It is evident thatrote recall is a factor of minute importance in ordinary use of language, that "a minimum of the sentences which we utter is learnt by heart as such— that most of them, on the contrary, are composed on the spur of the moment," and that "one of the fundamental errors of the old science of language was to deal with all human utterances, as long as they remain constant to the common usage, as with something merely reproduced by memory" (Paul, 1886, 97.8). In this remark, it is only the reference to "the old science of language" that is subject to qualification. In fact, the realization that this "creative" aspect of language is its essential characteristic can be traced back at least to the seventeenth century. Thus we find the Cartesian view that man alone is more than mere automatism, and that it is the possession of true language that is the primary indicator of this (see Descartes, Discourse on Method, part V), developed by a follower along these lines (Cordemoy, 1668): "if the organs ... had a certain settled order among them [i.e., if man were a "language-producing engine" such as, for example, an artificial speaking machine, rocks that produce an echo, or, to a confirmed Cartesian like Cordemoy, a parrot], they could never change it, so that when the first voice were heard, those that were wont to follow it would needs be heard also ... whereas the words which I hear uttered by Bodies, made like mine, have almost never the same sequel" (6) "... to speak, is not to repeat the same words, which have struck the ear, but to utter others to their purpose and suitable to them" (13). In any event, whatever the antiquity of this insight may be, it is clear that a theory of language that neglects this "creative" aspect of language is of only marginal interest.

On the basis of a limited experience with the data of speech, each normal human has developed for himself a thorough competence in his native language. This competence can be represented, to an as yet undetermined extent, as a system of rules that we can call the grammar of his language. To each phonetically possible utterance (cf. Section 4.2), the grammar assigns a certain structural description that specifies the linguistic elements of which it is constituted and their structural relations (or, in the case of structural ambiguity, several such structural descriptions). For some utterances, the structural description will indicate, in particular, that they are perfectly well-formed sentences. This set we can call the language generated by the grammar. To others, the grammar will assign structural descriptions that indicate the manner of their deviation from perfect well-formedness. Where the deviation is sufficiently limited, an interpretation can often be imposed by virtue of formal relations to sentences of the generated language.

The grammar, then, is a device that (in particular) specifies the infinite set of well-formed sentences and assigns to each of these one or more structural descriptions. Perhaps we should call such a device a generative grammar to distinguish it from descriptive statements that merely present the inventory of elements that appear in structural descriptions, and their contextual variants.

The generative grammar of a language should, ideally, contain a central syntactic component and two interpretive components, a phonological component and a
semantic component. The syntactic component generates strings of minimal syntactically functioning elements (following Bolinger, 1948, let us call them formatives) and specifies the categories, functions and structural interrelations of the formatives and systems of formatives. The phonological component converts a string of formatives of specified syntactic structure into a phonetic representation. The semantic component, correspondingly, assigns a semantic interpretation to an abstract structure generated by the syntactic component. Thus each of the two interpretive components maps a syntactically generated structure onto a "concrete" interpretation, in one case phonetic, and in the other, semantic. The grammar as a whole can thus be regarded, ultimately, as a device for pairing phonetically represented signals with semantic interpretations, this pairing being mediated through a system of abstract structures generated by the syntactic component. Thus the syntactic component must provide for each sentence (actually, for each interpretation of each sentence) a semantically interpretable deep structure and a phonetically interpretable surface structure, and, in the event that these are distinct, a statement of the relation between these two structures. (For further discussion, see Katz and Postal, forthcoming.) Roughly speaking, it seems that this much structure is common to all theories of generative grammar, or at least compatible with them. Beyond this loose and minimal specification, however, important differences emerge.

The generative grammar internalized by someone who has acquired a language defines what in Sausserian terms we may call langue (with a qualification to be specified below, in Section 1.2). In performing as a speaker or hearer, he puts this device to use. Thus as a hearer, his problem is to determine the structure description assigned by his grammar to a presented utterance (or, where the sentence is syntactically ambiguous, to determine the correct structural description for this particular utterance) and using the information in the structural description, to understand the utterance. Clearly the description of intrinsic competence provided by the grammar is not to be confused with an account of actual performance, as de Sausssure emphasized with such lucidity (cf. also Sapir, 1921; Newman, 1941). Nor is it to be confused with an account of potential performance.3 The actual use of language obviously involves a complex interplay of many factors of the most disparate sort, of which the grammatical processes constitute only one. It seems natural to suppose that the study of actual linguistic performance can be seriously pursued only to the extent that we have a good understanding of the generative grammars that are acquired by the learner and put to use by the speaker or hearer. The classical Saussrine assumption of the logical priority of the study of langue (and the generative grammars that describe it) seems quite inescapable.

In the background of the discussion below there will be two conflicting models of generative grammar. The first—which I will call the taxonomic model—is a direct outgrowth of modern structural linguistics. The second—which I will call the trans-

4 On the syntactic level, the taxonomic model is a generalization from Harris' morpheme-to-utterance statements which constitute the nearest approach to an explicit generative grammar on terms of rewriting rules with null context (i.e., context-free grammar). With respect to stated contexts. The second set (phonemic rules) states the phonetic constitution of phonemes with respect to stated contexts. Each of these sets is unordered.

The transformational model is explicitly a late-comer, more "concrete," and more "atomic" than the taxonomic model. We can characterize it briefly in the following way. Each rule is of the form: element A has the member (variant, realization) X in the context Z-W. Let us call such a rule a rewriting rule. The syntactic component consists of an unordered set of rewriting rules, each of which states the membership that it provides can be regarded as a labelled bracketing, obtainable automatically from a single derivation, a Phrase-marker of this set of formative grammatical components. The phonological component consists of two sets of rewriting rules. The first set (morpheme rules) states the phonetic constitution of phonemes with respect to stated contexts. The second set (phonemic rules) states the phonetic constitute of phonemes with respect to stated contexts. Each of these sets is unordered.

5 The common characterization of language as a set of "verbal habits" or as a "complex of present dispositions to verbal behavior, in which of the same language have performer to resemble one another" (Quine, 1960, 27) is totally inadequate. Knowledge of one's language is not reflected directly in linguistic habits and dispositions, and it is clear that speakers of the same language or dialect may differ enormously in dispositions to verbal response, depending on personal beliefs, and countless other extra-linguistic factors.
structure) subcomponent consists of an ordered set of rewriting rules that generate strings of formatives that we may call C-terminal strings. These constitute either a finite set, or a highly restricted infinite set. The second (transformational) subcomponent consists of a partially ordered set of complex operations called grammatical transformations, each of which maps a full Phrase-marker (or a pair, triple, etc., of terminal strings) into a new derived Phrase-marker of a T-terminal string. Some of the rewriting and transformational rules may be obligatory, while others are optional. Application of all obligatory and perhaps some optional rules of the syntactic component, observing the order, will give a T-terminal string with a derived Phrase-marker. The structural description of this string will be a set of Phrase-markers (one for each underlying C-terminal string) and a representation of its "transformational history," what we may call a Transformation-marker. We will see below that all of this information plays a role in determining the interpretation of an utterance. It is also essential to distinguish a lexicon, with rather different properties, but I will not go into this question here.

The phonological component of a transformational generative grammar consists of an ordered set of rewriting rules, an ordered set of transformational rules, and an ordered set of rewriting rules, in that order. The transformational rules, furthermore, apply in a cycle, first to the smallest constituents of a string, then to the next largest constituents, etc., until the maximal domain of phonological processes is reached. These are, technically, transformational rules since they involve the co-nstituent structure of the utterance. This transformational cycle determines the phonetic form of their components, using the manner of composition specified by the derived Phrase-marker.

Notice that in the case of the transformational model, the symbols and structures that are manipulated, rewritten, and transformed as a sentence is generated may bear no very direct relation to any of its concrete subparts, whereas in the case of the taxonomic model each of the symbols that is rewritten in the generation of a sentence stands for a category to which some subpart of this sentence belongs (or category symbol by which it is represented). It is in this sense that the taxonomic model is both more concrete and more atomistic.

Investigation of the semantic component of a transformational grammar is quite recent. It has proceeded from the assumption, implicit in all studies of transformational grammar, that the grammatical functions and relations that play the primary role in determining the semantic interpretation of a sentence are those that are represented (in the manner described in section 4.1, below) in the underlying Phrase-markers generated by the constituent structure subcomponent, so that these Phrase-markers constitute the basic "content elements" underlying the interpretation of actual sentences (cf. Harris, 1957, pp. 290, 339-40; Chomsky, 1957, p. 92). For investigation of the semantic component, in which these vague suggestions are refined, sharpened, and considerably elaborated and developed, see Katz and Fodor (1963); Katz and Postal (forthcoming).

In terms of the characterization of a generative grammar given above (p. 51), we can distinguish between the taxonomic and transformational models in the following simple way. The syntactic component of a taxonomic grammar provides a single Phrase-marker for each utterance (in each interpretation) which serves both as deep structure and surface structure. That is, this single labelled bracketing of a formative sequence contains all information relevant to its semantic or phonetic interpretation. In a transformational grammar, the Phrase-markers of the underlying strings and the Transformation-marker constitute, jointly, the deep structure, and contain all information relevant to semantic interpretation; while the labelled bracketing that constitutes the final derived Phrase-marker of the T-terminal string is the surface structure which, presumably, contains all and only the information relevant to semantic interpretation. Katz and Postal (forthcoming) have, furthermore, presented strong arguments for the view that singular transformations make no contribution to semantic interpretation, so that the contribution of the Transformation-marker to the deep structure is minimal. In fact, recent and still unpublished work suggests that it can be entirely eliminated, but I will not pursue this matter further here. The important point is that according to this conception of grammatical structure, the categories and grammatical functions represented in the actual labelled bracketing of the temporally given string will, in general, not be those that determine the semantic interpretation of this string, though they will be directly related to its phonetic interpretation. It is in the system of underlying structures, that are mapped on to the actual given string by transformational rules, that the semantically significant categories and functions are represented.

1.2 Before continuing, it is instructive to consider these notions from the point of view of traditional grammar, as well as that of classical linguistic theory and of modern taxonomic linguistics.

It would not be inaccurate to regard the transformational model as a formalization of features implicit in traditional grammars, and to regard these grammars as inexplicit transformational generative grammars. The goal of a traditional grammar is to provide its user with the ability to understand an arbitrary sentence of the language, and to form and employ it properly on the appropriate occasion. Thus it's goal is (at least) as far-reaching as that of a generative grammar, as just described. Furthermore, the rich descriptive apparatus of traditional grammar far exceeds the limits of the taxonomic model, though it is largely, and perhaps fully formalizable within the framework of the transformational model. However, it is important to bear in mind that even the most careful and complete traditional grammar relies on an essential way on the intuition and intelligence of the user, who is expected to draw the correct inferences from the many examples and hints (and explicit lists of irregularities) presented by the grammar. If the grammar is a good one, the user may...
succeed, but the deep-seated regularities of the language that he somehow discovers escape explicit formulation, and the nature of the abilities that enable him to perform this task remain a complete mystery. The vastness of these gaps can be appreciated only when one makes an attempt to construct explicit rules to account for the full range of structural information available to the mature user of a language.

Focusing on the notion of "creativity," one can distinguish two conflicting views regarding the essential nature of language in nineteenth-century linguistic theory. The Humboldtian view that "man must die Sprache nicht erz Problematisch ansehen" (Section 8, p. LXI) follows that "Das Erzeugnis ist durch den Begriff Form nichts Bestimmendes, ganz vorzüglich auch aus Methoden, die Arbeit des Geistes, welcher die Bahn und die Form vorzeichnet, weiter fortzusetzen" (Section 9, p. LXXI).

From this conception of the nature of language, Humboldt derives his views concerning understanding of speech and acquisition of language. Speaking and understanding are, in his view, differing manifestations of the same underlying capacity, the same generative principle, mastery of which provides the speaker-hearer with the ability to use and understand all of the infinite range of linguistic items ("Mit dem Verstehen verhält es sich nicht anders. Es kann in der Seele nichts, als durch eigne Tätigkeit vorhanden sein, und Verstehen und Sprechen sind nur verschiedenartige Wirkungen der nämlichen Sprachkraft. Die gemeinsame Rede ist nie mit dem Übergehen eines Stoffes vergleichbar. In dem Verstehenden, wie im Sprechenden, muss derselbe aus der eigenen, innern Kraft entwickelt werden; und was der erstere empfängt, ist nur die harmonisch stimmende Anregung. Es ist daher dem Menschen auch schon natürlich, das eben Verstandene wieder gleich auszusprechen. Auf diese Weise liegt die Sprache in jedem Menschen in ihrem ganzen Umfange, was aber nichts Anderes bedeutet, als dass jeder ein... geregeltes Streben besitzt, die ganze Sprache, wie es äussere oder innere Veranlassung herbeiführt, nach und nach aus sich hervorzu­zubringen und hervorgebracht zu verstehen"—Section 9, p. LXXI). Furthermore, since language consists essentially of a "System von Regeln" as well as a "Vorrath von Wörtern" (cf. Section 9, p. LXXVII), common to speaker and hearer, it follows that "Das Sprechenlernen der Kinder ist nicht ein Zumessen von Wörtern, Niederlegen im Gedächtnis, und Wiedernachahlen mit den Lippen, sondern ein Wachsen des Sprachvermögens durch Alter und Übung" (Section 9, p. LXXI).

