

# ASPECTS OF THE THEORY OF SYNTAX

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# I

## *Methodological Preliminaries*

### § 1. *GENERATIVE GRAMMARS AS THEORIES OF LINGUISTIC COMPETENCE*

THIS study will touch on a variety of topics in syntactic theory and English syntax, a few in some detail, several quite superficially, and none exhaustively. It will be concerned with the syntactic component of a generative grammar, that is, with the rules that specify the well-formed strings of minimal syntactically functioning units (*formatives*) and assign structural information of various kinds both to these strings and to strings that deviate from well-formedness in certain respects.

The general framework within which this investigation will proceed has been presented in many places, and some familiarity with the theoretical and descriptive studies listed in the bibliography is presupposed. In this chapter, I shall survey briefly some of the main background assumptions, making no serious attempt here to justify them but only to sketch them clearly.

Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance. This seems to me to have been the position of the founders of modern general linguistics, and no cogent reason for

modifying it has been offered. To study actual linguistic performance, we must consider the interaction of a variety of factors, of which the underlying competence of the speaker-hearer is only one. In this respect, study of language is no different from empirical investigation of other complex phenomena.

We thus make a fundamental distinction between *competence* (the speaker-hearer's knowledge of his language) and *performance* (the actual use of language in concrete situations). Only under the idealization set forth in the preceding paragraph is performance a direct reflection of competence. In actual fact, it obviously could not directly reflect competence. A record of natural speech will show numerous false starts, deviations from rules, changes of plan in mid-course, and so on. The problem for the linguist, as well as for the child learning the language, is to determine from the data of performance the underlying system of rules that has been mastered by the speaker-hearer and that he puts to use in actual performance. Hence, in the technical sense, linguistic theory is mentalistic, since it is concerned with discovering a mental reality underlying actual behavior.<sup>1</sup> Observed use of language or hypothesized dispositions to respond, habits, and so on, may provide evidence as to the nature of this mental reality, but surely cannot constitute the actual subject matter of linguistics, if this is to be a serious discipline. The distinction I am noting here is related to the *langue-parole* distinction of Saussure; but it is necessary to reject his concept of *langue* as merely a systematic inventory of items and to return rather to the Humboldtian conception of underlying competence as a system of generative processes. For discussion, see Chomsky (1964).

A grammar of a language purports to be a description of the ideal speaker-hearer's intrinsic competence. If the grammar is, furthermore, perfectly explicit — in other words, if it does not rely on the intelligence of the understanding reader but rather provides an explicit analysis of his contribution — we may (somewhat redundantly) call it a *generative grammar*.

A fully adequate grammar must assign to each of an infinite range of sentences a structural description indicating how this

sentence is understood by the ideal speaker-hearer. This is the traditional problem of descriptive linguistics, and traditional grammars give a wealth of information concerning structural descriptions of sentences. However, valuable as they obviously are, traditional grammars are deficient in that they leave unexpressed many of the basic regularities of the language with which they are concerned. This fact is particularly clear on the level of syntax, where no traditional or structuralist grammar goes beyond classification of particular examples to the stage of formulation of generative rules on any significant scale. An analysis of the best existing grammars will quickly reveal that this is a defect of principle, not just a matter of empirical detail or logical preciseness. Nevertheless, it seems obvious that the attempt to explore this largely uncharted territory can most profitably begin with a study of the kind of structural information presented by traditional grammars and the kind of linguistic processes that have been exhibited, however informally, in these grammars.<sup>2</sup>

The limitations of traditional and structuralist grammars should be clearly appreciated. Although such grammars may contain full and explicit lists of exceptions and irregularities, they provide only examples and hints concerning the regular and productive syntactic processes. Traditional linguistic theory was not unaware of this fact. For example, James Beattie (1788) remarks that

Languages, therefore, resemble men in this respect, that, though each has peculiarities, whereby it is distinguished from every other, yet all have certain qualities in common. The peculiarities of individual tongues are explained in their respective grammars and dictionaries. Those things, that all languages have in common, or that are necessary to every language, are treated of in a science, which some have called *Universal* or *Philosophical* grammar.

Somewhat earlier, Du Marsais defines universal and particular grammar in the following way (1729; quoted in Sahlin, 1928, pp. 29-30):

Il y a dans la grammaire des observations qui conviennent à toutes les langues; ces observations forment ce qu'on appelle la grammaire

générale: telles sont les remarques que l'on a faites sur les sons articulés, sur les lettres qui sont les signes de ces sons; sur la nature des mots, et sur les différentes manières dont ils doivent être ou arrangés ou terminés pour faire un sens. Outre ces observations générales, il y en a qui ne sont propres qu'à une langue particulière; et c'est ce qui forme les grammaires particulières de chaque langue.

Within traditional linguistic theory, furthermore, it was clearly understood that one of the qualities that all languages have in common is their "creative" aspect. Thus an essential property of language is that it provides the means for expressing indefinitely many thoughts and for reacting appropriately in an indefinite range of new situations (for references, cf. Chomsky, 1964, forthcoming). The grammar of a particular language, then, is to be supplemented by a universal grammar that accommodates the creative aspect of language use and expresses the deep-seated regularities which, being universal, are omitted from the grammar itself. Therefore it is quite proper for a grammar to discuss only exceptions and irregularities in any detail. It is only when supplemented by a universal grammar that the grammar of a language provides a full account of the speaker-hearer's competence.

Modern linguistics, however, has not explicitly recognized the necessity for supplementing a "particular grammar" of a language by a universal grammar if it is to achieve descriptive adequacy. It has, in fact, characteristically rejected the study of universal grammar as misguided; and, as noted before, it has not attempted to deal with the creative aspect of language use. It thus suggests no way to overcome the fundamental descriptive inadequacy of structuralist grammars.

Another reason for the failure of traditional grammars, particular or universal, to attempt a precise statement of regular processes of sentence formation and sentence interpretation lay in the widely held belief that there is a "natural order of thoughts" that is mirrored by the order of words. Hence, the rules of sentence formation do not really belong to grammar but to some other subject in which the "order of thoughts" is studied. Thus in the *Grammaire générale et raisonnée* (Lancelot



*et al.*, 1660) it is asserted that, aside from figurative speech, the sequence of words follows an "ordre naturel," which conforms "à l'expression naturelle de nos pensées." Consequently, few grammatical rules need be formulated beyond the rules of ellipsis, inversion, and so on, which determine the figurative use of language. The same view appears in many forms and variants. To mention just one additional example, in an interesting essay devoted largely to the question of how the simultaneous and sequential array of ideas is reflected in the order of words, Diderot concludes that French is unique among languages in the degree to which the order of words corresponds to the natural order of thoughts and ideas (Diderot, 1751). Thus "quel que soit l'ordre des termes dans une langue ancienne ou moderne, l'esprit de l'écrivain a suivi l'ordre didactique de la syntaxe française" (p. 390); "Nous disons les choses en français, comme l'esprit est forcé de les considérer en quelque langue qu'on écrive" (p. 371). With admirable consistency he goes on to conclude that "notre langue *pédestre* a sur les autres l'avantage de l'utile sur l'agréable" (p. 372); thus French is appropriate for the sciences, whereas Greek, Latin, Italian, and English "sont plus avantageuses pour les lettres." Moreover,

le bon sens choisirait la langue française; mais . . . l'imagination et les passions donneront la préférence aux langues anciennes et à celles de nos voisins . . . il faut parler français dans la société et dans les écoles de philosophie; et grec, latin, anglais, dans les chaires et sur les théâtres; . . . notre langue sera celle de la vérité, si jamais elle revient sur la terre; et . . . la grecque, la latine et les autres seront les langues de la fable et du mensonge. Le français est fait pour instruire, éclairer et convaincre; le grec, le latin, l'italien, l'anglais, pour persuader, émouvoir et tromper: parlez grec, latin, italien au peuple; mais parlez français au sage. (pp. 371-372)

In any event, insofar as the order of words is determined by factors independent of language, it is not necessary to describe it in a particular or universal grammar, and we therefore have principled grounds for excluding an explicit formulation of syntactic processes from grammar. It is worth noting that this naïve view of language structure persists to modern times in

various forms, for example, in Saussure's image of a sequence of expressions corresponding to an amorphous sequence of concepts or in the common characterization of language use as merely a matter of use of words and phrases (for example, Ryle, 1953).

But the fundamental reason for this inadequacy of traditional grammars is a more technical one. Although it was well understood that linguistic processes are in some sense "creative," the technical devices for expressing a system of recursive processes were simply not available until much more recently. In fact, a real understanding of how a language can (in Humboldt's words) "make infinite use of finite means" has developed only within the last thirty years, in the course of studies in the foundations of mathematics. Now that these insights are readily available it is possible to return to the problems that were raised, but not solved, in traditional linguistic theory, and to attempt an explicit formulation of the "creative" processes of language. There is, in short, no longer a technical barrier to the full-scale study of generative grammars.

Returning to the main theme, by a generative grammar I mean simply a system of rules that in some explicit and well-defined way assigns structural descriptions to sentences. Obviously, every speaker of a language has mastered and internalized a generative grammar that expresses his knowledge of his language. This is not to say that he is aware of the rules of the grammar or even that he can become aware of them, or that his statements about his intuitive knowledge of the language are necessarily accurate. Any interesting generative grammar will be dealing, for the most part, with mental processes that are far beyond the level of actual or even potential consciousness; furthermore, it is quite apparent that a speaker's reports and viewpoints about his behavior and his competence may be in error. Thus a generative grammar attempts to specify what the speaker actually knows, not what he may report about his knowledge. Similarly, a theory of visual perception would attempt to account for what a person actually sees and the mechanisms that determine this rather than his statements about what he sees and why, though these state-

ments may provide useful, in fact, compelling evidence for such a theory.

