is generable by a recursive set of phrase structure rules and transformations. (Dougherty, 1975: 178)

> The central task of syntax is to give a finite description of the infinite set of sentences of a given natural language. The basic observation underlying the acceptance of this task as central is that the native speaker of a language can produce and understand sentences he has never produced or encountered before, that there is in principle no upper bound on the length of sentences, and that the brain is finite. The form of a solution to this task, agreed on by linguists and philosophers alike, is to specify the finite set of lexical items of the language and a finite set of syntactic rules which, taken together, generate the infinite set of sentences. (Partee, 1979 : 196)

Chomsky's attribution of a doctrine of autonomy of form to structural linguistics stems in part from the widespread misperception or misunderstanding of Bloomfield's views about the role of meaning in linguistic analysis and of the relation of form and meaning in grammar, and in part from certain statements of several post-Bloomfieldian linguists who were, however, usually careful to point to the divergence with Bloomfield's own position (see the remarks of Hockett cited below). The tendency to see in the explicit formulation of distributional procedures an advocacy of a doctrine of the independence of form and meaning (thus leading to Chomsky's appelation of these as "mechanical discovery procedures") confuses the expressly stated purpose of these formal procedures. As we shall see presently, formal (distributional) procedures as developed by Harris, Bloch and others, were not intended to 'discover' grammatical structure. Instead, the considerable effort devoted to making these procedures fully explicit sought to provide a means of justifying grammatical statements arrived at by whatever 'shortcuts' including 'appeal to meaning' and even intuitive criteria. It is simply incorrect to see in rigorously formulated distributional procedures a version or incipient expression of the doctrine of autonomy of linguistic form. For distributionalism, as for the continental tradition of structural linguistics stemming from Saussure and represented in the United States perhaps most

prominently by Jakobson, grammatical form is not autonomous with respect to meaning, it indicates or signals meaning. [1] Bloomfield, again, was particularly adamant on this point:

> The grammatical forms are no exception to the necessary principle - strictly speaking, we should call it an assumption - that a language can convey only such meanings as are attached to some formal features: the speakers can signal only by means of signals. (1933: 168)

The origins of the doctrine of the autonomy of linguistic form, of a syntax somehow specified independently of meaning, do not lie in structural or distributional linguistics, in the writings of Saussure or Sapir or Bloomfield or Harris. The attribution of this doctrine to structural linguistics is yet plausible if indeed the goal of structural theorizing can be construed as proposing "mechanical discovery procedures" for grammars. We turn our attention to this issue in § 2.4 below.

2.3 'Post-Bloomfieldians'. That a wholesale 'exclusion of meaning' approach was espoused by all or nearly all the central figures active in structural or descriptive [2] linguistics in the immediate post-war period has become a central dogma of what passes as intradisciplinary history in American linguistics. As briefly indicated above, this conception has played an important role in the subsequent development of linguistic theorizing in generative grammar. Thus, distributionalism, as practiced by those considered to be followers of Bloomfield, seemed compatible with a rigid formalism, a view that linguistic analysis could and should be performed as a schedule of procedures applied to a corpus, without regard to meaning and even as

[1] For Saussure, see especially Godel (1957) on the problem of identity of linguistic elements, 136 ff. Also valuable are Godel (1966) and Engler (1974). For Jakobson, see e.g., (1952)

[2] On the relation of these two terms see Hymes and Fought (1981: 8-10).

-1 has told me he wanted to call MSL (1951) Methods in Descriptive Linguistics but the editors wanted the buzzword "structural". He didn't remember whether or not they asked his permission for the change!

eliminating the linguist or theorist altogether. [1] As in the case of Bloomfield, we find that this view does not withstand scrutiny. Such opinions were never, e.g., maintained by Harris, perhaps the major figure of this period, who presented the most rigorous and articulated survey of distributional procedures (1951a). We will not attempt to trace all of the details of the failure to understand the relation between distributional procedures and meaning. Instead, we will focus briefly on the role of formal procedures as attested by Harris and on a retrospective look at this period by Hockett, another central figure, which illustrates just how little shared was Harris' understanding of distributionalism.

For Harris, just as distributional procedures rather than some absolute scale of shortest sounds, or most frequent sounds, or sounds with certain articulatory or acoustic properties, were used to identify the phonemes of a language, so also distribution rather than meaning was the determinative criterion in setting up the morphemes. [2] Some influential writers (e.g., Putnam, 1961) have seen in Harris' procedure yielding the morphemes of a language without any reference to meaning or informant response an intent to provide a "uniform discovery procedure" for language structure.[3] However,

[1] Putnam (1961:94).

[2] Harris (1951a), Chapter 12.

[3] Roughly this procedure successively compares phonemic stretches of a test utterance with many other utterances which have the same initial phoneme, then the same first two initial phonemes and so on. If a sufficiently large number of utterances are compared in this way, a morphemic boundary may be tentatively imposed after the nth phoneme in the test utterance just in case the number of phonemes which follow the first n phonemes in the associated utterances is greater than the number which follow the first $n+1$ phonemes or the first $n-1$ phonemes.

even in this early work, Harris is careful to point to the insufficiency of a purely formal procedure for morphemic segmentation as "it leaves unstated many facts about these utterances, correlations between these utterances, and phenomena not described by current descriptive linguistics" (186),and, in a lengthy appendix Harris indicates that the goal in formulating distributional regularities is to establish "elements which will correlate with meanings" (188), and provides detailed examples of the sorts of considerations he has in mind.[1]

[1] To take only one example, Harris (193-4) raises the problem that gl-, sl- as in slide, slimy, glide, gleam have a seeming morphemic status in that there is a "partial similarity in meaning among the words beginning with sl-, gl- respectively"; however, "no adequate distributional basis can be found for supporting this segmentation." The problem with relying upon a common meaning criterion, of course, is that it does not provide any reliable basis for deciding, e.g., in the case of the gl- words, which have the common meaning and which do not. Are glimmer, gloss, glory gloom, glad, globe, gladiator to be included in this set ? Despite the vagueness of the common meaning criterion and the inability of distributional analysis to provide a formal basis for the morphemic status of gl-, sl- , the linguist cannot ignore this fact about meaning in his characterization of English.

> Difficult as it may be to argue for morphemic status for sequences like gl- , it is also unsatisfactory to leave unstated the fact that so many sequences beginning with gl- have partial similarity in meaning. The solution is not, of course, to cast a deciding vote one way or the other, but to relate this situation, precisely as it is, to the other facts about the language. The sequence gl- is not a distributionally separable element; therefore it is not a morpheme in the definition which applied to -er, -ceive, con-, yes. But gl- exhibits, in many morphemes, a correlation of meaning and phonemic form, of a type which is also true of most of the distributionally separable morphemes as a whole. At some point in our organization of the linguistic data, e.g., at the point where we say that most or all of the morphemes have assignable meaning, or at the beginnings of the gl-, sl- ... entries in the dictionary, we would state that very many of the morphemes beginning with gl- (...) have some reference to light, etc., and so for the other sets.

A few years later in ~~a rare~~ paper devoted entirely to discussion of theoretical and methodological issues, Harris returned to the topic of the insufficiency of purely formal distributional criteria in the characterization of language structure.

> Distribution suffices to determine the phonemes and morphemes, and to state a grammar in terms of these. However, both (a) in determining the elements and (b) in stating the relations between them, it turns out that the distributional structure does not give ideal coverage. It must either leave many details unsaid, or else become extremely complicated. (1954 : 784)

What the linguist seeks in investigating distributional regularities are "interesting distributional relations which tell us something about the occurrence of elements and which correlate with some aspect of meaning" (785). Since distributional differences could in many cases be seen to correlate with difference in meaning, the prospect was raised of actually giving meaning, or particular aspects of meaning, a formal description: "In certain important cases it will even prove possible to state certain aspects of meaning as functions of measurable distributional relations" (ibid). By the fact that distributional methods gave a precise content to the elements distinguished in a grammar, they also provided a needed check upon statements of language structure. Thus in requiring that structural statements be specified in distributional terms, the intent was to insure the objectivity of the final result, but this requirement, in itself, does not prescribe how results are to be obtained.

> As Leonard Bloomfield pointed out, it frequently happens that when we do not rest with the explanation that something is due to meaning, we discover that it has a formal regularity or 'explanation'. It may still be 'due to meaning' in one sense, but it accords with a distributional regularity. (785)

The interest in formal procedures and formal statement, as evidenced in these remarks as well as many others, is thus not at all "to eliminate the theorist altogether" in the attempt to construct correct grammars of a language, but rather to obtain objectively ascertainable statements of distributional regularities which, moreover, were "interesting" in that they provided a structural analysis, in part at least, of meaning. [1] Yet because Harris stressed that the determining criteria of any analysis always be distributional, he has been accused of having "cast out meaning altogether" or, as having "proposed, as a theoretical possibility, the total exclusion of the use of meaning in analysis" (Fries, 1961 : 212, 216).

There were 'post-Bloomfieldian' linguists who did write that the domain of meaning or semantics lay outside of linguistics proper, constituting a province where sociologists or perhaps anthropologists could find productive employment. In an influential paper of 1950, Martin Joos gave a characteristically sharp formulation to this ideal:

> Physicists describe speech with continuous mathematics, such as Fourier analysis or the autocorrelation function. Linguists describe language instead, using a discontinuous or discrete mathematics called 'linguistics'. It treats speech communication as having a telegraphic structure....The telegraphic code structure of language is examined from top to bottom, and at each of its several levels of complexity (compared to the two levels of Morse code) its structure is shown to be defined by possibilities and impossibilities of combination among units of this kind. Above the highest level we find, instead of such absolute restrictions, conditional probabilities of occurrence: this is the semantic field, outside linguistics, where sociologists can work. (349)

It is easy, with hindsight, to see in such comments the kind of enthusiastic overstatement typically issued by a burgeoning and confident youthful discipline intent on carving the sharpest possible demarcation with what it sees as the hidebound approaches of the past. Certainly, the confidence was misplaced in

[1] We defer to Chapters 5 and 6 below a discussion of the correlation of distributional structure and meaning.

light of the events of the next several decades in American linguistics, although the rhetoric seems mild indeed as contrasted with that issued by the subsequent generation of linguists. But it must be observed that the 'exclusion of meaning' program was never championed by those linguists schooled in or preoccupied by (or, in the case of Harris, in fraternity with) the anthropological tradition of the study of the relation of language and culture in the manner which Boas and his student Sapir had pioneered. [1]

Hockett (1968) has retrospectively summarized how "we post-Bloomfieldian descriptivists of the 1940's were dissatisfied with several aspects of Bloomfield's portrayal of language". He continues, "We believed that our views were derived at bottom from his, that we were clarifying confusing details in his views by applying procedures and ways of thinking that he had taught us." In fact, Hockett admits, "our views actually differed from his in (an) important respect":

> ...Bloomfield had repeatedly insisted that the discussion of meaning is beset with difficulties; from this he himself had inferred, not that scientific linguistics is impossible, but merely that our characterization of a language should always start from form rather than from meaning. The approach via meaning held too much danger of introducing irrelevant philosophical apriorisms, or of imposing on one language semantic categories actually only relevant for some other. During the 1940's some of us suspected that it might be possible to determine the forms of a language, and all the patterns by which they combine into larger forms, without any reference to meaning at all. Some decided that this was not only possible, but indeed, the only rigorous procedure, even if occasional resort to meaning might be a useful practical shortcut (A footnote identifies Harris (1951a) as a notable source of the 'shortcut' view - TR)....We also considerably shifted the meaning of the term 'grammar'. We came to think of 'grammar' largely as the patterns by which meaningful forms (not mere phonemes) combine or arrange into larger forms - an autonomous set of patterns, unrelated to meaning, or at least susceptible to analysis and description as though it had nothing to do with meaning. (24-5)

[1] On the "anthropological tradition" in American linguistics, see Hymes and Fought (1981) passim and e.g., 71: "...meaning was consistently important in the line of fieldwork from Boas and Sapir to Newman, Swadesh, and Voegelin, and...the outstanding field workers in the post-war period, Swadesh and Pike, were particularly prominent in opposition to the tendency to neglect meaning."

In this passage it is clearly stated by one of the main participants in theoretical discussions of the period that the view of language structure or grammar as "an autonomous set of patterns unrelated to meaning or at least susceptible to analysis and description as though it had nothing to do with meaning" originated not with Bloomfield but with some of his followers (who here remain unnamed) apparently concerned to make Bloomfield's views more rigorous. The attribution of this doctrine to Harris is characteristically made but, as indicated above, is without foundation (see also § 2.4 below).