It is just this point of view concerning the essential nature of language that underlies and motivates recent work in generative grammar. Furthermore, the Humboldtian views concerning perception and acquisition have re-emerged, in many particulars, in the course of this work (cf., e.g., Chomsky, 1957a, 48; 1960; 1961a, Sections 1, 2; and the references of note 50, below). A generative grammar, in the sense sketched above, is an attempt to represent, in a precise manner, certain aspects of the Form of language, and a particular theory of generative grammar is an attempt to specify those aspects of form that are a common human possession—In Hum-
boldian terms, one might identify this latter with the underlying general form of all language ("Die Formen mehrerer Sprachen können in einer noch allgemeineren Form zusammenkommen, und die Formen aller thun dies in der That, insofern man überall bloss von dem Allgemeinste ausgängt" “... dass man ebenso richtig sagen kann, dass das ganze Menschengeschlecht nur Eine Sprache, als das jeder Mensch eine besondere besitzt” — Section 8, p. LXIII). There is one respect (to which we return directly below) in which this work diverges in principle from the Humboldtian framework; beyond this, the narrower limitations within which it has concretely developed (in particular, insofar as very little has been said, until quite recently, concerning semantic or conceptual structure) is a result not of any point of principle, but rather of the fact that there has been little to say about these further matters that could withstand serious analysis (cf. Section 2.3).

Humboldt’s thoughts concerning the semantic aspect of linguistic form are, not surprisingly, rather obscure in certain respects. They are, however, original and suggestive and, in part, quite different from more recent and familiar views. I will only attempt a brief sketch, largely in paraphrase, of what seem to be their main outlines. For Humboldt, as for Frege and many others since, a word does not stand directly for a thing, but rather for a concept. There can, accordingly, be a multiplicity of expressions for the same object, each representing a way in which this object has been conceived through the workings of the process of “Spracherzeugung,” and Humboldt gives several Sanskrit examples, of the now familiar “morning star”—“evening star” type, to illustrate this (Section 11). The process of language-formation is, furthermore, constantly active. Thus, one cannot regard the lexicon of a language as a completed aggregate ("eine fertig daliegende Masse"), but rather only as "ein fortgehendes Erzeugnis und Wiedererzeugnis des wortbildenden Vermögens." (Section 13.) This is to say that the capacity of "Spracherzeugung" is constantly at work, not only in extending the system of concepts, but also in recreating it, in each perceptual act (thus memory limits are overcome, since the system of concepts is not stored in full detail, but only in terms of its "generating principle"). In two respects, then, a system of concepts is not to be regarded as constituting a store of well-defined objects (as, apparently, it is for Saussure). In particular, even with the system fixed, Humboldt denies that understanding a linguistic expression is simply a matter of selecting the fully specified concept from a "store of concepts." It is rather that the received signs activate within the listener a corresponding link in his system of concepts ("dass sie gegenseitig in einander dasselbe Glied der Kette ihrer sinnlichen Vorstellungen und inneren Begriffserzeugungen berühren"—Section 20, CCXIII), causing a corresponding, but not identical concept to emerge. When a "key of the mental instrument" is touched, in this way, the whole system will resonate, and the emerging concept will stand in harmony with all that surrounds it to the most remote regions of its domain. Thus, a system of concepts is activated in the listener, and it is the place of a concept within this system (which may differ somewhat from speaker to speaker) that, in part, determines the way in which the hearer understands a linguistic expression. Finally, the concepts so formed are systematically interrelated in an "inner totality," with varying interconnections and structural relations (Section 20). This inner totality, formed by the use of language in thought, conception, and expression of feeling, functions as a conceptual world interposed through the constant activity of the mind between itself and the actual objects, and it is within this system that a word obtains its value ("Geltung")—cf. as a means of communication (Austauschungsmittel), and the instrumental use of Humboldt, typical only of parasitic systems (e.g., "Sprachmischungen," as the lingua franca along the Mediterranean coast.) For further discussion of Humboldtian general linguistics, see Viertel (forthcoming).

In sharp contrast to the Humboldtian conception, in the general linguistics of the nineteenth century, is the view that is perhaps expressed most clearly by Whitney and phrases by which any man expresses his thought" (372); that study of speech is no element of language is nothing more than study of origin and development of these disappears. "... the acquisition of language by children does not seem to us any used some scores or hundreds of times, comes to understand what it means, and then, of language (its use for achieving concrete aims) is derivative and subsidiary. It is, for Paul’s quoted above are from a makes no clear distinction) between the kind of "creativity" that leaves the language an activity in which the adult is constantly engaged) and the kind that actually distinction. In fact, the technical tools for dealing with “rule-governed creativity,” have only become readily available dur- matics. But in the light of these developments, it is possible to return to the questions the underlying “Form of language,” insofar as it encompasses “rule-governed creativity,” by means of an explicit generative grammar. Saussure, like Whitney (and possibly under his influence—cf. Godel, 1957, 32-3), regards langue as basically a store of signs with their grammatical properties, that is, a store of wordlike elements, fixed phrases, and, perhaps, certain limited phrase types (though it is possible that his rather obscure concept of "mécanisme de la langue" was intended to go beyond this—cf. Godel, 1957, 250). He was thus quite unable to come to grips with the recursive processes underlying sentence formation, and he appears to regard sentence formation as a matter of parole rather than langue.
of free and voluntary creation rather than systematic rule (or perhaps, in some ob-
scure way, as on the border between *langue* and *parole*). There is no place in his
view (recently expressed, e.g., by Joos, 1961; Reichling, 1961; Mel'chuk, 1961; Juillard, 1961) that
current work in generative grammar is in some way an outgrowth of attempts to use
electronic computers for one or another purpose, whereas in fact it should be obvious
that its roots are firmly in traditional linguistics.

1.3 The issues involved can be clarified by setting linguistic theory within the general
framework of the study of human intellectual capacities and their specific
character. Still remaining within the classical framework, as modified above, we can
take as an objective for linguistic theory the precise specification of two kinds of
abstract device, the first serving as a perceptual model and the second, as a model
for acquisition of language.

The perceptual model A is a device that assigns a structural description D to a pre-
sented utterance U, utilizing in the process its internalized generative grammar G,

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1See Bolinger (1960) for an elaboration of this point of view. See also the Introduction to Joos (1957).
where \( G \) generates a phonetic representation \( R \) of \( U \) with the structural description \( D \). In Saussurian terms, \( U \) is a specimen of *parole* interpreted by the device \( A \) as a “performance” of the item \( R \) which has the structural description \( D \) and which belongs to the *langue* generated by \( G \). The learning model \( B \) is a device which constructs a theory \( G \) (i.e., a generative grammar \( G \) of a certain *langue*) as its output, on the basis of primary linguistic data (e.g., specimens of *parole*), as input. To perform this task, it utilizes its given *faculté de langage*, its innate specification of certain heuristic procedures and certain built-in constraints on the character of the task to be performed. We can think of general linguistic theory as an attempt to specify the character of the device \( B \). We can regard a particular grammar as, in part, an attempt to specify the information available in principle (i.e., apart from limitations of attention, memory, etc.) to \( A \) that makes it capable of understanding an arbitrary utterance, to the highly nontrivial extent that understanding is determined by the structural description provided by the generative grammar. In evaluating a particular generative grammar, we ask whether the information that it gives us about a language is correct, that is, whether it describes correctly the linguistic intuition of the speaker (Saussure’s “conscience des sujets parlants,” which to him, as to Sapir, provides the ultimate test of adequacy for a linguistic description). In evaluating a general theory of linguistic structure that is sufficiently explicit to offer an actual hypothesis about the character of \( B \), we ask whether the generative grammars that it selects meet the empirical criterion of correspondence to the speaker’s linguistic intuition, in the case of particular languages.

I will try to show that the taxonomic model (or any of its variants within modern study of language) is far too oversimplified to be able to account for the facts of linguistic structure and that the transformational model of generative grammar is much closer to the truth. To show that modern linguistics seriously underestimates the richness of structure of language and the generative processes that underlie it, it is necessary to sample the range of problems that cannot be attacked, or often even posed within the narrow limits that it sets. A variety of examples of this sort will be considered in the following sections. I will also try to show that these inadequacies and limitations may in part be traceable to an impoverished conception of the nature of human cognitive processes, and that a return to traditional concerns and viewpoints, with the higher standards of explicitness that have emerged in modern linguistics, can perhaps provide new insights concerning perception and learning.

II. LEVELS OF SUCCESS FOR GRAMMATICAL DESCRIPTION

2.0 Within the framework outlined above, we can sketch various levels of success that might be attained by a grammatical description associated with a particular linguistic theory. The lowest level of success is achieved if the grammar presents the observed primary data correctly.¹ A second and higher level of success is achieved when the grammar gives a correct account of the linguistic intuition of the native speaker, and specifies the observed data (in particular) in terms of significant general-level of success is achieved when the associated linguistic theory provides a general grammar that achieves the second level of success over other grammars consistent with the relevant observed data that do not achieve this level of success. In this case, we can say that the linguistic theory in question suggests an explanation for the linguistic intuition of the native speaker. It can be interpreted as asserting that data of the observed kind will enable a speaker whose intrinsic capacities are as presented in this general theory to construct for himself a grammar that characterizes exactly this linguistic intuition.

For later reference, let us refer to these roughly delimited levels of success as the levels of *observational adequacy*, *descriptive adequacy*, and *explanatory adequacy*, observational adequacy is concerned merely to give an account of the primary data descriptive adequacy is concerned to give a correct account of the linguistic intuition (1b); and a linguistic theory that aims for explanatory adequacy is concerned with the output of the device (1b); that is, it aims to provide a principled basis, independent of any particular language, for the selection of the descriptively adequate grammar of each language.

Modern linguistics has been largely concerned with observational adequacy. In particular, this is true of post-Bloomfieldian American linguistics (cf. below, Section 4.3–4), and apparently, of the London school of Firth, with its emphasis on the ad hoc character of linguistic description.⁸ Traditional grammar, on the other hand, was explicitly concerned with the level of descriptive adequacy (and this interest persists, Sapir, 1933; Long, 1960). This difference between traditional and modern points Nida, in his valuable study (1943) of English syntax within the immediate constituent of the formal and functional values in assigning to “the doctor’s house,” a structural description that indicates that the Subject-Verb frame appears in the former but not in the latter phrase. But clearly Jespersen’s account is correct on the level of descriptive adequacy, and the fact that the data-processing operations of modern linguistics fail to provide the correct information

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¹Innocuous as this comment may seem, it still requires qualification: What data is relevant is determined in part by the possibility for a systematic theory, and one might therefore hold that the

²Cf. Firth et al. (1937).
indicates only that they are based on an erroneous conception of linguistic structure, or that observational adequacy is being taken as the only relevant concern. On the other hand, Jakobson’s attempts to formulate universal phonological laws (cf. Section 4.2, below) might perhaps be regarded as indicating a concern for explanatory adequacy, on at least one level of grammar. It is clear that the question of explanatory adequacy can be seriously raised only when we are presented with an explicit theory of generative grammar that specifies the form of grammars and suggests a mechanism for selecting among them (i.e., an evaluation procedure for grammars of a specified form). The difference between observational and descriptive adequacy is related to the distinction drawn by Hockett (1958) between “surface grammar” and “deep grammar,” and he is unquestionably correct in noting that modern linguistics is largely confined in scope to the former.

2.1 Levels of Adequacy in Phonology. A few linguistic examples may help to clarify the distinction between these various levels of adequacy. Consider first the case of so-called “accidental gaps” in the lexicon. Thus in English there is a word “pick” / pik/; but no / bik/ or / fitik/. The level of observational adequacy would be attained by a grammar that contained the rule: N → / pik/; but no lexical rule introducing / bik/ or / fitik/. To attain the level of descriptive adequacy, a grammar would have to provide, in addition, a general rule that sets up a specific barrier against / fitik/, but not against / bik/ (which would thus qualify as an accidental gap, a phonologically permissible nonsense syllable). This level would be achieved by a grammar that contained the generalized that in initial position before a true consonant (a segment which is consonantal and nonvocalic, in terms of Jakobson’s distinctive features), a consonant is necessarily /s/. The level of explanatory adequacy would be attained by a linguistic theory that provides a principled reason for incorporating this generalization in a grammar of English, and for excluding the (factually correct) “rule” that in the context /b - ik/ a liquid is necessarily /f/. Thus the theory might provide a general evaluation measure (simplicity measure) which would show how the former, but not the latter rule gives a more highly valued grammar. Such a theory would suggest an explanation for the linguistic intuition that / bik/; but not / fitik/, is a “possible” word, though neither has been heard. This is the intuition that would result from observation of actual utterances by a learner who constructs the most highly valued grammar of the appropriate form, as specified by this theory.

Consider now the matter of predictable phonetic variants. Thus in my speech, the lexical item “telegraph” appears in many phonetic shapes, depending on context, in particular, the shapes (2i–iii) in the contexts # - #, -ic, -y, respectively:

téligráf

téligráf

télegfr.

Observational adequacy would be achieved by a grammar that merely states the facts, as I have just done, thus reproducing the observed data in a convenient arrangement.

In his review of Halle (1959b), Ferguson (1962, 232) describes Halle’s discovery of the role of morpheme structure rules as “a misfortune” not too different from certain defects of taxonomic grammars that Halle exposes (cf. Section 4.3, below). This is an extremely peculiar conclusion. No generalization is lost by distinguishing morpheme structure rules (which are obviously needed in a full grammar, and which, as Halle shows, play a distinctive role in accounting for an otherwise unexplained area of linguistic fact) from other phonetic rules differing from them both in formal properties and in the phenomena that they describe. On the other hand, the deficiency of taxonomism that is, their inability to achieve descriptive adequacy,

The theory of “morpheme structure rules” developed in Halle (1959a, 1959b) constitutes an attempt to reach the level of explanatory adequacy, in this case. Halle shows how consistent adherence to the principle of minimizing feature specifications in the phonological component provides a principled basis for the distinction between accidental and nonaccidental gaps. To my knowledge, this is the only attempt to provide a general basis for this distinction, though lists and charts that state much of the data that is to be explained have frequently appeared.