To avoid what has been a continuing misunderstanding, it is perhaps worth while to reiterate that a generative grammar is not a model for a speaker or a hearer. It attempts to characterize in the most neutral possible terms the knowledge of the language that provides the basis for actual use of language by a speaker-hearer. When we speak of a grammar as generating a sentence with a certain structural description, we mean simply that the grammar assigns this structural description to the sentence. When we say that a sentence has a certain derivation with respect to a particular generative grammar, we say nothing about how the speaker or hearer might proceed, in some practical or efficient way, to construct such a derivation. These questions belong to the theory of language use—the theory of performance. No doubt, a reasonable model of language use will incorporate, as a basic component, the generative grammar that expresses the speaker-hearer's knowledge of the language; but this generative grammar does not, in itself, prescribe the character or functioning of a perceptual model or a model of speech production. For various attempts to clarify this point, see Chomsky (1957), Gleason (1961), Miller and Chomsky (1963), and many other publications.

Confusion over this matter has been sufficiently persistent to suggest that a terminological change might be in order. Nevertheless, I think that the term "generative grammar" is completely appropriate, and have therefore continued to use it. The term "generate" is familiar in the sense intended here in logic, particularly in Post's theory of combinatorial systems. Furthermore, "generate" seems to be the most appropriate translation for Humboldt's term *erzeugen*, which he frequently uses, it seems, in essentially the sense here intended. Since this use of the term "generate" is well established both in logic and in the tradition of linguistic theory, I can see no reason for a revision of terminology.

## § 2. TOWARD A THEORY OF PERFORMANCE

There seems to be little reason to question the traditional view that investigation of performance will proceed only so far as understanding of underlying competence permits. Furthermore, recent work on performance seems to give new support to this assumption. To my knowledge, the only concrete results that have been achieved and the only clear suggestions that have been put forth concerning the theory of performance, outside of phonetics, have come from studies of performance models that incorporate generative grammars of specific kinds — that is, from studies that have been based on assumptions about underlying competence.<sup>3</sup> In particular, there are some suggestive observations concerning limitations on performance imposed by organization of memory and bounds on memory, and concerning the exploitation of grammatical devices to form deviant sentences of various types. The latter question is one to which we shall return in Chapters 2 and 4. To clarify further the distinction between competence and performance, it may be useful to summarize briefly some of the suggestions and results that have appeared in the last few years in the study of performance models with limitations of memory, time, and access.

For the purposes of this discussion, let us use the term “acceptable” to refer to utterances that are perfectly natural and immediately comprehensible without paper-and-pencil analysis, and in no way bizarre or outlandish. Obviously, acceptability will be a matter of degree, along various dimensions. One could go on to propose various operational tests to specify the notion more precisely (for example, rapidity, correctness, and uniformity of recall and recognition, normalcy of intonation).<sup>4</sup> For present purposes, it is unnecessary to delimit it more carefully. To illustrate, the sentences of (1) are somewhat more acceptable, in the intended sense, than those of (2):

- (1) (i) I called up the man who wrote the book that you told me about  
 (ii) quite a few of the students who you met who come from New York are friends of mine

- (iii) John, Bill, Tom, and several of their friends visited us last night
- (2) (i) I called the man who wrote the book that you told me about up
- (ii) the man who the boy who the students recognized pointed out is a friend of mine

The more acceptable sentences are those that are more likely to be produced, more easily understood, less clumsy, and in some sense more natural.<sup>5</sup> The unacceptable sentences one would tend to avoid and replace by more acceptable variants, wherever possible, in actual discourse.

The notion "acceptable" is not to be confused with "grammatical." Acceptability is a concept that belongs to the study of performance, whereas grammaticalness belongs to the study of competence. The sentences of (2) are low on the scale of acceptability but high on the scale of grammaticalness, in the technical sense of this term. That is, the generative rules of the language assign an interpretation to them in exactly the way in which they assign an interpretation to the somewhat more acceptable sentences of (1). Like acceptability, grammaticalness is, no doubt, a matter of degree (cf. Chomsky, 1955, 1957, 1961), but the scales of grammaticalness and acceptability do not coincide. Grammaticalness is only one of many factors that interact to determine acceptability. Correspondingly, although one might propose various operational tests for acceptability, it is unlikely that a necessary and sufficient operational criterion might be invented for the much more abstract and far more important notion of grammaticalness. The unacceptable grammatical sentences often cannot be used, for reasons having to do, not with grammar, but rather with memory limitations, intonational and stylistic factors, "iconic" elements of discourse (for example, a tendency to place logical subject and object early rather than late; cf. note 32, Chapter 2, and note 9, Chapter 3), and so on. Note that it would be quite impossible to characterize the unacceptable sentences in grammatical terms. For example, we cannot formulate particular rules of the grammar in such a way as

to exclude them. Nor, obviously, can we exclude them by limiting the number of reapplications of grammatical rules in the generation of a sentence, since unacceptability can just as well arise from application of distinct rules, each being applied only once. In fact, it is clear that we can characterize unacceptable sentences only in terms of some "global" property of derivations and the structures they define — a property that is attributable, not to a particular rule, but rather to the way in which the rules interrelate in a derivation.

This observation suggests that the study of performance could profitably begin with an investigation of the acceptability of the simplest formal structures in grammatical sentences. The most obvious formal property of utterances is their bracketing into constituents of various types, that is, the "tree structure" associated with them. Among such structures we can distinguish various kinds — for example, those to which we give the following conventional technical names, for the purposes of this discussion:

- (3) (i) nested constructions
- (ii) self-embedded constructions
- (iii) multiple-branching constructions
- (iv) left-branching constructions
- (v) right-branching constructions

The phrases *A* and *B* form a nested construction if *A* falls totally within *B*, with some nonnull element to its left within *B* and some nonnull element to its right within *B*. Thus the phrase "the man who wrote the book that you told me about" is nested in the phrase "called the man who wrote the book that you told me about up," in (2i). The phrase *A* is self-embedded in *B* if *A* is nested in *B* and, furthermore, *A* is a phrase of the same type as *B*. Thus "who the students recognized" is self-embedded in "who the boy who the students recognized pointed out," in (2ii), since both are relative clauses. Thus nesting has to do with bracketing, and self-embedding with labeling of brackets as well. A multiple-branching construction is one with no internal structure. In (1iii), the Subject Noun Phrase is multiple-branch-

ing, since "John," "Bill," "Tom," and "several of their friends" are its immediate constituents, and have no further association among themselves. In terms of bracketing, a multiple-branching construction has the form  $[[A][B]\cdots[M]]$ . A left-branching structure is of the form  $[[[\cdots]\cdots]\cdots]$  — for example, in English, such indefinitely iterable structures as  $[[[[John's\ brother]'s\ father]'s\ uncle]$  or  $[[[the\ man\ who\ you\ met]\ from\ Boston]\ who\ was\ on\ the\ train]$ , or (iii), which combines several kinds of left-branching. Right-branching structures are those with the opposite property — for example, the Direct-Object of (ii) or  $[this\ is\ [the\ cat\ that\ caught\ [the\ rat\ that\ stole\ the\ cheese]]]$ .

The effect of these superficial aspects of sentence structure on performance has been a topic of study since almost the very inception of recent work on generative grammar, and there are some suggestive observations concerning their role in determining acceptability (that is, their role in limiting performance). Summarizing this work briefly, the following observations seem plausible:

- (4) (i) repeated nesting contributes to unacceptability
- (ii) self-embedding contributes still more radically to unacceptability
- (iii) multiple-branching constructions are optimal in acceptability
- (iv) nesting of a long and complex element reduces acceptability
- (v) there are no clear examples of unacceptability involving only left-branching or only right-branching, although these constructions are unnatural in other ways — thus, for example, in reading the right-branching construction "this is the cat that caught the rat that stole the cheese," the intonation breaks are ordinarily inserted in the wrong places (that is, after "cat" and "rat," instead of where the main brackets appear)

In some measure, these phenomena are easily explained. Thus it is known (cf. Chomsky, 1959*a*; and for discussion, Chomsky, 1961, and Miller and Chomsky, 1963) that an optimal perceptual

device, even with a bounded memory, can accept unbounded left-branching and right-branching structures, though nested (hence ultimately self-embedded) structures go beyond its memory capacity. Thus case (4i) is simply a consequence of finiteness of memory, and the unacceptability of such examples as (2ii) raises no problem.

If (4ii) is correct,<sup>6</sup> then we have evidence for a conclusion about organization of memory that goes beyond the triviality that it must be finite in size. An optimal finite perceptual device of the type discussed in Chomsky (1959a) need have no more difficulty with self-embedding than with other kinds of nesting (see Bar-Hillel, Kasher, and Shamir, 1963, for a discussion of this point). To account for the greater unacceptability of self-embedding (assuming this to be a fact), we must add other conditions on the perceptual device beyond mere limitation of memory. We might assume, for example, that the perceptual device has a stock of analytic procedures available to it, one corresponding to each kind of phrase, and that it is organized in such a way that it is unable (or finds it difficult) to utilize a procedure  $\varphi$  while it is in the course of executing  $\varphi$ . This is not a necessary feature of a perceptual model, but it is a rather plausible one, and it would account for (4ii). See, in this connection, Miller and Isard (1964).

The high acceptability of multiple-branching, as in case (4iii), is easily explained on the rather plausible assumption that the ratio of number of phrases to number of formatives (the node-to-terminal node ratio, in a tree-diagram of a sentence) is a rough measure of the amount of computation that has to be performed in analysis. Thus multiple coordination would be the simplest kind of construction for an analytic device — it would impose the least strain on memory.<sup>7</sup> For discussion, see Miller and Chomsky (1963).

Case (4iv) suggests decay of memory, perhaps, but raises unsolved problems (see Chomsky, 1961, note 19).