It is perhaps not accidental that the proscription of meaning from grammar by certain 'post-Bloomfieldian descriptivists' coincides with Quine's attack on meaning as expressed in the essays collected in Quine (1953) and especially in "The Problem of Meaning in Linguistics" delivered as a lecture to an audience of linguists at Ann Arbor in 1951. In this paper (see Chapter 3 §2) Quine offers a novel characterization of the grammarian's task - that of accounting for the infinitely many well-formed sequences of phonemes of a language - and a novel approach to a solution - to devise a recursive description of just this class of "significant" sequences. And Quine (1960) later specifies that the grammarian is describing a language which is "previously unstudied" and is known only to the grammarian through his field work, a Gedankenexperiment which is intended to lend credence to Quine's contention that considerations of meaning play no role in demarcating the class K of significant sequences. The particular influence of this essay on Chomsky's first major formulation of generative grammar is discussed further in Chapter 3. For the moment, our purpose is simply to call attention to a coalescence of currents that

have led to a revival in generative grammar of a traditional conception of grammar as a theory of form-meaning pairings. [1]

2.4 "Mechanical discovery procedures". Undoubtedly the most influential source of many current views of structural and descriptive linguistics is Chomsky's argument in Syntactic Structures (1957) against what he termed "mechanical discovery procedures". There, in a celebrated discussion of the goals of linguistic theory, Chomsky attributed to linguists such as Harris, Bloch, Hockett, and Wells the view that the aim of structuralist theorizing was the construction of analytic procedures by means of which a grammar could be practically derived from the raw data of speech. In contrast, Chomsky argued that the goals of linguistic theory be set 'no higher' than the formulation of an evaluation procedure for choosing between alternative grammars. We reproduce this discussion in full, omitting only the familiar 'black-box' input-output representations of the various alternatives proposed.

> The strongest requirement that could be placed on the relation between a theory of linguistic structure and particular grammars is that the theory must provide a practical and mechanical method for actually constructing the grammar, given a corpus of utterances. Let us say that such a theory provides us with a discovery procedure for grammars.
>
> A weaker requirement would be that the theory must provide a practical and mechanical method for determining whether or not a grammar proposed for a given corpus is, in fact, the best grammar of the language from which this corpus is drawn. Such a theory, which is not concerned with the question of how this grammar was constructed, might be said to provide a decision procedure for grammars.
>
> An even weaker requirement would be that given a corpus and given two proposed grammars G_1 and G_2, the theory must tell us which is the better grammar of the language from which the corpus is drawn. In this case we might say that the theory provides an evaluation procedure for grammars.

[1] See Chomsky's laudatory remarks on Jespersen's version (1924) of this view of a grammar in his (1975b), and contrast Bloomfield's comment in his review of this work (1927b: 142): "In the study of linguistic forms, therefore, I should not appeal, as Jespersen sometimes does, to meaning as if it were separable from form..." See Chapters 3 and 4 below.

> ...The point of view adopted here is that it is unreasonable to demand of linguistic theory that it provide anything more than a practical evaluation procedure for grammars. That is, we adopt the weakest of the three positions described above. As I interpret most of the more careful proposals for the development of linguistic theory,(3) they attempt to meet the strongest of these three requirements. That is, they attempt to state methods of analysis that an investigator might actually use, if he had the time, to construct a grammar of a language directly from the raw data. I think it is very questionable that this goal is attainable in any interesting way, and I suspect that any attempt to meet it will lead into a maze of more and more elaborate and complex analytic procedures that will fail to provide answers for many important questions about the nature of linguistic structure. (50-53)

In footnote 3 Chomsky writes that "discovery procedures are the explicit goal" of Bloch (1948), Chomsky (1953), Harris (1951a) and (1955), Hockett (1952a)and (1947), Wells (1947) "and many other works".

The charge of forbidding complexity and unreasonable limitation of scope ironically echoes Harris' earlier remarks (cited above) on the insufficiency of a purely distributional structure. No actual mechanical discovery procedure, here or elsewhere, is presented and demonstrated to be inadequate; not surprisingly, because no such procedures had been formulated in structural linguistics. This discussion of goals, conducted entirely at the level of linguistic metatheory, is in fact a much abbreviated argument for a new approach to the justification of grammars in terms of an explicit theory of language structure - a metagrammar - incorporating a formal algorithm to evaluate and rank grammars according to ~~prior~~ previously defined notions of simplicity.[1] The subsequent discussion of simplicity as a criterion for evaluating grammars (54-5) is extremely sketchy; however, this is a central issue in the much larger work from

[1] See the discussion of Chomsky (1955a) in Chapter 3 §3 below.

which Syntactic Structures was, in part, "an informal outline" (Chomsky,1975a: 3). By translating "distributional analysis" into "mechanical discovery procedures" Chomsky imputes that the relevant criterion of adequacy for distributionalism is formulable as the development of a set of analytic procedures which would automatically yield a unique or optimal grammar of a language. But were "mechanical discovery procedures" the "explicit goal" of structural linguistics as represented in the writings of Harris, Bloch, Hockett and Wells? Or, in arguing for a new approach to determining the adequacy of grammars, has Chomsky created a convenient straw man to aid in the re-orientation of the metatheory of grammar?

Our examination of these well-known remarks of Chomsky must first be precise about what is claimed in the attribution of such a goal to structural linguistics. The key word "practical" must be singled out. Chomsky is not simply saying that 'in principle' or 'in theory' procedures of this kind might be possible; rather the claim is that the efforts of the purported adherents of this position are explicitly directed toward formulating such procedures for actual use. Thus Harris, Bloch, Hockett and Wells are charged with seeking formal analytic procedures which could, in a mechanical and step-by-step fashion, be practically applied to a corpus consisting of the raw data of speech (given, say, in a phonetic representation) to "discover" the correct grammar of the language of which the corpus is a (presumably) representative sample, and all without any knowledge of meaning or intuitive hunches or even without any active intervention on the part of the linguistic investigator.

Note first of all that such a methodology follows directly from a doctrine of the autonomy of form which Chomsky, mistakenly we believe, attributes to structural linguistics in general. We may also note that Chomsky does not name Bloomfield as envisaging or propounding "mechanical discovery procedures"; such an allegation - guilt, as it were, by association - has been and continues to be routinely made [1] but, as should be clear from the above discussion, is certainly false. Chomsky's 'argument' against "mechanical discovery procedures" has often been taken as directed especially against Harris.[2] In point of fact, as we shall see, the very notion of the grammar of a language was widely regarded to be suspect and Harris expressly disavowed that his distributional procedures eliminated non-uniqueness (§ 2.5 below). Among post-Bloomfieldians, Hockett and unnamed others apparently at one time entertained notions about 'autonomy of form' and the possibility of determining the grammar of a language "as though it had nothing to do with meaning". But textual evidence from the period does not support the charge of advocacy of "mechanical discovery procedures" by Hockett or any of the other sources Chomsky cites. To the contrary, each of the cited works explicitly disclaims that the procedures of analysis presented are intended to be mechanically applied and each is emphatic about the use of informal methods of analysis and of meaning in practical work.

1 E.g., Moore and Carling (1982 : 23): "Bloomfield's emphasis on methods of description was to make linguistics a science that sought objectivity by striving to derive its generalisations by rigorous procedures directly from observable data."

2 E.g. Lees (1957 : 38 fn. 3): "Pursuit of this goal is seen in perhaps its best and most resolute form in the works of Zellig S. Harris:..." ; Lyons (1970 : 34): "Harris' work also constituted the most ambitious and the most rigorous attempt that had yet been made to establish what Chomsky was...to describe as a set of "discovery procedures" for grammatical description."

We review these cited works in turn:

Bloch (1948):

> Our postulates are intended to state either empirical facts or what are assumed to be facts. They are NOT (orig. emphasis) intended to delineate procedures, or to constitute a list of practical rules to be followed step by step in one's work with an informant. On the other hand, the methods of analysis by which linguists usually proceed in arriving at the phonemic system of a dialect are implied in these postulates and can be justified by them....The basic assumptions that underlie phonemics, we believe, can be stated without any mention of mind and meaning; but meaning, at least, is so obviously useful as a shortcut in the investigation of phonemic structure - one might almost say, so inescapable - that any linguist who refused to employ it would be very largely wasting his time. (5)

We omit consideration of Chomsky (1953) which does not propose mechanical discovery procedures but alludes to the interest of "inquir(ing) seriously into the formality of linguistic method and the adequacy of whatever part of it can be made purely formal" and is actually concerned with "an attempt to develop an adequate notion of syntactic category within an inscriptional nominalistic framework" (242).

Harris (1951a; ms. completed January 1947):[1]

> These procedures are not a plan for obtaining data or for field work. ...The procedures also do not constitute a necessary laboratory schedule in the sense that each procedure should be completed before the next is entered upon. In practice, linguists take unnumbered short cuts and intuitive or heuristic guesses, and keep many problems about a particular language before them at the same time:...The chief usefulness of the procedures listed below is therefore as a reminder in the course of the original research, and as a form for checking or presenting the results, where it may be desirable to make sure that all the information called for in these procedures has been validly obtained. (1-2)

[1] This work is often cited as the locus classicus of "mechanical discovery procedures"; see, e.g., Lees (1957: 38 fn.3), Searle (1972: 2-4), Bach (1965: 121), Katz (1981: 35), Katz and Bever (1976: 15), Newmeyer (1980: 6-7), and Moore and Carling (1982: 30). Indeed, it hardly comes as a surprise to now read that Harris not only proposed mechanical discovery procedures but that he actually implemented them on computers which, however "were not very intelligent at that time":

> (Chomsky)studied at the University of Pennsylvania under the American structural linguist Zellig Harris, who not only deplored

Harris (1955):

> For methodological purposes and for special problems - though certainly not for practical work - this procedure can therefore replace the less orderly search for morphemic segments. (33)

Wells (1947):

> ...we do not propose our account as a mechanical procedure by which the linguist, starting with no other data than the corpus of all the utterances of the language and a knowledge of the morphemes contained in each one, may discover the correct I(mmediate) C(onstituent)-system. For any language, the number of possible IC-systems is very large; but in practice it is easy to see that most of the possibilities are negligible. Just as when working out the phonemics, the practicing linguist will discover many shortcuts. (193)

Hockett (1947):

> We now summarize the procedure of morphemic analysis worked out in the course of our discussion...Our summary of the procedure is given in steps...but in actually working with a particular language one has to skip back and forth, operating by trial and error.... Step 4. Two or more morphs are grouped into a single morpheme if they fit the following grouping-requirements: (a) they have the same meaning; (b) they are in non-contrastive distribution; (c) the range of the resultant morpheme is not unique. (241)

As for Hockett (1952a), Chomsky (1957a) remarks:

> Although discovery procedures are the explicit goal of these works, we often find on careful examination that the theory that has actually been constructed furnishes no more than an evaluation procedure for grammars. For example, Hockett states his aim in "A formal statement of morphemic analysis" as the development of "formal procedures by which one can work from scratch to a complete description of the pattern of a language"(p.27);...but what he actually does is describe some of the formal properties of a morphological analysis and then propose a "criterion whereby the relative efficiency of two possible morphic solutions can be

(continued from previous page)

> the intrusion of meaning into the science of language, but made every effort to shut it out completely by using mechanical methods of description that computers, which were not very intelligent at that time, were able to process. (Campbell, 1982 :171)

> determined; with that we can choose the maximally efficient possibility, or, arbitrarily, any one of those which are equally efficient but more efficient than all the others"(p.29). (52, fn.3)

If we turn to the indicated place (p.27) of the cited work, we find that this remark, in the opening sentence of the article, is not a statement of Hockett's "aim" as Chomsky claims, it is Hockett's characterization of what Hockett apparently takes to be Harris' aim:

> In his book Methods in Structural Linguistics Zellig S. Harris attempts to set up formal procedures by which one can work from scratch to a complete description of the pattern of a language, all without any reference (at least in theory) to meaning as a criterion.