In his review of Halle (1959b), Ferguson (1962, 292) describes Halle’s discovery of the role of morpheme structure rules as “a misfortune” not too different from certain defects of taxonomic grammars that Halle exposes (cf. Section 4.3, below). This is an extremely peculiar conclusion. No generalization is lost by distinguishing morpheme structure rules (which are obviously needed in a
sive investigations of English phonology in recent years, no attempt has even been made to meet them.

The point becomes even clearer when we consider phonetic variants that are syntactically conditioned. Thus English "törrent" /tørənt/ (cf. "torrential") has the reduced vowel [ə] in the second syllable, while the noun "törrent" /tørənt/re-\ alias the vowel [e]. The level of observational adequacy is attained by the preceding sentence. The level of descriptive adequacy would be achieved by a description that manages to relate these observations to the fact that there is a verb "törmente," but not "törrentine" in English, by means of general rules about stress shifts in nouns and verbs "törrentine" in English, stresses of which general rules about stress shifts in nouns are derived from verbs, and the role of stress in preventing vowel reduction. The level of explanatory adequacy requires a phonological theory that prescribes the general form of such syntactically determined phonetic theory that prescribes the general form.

Similarly, in the case of such familiar examples as "light house keeper" (word patterns 132, 213, 313), the level of descriptive adequacy requires, beyond a statement of these facts, a general account of the rules by which such stress patterns are assigned in syntactic constructions, and the level of explanatory adequacy will be achieved when any grammar that assigns the correct phonetic shape of the full phrase is determined by that of its constituents.

2.2 Levels of adequacy in syntax. Consider next a few syntactic examples. Suppose that the sentences

John is easy to please (3)
John is eager to please (4)

are observed and accepted as well formed. A grammar that achieves only the level of observational adequacy would, again, merely note this fact in one way or another (e.g., by setting up appropriate lists). To achieve the level of descriptive adequacy, however, a grammar would have to assign structural descriptions indicating that John in (3) is the direct object of please (the words are grammatically related as in "This pleases John"). While in (4) it is the logical subject of please (as in "I please someone"). A theory of grammar that does not allow structural descriptions of this sort cannot achieve the level of descriptive adequacy. In cases of this sort, the lexico-syntactic model of generative grammar discussed above (or any of its variants) cannot achieve the level of descriptive adequacy, since information of this kind cannot be represented in the Phrase-marker that provides as full structural description on the syntactic level. The transformational model does, however, make grammars in that contain such structural information of this sort, and therefore can, in this case at least, achieve the level of descriptive adequacy. In Section 4.1 we will return to the problem of assigning to (3) and (4) structural descriptions that provide the full range of syntactic information.

How might a transformational grammar achieve the level of explanatory adequacy in such a case as this? To achieve this level, the theory must provide for the selection of a descriptively adequate grammar, given such data as (3), (4), "John's eagerness (easiness) to please..." "to please John is easy (eager)", "John is an easy (eager) fellow to please", "it pleases John", "John pleases everyone", "John is easy (eager) for us to please", "it is easy (eager) to please John", "John is a person who it is easy to please", "this room is not easy to work in (to do decent work in)", "he is easy to do business with", "This knife is very difficult to cut (meat) with", "a hotel lobby is difficult (a difficult place) to meet people in", "he is not easy to get information from", "such flattery is easy to be fooled by", and many other similar and related structures.

The general theory, in other words, would have to make possible the formulation of the underlying generalizations that account for this arrangement of empirical data, and to distinguish these real generalizations from pseudo-simplifications that have no linguistic significance. In so doing, the theory would suggest an explanation for the linguistic intuition of native speakers as regards (3) and (4). This explanation would rest on the assumption that the concepts of grammatical structure and "significant generalization" made explicit in this theory constitute the set of tools used by the learner in constructing an internal representation of his language (i.e., a generative grammar), on the basis of presented linguistic data. There is fairly good reason to believe that in the case of (3), (4), the theory of transformational grammar can approach the level of explanatory adequacy by providing a partial explanation for the speaker's linguistic intuition. That is, the grammar that assigns the correct structural descriptions contains generalizations that are not expressed in grammars that fail to provide the correct structural descriptions, and is thus higher-valued, in a sense which can apparently be made precise without much difficulty.

As a second syntactic example, consider the following arrangements of sentences and non-sentences: ("John found the book", "John was a farmer"); ("the book was found by John", "a farmer was been by John"); ("did John find the book?", "did John be a farmer?"); ("found John the book?", "was John a farmer"); ("John didn't find the book", "John didn't be a farmer"); ("John found the book", "John wasn't a farmer"); ("John didn't find the book", "John did be a farmer"); ("Bill found the book and so did John", "Bill was a farmer and so did John"); ("Bill found the book and so found John", "Bill was a farmer and so was John"). In short, as is well known, there are a variety of respects, of which these are a sample, in which "be" behaves quite differently from "find." Similarly, "be", but not "find," is an Auxiliary. Traditional grammars merely list these facts as anomalous, and make no attempt to relate them. It can easily be shown, however, that a transformational grammar with a constituent structure subcomponent containing the rules:

\[
\begin{align*}
VP & \rightarrow \text{Aux} + VP_1 \\
\text{Aux} & \rightarrow \text{Aux}_1 (\text{Aux}_2) \\
\text{Aux}_1 & \rightarrow \text{Tense} (\text{Modal})
\end{align*}
\]


\[\text{(5i)}\]

\[\text{(5ii)}\]

\[\text{(5iii)}\]

See Miller and Chomsky (1963). Cf. also Lees (1960b) for detailed discussion of a class of similar cases. For discussion of measures of evaluation that select grammars with significant generalizations over those that do not contain such generalizations, cf. Chomsky (1955, chapter 3; 1962a); Halle (1961a); Haile and Chomsky (forthcoming).
(an analysis which has many independent motivations) will automatically provide for just this range of phenomena, thus reducing a mass of apparent idiosyncrasies to underlying regularity (cf. Chomsky, 1955, chapters 7, 9; 1957a). In fact, a transformational grammar would have to be complicated considerably to generate the excluded sentences. Here again, then, it seems that the level of explanatory adequacy can be met by a transformational grammar and the theory associated with it.\textsuperscript{14}

A similar problem is posed by certain English comparative constructions. We have such sentences as “John received a warmer welcome than Bill,” “John is a kinder person than Bill,” and “John knows a kinder person than Bill,” where only the last is ambiguous (“than Bill is,” “than Bill does”). Furthermore, although we can have such sentences as “Bill bought a bigger house than John did,” “Mary has a bigger red balloon than John,” we do not have “Bill bought the bigger house than John did,” “Mary has a red bigger balloon than John,” “Mary has a bigger redder balloon than John,” etc. At the level of observational adequacy, a grammar might simply state a variety of facts of this kind. But we can in fact reach a higher level of adequacy in this case. Suppose that we have a transformational grammar of English constructed so as to generate in the most economical way the full range of adjectival constructions, excluding comparatives. It can be shown (cf. Smith, 1961) that a large variety of constructions involving comparatives will be generated automatically by the grammar, with the right arrangement of ambiguities, instances and apparent “exceptions,” if we add to this grammar, at the appropriate point in the sequence of ordered rules, the generalized transformation that forms the simplest comparative constructions (namely, those of the form “John is taller than Bill (is)” from “John is tall,” “Bill is tall”). Here, then, is an interesting case where it seems proper to say that the general theory of transformational grammar provides an explanation for a complex array of superficially quite disordered data.

The possibilities for attaining higher levels of adequacy, and the difficulties that attend this project, are well illustrated by the problem of formulating in a precise way the rules for construction of relative clauses and interrogatives. These are related operations; a roughly adequate description would be the following. In each terminal string, zero or more Noun Phrases are assigned as a “prefix” the element Wh. To a string containing the Noun Phrase Wh + X we may now apply the transformation

\[ Y - Wh + X - Z \rightarrow Wh + Y - X - Z \] (6)

Thus from the string underlying “John admires Wh + someone,” we can form, by (6), the string underlying “Wh + someone Wh + John admires” (where Z, in this case, is null). The result of (6) is now subject to either the operation of Relativization, which embeds it in the Noun Phrase of a matrix sentence (giving, e.g., “he met someone Wh + someone John admires”) or the operation of Auxiliary Attraction which binds the first part of the Auxiliary to the position following Wh + X (giving “Wh + someone Wh + John admires”). Finally, the resulting string is subject to obligatory rules that replace Wh + X by “who . . .,” “what . . .,” etc. (giving “he met someone Wh + John admires,” “he who Wh + John admires”).\textsuperscript{14} Clearly Relativization can be applied only if the Noun of the phrase Wh + X is the same as that of the described still contains Wh-forms which do not introduce relative clauses (i.e., do not refer to a Noun which actually appears in the sentence in a designated position). If the string resulting from the operations just described still contains Wh-forms which do not introduce relative clauses (i.e., do not refer to a Noun which actually appears in the sentence in a designated position), then this string is an interrogative, and is to be answered by specification of the Noun Phrases that occupy the positions of these Wh-forms. Thus we may have such interrogatives as “who admires John?” (in which Auxiliary attraction has applied vacuously), “who(Wh) does John admire?,” “who(Wh) admires(Wh)m,” “he met someone who(Wh) admires(Wh)m,” “he(Wh) admires(Wh)m(Wh),” etc. Details aside, this much seems fairly clear and can be formalized without difficulty within the framework of transformational grammar.

When we investigate the matter more carefully, however, we find that certain additional conditions are necessary for descriptive adequacy. In particular, a closer can be formed only from singular indefinite Noun Phrases (although relatives and nondeleting interrogatives are not subject to this restriction; thus, “the boys who are in the room,” “which boys are in the room,” etc., are perfectly natural). We have such sentences as “you know a boy with (who has) a scar,” “who do I know with (who has) a scar?” “I know a boy who was expelled,” “who do I know who was expelled?,” etc. Notice, incidentally, that though such questions

\textsuperscript{14} The well-known (and different) apparent anomalies of “have” are also largely accounted for by (5) and the rules for forming questions, negations, etc. Notice that from these facts one is led to the conclusion that “be,” the modals and the auxiliary “have” are not Verbs, in contrast to the familiar treatment of these items as “defective verbs” (cf., e.g., Bloomfield, 1933, 223; or Austin, 1956, who describes the fact that modals have no progressive or participial forms, and compares them in this respect to “know,” etc.—actually, there is no more reason to comment on the lack of “to,” “ing,” or “en” forms of modals than on the fact that nouns do not appear in these positions). Notice also that there is no optional rule of the grammar that allows one to select “be” (though there is an optional rule that allows one to select “be + Predicate”). In this respect, “be” is quite different from most lexical items. In general, it seems reasonable to regard an item as meaning-bearing just in case selection of it is subject to an optional rule (thus most other lexical items are meaning-bearing, as are optional transformations and constructions given by rewriting rules, but not, e.g., phonemes). Where the grammar provides for an optional choice, it makes sense to search for the conditions under which it is appropriate to make the choice (this being one aspect of the study of meaning). Thus it would seem reasonable to inquire into the meaning of “Predicate” (i.e., choice of “be + Predicate” in rule (5)), but not into the meaning of “be,” which is no more subject to independent choice than are its particular variants or their individual phonemes.

\textsuperscript{15} More details are given in various places, for example, Chomsky (1957, 1961), Lees (1960). The formulation just outlined is suggested by remarks of E. S. Klima. Other sorts of questions can be forthcoming. It is hardly necessary to warn the reader of the informality of these descriptions. Other sentences by transformation,” what should be understood is that the abstract forms (category-sentences). Notice also that such rules as (6) should be regarded as constituting not a transformation, of which takes the kth analysis of a string that meets the structural condition and performs the speci-
where in the sentence. The abstract forms (that is, the categorized terminal strings) underlying a sentence with a relative clause are therefore determinable, given the sentence. This would not be true of interrogatives if a restriction such as that of the preceding paragraph were not imposed. This observation suggests the following general condition on transformational grammar. Each major category has associated with it a "designated element" as a member. This designated element may actually be realized (e.g., "it" for abstract Nouns, "some (one, thing)"), or it may be an abstract "dummy element." It is this designated representative of the category that must appear in the underlying strings for those transformations that do not preserve, in the transform, a specification of the actual terminal representative of the category in question. In other words, a transformation can delete an element only if this element is the designated representative of a category, or if the structural condition that defines this transformation states that the deleted element is structurally identical to another element of the transformed string. A deleted element is, therefore, always recoverable. 14

In conformity with this condition, the rules for forming interrogatives (but not relatives) must be limited in application to underlying strings containing Wh + Σ, where Σ is one of these designated elements. It is clear, on other grounds, that the elements "some (one, thing)" are to be identified as designated representatives of the nominal categories. It follows, then, that interrogatives must reflect the distributional limitations of these designated elements. Hence the general condition on deletion operations just proposed can provide a partial explanation for the peculiar restrictions on the formation of interrogatives in English.

There are indications of a different sort that this condition is necessary for descriptive adequacy. In general, a sentence from which a phrase has been deleted by a grammatical operation is not interpreted as structurally ambiguous. Thus the sentences "his car was stolen" (with agent deletion), "John is eating" (with deletion of Object— for discussion of this class of verbs, see Lees, 1960a; Chomsky, 1962a), and so on, are surely not to be considered in the same light as "flying planes can be dangerous," "John doesn’t know how good meat tastes," and other familiar examples of structural ambiguity. An elliptical sentence is not simply one that is subject to alternative interpretations. But if it is true that the interpretation of a sentence is determined by the structural descriptions of the strings that underlie it (as is supposed in the theory of transformational grammar), then the degree of ambiguity of a sentence should correlate with the number of different systems of structural description underlying it. In particular, if the condition that we have proposed is not met, the "elliptical sentences" given above should be multiply, in fact, infinitely ambiguous, since they should each have infinitely many sources. Thus "the car was stolen" could derive from "the car was stolen by the boy... by the tall boy... by the tallest of all the boys in the school," etc. In fact, the proposed condition establishes that each such sentence (similarly, "who did he see," etc.) is derived from a single source with an unspecified Noun Phrase instead of from infinitely many sources with different Noun Phrases, consistently with the manner in which these sentences are interpreted.