Case (4v) follows from the result about optimal perceptual models mentioned earlier. But it is unclear why left- and right-branching structures should become unnatural after a certain point, if they actually do.<sup>8</sup>



One might ask whether attention to less superficial aspects of grammatical structure than those of (3) could lead to somewhat deeper conclusions about performance models. This seems entirely possible. For example, in Miller and Chomsky (1963) some syntactic and perceptual considerations are adduced in support of a suggestion (which is, to be sure, highly speculative) as to the somewhat more detailed organization of a perceptual device. In general, it seems that the study of performance models incorporating generative grammars may be a fruitful study; furthermore, it is difficult to imagine any other basis on which a theory of performance might develop.

There has been a fair amount of criticism of work in generative grammar on the grounds that it slights study of performance in favor of study of underlying competence. The facts, however, seem to be that the only studies of performance, outside of phonetics (but see note 3), are those carried out as a by-product of work in generative grammar. In particular, the study of memory limitations just summarized and the study of deviation from rules, as a stylistic device, to which we return in Chapters 2 and 4, have developed in this way. Furthermore, it seems that these lines of investigation can provide some insight into performance. Consequently, this criticism is unwarranted, and, furthermore, completely misdirected. It is the descriptivist limitation-in-principle to classification and organization of data, to "extracting patterns" from a corpus of observed speech, to describing "speech habits" or "habit structures," insofar as these may exist, etc., that precludes the development of a theory of actual performance.

### § 3. *THE ORGANIZATION OF A GENERATIVE GRAMMAR*

Returning now to the question of competence and the generative grammars that purport to describe it, we stress again that knowledge of a language involves the implicit ability to understand indefinitely many sentences.<sup>9</sup> Hence, a generative grammar must be a system of rules that can iterate to generate an in-

definitely large number of structures. This system of rules can be analyzed into the three major components of a generative grammar: the syntactic, phonological, and semantic components.<sup>10</sup>

The syntactic component specifies an infinite set of abstract formal objects, each of which incorporates all information relevant to a single interpretation of a particular sentence.<sup>11</sup> Since I shall be concerned here only with the syntactic component, I shall use the term "sentence" to refer to strings of formatives rather than to strings of phones. It will be recalled that a string of formatives specifies a string of phones uniquely (up to free variation), but not conversely.

The phonological component of a grammar determines the phonetic form of a sentence generated by the syntactic rules. That is, it relates a structure generated by the syntactic component to a phonetically represented signal. The semantic component determines the semantic interpretation of a sentence. That is, it relates a structure generated by the syntactic component to a certain semantic representation. Both the phonological and semantic components are therefore purely interpretive. Each utilizes information provided by the syntactic component concerning formatives, their inherent properties, and their interrelations in a given sentence. Consequently, the syntactic component of a grammar must specify, for each sentence, a *deep structure* that determines its semantic interpretation and a *surface structure* that determines its phonetic interpretation. The first of these is interpreted by the semantic component; the second, by the phonological component.<sup>12</sup>

It might be supposed that surface structure and deep structure will always be identical. In fact, one might briefly characterize the syntactic theories that have arisen in modern structural (taxonomic) linguistics as based on the assumption that deep and surface structures are actually the same (cf. Postal, 1964a, Chomsky, 1964). The central idea of transformational grammar is that they are, in general, distinct and that the surface structure is determined by repeated application of certain formal operations called "grammatical transformations" to objects of a more

elementary sort. If this is true (as I assume, henceforth), then the syntactic component must generate deep and surface structures, for each sentence, and must interrelate them. This idea has been clarified substantially in recent work, in ways that will be described later. In Chapter 3, I shall present a specific and, in part, new proposal as to precisely how it should be formulated. For the moment, it is sufficient to observe that although the Immediate Constituent analysis (labeled bracketing) of an actual string of formatives may be adequate as an account of surface structure, it is certainly not adequate as an account of deep structure. My concern in this book is primarily with deep structure and, in particular, with the elementary objects of which deep structure is constituted.

To clarify exposition, I shall use the following terminology, with occasional revisions as the discussion proceeds.

The *base* of the syntactic component is a system of rules that generate a highly restricted (perhaps finite) set of *basic strings*, each with an associated structural description called a *base Phrase-marker*. These base Phrase-markers are the elementary units of which deep structures are constituted. I shall assume that no ambiguity is introduced by rules of the base. This assumption seems to me correct, but has no important consequences for what follows here, though it simplifies exposition. Underlying each sentence of the language there is a sequence of base Phrase-markers, each generated by the base of the syntactic component. I shall refer to this sequence as the *basis* of the sentence that it underlies.

In addition to its base, the syntactic component of a generative grammar contains a *transformational* subcomponent. This is concerned with generating a sentence, with its surface structure, from its basis. Some familiarity with the operation and effects of transformational rules is henceforth presupposed.

Since the base generates only a restricted set of base Phrase-markers, most sentences will have a sequence of such objects as an underlying basis. Among the sentences with a single base Phrase-marker as basis, we can delimit a proper subset called "kernel sentences." These are sentences of a particularly simple

sort that involve a minimum of transformational apparatus in their generation. The notion "kernel sentence" has, I think, an important intuitive significance, but since kernel sentences play no distinctive role in generation or interpretation of sentences, I shall say nothing more about them here. One must be careful not to confuse kernel sentences with the basic strings that underlie them. The basic strings and base Phrase-markers do, it seems, play a distinctive and crucial role in language use.

Since transformations will not be considered here in detail, no careful distinction will be made, in the case of a sentence with a single element in its basis, between the basic string underlying this sentence and the sentence itself. In other words, at many points in the exposition I shall make the tacit simplifying (and contrary-to-fact) assumption that the underlying basic string *is* the sentence, in this case, and that the base Phrase-marker is the surface structure as well as the deep structure. I shall try to select examples in such a way as to minimize possible confusion, but the simplifying assumption should be borne in mind throughout.

#### § 4. *JUSTIFICATION OF GRAMMARS*

Before entering directly into an investigation of the syntactic component of a generative grammar, it is important to give some thought to several methodological questions of justification and adequacy.

There is, first of all, the question of how one is to obtain information about the speaker-hearer's competence, about his knowledge of the language. Like most facts of interest and importance, this is neither presented for direct observation nor extractable from data by inductive procedures of any known sort. Clearly, the actual data of linguistic performance will provide much evidence for determining the correctness of hypotheses about underlying linguistic structure, along with introspective reports (by the native speaker, or the linguist who has learned the language). This is the position that is universally adopted in practice, although there are methodological discus-

language learning possible under the empirically given limitations of time and data.

### § 8. *LINGUISTIC THEORY AND LANGUAGE LEARNING*

In the preceding discussion, certain problems of linguistic theory have been formulated as questions about the construction of a hypothetical language-acquisition device. This seems a useful and suggestive framework within which to pose and consider these problems. We may think of the theorist as given an empirical pairing of collections of primary linguistic data associated with grammars that are constructed by the device on the basis of such data. Much information can be obtained about both the primary data that constitute the input and the grammar that is the "output" of such a device, and the theorist has the problem of determining the intrinsic properties of a device capable of mediating this input-output relation.

It may be of some interest to set this discussion in a somewhat more general and traditional framework. Historically, we can distinguish two general lines of approach to the problem of acquisition of knowledge, of which the problem of acquisition of language is a special and particularly informative case. The empiricist approach has assumed that the structure of the acquisition device is limited to certain elementary "peripheral processing mechanisms" — for example, in recent versions, an innate "quality space" with an innate "distance" defined on it (Quine, 1960, pp. 83f.),<sup>25</sup> a set of primitive unconditioned reflexes (Hull, 1943), or, in the case of language, the set of all "aurally distinguishable components" of the full "auditory impression" (Bloch, 1950). Beyond this, it assumes that the device has certain analytical data-processing mechanisms or inductive principles of a very elementary sort, for example, certain principles of association, weak principles of "generalization" involving gradients along the dimensions of the given quality space, or, in our case, taxonomic principles of segmentation and classification such as those that have been developed with some care in modern linguistics, in accordance with the Saussurian emphasis

on the fundamental character of such principles. It is then assumed that a preliminary analysis of experience is provided by the peripheral processing mechanisms, and that one's concepts and knowledge, beyond this, are acquired by application of the available inductive principles to this initially analyzed experience.<sup>26</sup> Such views can be formulated clearly in one way or another as empirical hypotheses about the nature of mind.

A rather different approach to the problem of acquisition of knowledge has been characteristic of rationalist speculation about mental processes. The rationalist approach holds that beyond the peripheral processing mechanisms,<sup>27</sup> there are innate ideas and principles of various kinds that determine the form of the acquired knowledge in what may be a rather restricted and highly organized way. A condition for innate mechanisms to become activated is that appropriate stimulation be presented. Thus for Descartes (1647), the innate ideas are those arising from the faculty of thinking rather than from external objects:

. . . nothing reaches our mind from external objects through the organs of sense beyond certain corporeal movements . . . but even these movements, and the figures which arise from them, are not conceived by us in the shape they assume in the organs of sense. . . . Hence it follows that the ideas of the movements and figures are themselves innate in us. So much the more must the ideas of pain, colour, sound and the like be innate, that our mind may, on occasion of certain corporeal movements, envisage these ideas, for they have no likeness to the corporeal movements . . . [p. 443].

Similarly, such notions as that things equal to the same thing are equal to each other are innate, since they cannot arise as necessary principles from "particular movements." In general,

sight . . . presents nothing beyond pictures, and hearing nothing beyond voices or sounds, so that all these things that we think of, beyond these voices or pictures, as being symbolized by them, are presented to us by means of ideas which come from no other source than our faculty of thinking, and are accordingly together with that faculty innate in us, that is, always existing in us potentially; for existence in any faculty is not actual but merely potential existence, since the very word "faculty" designates nothing more or less than a potentiality. . . . [Thus

ideas are innate in the sense that] in some families generosity is innate, in others certain diseases like gout or gravel, not that on this account the babes of these families suffer from these diseases in their mother's womb, but because they are born with a certain disposition or propensity for contracting them . . . [p. 442].