Here, Hockett is, as the above quotations from Harris demonstrate, simply misreading Harris; certainly, such an allegation flies in the face of the statement of purpose on the opening two pages of Methods cited immediately above. But in any event, Hockett himself is seemingly unsure that this characterization of Harris' aims is entirely accurate. For on the next page, Hockett writes:

> Harris only hints, at best, at the theory of morphemicization without meaning, and then operates on the half-formalized, half-intuitive level for most of his discussion. This, also, is not necessarily an adverse criticism; it depends on what Harris is trying to accomplish. (our emphasis) But it leaves the problem of complete formalization unsolved. (28)

Since Harris can be criticized for providing only a partial formalization only "depending on what (he) is trying to accomplish" i.e., on in fact whether his aim is to provide a complete formalization ("mechanical discovery procedure" in Chomsky's parlance) it is obvious that Hockett is in some doubt as to what the intent of Harris' procedures is. Hockett, on the other hand, is most lucid as to what his (Hockett's) aim is in

attempting a complete formalization of morphemic analysis; it is not the aim which Chomsky incorrectly cites as Hockett's and Hockett incorrectly attributes to Harris, but is, as Chomsky observed, a method of evaluating between different analyses. Hockett's concern in proposing a method for formalization is in justification, not the discovery of grammars, as is clear from the second passage cited by Chomsky:

> We have to have some criterion whereby the relative efficiency of two possible morphic solutions can be determined; with that we can choose the maximally efficient possibility, or, arbitrarily, any one of those which are equally efficient but more efficient than all others. (29)

Let us be quite clear as to what has transpired in this riot of quotation and counter-quotation. Chomsky, in an extraordinarily influential discussion, has counterposed two approaches to the goals of linguistic theory, and, as Lees was to point out in his widely-read review of Syntactic Structures, two conceptions of scientific theory as well. Chomsky has argued that "most of the more careful proposals for the development of linguistic theory", proposals which he was careful to identify and which we have reviewed above, have advocated practical "mechanical discovery procedures" as their "explicit goal", procedures which might actually be employed to inductively derive the correct grammar of a language from the raw data of speech. Lees, for his part, observes (rightly, we may add) that this is an impossibly strong requirement on any theory which cannot be satisfied "even in the most advanced of the physical sciences, not to mention the whole remaining less exact body of scientific knowledge" (1957 : 39-40). Promotion of such a rigidly inductivist account of the development of scientific theory is taken as indicative of "confusion" on the part of American linguists as to the nature of

scientific theories. Science, Lees reminds the uninitiated, employs the method of hypotheses and theories are free creations of the human mind, not inductively derived from observables as "the dull-cataloger of data", the descriptive linguist, maintains:

> And the theories by means of which we order our experiences, on the street or in the laboratory, are generated only by those flashes of insight, those perceptions of pattern, which mark off the brilliant scientist from the dull cataloger of data. (41)

But on examination of just those works cited by Chomsky, we do not find that the advocacy of "mechanical discovery procedures" is their "explicit goal"; instead, each directly disavows that the methods proposed are to be so construed, and each emphasizes that practical working methods depart from the formalized or partly formalized procedures presented; indeed, that this departure is a necessary one for actual work. The central purpose, repeatedly expressed in these works, is to provide justification - in terms of clarity of formulation and objectivity of result - for statements about language structure which could be arrived at by various means, including considerations of meaning. This is not a mere quibble of textual interpretation: the works cited by Chomsky are unambiguous on precisely this point. So too is exposed the caricature of descriptive lingusitics as a discipline adhering to an impossibly narrow inductivist conception of scientific method, a caricature which has, however, had a very real pragmatic success for the proponents of 'theoretical linguistics'. It is indeed striking that the mythology created by Chomsky and Lees in 1957 is still so widely accepted as accurate nearly three decades later (see, e.g., the sources cited in footnote 1 on p. 49), especially as the textual evidence required to document these claims is readily available to anyone curious enough to

spend several hours of his time satisfying himself as to their validity.[1] A fully satisfactory account of this state of affairs would undoubtedly extend beyond mere textual documentation and consider wider sociological and cultural currents.

2.5 Non-uniqueness and 'Game-Playing'. Since Y.R. Chao had first raised the issue explicitly in his 1934 paper on the non-uniqueness of phonemic solutions to a phonetic system, it had been recognized among some (American) linguists that the analytic methods of contrast and complementary distribution did not lead to a uniquely correct description of phonemic and morphemic (and possibly other levels of language) structure. That elements of conventionality played a role in the phonemicization of a language was implicitly inferable from Bloomfield's main work, the enormously influential LANGUAGE (1933). Whereas linguists of the previous decade had spoken of providing transcriptions of the sounds of a language in a notation replete with diacritical marks indicating various phonetic qualities of a perceived sound (aspiration, nasality, diphthongization, brightness and so on), Bloomfield's simplified phonemic notation reflected the fact that phonemes were considered merely in terms of their distinctive differences (see § 2.12 above).

Chao, however, addressed the issue of non-uniqueness directly by specifically inveighing against the employment of 'queer' orthography and diacritical marks in phonemic description. The choice of symbols, he argued, should not play a determinative role in the linguist's organization of phonemic structure, especially since the more complex and unwieldy the notation, the less chance of it being used in a standard manner by

[1] In this regard, Hymes and Fought (1981) is a valuable corrective.

investigators schooled in different manners. But the transition to a broader notation, in eliminating pointless controversies about the 'true' phonetic qualities of a sound, pointed to the fact that, depending upon how emphasis is allotted to one or another factor of size or grouping, "there is no such thing as the correct phonemic transcription for any given language". What the linguist considered a distinctive contrast is relative to the particular system of grouping that he employs.

> According as we emphasize one or another factor in the size of the unit, method of phonemic grouping, and choice of symbols, we arrive at one or another form of phonemic solution. There is nothing in our definition of a phoneme...that can decide for us....The definition permits us to devise ways and means of grouping together the distinguishable sounds that are not distinctive with respect to the particular system of phonemic grouping. It also implies that certain sounds in a language are never distinctive in that language by any reasonable manner of symbolic juggling, e.g., the difference between the [k] 's in keep, cool, coo, etc.,....(51)

The sole criteria governing phonemic descriptions were self-consistency, clarity of interpretation with respect to the intended purpose, and, significantly, the claim of non-exclusiveness (54). Since there was no unique resolution of phonemes into component sounds,[1] Chao's treatment of the issue of non-uniqueness implied that a certain relativity or conventionality governed the reducing of the sounds of a language into discrete combinatorial elements. However, Chao retained, in his definition of the phoneme as a class of sounds, the requirement that every word in the language be given as an ordered series of sound classes, and that words considered different in pronunciation, e.g., rider and writer, have different phonemic representations.

[1] almost twenty years later, Jakobson attempted this
~~Such as Jakobson later attempted~~ with a "distinctive feature" analysis of phonemes. E.g., Jakobson, Fant and Halle (1952: 7):

> By successively eliminating all redundant data (which do not convey new information) the analysis of language into distinctive features overcomes the "non-uniqueness of phonemic solutions". This pluralism, pointed out by Y.R. Chao, interfered with the analysis as long as the phoneme remained the ultimate operational unit and was not broken down into its constituents.

A further step away from associating the term 'phoneme' with either a physical reality or a mental ('psychologically real') one was taken by Twaddell (1935) who maintained that the phoneme was not a linguistic element of which one could speak of actual occurrences. Utterances were "phonetic events". Phonemes, on the other hand, were not the "positive, additive entities" of which words were composed, but "heuristic or pragmatic fictions", forms abstracted from utterances. A phoneme was defined as

> a negative, relational, differential abstraction; it is a unit of that sort of relation which de Saussure describes: 'Dans la langue il n'y a que des différences sans termes positifs'. (74)

The culmination in this trend towards a purely relational identity of linguistic elements [1] was reached by Harris who carried the logic of distributional analysis to its conclusion: instead of speaking of elements denoting sound qualities or stretches of sound or perceptual differences, one could just as well speak of positions or environments of occurrence and the relations between these:[2]

> Since each element is identified relatively to the other elements at its level, and in terms of particular elements at a lower level, our elements are merely symbols of particular conjunctions of relations: particular privileges of occurrence and particular relations to all other elements. It is therefore possible to consider the symbols as representing not the particular observable elements which occupy an environment but rather the environment itself, and its relation to other environments occupied by the element which occupies it. We may therefore speak of interenvironment relations, or of occupyings of positions, as being our fundamental elements. (1951a : 370-1)

[1] Which Cassirer (1945) in a paper read shortly before his death recognized as representing "a general tendency of thought that, in these last decades, has become more and more prominent in almost all fields of scientific research"(120), a "revaluation of our former logical and epistemological value" that "there is no opposition between what is 'formal' and what is merely 'factual' (104).

[2] Leading the Voegelins(1963 : 14) to speak of linguistic structure as "the geometrisation of recurrencies".

Descriptive methods did not only not provide uniquely 'correct' descriptions of the phonemes of a language, the most extensive survey of distributional procedures, Harris (1951a), pointedly offered many alternative methods for setting up linguistic elements of higher levels as well, including morphological, morphophonemic, morpheme class and so on. What particular procedure an investigator chose was immaterial from the point of view of distributionalism so long as the operations "dealt essentially with the description of features of speech relatively to the other features within the utterance, and as long as they did so explicitly and rigorously" (6). The sole criterion governing the procedures adopted was their restriction to statements of distribution. [1]

In limiting the methods of linguistic description to distributional analysis, Harris sought to avoid "the undesirable effect of forcing all languages to fit a single Procrustean bed, and of hiding their differences by imposing on all of them alike a single set of logical categories"(6), an injunction which Boas (whom Bloomfield had called "the teacher in one or another sense of us all" [2]) had urged in his concern to obtain accurate descriptions of the fast-disappearing American Indian languages.[3]

[1] The distribution of a segmental element (freedom of occurrence, Bloomfield: "privilege of occurrence") is defined as the totality of environments in which the element occurs (1951a : 61)

[2] Bloomfield (1944: 409)

[3] See Boas (1911). The linguistic views of Boas have suffered at the hands of the partisans of generative grammar (e.g., Chomsky (1964: 77), Bach (1965: 115), Newmeyer (1980: 5)) from Joos' sympathetic but one-sided characterization of them as expressing the principle that "languages can differ without limit as to extent or direction" (1957: 228, also 96). While correctly noting that a "leading principle" of "the Boas generation" was that "every language has to be explained from the inside out" (v), (Cf. Stocking (1974: 469): "In other words, 'grammatical categories' were to be derived internally from an analysis of the language itself rather than imposed from without. One must therefore 'strive to keep out the point of

In view of how elements are identified, distributionalism entailed a limitation to the generality of grammatical statement.

> The fact that the determination of elements is relative to the other elements of the language means that all such determining is performed for each language independently. All lists of elements, relations among them, and statements about them are applicable only to the particular language for which they are made. The research methods of the linguist may be roughly similar for many languages, but the statements that result from his work apply in each case to the language in question. (8, fn.6)

In addition to the relativity of grammatical statement to the language under investigation, the admission that distributional procedures could be employed in various ways depending on, among other things, matters of convenience or purpose (9, fn. 8) was not to say that no criterion governed the result of the application of these procedures to a corpus. As noted above (§ 2.3), Harris sought particularly to distributionally identify elements which correlated with aspects of meaning. But in addition, descriptive relevance was insured by the requirement that "the defining of the elements and the stating of the relations among them be based on distribution, and be unambiguous, consistent and subject to check" (9). If these strictures are followed, alternative distributional procedures will yield results which can be compared or put into correspondence

(continued from previous page)

> view of Indo-European languages as thoroughly as possible", citing Boas), Joos' summary statements give a misleading impression. Boas' overriding goal was ultimately to provide a comparative summary of the descriptions of individual languages in order to reveal what Boas considered to be culturally-conditioned psychological differences between languages. Stocking (1974), following presumably Voegelin (1952), speaks of this very Humboldtian enterprise as a "promissory note": "That these psychological differences might eventually be catalogued comparatively was indeed a promissory note" (469). Voegelin (1952, 450-1) elaborates: "a promissory note: a promise or prediction that we would at last obtain reliable data on the various Weltanschauungen as reflected in the various native languages of primitive man in the New World, and thus obtain an attested contrast to the Weltanschauungen derived from European languages". See also Stocking (19 on the influences of Kant and Dilthey on Boas' early ideas about culture. On Humboldt, badly misrepresented in Chomsky (1964; 1966a; 1968), see especially Aarsleff (1982) and Joly (1977).

with each other.[1]

Harris' frank admission of non-uniqueness for even the most (together with Bloch (1948)) rigorously formulated distributional procedures gave new impetus to the controversy of whether linguists could claim to characterize the structure of a language and led to the accusation - reminiscent of Brouwer's critique of Hilbert's formalism - that, if not, linguists were only engaging in a sophisticated kind of "hocus-pocus", playing mathematical games but making no empirical claims. Critics of this persuasion fall roughly into three camps which are, however, not mutually exclusive. There were those - primarily European - linguists who saw non-uniqueness as stemming from the 'neglect' of meaning; those, e.g., Householder (1952), who believed non-uniqueness was incompatible with the (naively) realist conception of theories held by the sciences; and there were linguists, such as Hockett, who objected that the apparently corpus-based limitations of procedural methods did not allow sufficient scope to the fact that empirical theories must be predictive, i.e., must make claims about phenomena beyond those already observed or analyzed. These are briefly examined in turn.