14This condition is, incidentally, particularly important for the study of the limits on generative capacity of transformational grammars. Cf. Matthews (1961).
In this case, then, it seems that we can formulate a well-motivated general condition that partially explains the facts stated in the descriptively adequate grammar. This condition predicts that such must be the linguistic intuition of anyone who constructs for himself a transformational grammar to deal with the linguistic data to which he has been exposed.

Further investigation of conditions on relatives and interrogatives raises interesting problems of a variety of different sorts. We have proposed that interrogatives were formed by rule (6) with X limited to Noun Phrases of the form "some (one, thing)" and W by rule (7). Thus from "I know someone who was expelled" (with W = "who was expelled"), we should derive "who who was expelled do you know?"; from "he has something of yours", we should derive "what of yours does he have?" (with W = "of yours"). In these cases there should derive "who from Philadelphia do you know?" and so on. In these cases there are preferred alternatives, namely, "who do you know who was expelled?" "what do he have of yours?" "who do you know from Philadelphia." Considering these three cases, we must either modify the rule (6) to allow it to apply only to the segment Determiner + Noun or a Noun Phrase of the form Determiner + Noun + Relative Clause, or we must conclude that the grammar contains a subsidiary rule (7), which applies after (6);

\[ X - \text{relative} - Y \Rightarrow X - Y - \text{relative}, \]  

(7)

where X is an indefinite Noun Phrase (note that "from Philadelphia," "of yours," does have the derived constituent structure of Relative Clause in the examples above, as does "who was expelled"). The choice between these alternatives is settled by the fact that "who (what) + Determiner + Noun is a Noun Phrase of the form Determiner + Noun + Relative Clause, or we must conclude that the grammar contains a subsidiary rule (7), which applies after (6);

Notice that although several Noun Phrases in a sentence may have Wh attached to them, the operation (6) must be limited to a single application to each underlying Wh. Thus we can have "who saw what?" "you met the man who saw the railroad station" "you read the book that was next to what?" "you saw the book which was next to what?" etc., but not "who saw what?" "you saw the book which was next to what?" (as a declarative), and so on, as could arise from multiple applications of this rule. These examples show that (6) cannot apply twice to a given string as a Relativization and once as an Interrogative transformation. Thus if rule (6) has applied to form a string which is embedded as a relative clause, it cannot reapply to this embedded string, preposing one of its Noun Phrases to the full sentence. Thus we can have the interrogative "he saw the man read the book that was on what?" but not "what did he see the man read the book that was on?" and we can have "he wondered where John put what?" but not "what did
succeed in excluding such nonsentences as "what for me to understand would be difficult," although the perfectly correct form "what would it be difficult for me to understand?" is still permitted. Thus this condition would account for a distinction between the occurrences of "for me to understand something" in the contexts—"would be difficult" and "it would be difficult"—so far as applicability of (6) is concerned.168

This discussion obviously does not exhaust the topic. For one thing, it by no means specifies the distributional peculiarities of relatives and interrogatives in full detail, and to the extent that this deficiency still remains, important questions of explanatory adequacy cannot even be raised. Furthermore, even where a partial explanatory account can be given, there are open questions that we have not dealt with. Thus in discussing designated members of categories we assumed that the representatives of the nominal categories were "someone," "something," and their variants, but the examples we gave to support this could equally well have been used to support the claim that the representatives are "everyone," "everything." In fact, in favor of the latter claim one might cite such examples as "whose reputations are at stake?" (suggested by P. Kiparsky), which have no source if "someone," "something" are taken as the unique designated elements. But if there are several alternative designated elements, the comments on "recoverability" must be slightly revised. In general, many aspects of relative and interrogative constructions remain to be accounted for, and it seems that the complex of problems relating to rule (6) should continue to provide a profitable testing ground for explanatory hypotheses concerning the form and applicability of grammatical rules.

Consider now one final example from the domain of syntax. Such sentences as

\[ \text{I don't approve of his drinking (cooking driving, etc.)} \]

(9)

are ambiguous (... the fact that he drinks, cooks, etc.; the manner in which he drinks, cooks, etc.).17 An explanation for this is proposed in Chomsky (1955), and it can now be given a much better formulation as well as stronger support by several ingenious observations of Lees (1960a, 64f.) and Klima (personal communication). Among the many ways of converting declarative sentences into NP's in English (cf. Lees, 1960a), we have, in particular, two that can be described informally as follows:

\[ \text{NP - Aux_Aux_ Verb (NP) cc NP} \]

(10)

\[ \text{NP - Aux - Verb - (NP) - Aux_ Poss - nom + Verb - (of + NP).} \]

(11)

The transformation (10) gives such noun phrases as "his refusing (having refused) to

168 Alternatively, one might attempt to account for this distinction by a condition that relies on the fact that in the illegitimate case the Noun Phrase to be proposed is contained within a Noun Phrase, while in the legitimate case, it is not. However, the condition that a Noun Phrase contained within a Noun Phrase is not subject to (6), though quite plausible and suggested by many examples, is apparently somewhat too strong, as we can see from such, to be sure, rather marginal examples as "who would you approve of my seeing?", "what are you uncertain about giving to John?", "what would you be surprised by his reading?", etc. There is certainly much more to be said about this matter.

17In the case of "cooking," there are, in fact, two more interpretations, since "cooking" is a Noun independently of the transformations (10), (11) below, and "cook" is one of those Verbs that undergo the transformation of NP - V - NP, to NP - V (cf. Gleitman, 1960; Chomsky, 1962a) giving "NP cooks" (which is then subject to (10)) from, e.g., "they cook NP."
derived phrase structure Determiner-Noun, as in the case of "his rejection of the offer." They must thus be formed by the transformation (11). And observe, in fact, that there is no other nominalized form of these verbs (as "refusal" and "rejection") contrast with "refusing" and "rejecting"). Hence we conclude that there is an obligatory rule that assigns to the nominalizing morpheme nom introduced in (11) the shape /ing/ when it is affixed to "drink," "drive," etc., just as it assigns to nom the shape /-el/ when it is affixed to "refuse" and the shape /-en/ when it is affixed to "reject."

It follows that "drinking," "driving," etc., will be formed in two distinct ways, by (10) and by (11). Since these verbs are, furthermore, optionally intransitive, the full NP "his drinking," "his driving," etc., will also be generated in two ways, once by (10) (with the derived structure NP and the interpretation "fact that") and once by (11) (with the derived structure Determiner + Noun, as well as NP, and the interpretation "manner of"). Noting that adjectives cannot be inserted in (12) (giving, e.g., "I don’t approve of his excessive drinking the beer"), we conclude that this is unambiguously derived by (10), consistent with its interpretation, in this case.

Notice that as the wh-question transformation was formulated, it does not yield "whose book (did you find)?", "which book (did you find)?", etc. To form these, it must be extended to apply also to underlyings strings of the form X – Determiner + Nom – Y (note that possessive NP’s are Determiners, replacing the definite article, in fact, by a transformation). Applying this observation to the present case, we see that this transformation will yield "whose excessive drinking surprised you?", etc., as it should, but that it will exclude "whose drinking the beer surprised you?", etc. (again, correctly), since the underlying NP in this case is not of the form Determiner + Noun. Similarly, "whose drinking surprised you?" will be derived from only one source (and it is, in fact, unambiguous), since only one of the potential sources is of the required form Determiner + Noun.

See Katz and Postal (forthcoming) for further discussion of the problems presented by such examples as (9). It seems clear that examples such as these are totally beyond the range of any version of the taxonomic model, as so far conceived. But again, it seems possible to achieve the levels of descriptive and even explanatory adequacy with a transformational grammar.

2.3 Levels of adequacy in semantics. I have given several examples of how a higher level of adequacy might be achieved by linguistic theory in the domains of phonology and syntax. It remains to consider the third major part of a synchronic description, namely, its semantic aspect. Here the problem is much more obscure. One might perhaps maintain that the condition of observational adequacy would be met by an account of situational regularities associated with actual discourse, and that the condition of descriptive adequacy is in part achieved by a set of appropriately interrelated dictionary entries, an explicit portrayal of the structure of certain "semantic fields," a list of terms that enter into specific meaning-relations, e.g., synonymy, etc. How might one hope to achieve a higher level of adequacy, in this case? It might plausibly be maintained that certain semantic features of a language can be partially explained in terms of underlying syntactic processes. As an example, consider the discussion of (9), above. Or consider the case of such adjectives as "astonishing, "intriguing, "etc., which have the semantic property that they are connected with a specific human "reaction," even where no explicit reference is made to the person who is interested, astonished, intrigued ("it was an intriguing plan," as distinct from "it was an elaborate plan, etc.). These adjectives have in common many important syntactic features that distinguish them from other Verb + ing forms (e.g., "the plan seems intriguing *failing), "a very intriguing *failing), etc.). Furthermore, they would be derived, in a transformational grammar, from sentences in which they appear as verbs ("the plan intrigues one," etc.—cf. transitives with human objects. Thus the structural description of the sentence containing the terminal string underlying "the plan intrigues one (i.e., unspecified human) will just as explicitly as it contains the past tense morpheme; and this fact might be suggested as the explanation for the cited semantic feature.

In general, syntactic description becomes deeper, what appear to be semantic questions fall increasingly within its scope, and it is not entirely obvious whether or one can draw a natural bond between grammar and "logical grammar," in the sense of Wittgenstein and the Oxford philosophers. Nevertheless, it seems clear that explanatory adequacy for descriptive semantics requires, beyond this, the development of an independent semantic theory (analogous, perhaps, to the theory of universal phonetics as mentioned below) that deals with questions of a kind that can scarcely be coherently formulated today, in particular, with the question: what are the substantive and formal constraints on systems of concepts that are constructed by humans on the basis of presented data? Observe that the problem posed in Section 1 for general linguistics is a special case of this question, where the system of concepts that is acquired consists of the notions "well-formed sentence of L," "grammatical relation in L," "sound pattern of L," etc. Perhaps it is not too much to hope that this particular problem may serve as a useful paradigm case. We return to this speculation below in Section 5. In any event, it seems that formulation of a general semantic theory of some sort, independent of any particular language, is perhaps not an unreasonable task to undertake, and is a precondition for any far-reaching attempt to attain a level of explanatory adequacy in semantic description. We might observe, at this point, that many problems of universal semantics (as of universal phonetics) were raised and quite seriously studied in the seventeenth century (cf., for example, Wilkins, 1668), though rarely since.

2Cf. Nowell-Smith (1954, 85). Other adjectives may also be characterizable in this way for some different reason, but this is irrelevant to the correctness of the present observation.
3That is, "intrigue," "astonish," etc., do not undergo optional deletion of the object, as do observations are not refuted by the fact that deviant utterances with object deletion can be attested (as, for example, "the American Legion to the category in question), just as the distinction between the classes of adjectives noted above is not obscured by instances such as "if the sea was not very raging. . . ." (B. Russell, Inquiry into Meaning and Truth, W. W. Norton & Co., 1940, 84). See note 2 and references there.
Katz and Postal (forthcoming).
2.4 Comprehensiveness of grammars. In the preceding discussion, three levels of adequacy have been loosely sketched that might be attained by a linguistic description. Of these, only the levels of adequacy in the areas of phonology, syntax, and semantics. Of these, only the levels of adequacy have been loosely sketched that might be attained by a linguistic description. Of these, only the levels of adequacy in the areas of phonology, syntax, and semantics. Of these, only the levels of adequacy have been loosely sketched that might be attained by a linguistic description. Of these, only the levels of adequacy in the areas of phonology, syntax, and semantics.

Adequacy and explanatory adequacy (and, ultimately, only the latter) are of sufficient interest to justify further discussion. Notice, however, that these levels of success are always possible to describe the linguistic intuition of the native speaker in a common isism. It is important to bear in mind that a grammar that assigns correctly the mass of structural descriptions (remeote or this is from present hopes) would still be of no particular linguistic interest unless it also were to provide some insight into those formal properties that distinguish a natural language from arbitrary, enumerable sets of structural descriptions.

Comprehensiveness of grammars. In connection with the question of levels of success, we must also briefly consider the matter of coverage data. Sapir's often quoted remark that "all grammars so called leak" is extremely misleading, insofar as it implies that there are grammars so complete that the question of completeness of coverage can seriously be raised. But this is patently false. In the case of traditional (i.e., inexplicit generative) grammars, the gaps are not easy to locate because of the vagueness of the rules and the essential reliance on the linguistic intuition of the reader. One of the merits of an explicit generative grammar is that these gaps are immediately exposed. Anyone who is actively at work on a linguistic description can cite innumerable examples that fall beyond the range of rules as so far formulated, or that are incorrectly handled by these rules—its, in fact, sufficient to open a book or to listen to a conversation at random to find countless examples of sentences and sentence types that are inadequately dealt with in traditional or modern grammars.

Comprehensiveness of coverage does not seem to me to be a serious or significant goal at the present stage of linguistic science. Gross coverage of data can be achieved. But this is little in many ways, by grammars of very different forms. Consequently, we learn little about the nature of linguistic structure from study of grammars that merely accomplish this. Higher levels of adequacy, in the sense described above, have been achieved.