Still earlier, Lord Herbert (1624) maintains that innate ideas and principles "remain latent when their corresponding objects are not present, and even disappear and give no sign of their existence"; they "must be deemed not so much the outcome of experience as principles without which we should have no experience at all . . . [p. 132]." Without these principles, "we could have no experience at all nor be capable of observations"; "we should never come to distinguish between things, or to grasp any general nature . . . [p. 105]." These notions are extensively developed throughout seventeenth-century rationalist philosophy. To mention just one example, Cudworth (1731) gives an extensive argument in support of his view that "there are many ideas of the mind, which though the cogitations of them be often occasionally invited from the motion or appulse of sensible objects without made upon our bodies; yet notwithstanding the ideas themselves could not possibly be stamped or impressed upon the soul from them, because sense takes no cognizance at all of any such things in those corporeal objects, and therefore they must needs arise from the innate vigour and activity of the mind itself . . . [Book IV]." Even in Locke one finds essentially the same conception, as was pointed out by Leibniz and many commentators since.

In the Port-Royal *Logic* (Arnauld, 1662), the same point of view is expressed in the following way:

It is false, therefore, that all our ideas come through sense. On the contrary, it may be affirmed that no idea which we have in our minds has taken its rise from sense, except on occasion of those movements which are made in the brain through sense, the impulse from sense giving occasion to the mind to form different ideas which it would not have formed without it, though these ideas have very rarely any resemblance to what takes place in the sense and in the brain; and there are at least a very great number of ideas which, having no connection with any

bodily image, cannot, without manifest absurdity, be referred to sense . . . [Chapter 1].

In the same vein, Leibniz refuses to accept a sharp distinction between innate and learned:

I agree that we learn ideas and innate truths either in considering their source or in verifying them through experience. . . . And I cannot admit this proposition: *all that one learns is not innate*. The truths of numbers are in us, yet nonetheless one learns them,<sup>28</sup> either by drawing them from their source when we learn them through demonstrative proof (which shows that they are innate), or by testing them in examples, as do ordinary arithmeticians . . . [*New Essays*, p. 75]. [Thus] all arithmetic and all geometry are in us virtually, so that we can find them there if we consider attentively and set in order what we already have in the mind . . . [p. 78]. [In general,] we have an infinite amount of knowledge of which we are not always conscious, not even when we need it [p. 77]. The senses, although necessary for all our actual knowledge, are not sufficient to give it all to us, since the senses never give us anything but examples, i.e., particular or individual truths. Now all the examples which confirm a general truth, whatever their number, do not suffice to establish the universal necessity of that same truth . . . [pp. 42–43]. Necessary truths . . . must have principles whose proof does not depend on examples, nor consequently upon the testimony of the senses, although without the senses it would never have occurred to us to think of them. . . . It is true that we must not imagine that these eternal laws of the reason can be read in the soul as in an open book . . . but it is sufficient that they can be discovered in us by dint of attention, for which the senses furnish occasions, and successful experience serves to confirm reason . . . [p. 44]. [There are innate general principles that] enter into our thoughts, of which they form the soul and the connection. They are as necessary thereto as the muscles and sinews are for walking, although we do not at all think of them. The mind leans upon these principles every moment, but it does not come so easily to distinguish them and to represent them distinctly and separately, because that demands great attention to its acts. . . . Thus it is that one possesses many things without knowing it . . . [p. 74].

(as, for example, the Chinese possess articulate sounds, and therefore the basis for alphabetic writing, although they have not invented this).



Notice, incidentally, that throughout these classical discussions of the interplay between sense and mind in the formation of ideas, no sharp distinction is made between perception and acquisition, although there would be no inconsistency in the assumption that latent innate mental structures, once "activated," are then available for interpretation of the data of sense in a way in which they were not previously.

Applying this rationalist view to the special case of language learning, Humboldt (1836) concludes that one cannot really teach language but can only present the conditions under which it will develop spontaneously in the mind in its own way. Thus the *form of a language*, the schema for its grammar, is to a large extent given, though it will not be available for use without appropriate experience to set the language-forming processes into operation. Like Leibniz, he reiterates the Platonistic view that, for the individual, learning is largely a matter of *Wiederzeugung*, that is, of drawing out what is innate in the mind.<sup>29</sup>

This view contrasts sharply with the empiricist notion (the prevailing modern view) that language is essentially an adventitious construct, taught by "conditioning" (as would be maintained, for example, by Skinner or Quine) or by drill and explicit explanation (as was claimed by Wittgenstein), or built up by elementary "data-processing" procedures (as modern linguistics typically maintains), but, in any event, relatively independent in its structure of any innate mental faculties.

In short, empiricist speculation has characteristically assumed that only the procedures and mechanisms for the acquisition of knowledge constitute an innate property of the mind. Thus for Hume, the method of "experimental reasoning" is a basic instinct in animals and humans, on a par with the instinct "which teaches a bird, with such exactness, the art of incubation, and the whole economy and order of its nursery" — it is derived "from the original hand of nature" (Hume, 1748, § IX). The form of knowledge, however, is otherwise quite free. On the other hand, rationalist speculation has assumed that the general form of a system of knowledge is fixed in advance as a disposition of the mind, and the function of experience is to cause this general

schematic structure to be realized and more fully differentiated. To follow Leibniz's enlightening analogy, we may make

. . . the comparison of a block of marble which has veins, rather than a block of marble wholly even, or of blank tablets, i.e., of what is called among philosophers a *tabula rasa*. For if the soul resembled these blank tablets, truths would be in us as the figure of Hercules is in the marble, when the marble is wholly indifferent to the reception of this figure or some other. But if there were veins in the block which should indicate the figure of Hercules rather than other figures, this block would be more determined thereto, and Hercules would be in it as in some sense innate, although it would be needful to labor to discover these veins, to clear them by polishing, and by cutting away what prevents them from appearing. Thus it is that ideas and truths are for us innate, as inclinations, dispositions, habits, or natural potentialities, and not as actions; although these potentialities are always accompanied by some actions, often insensible, which correspond to them [Leibniz, *New Essays*, pp. 45-46].

It is not, of course, necessary to assume that empiricist and rationalist views can always be sharply distinguished and that these currents cannot cross. Nevertheless, it is historically accurate as well as heuristically valuable to distinguish these two very different approaches to the problem of acquisition of knowledge. Particular empiricist and rationalist views can be made quite precise and can then be presented as explicit hypotheses about acquisition of knowledge, in particular, about the innate structure of a language-acquisition device. In fact, it would not be inaccurate to describe the taxonomic, data-processing approach of modern linguistics as an empiricist view that contrasts with the essentially rationalist alternative proposed in recent theories of transformational grammar. Taxonomic linguistics is empiricist in its assumption that general linguistic theory consists only of a body of procedures for determining the grammar of a language from a corpus of data, the form of language being unspecified except insofar as restrictions on possible grammars are determined by this set of procedures. If we interpret taxonomic linguistics as making an empirical claim,<sup>30</sup>

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 system. In contrast, the discussion of language acquisition in preceding sections was rationalistic in its assumption that various formal and substantive universals are intrinsic properties of the language-acquisition system, these providing a schema that is applied to data and that determines in a highly restricted way the general form and, in part, even the substantive features of the grammar that may emerge upon presentation of appropriate data. A general linguistic theory of the sort roughly described earlier, and elaborated in more detail in the following chapters and in other studies of transformational grammar, must therefore be regarded as a specific hypothesis, of an essentially rationalist cast, as to the nature of mental structures and processes. See Chomsky (1959*b*, 1962*b*, 1964) and Katz (forthcoming) for some further discussion of this point.

When such constrasting views are clearly formulated, we may ask, as an empirical question, which (if either) is correct. There is no a priori way to settle this issue. Where empiricist and rationalist views have been presented with sufficient care so that the question of correctness can be seriously raised, it cannot, for example, be maintained that in any clear sense one is "simpler" than the other in terms of its potential physical realization,<sup>31</sup> and even if this could be shown, one way or the other, it would have no bearing on what is completely a factual issue. This factual question can be approached in several ways. In particular, restricting ourselves now to the question of language acquisition, we must bear in mind that any concrete empiricist proposal does impose certain conditions on the form of the grammars that can result from application of its inductive principles to primary data. We may therefore ask whether the grammars that these principles can provide, in principle, are at all close to those which we in fact discover when we investigate

real languages. The same question can be asked about a concrete rationalist proposal. This has, in the past, proved to be a useful way to subject such hypotheses to one sort of empirical test.

If the answer to this question of adequacy-in-principle is positive, in either case, we can then turn to the question of feasibility: can the inductive procedures (in the empiricist case) or the mechanisms of elaboration and realization of innate schemata (in the rationalist case) succeed in producing grammars within the given constraints of time and access, and within the range of observed uniformity of output? In fact, the second question has rarely been raised in any serious way in connection with empiricist views (but cf. Miller, Galanter, and Pribram, 1960, pp. 145-148, and Miller and Chomsky, 1963, p. 430, for some comments), since study of the first question has been sufficient to rule out whatever explicit proposals of an essentially empiricist character have emerged in modern discussions of language acquisition. The only proposals that are explicit enough to support serious study are those that have been developed within taxonomic linguistics. It seems to have been demonstrated beyond any reasonable doubt that, quite apart from any question of feasibility, methods of the sort that have been studied in taxonomic linguistics are intrinsically incapable of yielding the systems of grammatical knowledge that must be attributed to the speaker of a language (cf. Chomsky, 1956, 1957, 1964; Postal, 1962*b*, 1964*a*, 1964*c*; Katz and Postal, 1964, § 5.5, and many other publications for discussion of these questions that seems unanswerable and is, for the moment, not challenged). In general, then, it seems to me correct to say that empiricist theories about language acquisition are refutable wherever they are clear, and that further empiricist speculations have been quite empty and uninformative. On the other hand, the rationalist approach exemplified by recent work in the theory of transformational grammar seems to have proved fairly productive, to be fully in accord with what is known about language, and to offer at least some hope of providing a hypothesis about the intrinsic structure of a language-acquisition system that will meet the condition of adequacy-in-principle and do so in a sufficiently

narrow and interesting way so that the question of feasibility can, for the first time, be seriously raised.