To some critics, since meaning was supposedly not the province of distributionalism, there could be no determinative criterion by means of which one analysis could be preferred to another. This position, often alleged to be that of Harris, was sharply criticized by European structuralists who, following Saussurian notions of la valeur linguistique and système (see the references cited on p. 38 , fn.1 above), objected to the point of a linguistics without concern for meaning. Such is

[1] Cf. Harris (1954: 777): "In any case, there is no harm is all this non-uniqueness, since each system can be mapped onto the others, so long as any special conditions are explicit and measurable."

the core of Benveniste's careful but ultimately misunderstanding comments on distributionalism, seen as epitomized in Harris' Methods:

> Schemes of distribution, no matter how rigorously they are established, do not constitute a structure, any more than inventories of phonemes and morphemes, defined by means of segmentation in chains of discourse, represent a description of a language....Let us emphasize especially that feature which, even more than the special technique of the procedure, characterizes the method; it is the principle that linguistic analysis, in order to be scientific, should ignore the meaning and apply itself solely to the definition and distribution of the elements....It is to be feared that if this method becomes general, linguistics may never be able to join any of the other sciences of man or of culture. The segmentation of the statement into discrete elements does not any more lead to an analysis of language than a segmentation of the physical universe leads to a theory of the physical world....One can then conceive of several types of description and several types of formalization, but all of them must necessarily assume that their object, language, is informed with meaning, which gives it its structure, and that condition is essential to the functioning of language among other systems of signs. (1954.:10-11)

The charge of ignoring meaning is, as we have seen, familiar; however, unlike those, e.g., Chomsky (§ 2.2 above) who have taken this as "a central idea of much of structural linguistics" in promoting a doctrine of the autonomy of linguistic form, Benveniste, a prominent Continental linguist, approaches eloquence in maintaining the centrality of meaning in linguistic analysis. For Benveniste, language structure - as it was for Bloomfield (and of course for Sapir) [1] - is a structure not 'autonomous' with respect to meaning, but 'given' by meaning. In fact (§ 2.3 above), this view is consistent with Harris' conception of distributionalism and is developed in much greater detail in the theory of grammatical transformations which results in operator grammar (Chapters 5 and 6 below). As we shall see, the connection of language structure

[1] This point could be developed at length; I refer the reader above all to the masterful discussion of Sapir in Harris (1951b). See below.

and meaning is crucially important for attendant metatheoretical considerations about the justification and validation of grammars. On the other hand, generative grammar, as a theory of autonomous linguistic form, originated in an attempt to validate grammars in terms of certain empirical criteria of adequacy (acceptability judgements) coupled with a metatheory of simplicity. This latter is subsequently accorded the status of a genetically-determined universal grammar, opening the way to a 'realist' view of grammars as "real-world" psychological or biological objects. [1]

Remarks similar in spirit to those of Benveniste were made at the same time by the Swiss Saussurian Henri Frei in a complaint that non-uniqueness was simply an indication of lack of interest in the notion of language as a system:

> Alors qu'un critère pertinent, appliqué correctement, doit à priori n'autoriser qu'un solution pour chaque problème, le critère distributionnel, s'il est employé exclusivement sans considération du signifié, permet en théorie, pour n'importe quel échantillon de chaîne parlée, n'importe quelle délimitation, sans autres restrictions que celles qui sont imposées individuellement par le degré d'imagination du savant.
>
> La non-unicité des solutions possibles est liée à l'atomisme. Dans la mesure où il forme un système cohérent, un état de langue n'autorise pas plusieurs résponses par problème, et, comme dans un jeu de mots croisés, toute solution fausse entraîne automatiquement la fausseté des autres. Admettre la non-unicité des solutions, c'est nier la notion de système linguistique; comme tout se tient, on devine pourquoi les distributionalistes s'intéressent au fond peu à ce concept. (1954: 142)

Both Benveniste and Frei make the point that "schemes of distribution", having purely formal significance, do not constitute a structure or adequately characterize language as a system. Formal arrangement of

[1] Chomsky (1983a) and (1984). See Chapter 4 §3.

data is not what the notion of structure or system in language is about. The importance of meaning in this regard is emphasized, yet neither provides a convincing illustration of the systemic character of the relation of meaning and structure, nor of how this system is to be determined. In retrospect, perhaps much confusion might have been averted if the term 'distribution' had been more prominently tied to the Sapirian notion of 'pattern' to which there are clear connections.[1] If these are recognized, the opposition between "schemes of distribution" and "structure" or "system", as drawn by Benveniste, Frei and others, falls.

Harris drew this connection explicitly in a lengthy review of a collection of Sapir's writings (1951b). Here he develops, in some detail, the strategy of distributionalism in revealing the patterning which is established by the functional relevance, or use, of linguistic elements, observing that "Sapir's greatest contribution to linguistics, and the feature most characteristic of his linguistic work, was...the patterning of data" (717). The association of pattern with the conception of language as a system is particularly stressed.

> Sapir's patterning is an observable (distributional) fact which he can discover in his data and from which he can draw those methodological and psychological considerations which he cannot observe directly, such as function and relevance, or perception and individual participation. He can the more readily do this because his patterning is established not directly on distributional classification but on an analysis in depth of the way in which the various elements are used in in language. The 'way the elements are used' is equivalent to their distribution; but talking about such use gives a depth which is lacking in direct classification of environments.

[1] Swadesh (1934) is apparently the source of the term 'distribution' in a technical linguistic context; see Diderichsen (1958: 158). In a letter, Swadesh indicates its Sapirian ancestry:"The source of the usage may have been Sapir, but I do not remember. At the time...I was not conscious of either adopting or inventing a technical term, but rather used the word simply as a way of describing the spread occurrences of a sound among the positions within the word. It was an application of the usage represented by 'geographic distribution', an expression which was much used by Sapir as by other anthropologists and linguists"(ibid, fn.4).

Thus Sapir uses the patterning of elements in order to express their function (their functional position within the language): "to say that a given phoneme is not sufficiently defined in articulatory acoustic terms but needs to be fitted into the total system of sound relations peculiar to the language is, at bottom, no more mysterious than to say that a club is not defined for us when it is said to be made of wood and to have such and such a shape and such and such dimensions. We must understand why a roughly similar object, not so different to the eye, is no club at all. ...To the native speaker and hearer, sounds (i.e., phonemes) do not differ as five-inch or six-inch entities differ, but as clubs and poles differ. If the phonetician discovers in the flow of actual speech something that is neither 'club' nor 'pole', he, as phonetician, has the right to set up a 'halfway between club and pole' entity. Functionally, however, such an entity is a fiction, and the native speaker or hearer is not only driven by its relational behavior to classify it as 'club' or 'pole', but actually hears and feels it as such" (quoting Sapir(1933a), 46-7).

Perception. In a related way, patterning is used as a basis for the structuring of perception. Sapir reports that English-speaking students often mistakenly hear p, t, or k instead of a final glottal stop; and after learning to recognize a glottal stop, they often mistakenly hear a glottal stop at the end of words ending in an accented short vowel (they write smɛ' for smɛ). He then points out (quoting Sapir (1933a),59-60) that the second type of error is simply a more sophisticated form of the first....

This effect upon perception is claimed not only for such phonemic hearing, but also for the structuring of experience in terms of the morphological and vocabulary patterns of the language: "Even comparatively simple acts of perception are very much more at the mercy of the social (more exactly: linguistic) patterns called words than we might suppose. If one draws some dozen lines, for instance, of different shapes, one perceives them as divisible into such categories as 'straight', 'crooked', 'curved', 'zigzag' because of the classificatory suggestiveness of the linguistic terms themselves" (quoting Sapir (1929), 162).

System. Sapir goes on to recognize patterning as one of the basic characteristics of language: "Of all forms of culture, it seems that language is that one which develops fundamental patterns with relatively the most complete detachment from other types of cultural patterning" (1929), 164). Had he used the descriptive term 'consists of' instead of the process word 'develops', he might have gone beyond this to add that we can even use this linguistic patterning to determine what is to be included in 'language'. There are scattered bits of speech-like noises - coughing, crying, shrieking, laughing, clucking - which may or may not be considered part of 'language' on one basis or another, but which we count out of language because they do not fit into its detached patterning.

Out of all this Sapir was able to make important generalizations about language as a system. Recognition of the detachment of linguistic

patterning leads to the statement that "the patterning of language is to a very appreciable extent self-contained and not significantly at the mercy of intercrossing patterns of a non-linguistic type" (quoting Sapir (1929), 165). This explicit talk about the fact of patterning makes possible the distinction between the grammar (specific pattern) and grammaticalness (degree of patterning) of language: "In spite of endless differences of detail, it may justly be said that all grammars have the same degree of fixity. One language may be more complex or difficult grammatically than another, but there is no meaning whatever in the statement which is sometimes made that one language is more grammatical, or form bound, than another" (Sapir (1933b),9-10).

From this, Sapir could go on to an interesting formulation of the adequacy of language. We all know the statement that any language can be used as the vehicle for expressing anything. Sapir removes the air of triviality from this by saying, "New cultural experiences frequently make it necessary to enlarge the resources of a language, but such enlargement is never an arbitrary addition to the materials and forms already present; it is merely a further application of principles already in use and in many cases little more than a metaphorical extension of old terms and meanings" (ibid, 10). In other words, the adequacy of language is not simply definitional, but derives from the possiblilites of extension and transference within the language structure, without either disregarding or destroying the structure. "The outstanding fact about any language is its formal completeness. ...No matter what any speaker of it may desire to communicate, the language is prepared to do his work. ...Formal completeness had nothing to do with the richness or poverty of the vocabulary. ...The unsophisticated natives, having no occasion to speculate on the nature of causation, have probably no word that adequately translates our philosophical term 'causation', but this shortcoming is purely and simply a matter of vocabulary and of no interest whatever from the standpoint of linguistic form. ... As a matter of fact, the causative relation...is expressed only fragmentarily in our modern European languages...(but) in Nootka...there is no verb or verb form which has not its precise causative counterpart" (Sapir (1924), 153-5). Sapir might have continued here to point out that the work of language in communication and expression can be carried out both by grammatical form and by vocabulary (though with different effect), since one can insert to cause to before any English verb somewhat as one can add a causative element to every Nootka verb. Hence what is important is not so much the distinction between grammatical form and vocabulary, as the fact that the distribution of grammatical elements, and so the grammatical structure, can change in a continuous deformation (the structure at any one moment being virtually identical with the immediately preceding structure), and that vocabulary can be added without limit (and changed in meaning). What we have, therefore, as the basic adequacy of language is not so much the static completeness of its formal structure, but rather its completability, or more exactly its constructivity without limit.

> The Fact of Patterning. A person who is interested in the various kinds and relations of patternings, for their own sake, can establish pattern and structure as bland distributional arrangements, and thence move toward the mathematical investigation of the combinatorial possibilities. Sapir, however, was interested in the fact of patterning, and what could be derived from the discovery that language was so patterned a bit of human behavior. This was not only because Sapir was above all an anthropologist, but also because of the particular development in linguistic science at the time.
>
> From de Saussure to the Prague Circle and Sapir and Bloomfield, the fact of patterning was the overshadowing interest. In the later work of this period in linguistics we find attempts to analyze and classify these patterns, but the big result was still the very existence of structure. This was the big advance in several sciences at the time. In the late depression years, when neither admiration of Russia nor war preparations in America had yet obscured the scientific and social results of Karl Marx, Leonard Bloomfield remarked to me that in studying Das Kapital he was impressed above all with the similarity between Marx's treatment of social behavior and that of linguistics. In both cases, he said, the activities which people were carrying out in terms of their own life situations (but in those patterns which were socially available) turned out to constitute tight patterns that could be described independently of what people were about. In language, they communicate, or pronounce words they have heard, but with the descriptive result of maintaining a patterned contrast between various subclasses of verbs or the like. In economic behavior, they may do various things just in order to make profit, but with the descriptive result that the producing population becomes increasingly removed from control over its production. Sapir saw this fact of patterning even more clearly - in language, in culture, and later in personality. Throughout his writings one sees how impressed he was with this fact, one which was also being stressed at the time (but with less happy success) in other social sciences. In his comments about language as patterned behavior he reached the heights of his subtlety, and pioneered a form of research which few have as yet taken up. (Harris (1951b: 719-722; footnotes suppressed))

This extended quotation illustrates, incontrovertibly, that for Harris - the arch-distributionalist, as it were - the significance of distributionally obtained results was not that of mere inventories of elements or segmentations of a corpus. Instead, such results reveal "the fact" of a "detached patterning" of the elements. Distributional procedures provide the means of giving a distinctive formulation to the system of relations ~~which~~ that establishes the functional position or relevance of each element. A structure

of distributional relations is then a structure of the functional relatedness - determined solely on grounds of occurrence, of which combinations of elements occur - of the linguistic elements themselves. It is this system of internal relations which enabled Harris (1954: 785) to speak of the possibility of stating "certain aspects of meaning as functions of measurable distributional relations". The systemic character of the patterning of elements indicates that a structure can be given to what is, in fact, called 'language'. We recall Harris' statements, cited earlier, that a distributional structure resulting from the procedures of segmentation and classification treated in Methods does not yield "ideal coverage". What is not directly stated here but might be inferred from the remark that "pattern and structure", conceived as "bland distributional arrangements", may be mathematically investigated as to the combinatorial possibilities, is an indication that a purely combinatorial account, i.e., in the sole terms of objects and relations among them, of the system which is language, is an open task for linguistic research. The patterns given by simple inventories of phonemes and morphemes and even higher-level elements resulting from the application of distributional procedures to a fixed corpus were not seen as a stopping point for linguistic analysis nor a definitive account of language structure, as Harris' concurrent researches into discourse analysis and grammatical transformations (1952a,b) showed. Rather the goal is the provision of a purely combinatorial account of the functionally-relevant pattern which characterized a language as a whole, including, but not restricted to, a specification of the notion of 'grammatical sentence of L' for arbitrary L. Having such a combinatorial

characterization of language structure would provide a general principle of analyzability in terms of which any corpus in the language -- a discourse or sublanguage [1] -- might be described via the restrictions on combinations (the functional relevance [2,3]) of its elements. Looking ahead a bit (Chapters 5 and 6), it is then possible to raise the question of the informational significance (in the sense specified there) of these combinatorially-characterized restrictions. The conception of a grammar of a language and of a general theory of language structure (the partially ordered system of word dependences, see Chapter 5) as pattern, the latter as manifested in a particular language as characterized by the former, is a most 'Sapirian' one, not at all antipathetic to distributionalism but rather more its combinatorially formulated generalization. Such a conception of structure markedly differs from the generative notion of a grammar as a set of 'rules' for generating 'all and only' the sentences of a language, an issue taken up in Chapter 4 §§ 2 and 3 and Chapter 5.