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It is important to bear in mind in considering the masses of linguistic data that are encompassed by its rules, where this data has no demonstrated bearing on the correctness of alternative formulations of the grammar of this language or on alternative theories of language. Until incorporated in an explicit generative grammar, such examples simply stand as exceptions, no more relevant to the correctness of the already formulated rules than strong verbs and irregular plurals. Listing of innumerable examples is neither difficult nor very interesting; it is quite another matter to find rules that account for them, or a general theory of such rules. It is necessary to distinguish between exceptions to a grammar, and counter-examples to a proposed general theory of linguistic structure. Examples that lie beyond the scope of a grammar are quite innocuous unless they show the superiority of some alternative grammar. They do not show that the grammar as already formulated is incorrect. Examples that contradict the principles formulated in some general theory show that, to at least this extent, the theory is incorrect and needs revision. Such examples become important if they can be shown to have some bearing on alternative conceptions of linguistic structure.

III. ON OBJECTIVITY OF LINGUISTIC DATA

When we discuss the levels of descriptive and explanatory adequacy, questions immediately arise concerning the firmness of the data in terms of which success is to be judged (nor are difficulties lacking even on the level of observational adequacy—cf. note 8). For example, in the case of (3), (4) one might ask how we can establish that the two are sentences of different types, or that "John's eagerness to please..." is well formed, while "John's easiness to please..." is not, and so on. There is no very satisfying answer to this question; data of this sort are simply what constitute the subject matter for linguistic theory. We neglect such data at the cost of destroying the subject. It is not that these introspective judgments are sacrosanct and beyond any conceivable doubt. On the contrary, their correctness can be challenged and supported in many ways, some quite indirect. Consistency among speakers of similar backgrounds, and consistency for a particular speaker on different occasions is relevant information. The possibility of constructing a systematic and general theory of

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24These comments apply, it seems to me, to most of the examples presented by Bolinger (1960, 1961). These lists of examples could be extended indefinitely. In the form in which they are presented, they have, for the most part, no obvious bearing on the correctness formulations of English grammar that have been proposed for certain fragments of the language, or of the theories that underlie them.

Bolinger does suggest (1961, 381) that his examples are in conflict with certain theories of generative grammar, and that they support an alternative view about the nature of language, about which he offers only the following hint: in a grammar of the sort he envisions, "constructions are not produced one from another or from a stock of abstract components, but flled side by side," and the speakers do not "produce" constructions, but rather "reach for them, from a preestablished inventory.

It is difficult to comment on the proposal in this form, because of the vagueness of the notions "construction" and "flled." If by "construction" Bolinger means something like "sequence of word classes," then his proposal is ruled out at once. It is clear that the variety of normal sentences is so great that the number of word class sequences associated with them is far larger than the number of seconds in a lifetime. For quantitative estimates beyond this question (which are furthermore highly conservative) see Miller, Galanter, Pribram (1960), Miller and Chomsky (1963). If he has in mind some more abstract principle by which constructions are "flled," it remains to be seen whether this proposal, when clearly formulated, will differ from current theories of generative grammar.
to account for these observations is also a factor to be considered in evaluating the probable correctness of particular observations (as in the case of any data—cf. note 8). Consequently the fact that a certain grammatical theory has had explanatory 8). Consequently the fact that a certain grammatical theory has had explanatory

It is sometimes assumed that operational criteria have a special and privileged
task. It seems rather obvious that the attempt to gain some insight into the range of data that we now have is likely to be far more fruitful than the attempt to make this data more firm, e.g., by tests for synonymy, grammatical-

In these and many other cases, what has not been shown is that the concept defined by the proposed operational criterion has some importance. In fact, at the present stage of the study of language, it seems rather obvious that the attempt to gain some insight into the range of data that we now have is likely to be far more fruitful than the attempt to make this data more firm, e.g., by tests for synonymy, grammatical-

IV. THE NATURE OF STRUCTURAL DESCRIPTIONS

A generative grammar contains a syntactic component, which generates strings of formative and specifies their structural features and interrelations; a phonological component, which converts a string of formatives with a specified syntactic

38A critical and still unanswered objection to any such approach has been given by Bar-Hillel (1954, 233). Frei also gives a "distributional" argument against the existence of homonyms (40), but again this is simply a proposal for terminological revision. He regards these terminological

39What seems open to question in this account is only the use of the words "arbitrary" and "un-

verifiable" to apply to empirical hypotheses that do not merely summarize evidence, that is, to all nontrivial assertions of science or common sense, to X's belief that Y is using "tomorrow" in the sense of its "history" (cf. comments by P. Foot, 1961, 47f.). The other notions defined in terms of "stair cases" are also relevant in the context of operational tests, among the many that might be proposed, has some unique significance; and the thesis of indeterminacy seems to amount only to the assertion that a significant empirical assertion has logically conceivable alternatives, which is true but unexciting.27
structure into a phonetic representation; and a semantic component, which assigns a semantic interpretation to a string of formatives with a specified syntactic structure. After a brief discussion of structural descriptions on the syntactic level, we will turn to a more detailed account of alternative views as to the nature of phonological representation. For discussion of semantic interpretation of structures generated by the syntactic component, see Katz and Fodor (1963), Katz and Postal (forthcoming).

4.1 The syntactic component. A structural description on the syntactic level must indicate how a string of formatives is subdivided into constituents of varying scope (from formatives, at one extreme, to the full sentence, at the other) and what are the categories to which these substrings belong (Noun, Verb, Noun Phrase, Relative clause, etc.). Such information can be presented as a labelled bracketing of a string of formatives or in some equivalent notation, e.g., a labelled tree such as (14) for the sentences (3), (4).

\[
\begin{array}{c}
\text{NP} \\
\text{John} \\
\text{VP} \\
\text{Adj} \\
\text{easy} \\
\text{to} \\
\text{Verb} \\
\text{Verb} \\
\text{to} \\
\text{NP} \\
\text{Subject} \\
\end{array}
\]

Such a representation is what we have called a Phrase-marker. In terms of such Phrase-markers, we can define grammatical relations, as certain subconfigurations. Thus the Subject-Predicate relation might be identified as the subconfiguration (Sentence; NP; VP), in which case it would hold between "John" and "is easy (eager) to please" in (14); and the Verb-Object relation could be defined as the configuration in (14) for which it may hold "John," with the obvious Phrase-marker; etc. Alternatively, these relations may hold between expressions (1) if they form part of a configuration of a Phrase-marker, as described above, or (II) if a "co-occurrence relation" of an appropriate sort holds between the pair in question and a pair that has this grammatical relation in the sense of (1). Accordingly, we would say that in

\[
\begin{align*}
\text{The gift pleased John.} \\
\text{did John expect to be pleased by the gift?}
\end{align*}
\]

For reasons such as this, the taxonomic model of modern linguistics (cf. Section 1, above), which provides a single Phrase-marker of the type (14) as the structural description of a string, must be regarded as descriptively inadequate. One might attempt to overcome this inadequacy by extending the definition of "grammatical relation" in the following way. Let us say that a grammatical relation holds of a certain pair (triple, etc.) of expressions if they form part of a configuration of a Phrase-marker, as described above, or if a "co-occurrence relation" of an appropriate sort holds between the pair in question and a pair that has this grammatical relation in the sense of (1). Accordingly, we would say that in

\[
\begin{align*}
\text{The gift pleased John.} \\
\text{did John expect to be pleased by the gift?}
\end{align*}
\]

The goal of traditional "universal grammar" was, in part, to give a substantive general account of these categories, thus fixing a universal "vocabulary" for the generative grammars of all languages. Presumably, such fixed universal category symbols would have to be defined in all languages. We apparently, language-independent semantic properties terms of formal properties of grammars and, perhaps, language-independent semantic properties will regard the category names for the time being as only conventional.

"For a careful definition of one such notion, see Hii (1961). This notion was introduced by Harris (1952a, 1952b) and studied in detail (Harris, 1957) as the basis for a theory of grammatical transformations. It is also mentioned in a similar connection by Bazell (1953), and is applied to Russian in Worth (1958). A grammatical transformation is defined, from this point of view, as a (symmetrical) relation holding between two sentence forms if corresponding positions in the two forms are filled by the same n-tuples of expressions. This relation is not part of generative grammar, as the notion "grammatical transformation" of Section 1 and the references cited there, but is a structural relation holding of sentences and sentence forms described by a taxonomic, IC grammar (as in Harris, 1951a, chapter 16). The notions of "co-occurrence relation" and "generative transformation" are rather different in formal properties as well as in their role in actual syntactic description, and a great deal of confusion can result from failure to distinguish them. Thus it makes no sense to arrange co-occurrence relations "in sequence," but generative transformations can (and, in practice, must) be ordered and applied in sequence. The examples of Section 2 depend essentially on the syntactic order and sequential application of transformational rules, and on appropriate choice of base versus derived forms (a distinction which is also not definable in terms of co-occurrence). Furthermore, co-occurrence is a relation defined on actual sentences, while generative transformations apply to abstract structures that often bear no close relation to actual sentences. Note also that in a generative transformational grammar, a direct, one-step transformational relation would hold between (16) and each of the sentences of (17); a somewhat more deviant relation would hold between (16) and (15), which is derived by a sequence of transformations from a pair of strings, one of which underlies (16); and no relation at all would hold between (15)-(17), or (17i)-(17ii), though all would be based on the terminal string underlying (16). From the point of view of co-occurrence, however, there is a "one-step" relation between both (16)-(17), and (17i)-(17ii), and no relation at all (because of "the gift brought happiness," etc.) between (15) and (16). Similarly, no co-occurrence relation would hold between (18ii) and (18ii) (because of "I met the boy"); though the latter is derived from the string underlying the former by a sequence of generative transformations. There are many other differences.

Harris' notion of transformation as a co-occurrence relation developed in the course of his work in the late 1940's on analysis of the structure of extended discourse. At the time, I was attempting to construct generative grammars for Modern Hebrew and English using Harris' morpheme-to-utterance procedures, with variables ranging over "long components," as a model for the syntactic component. There were serious difficulties in this, and the notion of grammatical transformation, when adopted and redesigned to enter the syntactic component of a generative grammar with ordered rules, seemed to overcome most of these.
did the gift please John?  
John was pleased by the gift.

(17i)  
(17ii)

where the grammatical relations are not expressible directly in terms of subconfigurations of the Phrase-marker, the Subject-Verb and Verb-Object relations hold of the pairs “the gift” – “please” and “please” – “John,” respectively, because any triple of expressions that can replace “the gift,” “please,” and “John” in (17) can also (with appropriate reordering) fill the positions of these expressions in (16), where the grammatical relations are definable directly in terms of the Phrase-marker.

However, this approach seems to me to face insurmountable objections. Thus although it may be true that a co-occurrence relation of the appropriate sort holds between (16) and (17), it does not hold between (15) and (16), or (18i) and (16). Thus “please” – “John” can be replaced by “bring” – “happiness” in (16), but not in (15) or in (18); but in all three cases these expressions are related as Verb-Object. And if some modification is proposed to deal with this discrepancy, will it be able to distinguish the grammatically related “please” – “John” in (15) from the same pair, grammatically unrelated, in “did John expect you to be pleased by the gift?” Or consider the sentences (18ii-iv):

the gift pleased John but not Bill  
the book is what I want  
I want the book  
the clever boy saw the friendly man

(18i)  
(18ii)  
(18iii)  
(18iv)

In both (18ii) and (18iii), the Verb-Object relation holds of the pair “want” – “the book”; but only in (18iii) can this pair be replaced by “met” – “the boy.” In (18iv), “clever” and “boy” are related as in “the boy is clever”; but in the latter, though not in (18iv), the pair “plan” – “intriguing” can replace “boy” – “clever.” Furthermore, it seems that any pair that can replace “clever” – “boy” in (18iv) can replace “clever” – “man” in the same sentence, though no grammatical relation at all holds of this pair.

It is, of course, impossible to show that no possible modification of the notion of co-occurrence could deal with such problems. However, for the present it seems clear that any theory which, like the theory of phrase structure grammar, assigns a single Phrase-marker such as (14) to an utterance is incapable of expressing deeper structural relations and must therefore be ruled out by considerations of descriptive adequacy.

In the case of a transformational grammar, the syntactic description of a string of formatives consists of a set of underlying Phrase-markers (one for each of the underlying simple strings from which the string is derived), a derived Phrase-marker such as (14) that gives its superficial constituent structure, and a Transformation-marker that expresses the manner of its derivation from underlying strings. The deeper structural information in the examples that we have discussed is provided by the underlying Phrase-markers (for further details concerning the particular example (14), see Miller and Chomsky, 1963; see the references cited previously for the other cases). These examples are quite typical in this respect. In general, the grammatical relations that are expressed (in the manner indicated above) in the underlying Phrase-markers are those that constitute the deeper structure and determine the semantic interpretation of an utterance. The categorization expressed in the derived Phrase-marker plays a role in determining the form of the utterance (thus the “grammatical Subject” determines the number of the Verb Phrase, and the derived Phrase-marker determines the functioning of the phonological rules—cf. the references of note 6), but are irrelevant to its content. The primary motivation for the theory of transformational grammar lies in the fact that the significant grammatical functions and relations are expressible, in a natural way, only in underlying elementary Phrase-markers. For the present, the transformational model for generative grammar is unique in that it allows for the generation of structural information of a variety rich enough to account for facts of the kind discussed here and in Section 2, above—and, furthermore, to do so in many cases in a principled way, thus reaching the higher level of explanatory adequacy—though it is by no means without its problems.

4.2 The Phonological component. The phonological component of the grammar can be regarded as an input-output device which operates on a string of formatives, provided with a structural analysis by the syntactic component, and assigns to this string a representation as a string of phones. It is, in part, an open question to what extent structural information on the syntactic level is relevant to determining the phonetic form of a string of formatives. There is no doubt that information of the kind provided in the derived Phrase-marker is essential, and there are scattered examples that suggest that deeper syntactic features may also play a role in determining the details of phonetic shape.