One might seek other ways of testing particular hypotheses about a language-acquisition device. A theory that attributes possession of certain linguistic universals to a language-acquisition system, as a property to be realized under appropriate external conditions, implies that only certain kinds of symbolic systems can be acquired and used as languages by this device. Others should be beyond its language-acquisition capacity. Systems can certainly be invented that fail the conditions, formal and substantive, that have been proposed as tentative linguistic universals in, for example, Jakobsonian distinctive-feature theory or the theory of transformational grammar. In principle, one might try to determine whether invented systems that fail these conditions do pose inordinately difficult problems for language learning, and do fall beyond the domain for which the language-acquisition system is designed. As a concrete example, consider the fact that, according to the theory of transformational grammar, only certain kinds of formal operations on strings can appear in grammars — operations that, furthermore, have no a priori justification. For example, the permitted operations cannot be shown in any sense to be the most “simple” or “elementary” ones that might be invented. In fact, what might in general be considered “elementary operations” on strings do not qualify as grammatical transformations at all, while many of the operations that do qualify are far from elementary, in any general sense. Specifically, grammatical transformations are necessarily “structure-dependent” in that they manipulate substrings only in terms of their assignment to categories. Thus it is possible to formulate a transformation that can insert all or part of the Auxiliary Verb to the left of a Noun Phrase that precedes it, independently of what the length or internal complexity of the strings belonging to these categories may be. It is impossible, however, to formulate as a transformation such a simple operation as reflection of an arbitrary string (that is, replacement of any string  $a_1 \cdots a_n$ , where each  $a_i$  is a single symbol, by  $a_n \cdots a_1$ ), or interchange of the  $(2n - 1)^{\text{th}}$  word with the  $2n^{\text{th}}$  word throughout a string of

arbitrary length, or insertion of a symbol in the middle of a string of even length. Similarly, if the structural analyses that define transformations are restricted to Boolean conditions on *Analyzability*, as suggested later, it will be impossible to formulate many "structure-dependent" operations as transformations — for example, an operation that will iterate a symbol that is the leftmost member of a category (impossible, short of listing all categories of the grammar in the structural analysis), or an operation that will iterate a symbol that belongs to as many rightmost as leftmost categories). Hence, one who proposes this theory would have to predict that although a language might form interrogatives, for example, by interchanging the order of certain categories (as in English), it could not form interrogatives by reflection, or interchange of odd and even words, or insertion of a marker in the middle of the sentence. Many other such predictions, none of them at all obvious in any a priori sense, can be deduced from any sufficiently explicit theory of linguistic universals that is attributed to a language-acquisition device as an intrinsic property. For some initial approaches to the very difficult but tantalizing problem of investigating questions of this sort, see Miller and Stein (1963), Miller and Norman (1964).

Notice that when we maintain that a system is not learnable by a language-acquisition device that mirrors human capacities, we do not imply that this system cannot be mastered by a human in some other way, if treated as a puzzle or intellectual exercise of some sort. The language-acquisition device is only one component of the total system of intellectual structures that can be applied to problem solving and concept formation; in other words, the *faculté de langage* is only one of the faculties of the mind. What one would expect, however, is that there should be a qualitative difference in the way in which an organism with a functional language-acquisition system<sup>82</sup> will approach and deal with systems that are languagelike and others that are not.

The problem of mapping the intrinsic cognitive capacities of

an organism and identifying the systems of belief and the organization of behavior that it can readily attain should be central to experimental psychology. However, the field has not developed in this way. Learning theory has, for the most part, concentrated on what seems a much more marginal topic, namely the question of species-independent regularities in acquisition of items of a "behavioral repertoire" under experimentally manipulable conditions. Consequently, it has necessarily directed its attention to tasks that are extrinsic to an organism's cognitive capacities — tasks that must be approached in a devious, indirect, and piecemeal fashion. In the course of this work, some incidental information has been obtained about the effect of intrinsic cognitive structure and intrinsic organization of behavior on what is learned, but this has rarely been the focus of serious attention (outside of ethology). The sporadic exceptions to this observation (see, for example, the discussion of "instinctual drift" in Breland and Breland, 1961) are quite suggestive, as are many ethological studies of lower organisms. The general question and its many ramifications, however, remain in a primitive state.

In brief, it seems clear that the present situation with regard to the study of language learning is essentially as follows. We have a certain amount of evidence about the character of the generative grammars that must be the "output" of an acquisition model for language. This evidence shows clearly that taxonomic views of linguistic structure are inadequate and that knowledge of grammatical structure cannot arise by application of step-by-step inductive operations (segmentation, classification, substitution procedures, filling of slots in frames, association, etc.) of any sort that have yet been developed within linguistics, psychology, or philosophy. Further empiricist speculations contribute nothing that even faintly suggests a way of overcoming the intrinsic limitations of the methods that have so far been proposed and elaborated. In particular, such speculations have not provided any way to account for or even to express the fundamental fact about the normal use of language, namely the speaker's ability to produce and understand instantly new

sentences that are not similar to those previously heard in any physically defined sense or in terms of any notion of frames or classes of elements, nor associated with those previously heard by conditioning, nor obtainable from them by any sort of "generalization" known to psychology or philosophy. It seems plain that language acquisition is based on the child's discovery of what from a formal point of view is a deep and abstract theory — a generative grammar of his language — many of the concepts and principles of which are only remotely related to experience by long and intricate chains of unconscious quasi-inferential steps. A consideration of the character of the grammar that is acquired, the degenerate quality and narrowly limited extent of the available data, the striking uniformity of the resulting grammars, and their independence of intelligence, motivation, and emotional state, over wide ranges of variation, leave little hope that much of the structure of the language can be learned by an organism initially uninformed as to its general character.

It is, for the present, impossible to formulate an assumption about initial, innate structure rich enough to account for the fact that grammatical knowledge is attained on the basis of the evidence available to the learner. Consequently, the empiricist effort to show how the assumptions about a language-acquisition device can be *reduced to a conceptual minimum*<sup>33</sup> is quite misplaced. The real problem is that of developing a hypothesis about initial structure that is sufficiently rich to account for acquisition of language, yet not so rich as to be inconsistent with the known diversity of language. It is a matter of no concern and of only historical interest that such a hypothesis will evidently not satisfy the preconceptions about learning that derive from centuries of empiricist doctrine. These preconceptions are not only quite implausible, to begin with, but are without factual support and are hardly consistent with what little is known about how animals or humans construct a "theory of the external world."

It is clear why the view that all knowledge derives solely from the senses by elementary operations of association and "gen-



eralization" should have had much appeal in the context of eighteenth-century struggles for scientific naturalism. However, there is surely no reason today for taking seriously a position that attributes a complex human achievement entirely to months (or at most years) of experience, rather than to millions of years of evolution or to principles of neural organization that may be even more deeply grounded in physical law — a position that would, furthermore, yield the conclusion that man is, apparently, unique among animals in the way in which he acquires knowledge. Such a position is particularly implausible with regard to language, an aspect of the child's world that is a human creation and would naturally be expected to reflect intrinsic human capacity in its internal organization.

In short, the structure of particular languages may very well be largely determined by factors over which the individual has no conscious control and concerning which society may have little choice or freedom. On the basis of the best information now available, it seems reasonable to suppose that a child cannot help constructing a particular sort of transformational grammar to account for the data presented to him, any more than he can control his perception of solid objects or his attention to line and angle. Thus it may well be that the general features of language structure reflect, not so much the course of one's experience, but rather the general character of one's capacity to acquire knowledge—in the traditional sense, one's innate ideas and innate principles. It seems to me that the problem of clarifying this issue and sharpening our understanding of its many facets provides the most interesting and important reason for the study of descriptively adequate grammars and, beyond this, the formulation and justification of a general linguistic theory that meets the condition of explanatory adequacy. By pursuing this investigation, one may hope to give some real substance to the traditional belief that "the principles of grammar form an important, and very curious, part of the philosophy of the human mind" (Beattie, 1788).

# Notes

## NOTES TO CHAPTER 1

1. To accept traditional mentalism, in this way, is not to accept Bloomfield's dichotomy of "mentalism" versus "mechanism." Mentalistic linguistics is simply theoretical linguistics that uses performance as data (along with other data, for example, the data provided by introspection) for the determination of competence, the latter being taken as the primary object of its investigation. The mentalist, in this traditional sense, need make no assumptions about the possible physiological basis for the mental reality that he studies. In particular, he need not deny that there is such a basis. One would guess, rather, that it is the mentalistic studies that will ultimately be of greatest value for the investigation of neurophysiological mechanisms, since they alone are concerned with determining abstractly the properties that such mechanisms must exhibit and the functions they must perform.

In fact, the issue of mentalism versus antimentalism in linguistics apparently has to do only with goals and interests, and not with questions of truth or falsity, sense or nonsense. At least three issues are involved in this rather idle controversy: (a) dualism — are the rules that underlie performance represented in a nonmaterial medium?; (b) behaviorism — do the data of performance exhaust the domain of interest to the linguist, or is he also concerned with other facts, in particular those pertaining to the deeper systems that underlie behavior?; (c) introspectionism — should one make use of introspective data in the attempt to ascertain the properties of these underlying systems? It is the dualistic position against which Bloomfield irrelevantly inveighed. The behaviorist position is not an arguable matter. It is simply an expression of lack of interest in theory and explanation. This is clear, for example, in Twaddell's critique (1935) of Sapir's mentalistic phonology, which used informant responses and comments as evidence bearing on the psychological reality of some abstract system of phonological elements. For Twaddell, the enterprise has no point because all

that interests him is the behavior itself, "which is already available for the student of language, though in less concentrated form." Characteristically, this lack of interest in linguistic theory expresses itself in the proposal to limit the term "theory" to "summary of data" (as in Twaddell's paper, or, to take a more recent example, in Dixon, 1963, although the discussion of "theories" in the latter is sufficiently vague as to allow other interpretations of what he may have in mind). Perhaps this loss of interest in theory, in the usual sense, was fostered by certain ideas (e.g., strict operationalism or strong verificationism) that were considered briefly in positivist philosophy of science, but rejected forthwith, in the early nineteen-thirties. In any event, question (b) poses no substantive issue. Question (c) arises only if one rejects the behaviorist limitations of (b). To maintain, on grounds of methodological purity, that introspective judgments of the informant (often, the linguist himself) should be disregarded is, for the present, to condemn the study of language to utter sterility. It is difficult to imagine what possible reason might be given for this. We return to this matter later. For further discussion, see Katz (1964c).