[1] The relation between the two is stated by Harris (1981:231)."The major difference between them is that discourses are the directly observable events which constitute the occurrence of language, whereas a sublanguage is a construct -- a structure that characterizes certain discourses, or certain parts of discourses, which occur in particular situations...."

[2] Speaking of 'function', 'system', or 'relevance' is legitimate as long as these notions can be specified on distributional or combinatorial grounds; cf. Harris (1941:707): "...talking about function, system, or the like, without defining them in terms of operations and relations, fools even the linguistic worker. For by satisfying him with undefined psychological terms it prevents him from continuing his analysis."

[3] Chomsky (e.g., (1978:304), (1979b:118), (1980a:107-8)) and others following Chomsky (e.g., Lightfoot (1982:30)) have viewed Sapir's use of

Although Benveniste alludes to the possibility of "several types of description and several types of formalization" of language structure which go beyond mere "schemes of distribution", Frei appears to endorse a particularly severe form of realism about theories as a consequence of his overriding belief in the paramountcy of "la notion de système linguistique": there is a systemic character to language structure and it admits of a uniquely correct formulation. But the question of the

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> "psychological" and "psychological reality" (as in his (1933)) an indication that Sapir gave a "realist psychological interpretation" to linguistic theory, which means, apparently, that he adopted "an essentially realist attitude" (Lightfoot: "explicitly took a realist stance") toward the "neural and biochemical systems with the properties expressed in these theories" and that he maintained that what "the linguist constructs is a representation of synaptic connections in the brain" (Lightfoot: "interpreted the procedures as represented in the mind in some way"). This is a serious misreading of Sapir's views as Harris (1951b: 744-6) had already pointed out:
>
> > A detailed examination of Sapir's use of psychology and kindred words shows they refer not to some new forces within the individual which can affect his language, culture, or personality, but simply to the fact that the individual participates in linguistic, cultural, and personality patterns. This is the meaning - i.e. the use - of the word; and it is quite different from what many thought it meant. Characteristically, the sentences containing psychological or its equivalents have two parts, the first in terms of formal pattern and the second in terms of the 'psychological' participation in the pattern. An example: "In other languages, with different phonologic and morphologic understandings...'m and 'p would have a significantly different psychologic weighting" (quoting from Sapir (1933a), 57-8).
> >
> > ....This individual participation in patterns is then said to be unconscious: "unconscious linguistic forms which in their totality give us regular phonetic change" (1929: 161,...); "unconscious phonologic pattern" (1933a: 58);"the subconscious character of grammatical classification" (1912: 101). ...He says that the development of an individual's participation in a pattern is unconscious: "in each case an unconscious control of very complicated configurations or formal sets is individually acquired" (1927:555); "the language-learning process, particularly the acquisition of a feeling for the formal set of a language, is very largely unconscious and involves mechanisms that are quite distinct in character from either sensation or reflection" (1924:156). ...We can now understand why Sapir had to stress the fact that the individual's participation in these patterns is unconscious. It is precisely because the individual is not aware of the way his behavior is patterned that he cannot explicitly compare his patterning with that of others, and so has his perception of other's behavior determined in advance.

'truth' of the linguist's characterization of language structure in the face of non-uniqueness, or of alternative distributional presentations, was addressed directly and with considerably more influence by Householder (1952) in a review of Harris' Methods.

It was possible, Householder argued, to categorize linguists according to "two metaphysical viewpoints", termed respectively, "God's truth" and "hocus-pocus". The "God's truth" position, it develops, is unabashedly that of transcendental realism:

> The theory of the 'God's truth' linguists (and I regret to say I am one) is that a language has a structure and the job of the linguist is (a) to find out what that structure is, and (b) to describe it as clearly, economically and elegantly as he can without at any point obscuring the God's truth structure (260).
>
> The God's truth man doesn't believe he'll ever find God's truth, but he does believe it exists, and that by trying and working he can gradually approach it asymptotically (261).

Oppositely, "hocus-pocus" linguists are viewed both as instrumentalists about theories and, at the same time, as not really engaged in the enterprise of science since, according to their own conception of what they are doing (i.e., non-uniqueness), questions of correctness do not arise and structure is imposed, not discovered.

> The hocus-pocus linguist believes...that a language (better, a corpus, since we describe only the corpus we know) is a mass of incoherent, formless data, and the job of the linguist is somehow to arrange and organize this mass, imposing on it some sort of structure...(260).

While admitting that Harris "here and there pays his respects to (the God's truth) point of view", Householder charges that "many, many parts of the book seem to be pure hocus-pocus" (261), that Harris, in explicitly proclaiming non-uniqueness and offering many alternative procedures for obtaining distributional results, is only engaging in eleborate tricks of

data manipulation. The problem with hocus-pocus, however entertaining it might be to practice, is that no empirical claim seems involved: "it is difficult to see what a hocus-pocus linguist is making successive approximations to" (261).

Householder does not specify what analytic procedures of segmentation or grouping or else synthetic theories of item and process he includes in holding that structure is, for hocus-pocus linguists, imposed. So it is unclear whether he faults distributional procedures per se or only their failure, as formulated by Harris, to provide a unique result, perhaps as might be signaled by a conjoint declaration that the result obtained is a 'true' or 'correct' characterization of structure. No mention is made of Harris' various reminders that distributional results, as long as clearly stated, could be put in correspondence with one another and thus that non-uniqueness does not entail 'anything goes' is describing a language structurally (the matter of restriction to a corpus is taken up below).

On the other hand, Householder assumes the "God's truth" viewpoint without argument; its adequacy, it seems, is self-evident. Yet it is (notoriously) uncertain that this conception is coherent, or even what it could mean. What other than the linguist's characterization, which can be (or should be) empirically confronted in various ways, could language structure be? The inherent danger of raising the issue of adequacy of theories in the stark terms of realism is, of course, that of indulging in the fallacy of "something more" in Hempel's felicitous phrase,[1] of attempting to contrast the linguist's theory and analysis with some privileged insight into the 'real' structure of language. How does the

[1] As reported by Stein (n.d.).

linguist know he is not "obscuring" the "God's truth structure" by his efforts unless he is able, somehow, to compare his results with such a structure ? How does he know what such a structure is ? And if he does know, what is the point of his labor ? In a subsequent paper which might, in part, be considered a response to this review and to Hockett (1952b), Harris displayed an unwillingness to consider the dichotomy of "God's truth" and "hocus-pocus" and, by implication, of realism and instrumentalism (at least as posed in these terms), a legitimate one.

> An opposition has sometimes been claimed between real facts and mathematical manipulation of structure. This claim ignores the fact that science is (among other things) a process of indicating much data by few general statements, and that mathematical methods are often useful in achieving this. Mathematical and other methods of arranging data are not a game but essential parts of the activity of science (1954: 793, fn. 6).

To Hockett, Harris' views on the non-uniqueness of distributional procedures seem to mean that the linguist was only playing "mathematical games" with a corpus of data. These points are brought out with respect to the Saussurian notions of 'langue' and 'parole' in his (1952b):

> Harris is wrong in defining the 'system' (i.e., language structure, or 'langue' - TR) as what the analyst does with the data he gathers through observation of behavior. [1] We do not allow the analyst simply to play mathematical games with his data (98).

Since the data of a corpus could be distributionally arranged in various ways, raising the issue of the adequacy or correctness of its description did not really make sense. [2] And earlier, in a paper which Chomsky has

[1] A curious interpretation of Harris (1941: 706-7): 'The Prague Circle terminology, however, has two dangers: First, it gives the impression that there are two objects of possible investigation, the Sprechakt (speech) and the Sprachgebilde (language structure), whereas the latter is only the scientific arrangement of the former."

[2] An inference made plausible by the doctrine of the 'autonomy of linguistic form', discussed in § 2.2 supra.

cited as presaging the 'realist' position in linguistic theory (see fn. 1 below), Hockett (1948) distinguishes "scientific linguistics" from such game-playing by the fact that scientific linguistics makes predictions about utterances of the language in a manner that is clearly testable. The linguist was not merely playing games if he predicted, rightly or wrongly, utterances which were not in the corpus or which had not been analyzed at the time of the prediction. Significantly for the direction linguistic theory was to follow in subsequent years, Hockett introduces the situation of language acquisition as a parallel to the predictive analysis the linguist should produce:

> The analytical process thus parallels what goes on in the nervous system of a language learner, particularly, perhaps, that of a child learning his first language. The child hears, and eventually produces, various whole utterances. Sooner or later, the child produces utterances he has not previously heard from someone else (279).
>
> The child's 'analysis' consists...of a mass of varying synaptic potentials in his central nervous system. The child in time comes to <u>behave</u> the language; the linguist must come to state it (280).

On the basis of these remarks alone, one might well understand Chomsky's statements that Hockett has put forward "an explicitly 'realistic' interpretation of discovery procedures" or, that Hockett has here taken "a very strong realist position". [1] But such conclusions can be reached

[1] E.g. Chomsky (1979b; 1978):

> It seems to have been generally assumed that the discovery procedures could be justified only in "pragmatic" terms, as providing an organization of the corpus that would be useful for one or another purpose. There were exceptions, for example, Charles Hockett, who put forth an explicitly "realistic" interpretation of discovery procedures, in an important brief article, in the <u>International Journal of American Linguistics</u> (1979b: 115).
>
> In American linguistics - in fact also in European structuralism of the thirties, forties, and early fifties - there was very intensive

only ~~via~~ an extremely selective reading of Hockett's article and are not at all consistent ~~in the light of~~ remarks which Hockett makes on precisely this issue in subsequent papers (1952b; 1954). These make it plain that Hockett is here speaking of an operational parallel between the child who eventually produces 'new' utterances that are acceptable in his speech community and a linguist's grammar which, as the corpus is expanded upon which it is based, in theory should become more and more accurate in its predictions about the sentences (utterances) of the language. In the (1948) article cited by Chomsky, Hockett writes:

(continued from previous page)

> work, as you know, in developing procedures that in principle, one hoped could be applied in a mechanical way to a corpus of data so as to produce, finally, a grammar of that corpus. Well, a crucial question arises at that point; it is essentially the question of realism, you might say. That is the question, What is the nature of these procedures ? Are they simply a device for bringing organization to chaos ? And, is it the case that one set of procedures is as good as any other set ? Or, is there a kind of truth claim involved in those procedures ? Well, if there is a truth claim, then that means that the system that arises by applying the procedures is claimed to be represented in the mind in some fashion. (Recall Lightfoot (1982: 30): "interpreted the procedures as represented in the mind in some way", cited fn. 3, p. 67 -TR) That is, one claims, at least, that the procedures correspond in some fashion to the mental representation of the language in his brain. And in fact that conclusion had been drawn. For example, it had been drawn by Charles Hockett in a very perceptive, brief paper that appeared in the late 1940's, where he took a very strong realist position and said, in effect, that the grammar that the linguist constructs is a representation of synaptic connections in the brain and that the procedures of analysis correspond to what the child is doing when he works with the data and develops that grammar. Hockett is quite unusual, I think, in taking that position (1978: 303-4).