A rather classical view of the structure of the phonological component might be something like this. Formatives are of two types: grammatical and lexical (among the grammatical we count, as subtypes, class markers and junctural elements introduced by syntactic rules, e.g., word boundary). Each grammatical formative is represented by a single symbol. Each lexical formative is represented in a systematic orthography as a string of symbols, each of which is assigned to certain categories (Vowel, Consonant, Voiced, etc.). Each symbol can, in fact, be regarded as an abbreviation for the set of categories to which it belongs, and each lexical item can thus be represented by a classificatory matrix in which the columns stand for what we can call “segments” and the rows, for categories; the entry in the \( i \)th row and \( j \)th column indicates whether the \( j \)th segment belongs to the \( i \)th category. These categories we may call (classificatory) distinctive features. Some squares of the matrix may be blank, where the feature in question can be supplied by a general rule (e.g., the entry for Rounding in the case of English Lax Front Vowels, which become automatically, unrounded).

The rules of the phonological component are ordered, and apply in sequence to
the string of formatives (utilizing, when this is relevant, the associated syntactic information) until ultimately a representation is reached in terms of a universal phonetic alphabet. The symbols of this alphabet are specified in terms of a set of phonetic features; hence the output of the phonological component can again be regarded as a matrix in which columns represent phones and rows, phonetic features of the universal system. The entry in the th row and th column indicates whether of the universal system. The entry in the row and column of th feature, or the degree to which th phone of the generated utterance possesses the th feature, or just features.

Classificatory distinctive features are by definition "binary"; phonetic features may or may not be distinctive. In the case of such features as stress, Classificatory distinctive features are found in the possible combinations and contrasts. Steps toward such a theory are found in the work of the classical British phoneticians (Bell, Ellis, Sweet); in the "phonology" of de Saussure's 1897 lectures, and again in Jakobson's theory of distinctive features of phonetics. This theory constitutes a universal phonetic theory, with a fixed alphabet, as the condition of phonetic distinctiveness. Note that a universal phonetic alphabet is the counterpart of a substantive theory of syntactic categories (see above, note 28) that assigns a fixed significance to the labels used in the syntactic component; but in the case of a phonetic alphabet, the phonetics of Bloomfield's practice (1933) (in particular, when his "secondary phonemes" are not represented), though it is difficult to say whether it is inaccord with his phonological theory, which is hardly a model of clarity. Systematic phonemics would now generally be called "morphophonemics," in view of the several senses of the term. This terminological innovation is justified if there is a third, intermediate level of systematic representation, more closely related to sound and quite independent of syntactic structure, such as the system of representation now called "phonemic." However, as I will attempt to show below, the existence of an additional level is highly dubious, and for this reason I have preferred to keep the older term, modified by "systematic" to avoid confusion.

It is instructive, in this connection, to recall the controversies aroused by Bloomfield's Language.

In particular, Kent's review (1934) criticized it from the point of view of traditional (systematic) phonetics. Kent argues that "the difference between [s] and [z] is functional in English: shall we disregard it in citing Japanese, because it is not functional— even though we have the machinery for marking the distinction." In this vein, he criticizes Bloomfield's phonemicization of "secretary" ([sekrət̪i] vs. [sekrət̪i] vs. [sekrət̪i]), etc. In responding to the review, Bolling (1934) comments that to mark predictable variants, "would be like the meaningless underlining of a schoolgirl;" and he supports Bloomfield's phonemicizations by the argument that they mark only what is not predictable. It is interesting to note that the position that Bolling is attacking is, on many points, just the one that is adopted by the "Neo-Bloomfieldian" linguists of the 1940's and 1950's, who characteristically criticize Bloomfield for failure to separate levels, and who return to a much "narrower" transcription. In particular, the marking of reduced variants of unstressed vowels is considered one of the major innovations in this development. We return to this issue directly.

The controversy between Kent and Bloomfield-Bolling concerns the choice between systematic phonetics and systematic phonemics. But it is clear that these are not alternatives, and that in fact both are significant in the description of a language. It was Bloomfield's summary rejection of phonemics as without scientific value or status, rather than his development of a higher level of representation, that should really have been at issue here.
In general, we can say, with Palmer (1958), that the place of the phonological component is "that of an ancillary technique; it provides a bridge between the grammatical statement and the direct observations that are reported in phonetics." For linguistic theory, the significant questions concerning the phonological component have to do with the choice of phonetic features (and, more generally, the universal phonetic theory), and with the conditions on the form and ordering of rules. The latter question, in particular, is of great importance, and phonological theory has suffered seriously from its neglect. As soon as the attempt to construct explicit rules to determine the phonetic shape of a string of formatives passes the most superficial and introductory stage, it becomes obvious that a fairly strict ordering must be imposed on phonological processes, if they are to be describable in full generality. Thus most of the examples in Sapir (1933) involve ordering, though he does not explicitly mention this fact. Bloomfield was much concerned with questions of ordering, and his Menomini morphophonemics (1939) is the first modern example of a segment of a generative grammar with ordered rules. Bloomfield does not discuss the extent or depth of ordering in this grammar, and it is not easy to determine this from the examples that he gives. It apparently does not exceed five (cf. Bever, 1963). In the segment of the phonological component of Modern Hebrew presented in Chomsky (1951), a depth of ordering that reaches the range of twenty to thirty is demonstrated, and this is surely an underestimate. Recent work (see note 32) gives strong support to the belief that ordering relations among phonological processes are quite strict; and, furthermore, it provides evidence that the ordering is not strictly linear, but is in part cyclic (see Section 1). Resolution of these questions seems to me the outstanding problem for contemporary phonology. Although several cases of ordering will be presented below, it is important to bear in mind that scattered examples cannot give an accurate indication of the extent or significance of ordering in a full grammar.

To make the discussion somewhat more concrete, consider the following simple example from English. We find such phonological regularities as the following

(20i) \[ \text{\{t\}} \rightarrow \text{s in the context: } - + [i, y] \]
(20ii) \[ [s, z] + [i, y] \rightarrow [s, z] \text{ in the context: } - \text{ Vowel} \]

Thus we have "opaque" - "opacity," "logic" - "logicism," "demoncracy," "piracy" - "piracy," in case (i); "race" - "racial," "express" - "expression," "erase" - "erasure," "enclave" - "enclose," "revise" - "revision," in case (ii). Although various qualifications are needed, clearly rules such as these are needed in any grammar. But if these are regarded as purely classificatory, unordered rules to the effect that "morphophoneme" X has the "phoneme" Y as member (or realization, etc.) in the context Z-W, then they must be supplemented by the additional rule:

(21) \[ \text{(20ii)} \]

To account for "logician," "delicious" (cf. "delicacy"), "relate" - "relation," "ignite" - "ignition," etc. But clearly this rule is unnecessary if (20ii) can apply to the result of application of (20i), that is, if the rules are ordered as in (20).

The grammar containing just (20i), (20ii), in that order, will provide such derivations as:

(22) \[
\begin{align*}
\text{la}i\text{jik} + \text{yin} & \rightarrow \text{president + i} \\
\text{la}i\text{jis} + \text{yin} & \rightarrow \text{presidens + i} \\
\text{la}i\text{jish} + \text{yin} & \rightarrow \text{presidenz + ael} \\
\end{align*}
\]

The top line in (22) is the systematic phonemic representation, in each case, and the last line becomes the systematic phonetic by additional rules. But none of the intermediate stages has any systematic status at all, apparently. For each linguistic form, the number of intermediate representations will depend on the number of rules in the ordered sequence that apply to it, and this number will differ for different forms, indeed, for different subparts of the same sentence, phrase, or word.

Clearly a grammar that contains (21) as a rule is missing a generalization. In fact, consideration of additional examples shows immediately that several generalizations are being missed. Thus observe that alongside of (20) there is also the rule

(23) \[ z \rightarrow s \text{ in the context: } - + iv \]

as in "abuse" - "abusive." But consider the forms "persuade" - "persuasive," "corrode" - "corrosive," "erase" - "erasure," etc. In a taxonomic grammar with no provision for applying rules in sequence, these regularities must be

\[ \text{Cf. Bloomfield (1933, 213). He regarded ordering of rules as an artifact—an invention of the linguist—as compared with order of constituents, which is "part of language." But this deprivation of the role of order of synchronic processes is just one aspect of the general antipathy to theory (the so-called "anti-mentalism") that Bloomfield developed and bequeathed to modern linguistics. This tendency fitted well with the operationalism, verificationism, and behaviorism that Harris showed (1951a, 237) that some of Bloomfield's examples of ordering can be handled by unordered rules that state the phonemic composition of a morphophoneme in a strictly morphophonemic context. But his method does not generalize to such examples as the one given directly below; and, furthermore, it is not clear whether the italicized condition on morphophonemic rules is compatible with the procedures by which they are established, since these procedures set up morphophonemes (similarly, phonemes) in terms of phonemic (respectively, phonetic) or mixed environments. There are important questions of principle here that have not been sufficiently clarified.}

\[ \text{That is, it is shown that a sequence of some twenty-five rules can be formed such that any interchange of adjacent rules will lead to a reformulation that increases complexity (and hence reduces generality). In the light of more recent work, the grammar presented there would have to be modified in many respects, but the conclusion concerning ordering, so it appears, would, if anything, be strengthened.}

\[ \text{A natural evaluation measure ("simplicity" measure) for the phonological component (cf. Hale, 1964a) is the number of feature specifications it contains. In particular, then, the grammar is more, "generalized" archiphonemes such as C, V, etc.) rather than segments.}

\[ \text{As throughout, irrelevant details are omitted. In particular, for reasons beyond the scope of this discussion, the first vowel in "logic" should actually be not /a/ but /e/ (cf. note 46), and /i/ should actually be the "archiphoneme" lax vowel.} \]
accounted for by two entirely new rules, independent of (20), (21), (23), namely:

\[
\begin{align*}
\text{d} & \rightarrow \text{s} \quad \text{in the context: } - + \text{iv} \\
\text{d} + [i, y] & \rightarrow \text{z} \quad \text{in the context: } - \text{Vowel}.
\end{align*}
\]

(24i) (24ii)

If we allow rules to apply in sequence, the rules (24) are entirely superfluous. It is simply necessary to generalize (20i) to apply to \([d, t]\) instead of simply \(l/\), thus giving for “persuasive” the derivation (25) and for “persuasion” the derivation (26):

\[
\begin{align*}
\text{perswéed} + \text{iv}, \text{perswéz} + \text{iv} \quad \text{(by (20i)), perswéisiv \quad (by (23))} \\
\text{perswéd} + \text{yin}, \text{perswéz} + \text{yin} \quad \text{(by (20i))}, \text{perswézin \quad (by (20ii))},
\end{align*}
\]

(25) (26)

where again the first is the systematic phonemic and the last the systematic phonetic representation (details omitted).

Again, it is obvious that a grammar that accounts for this variety of phonetic facts by the rules (20) (suitably generalized) and (23), which are independently motivated, is much to be preferred, on grounds of descriptive adequacy, to one which contains in addition the rules (21), (24). The latter grammar is simply leaving significant generalizations unexpressed. But a descripative adequate account in this case again requires that the rules be applied in the sequence given.

Finally, let us extend the analysis to include the forms (27), illustrating a point to which we will return below:

\[
\begin{align*}
\text{decide} & \rightarrow \text{disa-\text{yd}} \\
\text{decided} & \rightarrow \text{disa-\text{yDid}} \quad - \text{[D]} = \text{alveolar flap} \\
\text{decisive} & \rightarrow \text{disaysiv} \\
\text{delight} & \rightarrow \text{dilayt} \\
\text{delighted} & \rightarrow \text{dilayDid}.
\end{align*}
\]

(27i) (27ii) (27iii) (27iv) (27v)

To account for such facts as these, we must add to the phonological component containing the rules (20) and (23), the rules (28) and (29), where the order is now: (20i), (20ii), (23), (28), (29).

\[
\begin{align*}
\text{a} & \rightarrow \text{\text{a}} \quad \text{in the context: } - \text{(Glide) Voiced} \\
\text{[t, d]} & \rightarrow \text{\text{D} in the context: Stressed Vowel \quad Unstressed Vocalic.}
\end{align*}
\]

(28) (29)

Again, these can be generalized in familiar ways, and each is required, independently, by many other examples. With the rules so ordered we have such derivations as the following:

\[
\begin{align*}
decide & \rightarrow \text{disa-\text{yd}} \\
\text{decided} & \rightarrow \text{disa-\text{yDid}} \\
\text{decisive} & \rightarrow \text{disaysiv} \\
\text{delight} & \rightarrow \text{dilayt} \\
\text{delighted} & \rightarrow \text{dilayDid}.
\end{align*}
\]

(30)

\[
\begin{align*}
\text{decide} & \rightarrow \text{disa-\text{yd}} \\
\text{decided} & \rightarrow \text{disa-\text{yDid}}
\end{align*}
\]

(31)

(20i) (20ii) (23) (28) (29)

As we enlarge the range of examples considered, the depth of required ordering increases (as does its complexity, when we introduce the transformational cycle). Investigation of this question has, so far, failed to reveal any systematic set of representation that might be taken as constituting a “level of representation” at any intermediate point in the operation of the phonological component, and therefore it seems necessary to conclude that systematic phonemics and systematic phonetics are the only two levels of representation that appear in structural descriptions provided by the phonological component. To fortify this conclusion, I would like to consider briefly the status of modern taxonomic phonemics, as seen from this point of view.

4.3 Taxonomic phonemics. Sound pattern has been taken as the primary object of study in modern, structural linguistics; it has, furthermore, been studied in relative or complete isolation from the syntactic setting within which phonological processes operate. In both of these respects, structural linguistics marks a departure from a more traditional point of view, which again emerges in recent work in generative grammar, as sketched above. Though modern phonologists have not achieved anything like unanimity, a body of doctrine has emerged to all or part of which a great many linguists would subscribe. Abstracting away from much variation, let us coin the term “taxonomic phonemics” to refer to this body of doctrine, thus emphasizing its striking reliance, in almost all versions, on procedures of segmentation and classification (identification of variants).