2. This has been denied recently by several European linguists (e.g., Dixon, 1963; Uhlenbeck, 1963, 1964). They offer no reasons for their skepticism concerning traditional grammar, however. Whatever evidence is available today seems to me to show that by and large the traditional views are basically correct, so far as they go, and that the suggested innovations are totally unjustifiable. For example, consider Uhlenbeck's proposal that the constituent analysis of "the man saw the boy" is [*the man saw*] [*the boy*], a proposal which presumably also implies that in the sentences [*the man put*] [*it into the box*], [*the man aimed*] [*it at John*], [*the man persuaded*] [*Bill that it was unlikely*], etc., the constituents are as indicated. There are many considerations relevant to the determination of constituent structure (cf. note 7); to my knowledge, they support the traditional analysis without exception against this proposal, for which the only argument offered is that it is the result of a "pure linguistic analysis." Cf. Uhlenbeck (1964), and the discussion there. As to Dixon's objections to traditional grammars, since he offers neither any alternative nor any argument (beyond the correct but irrelevant observation that they have been "long condemned by professional linguists"), there is nothing further to discuss, in this case.
3. Furthermore, it seems to me that speech perception is also best studied in this framework. See, for example, Halle and Stevens (1962).

4. Tests that seem to determine a useful notion of this sort have been described in various places — for example, Miller and Isard (1963).
5. These characterizations are equally vague, and the concepts involved are equally obscure. The notion “likely to be produced” or “probable” is sometimes thought to be more “objective” and antecedently better defined than the others, on the assumption that there is some clear meaning to the notion “probability of a sentence” or “probability of a sentence type.” Actually, the latter notions are objective and antecedently clear only if probability is based on an estimate of relative frequency and if sentence type means something like “sequence of word or morpheme classes.” (Furthermore, if the notion is to be at all significant, these classes must be extremely small and of mutually substitutable elements, or else unacceptable and ungrammatical sentences will be as “likely” and acceptable as grammatical ones.) But in this case, though “probability of a sentence (type)” is clear and well defined, it is an utterly useless notion, since almost all highly acceptable sentences (in the intuitive sense) will have probabilities empirically indistinguishable from zero and will belong to sentence types with probabilities empirically indistinguishable from zero. Thus the acceptable or grammatical sentences (or sentence types) are no more likely, in any objective sense of this word, than the others. This remains true if we consider, not “likelihood,” but “likelihood relative to a given situation,” as long as “situations” are specified in terms of observable physical properties and are not mentalistic constructs. It is noteworthy that linguists who talk of hardheaded objective study of use of sentences in real situations, when they actually come to citing examples, invariably describe the “situations” in completely mentalistic terms. Cf., e.g., Dixon (1963, p. 101), where, in the only illustrative example in the book, a sentence is described as gaining its meaning from the situation “British Culture.” To describe British culture as “a situation” is, in the first place, a category mistake; furthermore, to regard it as a pattern abstracted from observed behavior, and hence objectively describable in purely physical terms, betrays a complete misunderstanding of what might be expected from anthropological research.

For further discussion, see Katz and Fodor (1964).

6. That it may be true is suggested by several (for the moment, quite untested) observations. For example, in Chomsky and Miller (1963, p. 286) the following example is cited: “anyone who feels that if so many more students whom we haven’t actually admitted are sitting in on the course than ones we have that the room had

to be changed, then probably auditors will have to be excluded, is likely to agree that the curriculum needs revision." This contains six nested dependencies (along with other dependencies that go beyond nesting) with no self-embedding. Though hardly a model of felicitous style, it seems fairly comprehensible, and not extremely low on the scale of acceptability. In comparison, self-embedding of degree two or three seems to disturb acceptability much more severely. The matter is worth studying, since a positive result concerning (4ii) would, as noted, support a conclusion about organization of memory which is not entirely obvious.

7. It has sometimes been claimed that the traditional coordinated structures are necessarily right-recursive (Yngve, 1960) or left-recursive (Harman, 1963, p. 613, rule 3i). These conclusions seem to me equally unacceptable. Thus to assume (with Harman) that the phrase "a tall, young, handsome, intelligent man" has the structure [ [[[tall young] handsome] intelligent] man] seems to me no more justifiable than to assume that it has the structure [tall [young [handsome [intelligent man]]]]. In fact, there is no grammatical motivation for any internal structure, and, as I have just noted, the assumption that there is no structure is also supported on grounds of acceptability, with extremely weak and plausible assumptions about organization of memory. Notice that there are cases where further structure might be justified (e.g., [intelligent [young man]] or, perhaps [YOUNG [intelligent man]], with contrastive stress on "young"), but the issue is rather whether it is always necessary.

The same is true if we consider the very different type of Adjective-Noun construction that we find in such phrases as "all the young, old, and middle-aged voters" (for an interesting discussion of these various kinds of modification relations, see Ornan, 1964). Here, too, neither the structure [[young, old] and middle-aged] nor [young [old and middle-aged]] has any justification.

Similarly, it is surely impossible to assume, with Yngve, that in the phrase "John, Mary, and their two children" the structure is [John] [[Mary] [and their two children]], so that "John" is coordinated with "Mary and their two children," the latter being analyzed into the coordinated items "Mary" and "their two children." This is entirely counter to the sense. Notice, again, that conjunction *can* have this structure (e.g., "John, as well as Mary and her child"), but surely it is false to claim that it *must* have this structure.

In these cases all known syntactic, semantic, phonetic, and perceptual considerations converge in support of the traditional view that these constructions are typically coordinating (multiple-

branching). Notice also that this is the weakest assumption. The burden of proof rests on one who claims additional structure beyond this. There are various ways of justifying assignment of constituent structure. For example, in such a phrase as "all (none) of the blue, green, red, and (or) yellow pennants," if one wanted to argue that "blue, green, red" is a constituent (i.e., that the structure is left-branching), or that "green, red, and (or) yellow" is a constituent (that the structure is right-branching), then he would have to show that these analyses are required for some grammatical rule, that the postulated intermediate phrases must receive a semantic interpretation, that they define a phonetic contour, that there are perceptual grounds for the analysis, or something of this sort. All of these claims are patently false in this case, and the other cases mentioned here. Thus no semantic interpretation can be assigned to "old and middle-aged" in "young, old, and middle-aged voters" or to "green, red, or yellow" in "none of the blue, green, red, or yellow pennants" or to "Mary and their two children" in "John, Mary, and their two children"; the phonetic rules explicitly preclude such constituent analysis; there are no grammatical rules that require these analyses; there are no perceptual or other arguments to support them. It seems difficult, then, to see any grounds for objecting to the traditional analysis and insisting on additional intermediate categorization, in such cases as these.

8. Yngve (1960, and several other papers) has proposed a different theory to account for certain observations such as those of (4). Beyond the obvious condition of finiteness of memory, his theory assumes also that order of generation is identical with order of production — that the speaker and hearer produce sentences "from top-to-bottom" (they first decide on the major structures, then the substructures of these, etc., leaving to the very end of the process the choice of lexical items). Under this highly restrictive additional assumption, the optimal perceptual device mentioned earlier is no longer constructible, and left-branching and multiple-branching, as well as nesting and self-embedding, contribute to "depth" in Yngve's sense, hence to unacceptability. To support this hypothesis, it would be necessary to show (a) that it has some initial plausibility, and (b) that left-branching and multiple-branching in fact contribute to unacceptability exactly as do nesting and self-embedding. As to (a), I see no plausibility at all to the assumption that the speaker must uniformly select sentence type, then determine subcategories, etc., finally, at the last stage, deciding what he is going to talk about; or that the hearer should invariably make all higher-level decisions before doing any lower-level analysis. As

to (b), the hypothesis is supported by no evidence at all. The examples given by Yngve all involve nesting and self-embedding and hence are irrelevant to the hypothesis, since the unacceptability in this case follows from the assumption of finiteness alone without the additional assumption of "top-to-bottom" production for speaker and hearer. Furthermore, the hypothesis is contradicted by the observation (4iii) that multiply coordinated structures (cf. note 7) are the most acceptable (rather than the least acceptable, as predicted) and that left-branching structures are far more acceptable than nested structures of equal "depth," in Yngve's sense. It also fails to explain why examples of type (4iv), such as (2i), though very low in "depth," are still unacceptable.

However, Yngve makes one important point in these papers, namely, that some transformations can be used to decrease nesting, hence to reduce the perceptual load. This suggests an interesting argument as to why grammars should contain transformational rules. Some additional weight to this argument is given by the discussion of performance models involving transformational grammars in Miller and Chomsky (1963, Part 2).