> At first these newly coined utterances may be rejected by those about him; but by a process of trial and error, supplemented by the constant acquisition of new whole utterances from those who already speak the language, the child eventually reaches the point of no longer making 'mistakes'. Lapses there still may be.... But by the time the child has achieved linguistic adulthood, his speech no longer contains errors; for he has become an authority on the language, a person whose ways of speaking determine what is and what is not an error.
>
> The child's coining of an utterance he has not heard is, of course, a kind of prediction: 'If I make such-and-such noises, those about me will react in a certain way.' (We do not imply that any such 'thought' passes through the 'mind' of the child.) The parallel between this and the process of analysis performed by the linguist is close. When the child is just beginning, his coinage of utterances is often ineffective; when the linguist's corpus is small, his predictions are inaccurate. As the child continues to learn, or as the corpus grows and analysis is modified, prediction becomes more and more accurate. In theory, at least, with a large enough corpus there would no longer be any discernible discrepancy between utterances the linguist predicted and those sooner or later observed (279-80).

The point Hockett is addressing is that the acquisition of language can be considered as acquisition of a set of habits of speaking - this follows from noting that the acquired ways of speaking determine what is and what is not an error - just as the linguist's grammar specifies what is and what is not an utterance of the language. A clearer formulation is given in Hockett (1952b):

> We do not allow the analyst to play mathematical games with his data. We require him to produce a systematization which IN AN OPERATIONAL SENSE (original capitals) matches the habits which we ascribe to the speaker: just as the speaker can produce any number of new utterances from essentially the same set of underlying habits, so the analyst's description must be capable of producing any number of new utterances, each capable of passing the test of casual acceptance by a native speaker (98).

Despite the proclaimed significance for 'realism' of this reference to the "synaptic potentials of the central nervous system", thereby fore-

shadowing the view of linguistic theory as a theory of "the biological basis of language capacities" [1], it seems apparent that Hockett is only alluding to the hardly contestable fact that behavior has an 'underlying' or 'internal' neurophysiological correlate, i.e., that some physiological mechanism, including of course the central nervous system, is involved in the production of linguistic (and other) behavior. However, for Hockett and, in accord with Bloomfield's position about the social factors, prominently termed 'meaning', which determined language structure, the internal mechanism of language is not a desideratum (even as abstractly characterized as "rules" and "representations", we might add) of linguistic theory nor of more than peripheral concern to the linguist. The pattern (or system of habits, as Hockett is wont to say [2]) of language structure is not in any interesting sense innately specified, but is socially-acquired; furthermore, "mentalist" or "little man" terminology is inappropriate in describing or making reference to this

[1] Cf. Chomsky (1980a: Chapter 5): "On the Biological Basis of Language Capacities".

[2] Hockett (1952b: 98) had noted that "None of us, including Bloomfield, has any objection to speaking of 'habits'. 'Langue' is then a set of habits, and 'parole' is the behavior which manifests these habits, the behavior through the examination of which the analyst declares what the habits are." In response, however, Harris remarked that although there is evidence "enough to make us feel that the bulk of the major structural features are indeed reflected in speaking habits - habits which are presumably based, like the linguist's analysis, on the distributional facts", there is nothing gained when predicting new data or new formations by speaking of habits which is not better served by speaking of the distributional structure instead, since the linguist is in no position to say that a parallel system of habits exists until there is linguistic evidence for it, and not before (1954: 778-80; 780).

structure. Again, the focus of the discussion are the terms 'langue' and 'parole': [1]

> The relation between langue and parole, then, is the relation between habits and behavior. The habits involved in language - the habits which constitute langue - are socially acquired, not passed down through the germ plasm; this means that they are cultural habits, or simply culture. Culture as a whole, like its proper part, langue, is not directly observable: the ethnographer can only observe behavior, and has to deduce culture from the regularities of recurrence.
>
> However, in all of this, 'mental' is an unnecessary term. To speak of habits as 'mental' is to indulge in the 'little man' terminology:...The problem which the 'little man' terminology is most apt to shunt aside is that of discovering just how this internal mechanism operates, so that under impact from outside, it becomes a 'carrier' of culture and linguistic habits. In other words, how does the central nervous system work? Of course, this is not the problem of the anthropologist or the linguist but it is the duty of those specialists to turn over to those whose problem it is a report unprejudiced by 'little man' talk. ... What is left to us is the specific form, in linguistics, of a general problem of cultural anthropology: the development of operational techniques for deducing habits from behavior (1952b: 98-9).

To complete the operational parallel between the child's new utterances accepted as correct by his speech community and the situation of the linguist requires, as Hockett observes, that each new utterance predicted by his analysis must pass "the test of casual acceptance by a native speaker". Given that empirical adequacy is located exclusively in predictiveness, the evidential support which is clearly relevant is that of the behavior of (native) speakers of the language in question including, among other observable properties of behavior, their judgements of acceptability. This

[1] Chomsky, of course, has sought to see in Saussure's distinction a precursor of his "competence/performance" dichotomy; see his (1963 : 326-331).

[2] On 'little man' talk, 'in the air' around this period perhaps because of Ryle (1949), see especially Morgenbesser (1969).

is the route followed in Quine (1951) and subsequently by Chomsky and leads, rather naturally though not necessarily, to the view that a grammar is a theory of the "intuitions of linguistic form" or of the "linguistic intuition" of the native speaker (these alternative formulations are not equivalent as will be pointed out later). In this way, claims about justification, the 'truth' or 'correctness' of a grammar, as a predictive theory, are ultimately to be anchored in an evidential basis of acceptability judgements of native speakers. We shall argue in Chapter 4 that short of someone actually developing acceptability tests which do accurately distinguish e.g., between selectionally deviant sequences and ungrammatical sequences, [1] there is little prospect of attempting to ground the notion of 'grammaticality' (or 'grammatical sentence of L') in acceptability judgements, a conclusion Chomsky also reaches but with different result. Clearly there must be some behavioral correlate or evidence of a specification of 'grammaticality', i.e., grammatical sequences must be distinguished as useable communications or be 'sayable' or the like, but from this nothing necessarily interesting follows (as Hockett has argued) about the biological endowment of the individual speaker. Rather the restrictions on word combinations (the characterization of which provides a structure for language) can be viewed - to speak incautiously for the moment in a teleological vein - as constraints required to distinguish information, required for the use of language as an instrument for the social transmission of information, a theme to which we return in Chapter 5.

1 Such as exists between The universe plays the oozing slime astutely and Go and the up to, e.g.. See further Chapter 4 §2 on this distinction.

Chomsky's retrospective willingness to read a 'realist' interpretation into these remarks of Hockett's are in step with his identification of the notions of 'making an empirical claim' and 'making a claim about biological structure', i.e., the "innate language faculty". Thus he has sought to interpret structural and descriptive linguistics as, to the extent that this discipline can be said to have made empirical claims at all, making claims - which are not very plausible, he adds - about the nature of this device. [1] Such a construal of structural linguistics,

[1] E.g., Chomsky (1965a) and (1975a):

> If we interpret taxonomic linguistics as making an empirical claim, this claim must be that the grammars that result from application of the postulated procedures to a sufficiently rich selection of data will be descriptively adequate - in other words, that the set of procedures can be regarded as constituting a hypothesis about the innate language-acquisition device (1965a: 52-3).
>
> General properties of language, if not merely historical accident and thus of no real interest, must be attributable to an intersection of (1) genetically determined mechanisms of mind and (2) uniformities in the empirical conditions of language use and acquisition. An explicit linguistic theory of either the taxonomic or nontaxonomic variety can be understood as an empirical hypothesis concerning factors of the former sort....Thus if one takes a realist interpretation of the work of the post-Bloomfieldian theorists, they are proposing quite deep linguistic universals: the principles implied by their procedural methods. Under this interpretation, it is postulated that human languages must have the properties determined by application of these procedures to a corpus of data. My own decision to abandon taxonomic approaches resulted from an increasingly firm belief that languages simply do not have these properties (1975a: 37).

as we have argued throughout this chapter, is not textually supported in the writings of the structuralists themselves. Indeed, it is perverse in the light of what structural linguists say about the intent and interpretation of their procedures and, moreover, of their understanding of the notion of language structure.

2.6 Prediction. The issue of empirical adequacy has been linked, by Hockett, to the notion of predictiveness: 'game-playing' or non-uniqueness is simply an artifact of corpus-confined analysis. If the concern of the linguist is merely to rearrange or list in various ways the data of a fixed corpus, then, whatever he is doing, he is not making any empirical claims. The criteria governing his description have nothing to do with correctness or 'truth' but rather with his inclination or particular purpose in mind. However, if the character of his description is such that he makes (or can make) predictions about utterances which are not included in the analyzed corpus, he is making explicit empirical claims which can be tested.

Chomsky has also argued this way against Harris although posed in this fashion, a crucial additional aspect of determining adequacy is omitted. For Chomsky, that a grammar makes predictions is not a sufficient criterion of adequacy but only a necessary one; there may be many such grammars which are empirically equivalent descriptions of a corpus and extensions of a corpus. This situation requires the imposition of a metatheory of grammar, a general theory of language structure, which includes a formal algorithm to select, on grounds of simplicity (a.k.a. generality), the simplest such grammar compatible with the empirical data.

This is the two-tiered program of adequacy first articulated in the unpublished typescript THE LOGICAL STRUCTURE OF LINGUISTIC THEORY, dated June, 1955, and discussed further in Chapter 3.

For the present we are concerned with the alleged solution to non-uniqueness posed by predictive grammars. We may note first of all that an assumption has been made that there are (or were) linguists who, in describing a corpus, intend or entail nothing regarding utterances of the language which are not included in the corpus under analysis. Hockett apparently has Harris in mind as an exemplar of this position but the attribution has been later broadened to the entire discipline of descriptive linguistics (ironically including Hockett) in the widespread employment of the term 'taxonomic linguistics'. [1] Thus a generally disseminated characterization of 'taxonomic linguistics' is that its primary concern lay in the formulation of techniques and procedures for describing a closed corpus of utterances. Consequently, we have received a view of descriptive linguists as "dull catalogers of data" (Lees, 1957: 41), of descriptive linguistics as the practice of "pre-Darwinian taxonomy" (Chomsky, 1964: 25) which occupies a correspondingly low-level "Baconian" stage of science (Bach, 1965), and of the "only alternative" to the

1 The origin of the term (as 'taxonomic model') is Chomsky (1964) who, however, sees structural and descriptive linguistics as 'summarized' in a model of phrase structure grammar which is "in the spirit of modern procedural and descriptive approaches": "It should be noted, however, that modern grammars are typically not conceived as generative grammars, but as descriptive statements about a given corpus (text). Hence, the taxonomic model, as described below, is no more than an attempt to formulate a generative grammar which is in the spirit of modern procedural and descriptive approaches (11)." The reformulation was carried out in detail in Postal (1964).

"radical idealizations" of a theory of "internally represented" grammar as being "a form of natural history, tabulation and arrangement of facts, hardly a very serious pursuit however engaging the data" (Chomsky, 1980a: 218-20).

But on examination this view of descriptive ('taxonomic') linguistics seems little more than a cliff over which to push one's opponents. Boas, to begin at the beginning, regularly sought to determine whether presented descriptions of linguistic structure were both complete and correct (the tests of "exhaustiveness" and "vulnerability" in the Voegelin's (1963: 14) terminology) and this involved matching the results obtained from elicited utterances with non-elicited texts which were freely selected by the informant from some internal cultural domain or folklore. These latter texts were essential, as Boas considered them as the test of a grammar. Bloomfield, as Chomsky has recognized [1], wrote a 'generative' formulation of the morphophonemics of Menomini, a language of the Algonquin family spoken by some 1700 people in Wisconsin. In this work, there is explicit employment of base and derived forms to characterize any utterance of the language:

> The process of description leads us to set up each morphological element in a theoretical basic form, and then to state the deviations from this basic form which appear when the element is combined with other elements. If one starts with the basic forms and applies our statements (...) in the order in which we given them, one will finally arrive at the form of words as they are actually spoken (1939b: 352).

To take another example, Harris' "Structural Restatements" of Swadesh's Eskimo, Newman's Yawelmani, and Voegelin's Delaware grammars had as

[1] Chomsky (1975a: fn. 45, 50-1)

their express purpose "the testing and exploring of statements of morphological structure" from the perspective of distributional analysis, adopted as a check on other methods of linguistic description.

> The present restriction to distributional relations carries no implication of the irrelevance or inutility of other relations of the linguistic elements, in particular their meanings. Information concerning the meanings is not derivable from the distributional statements and is clearly necessary for any utilization of the language. However, because of the differences between the distributional relations and such other relations as those of meaning and phonemic similarity, and because of the independence of each type of relation in respect to the others, it becomes desirable to examine each type of relation separately (1947: 217).

Perhaps no statement of descriptive linguistics [1] has aroused more critical ire than Harris':

> The overall purpose of work in descriptive linguistics is to obtain a compact one-one representation of the stock of utterances in the corpus (1951a: 366).