Taxonomic phonemic theory constitutes the first attempt to formulate a linguistic theory with sufficient clarity and care so that questions of theoretical adequacy can seriously be raised. The development of taxonomic phonemics has led to standards of explicitness and precision that had rarely been attained in previous linguistic description, and to many new insights into sound structure. Furthermore, the period of its dominance has also been one of unparalleled extension of the range of linguistic investigation. For these reasons, the methodological and substantive assumptions that underlie this theory deserve careful and critical scrutiny. It seems to me, how-

\[47\]

To this extent, this adjustment of (20i) simplifies the grammar (cf. note 37). Several qualifications are needed, however, which make the effect of the adjustment neutral, as regards complexity. Note that these rules should properly be stated strictly in terms of features. Thus, for example, rule (23) should assert, simply: \([ + \text{Continuant}] \rightarrow [- \text{Voiced}] \) in the context: \(- + \text{iv} \).
ever, that they have not received the kind of critical appraisal that this position merits. In this discussion of taxonomic phonemics, I will attempt to show that several of the main methodological and substantive assumptions that have played a crucial role in taxonomic phonemics are invalid, and that, in several important respects, the theory of taxonomic phonemics, as it has emerged during the last thirty years, is less adequate than the phonemic theory that was implicit in the work of such pioneers of modern phonology as, for example, Edward Sapir.

Under discussion, then, are four potential levels of representation associated with the phonological component, namely, the levels of:

- physical phonetics (3li)
- taxonomic phonetics (3lii)
- systematic phonetics (3liii)
- systematic phonemics (3liv)

Physical phonetics is the study referred to by Troubetzkoy (1939) as “the science of the sounds of parole,” a study with methods and goals entirely different from those of phonology (the “science of the sounds of langue”). It provides Bloomfield’s “mechanical record of the gross acoustic features, such as is produced in the phonetics laboratory” (1933, 85); its status is not in question here, and no further attention will be given to it.

I will assume, for the purposes of this discussion, that the status of systematic phonemics (“morphophonemics,” in one sense of the more usual modern phrase) is also not in question.

The status of systematic phonetics and the condition of phonetic specifiability (cf. p. 86, above), however, has been very much in question, and this level has, in fact, been explicitly rejected in many theoretical discussions. Thus for Bloomfield (op. cit.), the only kind of linguistic record that is “scientifically relevant,” aside from that provided by physical phonetics, “is a record in terms of phonemes, ignoring all features that are not distinctive in the language.” Phonetic transcription is dismissed as haphazard, limitless, accidental, and of no scientific value; and Bloomfield maintains that in phonology “we pay no heed to the acoustic nature of phonemes but merely accept them as distinct units and study their distribution” (p. 137). Troubetzkoy sometimes refers to phonemes as completely “abstract” units serving only a distinctive function. But elsewhere, he pays a great deal of attention to the systematization of the universal phonetic features that play a distinctive role in some language (structural phonetics—cf. 1939, 93f.). Bloomfield’s apparent rejection of the level of structural phonetics reappears in an extreme form in Joos’ (1957) summary of what he takes to be the characteristic view of American linguistics, namely, that “languages could differ from each other without limit and in unpredictable ways” (96), that “distinctive features are established ad hoc for each language or even dialect,” and that “no universal theory of segments can be called upon to settle the moot points” (228). Similarly, Hjelmslev appears to deny the relevance of phonetic substance to phonological representation.

Nevertheless, it seems to me correct to regard modern taxonomic phonemics, of all varieties, as resting squarely on assumptions concerning a universal phonetic theory of the sort described above. Analysis of actual practice shows no exceptions to the reliance on phonetic universals. No procedure has been offered to show why, for example, initial [pʰ] should be identified with final [p] rather than final [t], in English, that does not rely essentially on the assumption that the familiar phonetic properties (Stop, Labial, etc.) are the “natural” ones. Harris might be interpreted as suggesting that a nonphonetic principle can replace reliance on absolute phonetic properties when he concludes (1951a, 66) that “simplicity of statement, as well as phonetic similarity, decide in favor of the p-pʰ grouping”; but this implication, if intended, is surely false. The correct analysis is simpler only if we utilize the familiar phonetic properties for phonetic specification. With freedom of choice of features, any arbitrary grouping may be made simpler. From innumerable examples of this sort, it seems that we must conclude that, despite disclaimers, all varieties of taxonomic phonemics rely essentially on the condition of phonetic specifiability. Furthermore, actual practice shows remarkable agreement as to which features constitute the universal phonetic system that is implicitly assumed.

It appears, then, that the status of systematic phonetics is also beyond dispute, though there is room for much discussion as to what is the actual character of the universal phonetic theory that underlies all descriptive practice. In any event, we can assume that each utterance of any language can be uniquely represented as a sequence of phones, each of which can be regarded as an abbreviation for a set of features (those that constitute the universal theory in question), in terms of which “phonetic similarity,” “simplicity of statement,” “pattern congruity,” and so on, are defined.

Let us turn then to a more detailed investigation of taxonomic phonemics, taking this to be a theory that requires that phonological representations must, in addition to the condition of phonetic specifiability, meet conditions which, for the sake of this discussion, I will designate by the following terms:

- linearity (32i)
- invariance (32ii)
- biuniqueness (32iii)
- local determinacy (32iv)

The linearity condition (32i) requires that each occurrence of a phoneme in the phonemic representation of an utterance be associated with a particular succession of (one or more) consecutive phones in its representing matrix, as its “member” or “realization”; and, furthermore, that if A precedes B in the phonemic representation, then the phone sequence associated with A precedes (is to the left of) that associated with B in the phonetic matrix. This condition follows from definitions of the phoneme as a class of phone sequences (as in post-Bloomfieldian American linguistics, typically42) or as a bundle of distinctive features (Bloomfield, Jakobson) or a minimal term in a phonological opposition (Prague circle).

42In the case of Bloch’s very careful system of definitions (cf. Bloch, 1939, for a lucid sketch), the linearity condition is not necessarily met, but it is met, apparently, insofar as linear order is defined. Thus as the definitions stand, it is impossible for English [pʰ] to be a member of the phoneme /p/ (with [p]), since the defining qualities for /p/ are not coextensive with [pʰ] (or if a defining quality
The invariance condition (32ii) asserts that each phoneme \( P \) has associated with it a certain set \( \varphi(P) \) of defining features (that is, \( P = Q \) if and only if \( \varphi(P) = \varphi(Q) \)) and that wherever \( P \) occurs in a phonemic representation, there is an associated occurrence of \( \varphi(P) \) in the corresponding phonetic representation. The invariance condition has no clear meaning unless the linearity condition is also met; I will assume, then, that it is inapplicable when linearity is violated. The invariance condition, in the form stated above, is required explicitly by Bloomfield, Troubetzkoy, Jakobson, and Bloch, for example, and appears to be implicit in many other conceptions. Where linearity and invariance are both met by a taxonomic phonemic representation, the string of phones is segmented into successive segments, each of which contains, along with redundant (determined) features, the defining features \( \varphi(P) \) of some phoneme \( P \), and the phonemic representation is just the sequence of these phonemes.

One can distinguish two versions of the invariance condition, depending on whether the features are taken to be relative (i.e., more or less along a certain phonetic dimension) or absolute. Jakobson explicitly accepts the relative version of the invariance condition, and Bloch, as I understand his account, seems to accept the absolute version. Under the absolute invariance condition, partial overlapping is excluded. If a certain occurrence of a phone \( P \) is assigned to a phone \( P \), then every other occurrence of \( P \) must be assigned to \( P \). Under the relative version of the invariance condition, certain cases of partial overlapping are permissible.

There are, however, some unresolved conceptual difficulties concerning the formulation of the relative invariance condition. Consider, e.g., a binary feature \( F \) such that a phone \( P \) in a certain context \( X-Y \) is assigned the feature \( +F \) or \(-F \) depending on its relation, in terms of the feature \( F \), to some other phone \( Q \) in the context \( X-Y \). But how is the context \( X-Y \) in question to be specified? If in terms of phones, then in general we can expect that the contrasting element \( Q \) will not appear in the context \( X-Y \), but in a context \( X'-Y' \), where \( X' \) belongs to the same phoneme as \( X \) and \( Y' \) to the same phoneme as \( Y \). If in terms of phonemes, then what happens when features that define \( X \) and \( Y \) are again relative to a context which, in this case, includes \( P \) and \( Q \)? For some discussion, see Chomsky (1957b).

Technically, the biuniqueness condition (32iii) asserts that each sequence of phones is represented by a unique sequence of phonemes, and that each sequence of phonemes represents a unique sequence of phones. The biuniqueness condition is very widely maintained by modern phonologists, in particular, by those mentioned above.

However, it is very difficult to formulate this condition in a manner that is actually in accord with their intentions. Consider, for example, Hockett's explicit discussion of it (1951). He considers a hypothetical language with no morphophonemic contrast between voiced and voiceless stops and with the rule:

\[
\text{Stop} \rightarrow \text{Voiced, medially, in words.}
\]

Thus morphophonemic \( \text{patata}k \) becomes phonetic \( [\text{patadak}] \), while morphophonemic \( \text{patata}k \) becomes phonetic \( [\text{pata}k] \). But, Hockett argues, if we hear [\text{pata}k] we do not know whether to transcribe /\text{pata}k/ or /\text{patata}k/.

Consequently the morphophonemic representation fails the biuniqueness condition, and cannot be taken as the phonemic representation, which, in this case, must mark the distinction between voiced and voiceless consonants. This illustrative example, however, leaves many questions unanswered. Suppose, following Hockett, "that there is no word /\text{pada}k/ and no word /\text{tak}/, or that, both of these words existing, they that would not occur in this sequence." Or, suppose that there is a general rule to the effect that no word ends in a vowel. In any such case, "we can conclude that the proper representation would be \( \text{patat ak} \) (\( /\text{pata}k/ \)), and the morphophonemic representation would, technically, meet the biuniqueness condition and would thus qualify as phonemic, if we take this condition literally.

Hockett does not state whether he would accept this system as phonemic, in this case, but it is fairly clear from the context that he would not. In fact, a decision to accept it as phonemic would seem to be inconsistent with his principle of separation of levels, to which we return below, under any reasonable interpretation of this. It is fairly clear that linguists who accept the so-called biuniqueness condition would regard the situation just described as still being a violation of "biuniqueness" in the intended sense. That is, they do not mean by "biuniqueness" simply one-one correspondence, but rather a correspondence such that the unique phonemic representation corresponding to a given phonetic form can be determined by "purely phonetic" considerations, or perhaps, considerations involving only "neighboring sounds." This convention, which is rather difficult to state precisely, is what I have called the condition of local determinacy (32iv). Apparently it is this, rather than literal biuniqueness in the technical sense, that is required in taxonomic phonemics.

Notice that from the linearity and absolute invariance condition one can deduce a particularly strong form of the biuniqueness and local determinacy conditions, namely, as noted above, the condition that the phoneme corresponding to a particular phone can be determined independently of the context of this phone. That is, even partial overlapping is disallowed, and (32iv) is vacuous. Although, as noted above, the situation is still some what confused in the case of the relative invariance condition, it is clear that proponents of such positions (e.g., Jakobson, Harris) would disallow complete overlapping but not partial overlapping, since however they interpret the invariance condition, they do insist on some sort of "biuniqueness."

Although conditions (32i-iv) are (with a qualification to which I return below in Section 4.3) quite generally accepted, and though they do follow from familiar definitions of the phoneme, there are many examples showing that they are untenable. Consider first the linearity condition. Of the many examples that illustrate its...
Nasalized in the context: Nasal Consonant

though phonemic /hand/ is phonetic [hænd]. In the face of this evidence, no linguist would conclude that vowel nasalization is distinctive in English, and that “can’t” – “cat” constitute a minimal pair, while “can’t” – “canned” do not. Rather, in such a case, the linearity condition would be disregarded. Furthermore, there can be no doubt that this decision is correct. The phonetic representation can be derived from the phonemic, in this case, by the phonetic rules (34), ordered as given:

Vowel → Nasalized in the context: Nasal Consonant (34i)
Nasal → Ø in the context: Lax Vowel – Unvoiced Stop (34ii)

Though perfectly general and straightforward, these rules happen to lead to a violation of the linearity condition.

A second and more extreme example of the violation of linearity is the case of the a-a’ contrast, discussed above (p. 85, Section 4.2). The rules (28), (29), applied in this order, convert the systematic phonemic representations of row (I) of (35) first to row (II) and then to the systematic phonetic representation of row (III):

rayDfr rayDr ("writer," “rider,” respectively) 35(I)
rayDr ra-yDfr (by (28)) (II)
rayDr ra-yDfr (by (29), etc.) (III)

But here words which differ phonemically only in their fourth segments differ phonetically only in their second segments. Hence if phonemic representation is to play any significant role in linguistic description (if it is to be part of a grammar that achieves descriptive adequacy), the linearity condition must be rather grossly violated.

These violations of the linearity condition incidentally show, in yet another way, the incorrectness of the claim that phonology can (or, even more unaccountably, that it must) be based on synonymy, in its usual formulation to the effect that phonetically similar sounds are not assigned to the same phoneme if and only if replacement of one by the other in some context leads to a change of meaning (cf., e.g., Diderichsen, 1949). If what is meant by “context” is “phonetic context,” then the criterion would give the result that V-V and a-a’ constitute a phonological opposition (contrast) in English. If what is meant is “phonemic context,” then obviously the question at issue is simply being begged. In general, it should be observed that “minimal pair” is not an elementary notion. It cannot be defined in phonetic terms, but only in terms of a completed phonemic analysis. Consequently, the “commutation test” is of only marginal interest if formulated, in the usual manner, as a procedure for phonemic analysis.