9. It is astonishing to find that even this truism has recently been challenged. See Dixon (1963). However, it seems that when Dixon denies that a language has infinitely many sentences, he is using the term "infinite" in some special and rather obscure sense. Thus on the same page (p. 83) on which he objects to the assertion "that there are an infinite number of sentences in a language" he states that "we are clearly unable to say that there is any definite number,  $N$ , such that no sentence contains more than  $N$  clauses" (that is, he states that the language is infinite). Either this is a blatant self-contradiction, or else he has some new sense of the word "infinite" in mind. For further discussion of his remarks in this connection, see Chomsky (in press).
10. Aside from terminology, I follow here the exposition in Katz and Postal (1964). In particular, I shall assume throughout that the semantic component is essentially as they describe it and that the phonological component is essentially as described in Chomsky, Halle, and Lukoff (1956); Halle (1959a, 1959b, 1962a); Chomsky (1962b); Chomsky and Miller (1963); Halle and Chomsky (1960; forthcoming).
11. I assume throughout that the syntactic component contains a lexicon, and that each lexical item is specified in the lexicon in terms of its intrinsic semantic features, whatever these may be. I shall return to this matter in the next chapter.
12. In place of the terms "deep structure" and "surface structure," one might use the corresponding Humboldtian notions "inner form"

of a sentence and "outer form" of a sentence. However, though it seems to me that "deep structure" and "surface structure," in the sense in which these terms will be used here, do correspond quite closely to Humboldtian "inner form" and "outer form," respectively (as used of a sentence), I have adopted the more neutral terminology to avoid the question, here, of textual interpretation. The terms "depth grammar" and "surface grammar" are familiar in modern philosophy in something roughly like the sense here intended (cf. Wittgenstein's distinction of "*Tiefengrammatik*" and "*Oberflächengrammatik*," 1953, p. 168); Hockett uses similar terminology in his discussion of the inadequacy of taxonomic linguistics (Hockett, 1958, Chapter 29). Postal has used the terms "underlying structure" and "superficial structure" (Postal, 1964*b*) for the same notions.

The distinction between deep and surface structure, in the sense in which these terms are used here, is drawn quite clearly in the Port-Royal *Grammar* (Lancelot *et al.*, 1660). See Chomsky (1964, pp. 15-16; forthcoming) for some discussion and references. In philosophical discussion, it is often introduced in an attempt to show how certain philosophical positions arise from false grammatical analogies, the surface structure of certain expressions being mistakenly considered to be semantically interpretable by means appropriate only to other, superficially similar sentences. Thus Thomas Reid (1785) holds a common source of philosophical error to lie in the fact that

in all languages, there are phrases which have a distinct meaning; while at the same time, there may be something in the structure of them that disagrees with the analogy of grammar or with the principles of philosophy. . . . Thus, we speak of feeling pain as if pain was something distinct from the feeling of it. We speak of pain coming and going, and removing from one place to another. Such phrases are meant by those who use them in a sense that is neither obscure nor false. But the philosopher puts them into his alembic, reduces them to their first principles, draws out of them a sense that was never meant, and so imagines that he has discovered an error of the vulgar [pp. 167-168].

More generally, he criticizes the theory of ideas as based on a deviation from the "popular meaning," in which "to have an idea of anything signifies nothing more than to think of it" (p. 105). But philosophers take an idea to be "the object that the mind contemplates" (p. 105); to have an idea, then, is to possess in the mind such an image, picture, or representation as the immediate object of thought. It follows that there are two objects of thought:



the idea, which is in the mind, and the thing represented by it. From this conclusion follow the absurdities, as Reid regards them, of the traditional theory of ideas. One of the sources of these absurdities is the failure of the philosopher to attend "to the distinction between the operations of the mind and the objects of these operations . . . although this distinction be familiar to the vulgar, and found in the structure of all languages . . ." (p. 110). Notice that these two senses of "having an idea" are distinguished by Descartes in the Preface to the *Meditations* (1641, p. 138). Reid's linguistic observation is made considerably earlier by Du Marsais, in a work published posthumously in 1769, in the following passage (pp. 179-180):

Ainsi, comme nous avons dit *j'ai un livre, j'ai un diamant, j'ai une montre*, nous disons par imitation, *j'ai la fièvre, j'ai envie, j'ai peur, j'ai un doute, j'ai pitié, j'ai une idée*, etc. Mais *livre, diamant, montre* sont autant de noms d'objets réels qui existent indépendamment de notre manière de penser; au lieu que *santé, fièvre, peur, doute, envie*, ne sont que des termes métaphysiques qui ne désignent que des manières d'être considérés par des points de vue particuliers de l'esprit.

Dans cet exemple, *j'ai une montre, j'ai* est une expression qui doit être prise dans le sens propre: mais dans *j'ai une idée, j'ai* n'est dit que par une imitation. C'est une expression empruntée. *J'ai une idée*, c'est-à-dire, *je pense, je conçois de telle ou telle manière. J'ai envie*, c'est-à-dire, *je désire; j'ai la volonté*, c'est-à-dire, *je veux*, etc.

Ainsi, *idée, concept, imagination*, ne marquent point d'objets réels, et encore moins des êtres sensibles que l'on puisse unir l'un avec l'autre.

In more recent years, it has been widely held that the aims of philosophy should, in fact, be strictly limited to "the detection of the sources in linguistic idioms of recurrent misconstructions and absurd theories" (Ryle, 1931).

13. These descriptions are not fully accurate. In fact, the sentential complement in (10) should, more properly, be regarded as embedded in a Prepositional-Phrase (cf. Chapter 3); and, as Peter Rosenbaum has pointed out, the sentential complement of (11) should be regarded as embedded in the Noun-Phrase Object of "expect." Furthermore, the treatment of the Verbal Auxiliaries in (10) and (11) is incorrect, and there are other modifications relating to the marking of the passive transformation, to which we shall return in the next chapter.
14. It seems clear that many children acquire first or second languages

quite successfully even though no special care is taken to teach them and no special attention is given to their progress. It also seems apparent that much of the actual speech observed consists of fragments and deviant expressions of a variety of sorts. Thus it seems that a child must have the ability to "invent" a generative grammar that defines well-formedness and assigns interpretations to sentences even though the primary linguistic data that he uses as a basis for this act of theory construction may, from the point of view of the theory he constructs, be deficient in various respects. In general, there is an important element of truth in the traditional view that "the pains which everyone finds in conversation . . . is not to comprehend what another thinketh, but to extricate his thought from the signs or words which often agree not with it" (Cordemoy, 1667), and the problem this poses for speech perception is magnified many times for the language learner.

15. For example, Russell (1940, p. 33: "from a logical point of view, a proper name may be assigned to any continuous portion of space-time"), if we interpret his notion of "logically proper name" as embodying an empirical hypothesis. Interpreted in this way, Russell is stating what is, no doubt, a psychological truth. Interpreted otherwise, he is giving an unmotivated definition of "proper name." There is no logical necessity for names or other "object words" to meet any condition of spatiotemporal contiguity or to have other Gestalt qualities, and it is a nontrivial fact that they apparently do, insofar as the designated objects are of the type that can actually be perceived (for example, it is not true of "United States" — similarly, it need not be true of somewhat more abstract and functionally defined notions such as "barrier"). Thus there are no logical grounds for the apparent nonexistence in natural languages of words such as "LIMB," similar to "limb" except that it designates the single object consisting of a dog's four legs, so that "its LIMB is brown" (like "its head is brown") would mean that the object consisting of the four legs is brown. Similarly, there is no a priori reason why a natural language could not contain a word "HERD," like the collective "herd" except that it denotes a single scattered object with cows as parts, so that "a cow lost a leg" implies "the HERD lost a leg," etc.
16. Thus for Aristotle (*De Anima*, 403b), the "essence of a house is assigned in such a formula as 'a shelter against destruction by wind, rain, and heat,'" though "the physicist would describe it as 'stones, bricks, and timbers.'" For interesting comments on such definitions, see Foot (1961), Katz (1964d).
17. By a "reasonable procedure" I mean one that does not involve

extralinguistic information — that is, one that does not incorporate an “encyclopedia.” See Bar-Hillel (1960) for discussion. The possibility of a reasonable procedure for translation between arbitrary languages depends on the sufficiency of substantive universals. In fact, although there is much reason to believe that languages are to a significant extent cast in the same mold, there is little reason to suppose that reasonable procedures of translation are in general possible.

18. Actually, a set of structural descriptions should be assigned by  $f$  to each  $s_i$  (and each structural description must be assigned to exactly one  $s_i$ ), given  $G_j$ , one for each way of interpreting the sentence  $s_i$  with respect to  $G_j$ . Thus an unambiguous sentence should receive one structural description, a doubly ambiguous sentence two structural descriptions, etc. We assume that mappings are effective — that there is an algorithm for enumerating sentences, structural descriptions, and grammars and (throughout this is less obvious) for determining the values of  $f$  and  $m$  in all cases.
19. Obviously, to construct an actual theory of language learning, it would be necessary to face several other very serious questions involving, for example, the gradual development of an appropriate hypothesis, simplification of the technique for finding a compatible hypothesis, and the continual accretion of linguistic skill and knowledge and the deepening of the analysis of language structure that may continue long after the basic form of the language has been mastered. What I am describing is an idealization in which only the moment of acquisition of the correct grammar is considered. Introduction of these additional considerations might affect the general discussion in many ways. For example, in some limited but nevertheless real way, the preconditions (i)–(v) themselves might possibly be developed on the basis of deeper innate structure, in ways that depend in part on primary linguistic data and the order and manner in which they are presented. Furthermore, it might very well be true that a series of successively more detailed and highly structured schemata (corresponding to maturational stages, but perhaps in part themselves determined in form by earlier steps of language acquisition) are applied to the data at successive stages of language acquisition. There are, a priori, many possibilities that can be considered here.
20. It is instructive to see how modern structural linguistics has attempted to meet these conditions. It assumes that the technique for discovering the correct hypothesis (grammar) must be based on procedures of successive segmentation and classification of the items in the corpus (which constitutes the primary linguistic data, when supplemented, perhaps, by certain kinds of semantic in-

formation the exact relevance of which to the problem at hand has never been clarified). To compensate for this extremely strong demand on the procedure of grammar discovery, it was necessary to sacrifice descriptive adequacy, over a wide range of cases. In fact, the methodological discussions of modern linguistics pay very little attention to considerations (ii)–(iv) (though they do imply certain conclusions about them) and concentrate almost solely on development of constructive, step-by-step procedures of classification and segmentation. For discussion, see Lees (1957), Chomsky (1964).