To Chomsky, and others following Chomsky's lead, this statement constitutes sufficient warrant for the claim that 'taxonomic linguistics' could not be making empirical claims, that matters of rearranging or listing the data of a corpus are outside the ken of empirical science, whose concerns have to do with matters of truth or falsity. For Bach, e.g., such a statement reflects the fact that

> Most of the time Harris talks as if the linguist could do without generalizations at all, in other words, as if the purpose of linguistic analysis were merely to rearrange the original data:... (1965: 121-2)

[1] Although Bloomfield's "The only useful generalizations about language are inductive generalizations" (1933: 20) would have to be considered also a contender.

The implication is that distributional analysis yields results which can be considered only in terms of convenience or convertibility for one purpose or another, that generalizations are not sought, and that no evidence can either conflict with a distributional statement or be offered in its support. [1] However, none of the critical sources we have found proceed to cite the context of this remark of Harris' or the two immediately following sentences, which give an idea of what Harris meant by referring to a "one-one representation":

> Since the representation of an utterance or its parts is based on a comparison of utterances, it is really a representation of distinctions. It is this representation of differences which gives us discrete combinatorial elements (each representing a minimal difference) (367).

[1] Chomsky (1960):

> There is a serious point at issue. Procedures that merely lead to a one-one representation of the corpus have no empirical import and can be neither criticized nor supported by any evidence. They are merely a convenience for the analyst, and he can select those he likes at will. Procedures that take "an inductive step", however, as the morpheme-to-utterance procedures of Harris' Methods, make an important empirical claim (i.e., that such-and-such items not in the corpus are grammatical sentences - and are, furthermore, sentences of a particular structural type), and thus can be judged in terms of truth and falsity. This distinction has not been clearly drawn in procedural linguistics...(fn. 17, 538).

Chomsky is thus of apparently two minds as to whether empirical claims are being made by the procedures of Methods, indicating that this point has not been sufficiently appreciated in descriptive linguistics. Elsewhere, he speaks of the incompatibility of the statement of Harris cited above and Harris (1951a: 372-3) that "the work of analysis leads right up to the statements which enable anyone to synthesize or predict utterances in the language", noting that "these conflicting remarks...illustrate a general ambivalence concerning goals that makes evaluation of modern taxonomic linguistics on its own terms rather difficult" (1964: 98). However, here Chomsky seems to have forgotten his earlier appreciation of the morpheme-to-utterance procedures (of chapter 16) of Harris' Methods as he writes regarding the goal of a predictive grammar: "there are no known procedures which lead to this more ambitious, and far more significant goal" (ibid).

In other words, to require a one-one representation of the stock of utterances of a corpus is to require that grammatical statements represent only distributional differences established by "comparison of utterances"; as observed above (p.56) this means that grammatical symbols can be taken as representing not "particular observable elements which occupy an environment but rather the environment itself", i.e., positions in a structure. To require a one-one representation is thus to require that a difference in grammatical position (relative to all other positions) is to be represented by a distinct grammatical symbol. It is not a demand that every item identified in a corpus be uniquely listed. Such an interpretation betrays a hardly excusable unfamiliarity with the methodological esprit which motivates this work.

To the contrary, in formulating grammatical statements the successive levels of analysis and choice of various procedures are offered precisely to remove redundancy from linguistic description so as to "not say the same thing twice". [1] Utterances are described by continually replacing elements that have greater and more complex restrictions on their occurrence by elements of wider combinability, a principle whose significance we return to in Chapters 5 and 6. Harris summarized this methodology as follows:

> As a result of these operations, we not only obtain initial elements but are also able to define new sets of elements as classes or combinations (sequences, etc.) of old ones. While the successive classifications are based on differences in occurrence, these differences are expressed in the particular definitions of each class, and the relations

[1] Wells (1963: 42).

> among these classes can then be investigated without regard to the differences in their definitions....
>
> This leads ultimately to sets of few elements having complex definitions but as nearly as possible random occurrence in respect to each other, replacing the original sets of many elements having simple definitions but complexly restricted distribution (1951a: 369-370).

As for the charge that "most of the time Harris talks as if the linguist could do without generalizations at all", we can only conclude that Bach had no first-hand familiarity with Methods, for even a casual perusal of this book belies such a conclusion; e.g.,

Chapter 7 ("Phonemes"):

> We now seek a more efficient set of symbols for our segments, one in which there are fewer elements, and in terms of which we can state more compactly which sequences of these occur (59).

Chapter 8 ("Junctures"):

> We reduce the number of phonemes, and simplify the statement of restrictions upon the environments in which they occur by considering those restrictions of environment which apply to large numbers of phonemes (79).

Chapter 9 ("Rephonemicization"):

> We would like to eliminate some of those exceptional restrictions not by modifying our operational definition of a phoneme (§ 7.5), nor by changing the criteria we seek to satisfy, but by performing a further operation, if possible, on the restricted segments in order to make them amenable to those phonemic groupings which would satisfy our preference (90).

Chapter 10 ("Phonemic Long Components"):

> We seek to express the limitations of distribution among phonemes and to obtain less restricted elements (125).

Chapter 11 ("Phonological Structure"):

> However, we may also wish to have a compact statement of how these elements occur in any utterance of the corpus, so that we can make general statements not only about the elements but also about the utterances which we represent by these elements (150).

Chapter 12 ("Morphological Elements: Morphemic Segments):

> We therefore seek a way to treat sequences of phonemes as single longer elements (157).
>
> Having established in what way our utterance differs minimally from others, we choosethat manner of distinguishing our utterance from the others which has the greater generality; i.e., we define the elements that distinguish our utterance in such a way that general things can be said about the distribution of those elements (163).

Chapter 13 ("Morpheme Alternants"):

> The following chapters present a series of operations designed chiefly to reduce the number of elements for linguistic description ... We seek to obtain fewer elements having fewer restrictions on occurrence (197).

Chapter 14 ("Morphophonemes"):

> In general, the setting up of such new morphophonemic elements will be easier, the greater the phonemic similarity among the members of a morpheme. And, over the whole corpus, if more of the morphemes have, in identical environments, identical alternations among their members, fewer morphophonemic elements will be set up; for then the morphophonemes set up for one morpheme will also serve for many other morphemes. It is therefore important to discover which alternations occur in many morphemes (219).

Chapter 15 ("Morpheme Classes"):

> We seek to reduce the number of elements, in preparation for a compact statement of the composition of utterances. We furthermore seek to avoid repeating almost identical distributional statements for many morphemes individually (243).

Chapter 16 ("Morpheme Sequences"):

> We seek to reduce the number of classes which we require when we state the composition of each utterance of the language; and to make it unnecessary to state in chapter 19 the special restrictions of certain subclasses (262).
>
> The comparison of all the sequences containing a particular class permits various generalizations concerning that class (276).

Chapter 17 ("Morphemic Long Components"):

> We seek to express compactly the remaining relations among morpheme classes, other than those which are explicitly indicated in 13-6 (299).

Chapter 18 ("Constructions"):

> We note recurrent sets of similar morpheme classes, independently of how these classes or arrangements fit into the utterance.... To a large extent this attempt to summarize the recurrent arrangements of classes combines, or may conveniently begin by combining, the results of 16.5 and 17. The considerations of both of those sections lead to recognizing various larger-than-one morpheme-length portions of utterances: in 16.5, these portions are the immediate constituents (at successive levels of analysis) of an utterance or stretch of speech; in chapter 17, the domains of the components. Here we will go beyond these combined results, in seeking identities and similarities in other features as well as in those previously considered....
>
> We classify into one construction all sequences which are similar in respect to stated features (325).

Chapter 19 ("Morphological Structure"):

> We state which sequences of the resultant position classes of chapter 16 or the constructions of chapter 18 occur as utterances in the corpus.
>
> This procedure, like that of chapter 11, consists in making an assertion of occurrence rather than a relational statement: not that X occurs next to or is substitutable for Y, but that utterances consisting of XY occur. In order to make these assertions as condensed and as general as possible, they are put in the most general terms: i.e., they state the occurrence of the most general classes or constructions (349).

We should think it would be extremely difficult to fabricate a greater calumny than that "most of the time Harris talks as if the linguist could do without generalizations at all" and must defer to more Kuhnian or sociologically-inspired endeavors any further comments upon it. But more generally, against the insinuation that distributional procedures yield only a listing of elements [1], Harris directly states the reason - not all combinations of elements occur - why distributional structure is not a mere listing. And contrary to the purported non-empirical character of "taxonomic linguistics", he provides as explicit an empirical

[1] E.g., Leiber (1975: 34)

claim as can be found in the literature of linguistics:

> If all combinations of our elements occurred, there would be nothing to say except a listing of the elements and the statement that all combinations of them occur,...
>
> However, it is almost impossible for all sequences of all simultaneous combinations of all elements (in all degrees of repetition) to occur, in any language....
>
> Our statement of all the combinations of elements which occur in any utterance of a corpus is shorter than an actual list of all the utterances in it: first, because we do not distinguish between sequences which are composed of the same elements in the same order; and second, because all elements which occur in the same environment are included in the same general statement of occurrences, and may be indicated by the same mark....
>
> We now try to find a sequence of phoneme classes which is constantly repeated, so that we can say that every utterance and the whole succession of utterances in our corpus is merely a repetition many times over of this one sequence.
>
> Thus for Yokuts it is possible to state the following formula: [1]
>
> $$\#[C\ V\ (C)]\ C\ V\ (C)\#$$
>
> where ## indicates utterance juncture and any utterance contour over the preceding stretch, up to the next # ; C any consonant, V any vowel, . the length phoneme;...; sections in parenthesis ( ) sometimes occur and sometimes do not; the section in square brackets [ ] occurs any number of times from zero up. ...Repeating this entire formula any number of times, and substituting for each mark any phoneme (or in the last analysis any segment) which that mark represents, we would obtain any utterance of Yokuts. Conversely, all Yokuts utterances can be represented by this sequence repeated the required number of times (1951a: 150-2).

[1] On Yokuts, a language of California, see Harris (1944) where this formulation is first presented on the basis of Newman's data. We note in the present context a passage at p. 199: "Both Newman's method and the alternative (distributional - TR) methods indicated above are essentially similar in that they describe particular events or relations in terms of general systemic relations. This was indeed the great contribution of Sapir's talking about configuration and pattern."

As regards Methods in particular, the allegation that the issue of predictiveness is ignored or absent or not clearly distinguished is simply false. The issue is repeatedly raised in terms of testing the adequacy of the linguist's characterization of structure and stress is placed upon the point that linguistic description is of interest insofar as it serves as a predictive sample of the langauge under analysis.[1] What is really of interest, from the point of view of linguistic metatheory, as opposed to correcting widespread inaccuracies about the methods, aims and results of descriptive linguistics, is the nature of grammatical prediction: what is it for a grammar to make a prediction ?

Harris had recognized two related ways in which a grammar could be considered predictive: as stating regularities whose domain was posited as extending over the language as a whole, or as a means of synthesizing utterances in the language.

> There is in general a choice of purposes facing the investigator in linguistics. He may seek all the regularities which can be found in any stretch of speech so as to show their interdependencies (e.g., in order to predict successfully features of the language as a whole); or he may seek just enough information to enable anyone to construct utterances in the language such as those constructed by native speakers (in order to predict the utterances, or to teach a person how to speak the language) (1951a: 365).

[1] E.g. (244): "The interest in our analysis of the corpus derives primarily from the fact that it can serve as a predictive sample of the language." Cf. (17): "When a linguist offers his results as a system representing the language as a whole, he is predicting that the elements set up for his corpus will satisfy all other bits of talking in that language."

An example of the former was given above (p. 88) in the formulaic statement of vowel-consonant patterning to which any Yokuts utterance is predicted to conform. But we shall discuss the latter alternative first as the notion of 'synthesizing utterances' has been subjected to a variety of interpretations, particularly as regards the psychological relevance of the predictive theory, i.e., the grammar.

A grammar which synthesizes utterances might seem to have rather obvious psychological implications when considered as a model of competence [1] or a model of language use or verbal behavior. But, as may be inferred from our previous discussion, Harris in raising the issue in Methods was not alluding to psychological considerations or any particular psychological theory. Harris had indeed observed that a grammar could be formulated as a deductive system, a statement often seen as precursing generative grammar. But such a system was not to be considered "as an empirical hypothesis with regard to the language faculty", as Chomsky surmises. [2] It is perhaps by turning to two papers of 1952 that we can better illustrate what Harris may have intended by speaking of predicting utterances by synthesizing them.

[1] Cf. Langendoen (1979: 150): "...the theory of competence is not a theory about linguistic performance (rather, it is a theory about sentence types, of which tokens may be manifested when people use language to talk to themselves, or to one another),..."