21. This point has some historical interest. In fact, as has generally been noted by commentators, Locke's attempt to refute the doctrine of innate ideas is largely vitiated by his failure to observe the distinction we have just been discussing, although this was clear to Descartes (and was later re-emphasized by Leibniz, in his critique of Locke's *Essay*). Cf. § 8.
22. See note 19. An actual acquisition model must have a strategy for finding hypotheses. Suppose, for example, that the strategy is to consider only grammars that have better than a certain value (in terms of the evaluation measure (v)), at each stage in the process of language learning. What is required of a significant linguistic theory, then, is that given primary linguistic data *D*, the class of grammars compatible with *D* be sufficiently scattered, in terms of value, so that the intersection of the class of grammars compatible with *D* and the class of grammars which are highly valued be reasonably small. Only then can language learning actually take place.
23. See references of note 10.
24. Failure of attempts to justify an explanatory theory may be interpreted in various ways, of course. It may indicate that the theory is wrong, or that its consequences were incorrectly determined — in particular, that the grammar tested for descriptive adequacy was not the most highly valued one. Since any reasonable evaluation measure must be a systematic measure, and since language is a tightly interconnected system, the latter possibility is not to be discounted. In short, justification of linguistic theory does not avoid the problems faced by justification of any substantive and nontrivial empirical hypothesis.
25. Actually, it is not clear that Quine's position should be taken as in any real sense an empiricist one. Thus he goes on to propose that in the innate quality space a red ball might be less distant from a green ball than from a red kerchief, so that we have not just a pre-experiential characterization of distance but also an innate analysis of this into distance in various respects. On the

basis of these few comments, one might interpret him as proposing that such concepts as "ball" are innate ideas, hence as adopting an extreme form of nativism; at least, it is difficult to see wherein the cited proposal differs from this. In further support of such an antiempiricist interpretation, one may point to Quine's virtual renunciation of reinforcement theory (cf. my note 26).

Unfortunately, what are intended as empiricist views have generally been formulated in such an indefinite way that it is next to impossible to interpret them with any certainty, or to analyze or evaluate them. An extreme example, perhaps, is Skinner's account of how language is learned and used (Skinner, 1957). There seem to be only two coherent interpretations that one can give to this account. If we interpret the terms "stimulus," "reinforcement," "conditioning," etc., which appear in it, as having the meanings given to them in experimental psychology, then this account is so grossly and obviously counter to fact that discussion is quite beside the point. Alternatively, we may interpret these terms as metaphoric extensions of the (essentially homonymous) terms used in experimental psychology, in which case what is proposed is a mentalist account differing from traditional ones only in that many distinctions are necessarily obscured because of the poverty of the terminological apparatus available for paraphrase of the traditional mentalistic notions. What is particularly puzzling, then, is the insistent claim that this paraphrase is somehow "scientific" in a way in which traditional mentalism is not.

26. This application is perhaps mediated by "reinforcement," though many contemporary behaviorists use this term in such a loose way that reference to reinforcement adds nothing to the account of acquisition of knowledge that they propose. For example, Quine suggests (1960, pp. 82-83) that "some basic predilection for conformity" may take the place of "ulterior values," and that society's reinforcement of the response may consist "in no more than corroborative usage, whose resemblance to the child's effort is the sole reward." As Quine correctly notes, "this again is congenial enough to Skinner's scheme, for he does not enumerate the rewards" (this being one of the contributory factors to the near vacuity of Skinner's scheme). What this proposal comes to is that the only function of "reinforcement" may be to provide the child with information about correct usage; thus the empirical claim of "reinforcement theory" will be that learning of language cannot proceed in the absence of data. Actually, Skinner's concept of "reinforcement" is apparently still weaker than this, for he does not even require that the "reinforcing stimulus" impinge on the responding organism; it is sufficient that it be hoped for

or imagined (for a collection of examples bearing on this matter, see Chomsky, 1959b).

27. These mechanisms, as is now known, need not be at all elementary. Cf., for example, Lettvin *et al.* (1959), Hubel and Wiesel (1962), Frishkopf and Goldstein (1963). This work has demonstrated that peripheral processing in the receptor system or in lower cortical centers may provide a complex analysis of stimuli that, furthermore, seems to be rather specific to the animal's life-space and well correlated with behavior patterns. Thus it seems that not even peripheral processing can be described within the unstructured and atomistic framework that has been presupposed in empiricist thinking.
28. I depart here from the Langley translation, which renders this passage inaccurately. The French original is as follows: ". . . je demeure d'accord que nous apprenons les idées et les vérités innées, soit en prenant garde à leur source, soit en les vérifiant par l'expérience. Ainsi je ne saurois admettre cette proposition, *tout ce qu'on apprend n'est pas inné*. Les vérités des nombres sont en nous, et on ne laisse pas de les apprendre, soit en les tirant de leur source lorsqu'on les apprend par raison démonstrative (ce qui fait voir qu'elles sont innées) soit en les éprouvant dans les exemples comme font les arithméticiens vulgaires. . . ."
29. Cf. Chomsky (1964) for additional discussion and quotations illustrating Humboldt's views on these questions.
30. That this is a fair interpretation of taxonomic linguistics is not at all clear. For one thing, structural linguistics has rarely been concerned with the "creative" aspect of language use, which was a dominant theme in rationalistic linguistic theory. It has, in other words, given little attention to the production and interpretation of new, previously unheard sentences—that is, to the normal use of language. Thus the suggestion that the various theories of immediate constituent analysis might be interpreted as generative, phrase structure grammars (as in Chomsky, 1956, 1962a, or Postal, 1964a) certainly goes beyond what is explicitly stated by linguists who have developed these theories, and very likely beyond their intentions as well. Hence, the central problem of descriptive adequacy is not really raised within structural linguistics. Secondly, many "neo-Bloomfieldian" linguists, accepting Bloomfield's behaviorism under interpretation (b) of note 1 (as well as Firthians and "neo-Firthians" and many others), have thereby explicitly rejected any concern for descriptive adequacy, limiting the task of grammatical description, at least in theory, to organization of the primary linguistic data. Others have held that a grammar should at least describe the "habits" or "disposi-

tions" of the speaker, though the sense in which language use might be regarded as a matter of habit or disposition has never been satisfactorily clarified. To be more precise, there is no clear sense of the term "habit" or "disposition" in accordance with which it would be correct to describe language as a "habit structure" or a "system of dispositions."

In general, it is not clear that most behaviorist tendencies should be regarded as varieties of empiricism at all, since, as distinct from classical empiricism, they renounce any interest in mental processes or faculties (that is, in the problems of descriptive or explanatory adequacy).

31. This is the only respect in which a comparison of such alternatives is relevant, apart from their relative success in accounting for the given facts of language acquisition. But this consideration apparently offers no information that has any bearing on the choice among alternative theories.

In general, it is important to bear in mind that an extremely specialized input-output relation does not necessarily presuppose a complex and highly structured device. Whether our assumption about the mind is that it contains the schema for transformational grammar or that it contains mechanisms for making arbitrary associations or for carrying out certain kinds of inductive or taxonomic operations, there is apparently little knowledge about the brain and little engineering insight into plausible physical systems that can be used to support these hypotheses. Similarly, there is no justification for the common assumption that there is an asymmetry between rationalist and empiricist views in that the former somehow beg the question, not showing how the postulated internal structure arises. Empiricist views leave open precisely the same question. For the moment, there is no better account of how the empiricist data-processing operations might have been developed, as innate structure, in a species, than there is of how the rationalist schema may arise through evolutionary processes or other determinants of the structure of organisms. Nor does comparison with species other than man help the empiricist argument. On the contrary, every known species has highly specialized cognitive capacities. It is important to observe that comparative psychology has not characteristically proceeded on empiricist assumptions about knowledge and behavior, and lends no support to these assumptions.

32. There is reason to believe that the language-acquisition system may be fully functional only during a "critical period" of mental development or, more specifically, that its various maturational stages (see note 19) have critical periods. See Lenneberg (forth-

coming) for an important and informative review of data bearing on this question. Many other aspects of the problem of biologically given constraints on the nature of human language are discussed here and in Lenneberg (1960).

Notice that we do not, of course, imply that the functions of language acquisition are carried out by entirely separate components of the abstract mind or the physical brain, just as when one studies analyzing mechanisms in perception (cf. Sutherland, 1959, 1964), it is not implied that these are distinct and separate components of the full perceptual system. In fact, it is an important problem for psychology to determine to what extent other aspects of cognition share properties of language acquisition and language use, and to attempt, in this way, to develop a richer and more comprehensive theory of mind.

33. It is a curious fact that empiricism is commonly regarded as somehow a "scientific" philosophy. Actually, the empiricist approach to acquisition of knowledge has a certain dogmatic and aprioristic character that is largely lacking in its rationalist counterpart. In the particular case of language acquisition, the empiricist approach begins its investigation with the stipulation that certain arbitrarily selected data-processing mechanisms (e.g., principles of association, taxonomic procedures) are the only ones available to the language-acquisition device. It then investigates the application of these procedures to data, without, however, attempting to show that the result of this application corresponds to grammars that can be shown, independently, to be descriptively adequate. A nondogmatic alternative to empiricism would begin by observing that in studying language acquisition, what we are given is certain information about the primary data that are presented and the grammar that is the resulting product, and the problem we face is that of determining the structure of the device that mediates this input-output relation (the same is true of the more general problem of which language acquisition is a special case). There are no grounds for any specific assumptions, empiricist or otherwise, about the internal structure of this device. Continuing with no preconceptions, we would naturally turn to the study of uniformities in the output (formal and substantive universals), which we then must attribute to the structure of the device (or, if this can be shown, to uniformities in the input, this alternative rarely being a serious one in the cases that are of interest). This, in effect, has been the rationalist approach, and it is difficult to see what alternative there can be to it if dogmatic presuppositions as to the nature of mental processes are eliminated.
34. That is, a theory that meets conditions (i)-(iv) of p. 31. I shall