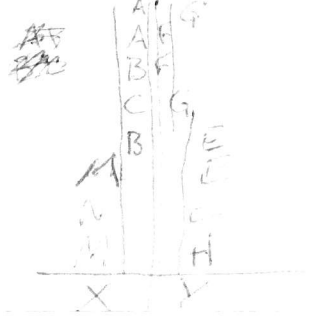
[2] "The work of analysis leads right up to the statements which enable anyone to synthesize or predict utterances in the language. These statements form a deductive system with axiomatically defined initial elements and with theorems concerning the relations among them. The final theorems would indicate the structure of the utterances of the language in terms of the preceding parts of the system " (372-3). For the interpretation, see e.g., Chomsky (1975a: 11 and fn. 16) from whence the quotation.

These papers are the first widely available reports on the work Harris had conducted, beginning in the late 1940's, on the analysis of discourse (i.e., connected speech or writing) which initiated investigations of grammatical transformations. In brief, the object of discourse analysis (as presented at that time) was to determine the interrelations of the elements (morphemes or morpheme sequences) of a text on the basis of their occurrence in that text alone. This was done by considering which morpheme sequences were equivalent (with respect to a given text) by the fact of their occurring either in identical sentence environments (as when sentences or parts of sentences are repeated) or in environments which could be demonstrated to be equivalent by substitutional comparisons with other sentences of the text.[1]

In so doing, not only would the occurring sentences of a text be represented as sequences of morpheme sequence classes, information which might be compared to a dictionary of words and a listing of word classes, but, in addition, the structural analysis could be considered as a 'grammar' which, in specifying the possible, not merely occurring, sequences of morpheme sequences, permits the 'derivation' of new sentences not occurring in the text but nevertheless in conformity with the established restrictions. Thus, in the sample text analyzed, a sentence is derived[2] —

[1] E.g., in a text consisting of the following sentences (represented as morpheme class sequences): AF : BE : CG : BF : ME : AG : NE : NG : MH, two equivalence classes, X and Y, may be set up. X = {A, B (because of AF and BF), C (because of AG and CG), M and N (because of BE and ME and NE)} and Y = {F, E (because of BF and BE), G (because of AF and AG), and H (because of ME and MH)} . Adapted from Harris (1952b: 349).

[2] (1952b: 368)



The existence of monopoly enterprise is enough to build economic crisis - obviously unintended by the author, as satisfying the structural analysis of the text. Grammatical prediction as illustrated in discourse analysis serves, in this case, to critique the facile argumentation of a text through the synthesis of an unintended sentence which nevertheless meets just those intratextual structural conditions satisfied by (and characterizing) the occurring sentences. In these terms it makes little sense to raise the issue of the psychological relevance of the discourse grammar. In fact, the problem Chomsky has repeatedly identified as central to linguistic theory but as unresolved in structural linguistics [1] - that of how a grammar is to "take an inductive step", to "project" from a corpus of utterances (or "primary linguistic data" in the later innatist version of generative grammar) to the infinitely many 'remaining' sentences of a language (see Chapter 3) - is not formulable in the terms of discourse analysis, as presented here. Whatever sense can be made of the notion of synthesizing arbitrary sentences of a language (given the presence of dialect variation, borrowings, slang and the like), this was not a issue in discourse analysis, the area where Harris had turned to further extend analytic procedures.

In as much as a grammar can be considered as a theory of the language as a whole, the task of the linguist can be seen as that of attempting to specify the notion 'grammatical sentence of L'. One way of thinking about

1 E.g., Chomsky (1964: 23); (1965 : 202, fn. 20); (1975a: 30). Chomsky's more recent downgrading of the notion of 'language' together with an admission that grammars may not generate 'languages' at all (e.g., (1980a: 126) has obvious bearing here and is considered further in Chapter 4.

this which Chomsky has been extremely influential in promoting is to consider a language as an infinite set of sentences and to view a grammar as a formal device that 'selects' (or: 'recognizes', 'generates') [1] all and only the grammatical strings of symbols (words) of a language. That is, given a finite vocabulary and the infinite set of strings formed by arranging the words of the vocabulary in all possible combinations, a grammar will select some subset of these strings, the language generated (recognized, defined, characterized, etc.) by the grammar. We may note that the condition of membership in the set of selected strings is not 'fuzzy' or probabilistic; a string is a grammatical sentence iff it is recognized or selected (is in or out) by the device.

It is only natural to raise the issue of the psychological implications of such a formal device, but this need not entail, as Chomsky has repeatedly admonished, that such a device should be considered as a model of linguistic behavior, of either a speaker or a hearer. Chomsky, of course, was not the first to point out the possible psychological relevance of a formal grammar conceived in this way. For example, Skinner had said that provided sufficient information about the environment of the speaker and the situation of utterance and the speaker's prior verbal behavior in response to past stimuli (here we gloss details), it would be possible to predict the probability of occurrence of particular

[1] These alternative formulations may suggest we are ignoring the issue of whether a grammar generates recursive or only recursively enumerable sets, about which there is a long literature beginning with Putnam (1961). We do this because we think this is a pseudoproblem stemming from thinking of a language as a (well-defined) set of sentences. See Scott (1973) and further in Chapter 4 §2.

utterances. [1] We noted above that Bloomfield, for all his behaviorism, real and alledged, held this to be an unattainable objective. However, Shannon (1948) had suggested that the source of messages in a discrete communication system was representable as a stationary stochastic (Markov) process that selected successive elements of the message from a finite vocabulary according to fixed probabilities. In particular, all information about the history of a sequence is given when the single, immediately preceding selection is known. This seemed to some psychologists and linguists [2] to provide a model of verbal behavior quite compatible with Skinner's behaviorist account. Hypothetically, if an environmental stimulus resulted in a verbal response, say the uttering of a word, that event would in turn provide a further stimulus for another verbal response and so on. If it were possible to state the rules of a grammar in terms of the possible continuations of a string of word responses, those rules could be interpreted directly as the result of stimulus-response learning. [3] Chomsky, in an early paper which attracted considerable attention, demonstrated the in principle inadequacy of the conception of grammatical structure provided by strictly linear (in English, left-to-right) scanning devices such as Markov and other finite state sources. He showed, notably, that such devices could not express grammatical

[1] Skinner (1957: 22): "Every verbal operant may be conceived of as having under specified circumstances an assignable probability of emission...." Cf. (28): "The probability that a verbal response of a given form will occur at a given time is the basic datum to be predicted and controlled."

[2] E.g., Miller and Selfridge (1950), Hockett (1955).

[3] Miller (1977: 118).

dependencies (such as number agreement) across unbounded domains such as existed in self-embedded sentences. [1]

The problem is then to characterize this hypothetical selective device. Since speakers are finite beings, it is required that the grammar be finitely representable. On the other hand, under the usual assumption that there is no longest grammatical sentence, a grammar must allow for operations which may iterate indefinitely many times. Accordingly, the linguist's task will be the specification of the finitely many restrictions or 'rules' which determine the selected set of strings.

In what sense, then, can a grammar - viewed as a formal device - be said to make an empirical claim? Obviously, just in case the sequences it selects are determined to be grammatical sentences by native speakers of the language. This condition can be expressed more precisely as follows: given a sequence of words antecedently specified as grammatical by a reliable native informant, this sequence will eventually 'turn up' on the list of selected sequences. In other words, empirical adequacy requires that the set of 'correct' strings be somehow specified as a 'target' set, i.e., specified as grammatical by native speakers, either by using elicitation techniques, or by querying acceptability judgements, or, as is more usual in the case where the language is the linguist's native tongue, by the linguist consulting his own 'intuition' as to the well-formedness of particular strings.

[1] Chomsky (1956). E.g., The men that John recommended that Mary hire are here, The men that John recommended that Mary hire on the basis of Bill's report are here, etc. The inadequacies of a strictly linear account of sentence structure had already been discussed by Harris in Methods (271-2) with She made him a good husband because she made him a good wife where the hearer has to interpret the first made (made him into) on the basis of the later occurrence (made for him) . Non-linear dependencies were also

Notice that since obviously only finitely many strings of elements can be operationally ascertained to be sentences of the language as determined by informant's judgements or reactions (including the linguist's own) and since there are, by hypothesis, infinitely many grammatical sentences [1], there are infinitely many sentences predicted by the grammar which will never be tested as to their well-formedness. Thus empirical adequacy seems to require that a grammar, viewed as a selective device, predict or generate sequences antecedently determined to be well-formed by native speakers and, on the basis of rules characterizing these sentences and perhaps other considerations (including simplicity), that it 'project' from this set of sentences to the infinitely many sentences of the language. It has been found to be, as we shall discuss in Chapter 4, too strong a requirement to hold that every antecendently-determined grammatical sentence be generated by a grammar in this manner. In fact, 'S_i is a grammatical sentence of L' has been held to mean only 'S_i is generated/characterized by the rules of the grammar' even if it has been determined that S_i is not an acceptable sentence of the language. In raising this issue here, we wish only to establish the point that the notion of prediction, as applied to a grammar viewed as a formal device, requires a prior determination of well-formedness, at least for some sentences. The nature of this relation is, however, not straightforward. For example, is it required that the grammar predict all the sentences determined to be well-formed in the linguist's corpus of data? Or only some

(continued from previous page)

pointed to by K.S. Lashley's (1951) classic example: Rapid righting with his uninjured hand saved from loss the contents of the canoe.

[1] Since there is no longest grammatical sentence. Cf. Chomsky (1956: 109): "In general, the assumption that languages are infinite is made for the purpose of simplifying the description."

This looks ahead to chapters 5 & 6 and will be unintelligible to most readers. Give them some help!

of these, e.g., "clear cases" ? [1]

It remains to recall the first alternative outlined by Harris above pertaining to the predictiveness of grammars, that of "seeking all the regularities... so as to show their interdependence". This mode of testing the adequacy of grammars is directly the descendant of that adopted in anthropological linguistics (see above) and is firmly corpus-based. The test consists in determining whether the grammatical restrictions as determined in a corpus of sentences, or utterances or texts on distributional (and transformational - for the distinction, see Chapter 5) grounds account for (are observed to obtain in) other sentences or utterances later added to the corpus. Only a test of this kind could apply to, e.g., the grammatical analysis of written discourse, as in a sublanguage of science (Chapter 6). Here, the only sense that one can make of 'predict' is that new sentences of the sublanguage can be 'housed' within the restrictions established for previous sentences. But it is also the character of the grammatical description (which is of restrictions on combinations of elements) that is empirically significant. In particular, since grammatical description is of redundancies (restrictions) not eliminable by a determinate set of transformational operations, specified as to the domain of their application and meeting the semantic criterion of paraphrase (which is operationally controlable), the description has an informational character.

To specify the notion of 'grammatical sentence of L' is then to provide a principle of analyzability, a combinatorial means of analysis, which, applied to any sentence of the language (or any string of elements)

[1] See the discussion in Chapter 3 §2 and Chapter 4 §2 below.

provides its decomposition into recurrent elements, i.e., residual restrictions on combinations (or shows the elements are not a possible combination of the language). But the analysis of a sentence in terms of its recurrent elements always requires in addition a specification of the domain of the operations which identify the recurrent elements. For a 'sentence-grammar' of the language as a whole (e.g., for English, Harris (1982)) which idealizes away from the actual occurrence of language in discourses, it turns out that an operator grammar of word dependences gives a general solution (in terms of a partially ordered predicational constraint) to stating the sentence-bounded restrictions, a solution which would hardly be possible with a morpheme or morpheme class analysis. But the additional restrictions on combinations of elements which characterize connected discourse, in a particularly striking fashion in a semantically restricted domain such as a sublanguage of a science, provide the basis for a sufficiently articulated structure having a palpably meaningful character: the 'grammar' of the sublanguage serves as a compact but informationally (in part, paraphrastically) equivalent representation of the texts themselves, as confirmed by special sublanguage 'informants' who are researchers in the field. [1]

The general point is that 'predictiveness' here is with respect to the stated domain of the restrictions on combinations of elements. If additional restrictions can be identified in a subdomain of a language, then on the basis of these additional restrictions, a particular type of sentence is predicted, viz., one in which the stated restrictions - or statable departures from these - can be determined to hold.

[1] Chapter 6.

It is then possible to provide empirical validation of the general theory of language structure which guides the construction of grammars with these informational properties. In a specific application to the sublanguage of a science, both the general theory of language structure (the predicational partial ordering and reductions - in phonemic shape - on grounds of low information) and the grammar of the language as a whole upon which the construction of particular sublanguage grammars rely, receive an empirical confirmation in the resultant informational character of the grammatical description. This confirms the posited theoretical link between language structure and information which implicitly guided the redundancy-eliminating methods of distributionalism, ~~can be seen to be established~~. But before discussing these matters in Chapters 5 and 6, we turn first to the program of validation set out in generative grammar, beginning with the earliest, and still in many ways the most complete formulation of a generative grammar.