Such violations of the linearity condition have not gone unnoticed by careful taxonomic phonologists, and it is instructive to consider the steps that have been taken to meet them. Troubetzkoy gives an example quite analogous to (34) both in the Anleitung and the Grundzüge (1939, 46). He observes that the following phonological rules operate in Russian:

o → Ø in the context: Vowel – Nasal Consonant (36i)
1 → Ø in the context: Lax Vowel – Unvoiced Stop (36ii)

Thus phonemic /sônca/ ("sun") and there is no necessity to set up /0/ in contrast to /0/ as a new phoneme. Here the linearity condition is violated, as in (34); and, furthermore, the rules must be ordered as given. To account for such violations of linearity, Troubetzkoy proposes a general rule for phonemicization which we can state as follows:

If the phone A is phonetically similar to the phone sequence BC, and A-BC are in free variation or complementary distribution, and BC is a realization of the phoneme sequence PQ, then A is to be regarded as a realization of PQ.

Thus [g] is phonetically similar to and in complementary distribution with [ol], which is a realization of /ol/; thus [g] is a realization of /ol/. Similarly, nasalized vowels, in some English dialects, are in complementary distribution with Vowel + Nasal, and could thus be regarded as a realization of Vowel + Nasal, thus dealing with the violation of linearity caused by (34), in these dialects. Similarly, one might use the same argument to justify representing intervocalic and word final English /g/ as /ŋ/ (though to apply the argument in this case, complementary distribution...
would have to be defined in terms of phonemically specified, rather than phonetically
specified contexts).

However, the rule (37) seems to me not at all satisfying. It is entirely *ad hoc,* and
it can only be taken as indicating that the definition of the phoneme as a minimal
term of a phonological opposition is incorrect. More seriously, it cannot be applied in
general, without absurdity. Thus, in English, the pairs [ŋ]-[ny], [y]-[y] are
phonetically similar and in complementary distribution, but it would be absurd, following
the rule, to phonemicize [kitn] ("kitten") as /kitny/ or /yatl/ ("yacht") as /yatl/.
Even more serious for the taxonomic phonemicist is the fact that this rule can lead to
a violation of biuniqueness. Thus consider the English [l]-[l] contrast ("write" –
"ride"), discussed above. [iy] appears only initially or after a consonant, and before
an unvoiced consonant; [y] can never appear in this position. Since [y] and [iy] are
phonetically similar and [iy] is a realization of /ay/, by Troubetzkoy’s rule, [y] is
a realization of /ay/. Aside from the absurdity, this leads to a violation of biuniqueness,
in this case, since /y/ and /ay/ contrast ("ion" /yjan/ – "yon" /yan/). Hence aside from
being *ad hoc,* this rule cannot be regarded as an extension of the notion “phoneme”
to deal with the case of violation of linearity.

Troubetzkoy’s informal comments and discussion of examples indicate that the
rule, as he stated it, perhaps does not conform to his actual intentions. Suppose, in
fact, that we were to restrict application of the rule (37) to the case in which B is a
Lax Vowel and C a Liquid or Nasal. Then the violations of linearity in the Russian
element (36) and the English example (34) (but not the example of English /ng/) would
still be handled, while the counterexamples of the preceding paragraph would be
ruled out. But now the entirely *ad hoc* character of the rule becomes even
more clear, and surely with such a restrictive formulation as this no one would
seriously regard it as constituting part of the definition of the fundamental concept
“phoneme.” Furthermore, it is still not difficult to construct counterexamples. Thus
in many American dialects, [e] of “get” is in complementary distribution with [er] of
“berry,” which is a realization of /ar/; so that the rule, even as amended, [e] must
be regarded as a realization of /er/, and “get” must be phonemized /er/.

The rule (37) is a typical example of an *ad hoc* device invented to remedy an inadequacy
of some general notion of “taxonomic phoneme,” and this discussion of
difficulties that it faces could be duplicated for other principles of this sort. These
*ad hoc* revisions of a basically inadequate notion do not succeed in reaching the
central issue. In such cases as those discussed above, it is clear that the acceptability
of an analysis hinges on its effect on the grammar as a whole. Thus the rules (34i)
and (34ii) are quite general and are independently motivated. A grammar that incorpo-
rates them is materially simpler than one that does not. But the rules: /yu/ → [y]
before Vowels, or /er/ → [e] before Consonants, as in the absurd examples given
above, obviously do not simplify the grammar of English. Similarly, Troubetzkoy’s
Russian example is well motivated by general systematic considerations; e.g., by the
existence of such forms as /sölëniʃiʃ, /sölëniʃiʃ/, and by the fact that were (36) not
incorporated in the grammar, then each occurrence of /o/ in the lexicon would have
to be marked as distinct from /o/, greatly complicating the grammar (cf. note 37).
Similarly, the necessity of assigning English [ŋ] to /n/ (more accurately, to the archi-
phoneme Nasal) becomes obvious only when the full range of examples involving

Nasal + Stop in various syntactic positions comes under investigation. The fact that
considerations of this sort are crucial suggests that any such “atomistic” rule as the
one that Troubetzkoy suggests will fail.

General systematic considerations are, however, foreign to the point of view of
taxonomic phonemics, and, in fact, they have often been criticized as circular (cf.,
e.g., Twaddell, 1935, 66). This criticism is correct, given the general “procedural”
bias of modern phonology; but it shows only that the attempt to develop a taxo-
nomic phonemics on the basis of analytic procedures of segmentation and classifica-
tion, supplemented by such *ad hoc* rules as (37), is ill-conceived from the start.

The more extreme case of violation of linearity posed by “writer” – “rider” (which
is beyond the range of (37) or any modification of it) is discussed by Harris (1951a),
who proposes that [a] be assigned to /ay/ as a unit, and [a]-[y] to /ayd/ as a unit,
on general grounds of symmetry of distribution. But this is a rather vague
statement, which somehow a criterion of distributional symmetry can be formulated that has just
the desired effect in this case. This result would still seem to be accidental and beside
the narrow scope of taxonomic phonemics. Finally, notice that Harris’
proposal appears to involve an inconsistency with respect to the notion “distribution.” Phonemes are to be defined in purely distributional terms. If the distribution
is with respect to phonetic context, then the definition of “phoneme” is violated by
his assignment of [a] and [a] to /a/, since these phones contrast in the phonetic context [— D]. If the distribution is with respect to phonemic context (an assumption
difficult to reconcile with a procedural approach, as noted above), then the
definition is violated by the assignment of [Y] to /y/ or /d/, depending on the phonetic
context in this case.

It seems to me, then, that the *ad hoc* devices for dealing with the violations of
linearity are not defendable, and that the definition of a phoneme as “a bundle of
phonetic distinctive features,” “a class of phones in free variation or complemen-
tary distribution,” or “a minimal term in a phonological opposition” can be
maintained only if we are willing to tolerate such absurdities as the phonemic
representations /kitn/, /rayDir/, /ra-yDir/ for “can’t,” “writer,” “rider,” and so on.
in many other cases.

Consider now the invariance condition. Notice first that it fails in the case of viola-
tions of linearity such as those discussed above. However, it seems to me untenable
when linearity is preserved. Phonemic overlapping provides the clearest example of
“throw” and of /t/ in “Betty” (where it contrasts with the /t/ of “berry” – cf. Bloch,
in 1941). Following the principle of invariance, we must assign [D] to /t/ in the context
# 69, contrary not only to the speaker’s intuition but also to the otherwise
valid rules of consonant distribution. The situation is worse in dialects in which [D]
and [r] are in free variation in this context and in intervocalic contrast, in which case no coherent solution is possible within the framework of (32), although the description of the facts is perfectly straightforward. The situation is still worse if we accept the absolute invariance condition, particularly if (as in Bloch, 1950) the features ("qualities") are defined in auditory terms. For it is known that in this case, not even the correct analysis of English stops is tenable, since /p/, /t/ and /k/ overlap (Schatz, 1953). For reasons such as these, then, it seems that the invariance condition cannot be accepted, however the condition of linearity is treated.

The biuniqueness condition is difficult to discuss because of the unclarity of formulation noted above. Nevertheless, certain consequences of accepting it are clear, and it seems to me that these are quite devastating, for anyone concerned with descriptive adequacy. Halle has pointed out that it is generally impossible to provide a level of representation meeting the biuniqueness condition without destroying the generality of rules, when the sound system has an asymmetry. Thus he gives the following, quite typical example from Russian (Halle, 1959b). In (38) the four forms in column I are given in systematic phonemic representation and in column III in systematic phonetic representation:

<table>
<thead>
<tr>
<th>I</th>
<th>II</th>
<th>III</th>
</tr>
</thead>
<tbody>
<tr>
<td>d'at, l, i</td>
<td>d'at, l, i</td>
<td>d'at, l, i</td>
</tr>
<tr>
<td>d'at, bi</td>
<td>d'ad, bi</td>
<td>d'ad, bi</td>
</tr>
<tr>
<td>z'êl, i</td>
<td>z'êl, i</td>
<td>z'êl, i</td>
</tr>
<tr>
<td>z'êbi</td>
<td>z'êbi</td>
<td>z'êbi</td>
</tr>
</tbody>
</table>

The forms of column III are produced from those of column I by the general rule:

Obstruent → Voiced in the context; − Voiced Obstruent.

But the representations in column I fail the condition of biuniqueness as usually construed (in terms of local determinacy), and consequently would not be accepted as taxonomic phonemic. The representations in column II would be accepted as "phonemic" by taxonomic phonologists, because of the fact that t,d, contrast, while ū-j do not. But if the grammar is to provide II as a level of representation, then it cannot incorporate the general rule (39), but must have in its place the two rules (40) and (40ii), the first of which is taken as a rule relating "morphophonemic" to "phonemic" representation, and the second as relating "phonemic" to phonetic representation:

Obstruent → Voiced in the context; − Voiced Obstruent, except for c, ě, x;  
(c, ě, x → Voiced in the context; − Voiced Obstruent. (40i)  
(c, ě, x → Voiced in the context; − Voiced Obstruent. (40ii)

It seems to me that the force of this example has not been sufficiently appreciated by taxonomic phonemicists. Where it has been noted at all, the discussion has not been adequate. Ferguson, in his review (1962) of Halle (1959b), discusses not the example given in the book under review (and reproduced above), but instead a Turkish example that had at first been proposed by Lees as analogous to Halle's, and then withdrawn by Lees as inappropriate (Lees, 1961, p. 63). Insofar as Ferguson's discussion carries over to the correct example that Halle gives, it amounts only to the observation that from the phonetic record alone it is possible to determine the underlying systematic phonemic (in his terms, morphophonemic) form in the case of c, ě, x, but not in the case of the other obstruents. This is correct but irrelevant, since this information is provided just as explicitly in the grammar which incorporates only systematic phonemics and systematic phonetics as in the grammar which, in addition, adds an intermediate level of taxonomic phonemics. Thus the fact remains that in this case, the only effect of assuming that there is a taxonomic phonemic level is to make it impossible to state the generalization.

In the face of Halle's example, I do not see how one can fail to be uncomfortable in attributing to Russian a level of taxonomic phonemics. Furthermore, similar examples are not difficult to find in other languages. Bloch, in fact, gave a rather similar example in his discussion of phonemic overlapping (Bloch, 1941). In his dialect of English there are forms that might have the systematic phonemic representations of column I and the systematic phonetic representations of column III of (41):

<table>
<thead>
<tr>
<th>I</th>
<th>II</th>
<th>III</th>
</tr>
</thead>
<tbody>
<tr>
<td>&quot;nod&quot;: nad na·d na·d</td>
<td></td>
<td></td>
</tr>
<tr>
<td>&quot;knot&quot;: nat nat nat</td>
<td></td>
<td></td>
</tr>
<tr>
<td>&quot;bed&quot;: bed bed be·d</td>
<td></td>
<td></td>
</tr>
<tr>
<td>&quot;bet&quot;: bet bet bet</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Column I does not meet the biuniqueness condition because of such contrasts as "balm" - "bomb," "stary" - "sorry," "father" - "bother," and because of the fact that the vowel of "Pa(d) (do it)" is that of "pod," phonetically. Column III can be derived from column I by the familiar rule of lengthening before voiced segments (of which (28) is a special case). But Bloch is forced, by the biuniqueness condition, to accept II as the phonemic level of representation. Thus a full grammar of English, meeting this condition, would have to replace the general rule of vowel lengthening by two rules, the first of which applies only to /a/ and the second to all other vowels. The first would relate "morphophonemic" and "phonemic," and the second "phonemic" and phonetic representations. The situation is exactly analogous to the Russian example just given, and again we see that the effect of the biuniqueness condition is to complicate the grammar, that is, to prevent it from achieving descriptive adequacy.

The complicating effect of the biuniqueness condition has been commented on by several of its proponents. Thus Bloch remarks at once, in discussing the preceding example, that it leads to a loss of symmetry. Similarly, he remarks (1950, note 3) that the National Romanization which influenced his earlier, nonbiunique analysis...
of Japanese, though "neat and systematic," is not as close to a "phonemic notation" as the Hepburn Romanization, "unsystematic and cumbersome as it seems to be." Similarly, Hockett (1951) compares Bloch's "deceptively simple" nonbiunique analysis with his later "quite complicated ... but obviously more accurate" taxonomic phonemic analysis. In fact, however, the "greater accuracy" of the latter seems to reside in nothing other than its observance of conditions (321-IV). We return below to the question of why this is regarded as a sign of greater accuracy.

We have, as yet, said nothing about the principle of complementary distribution, which is the central concept of taxonomic phonemics as developed, for example, by Jones, Troubetzkoy, Harris, and Bloch. This principle is, basically, the principle of biuniqueness converted into a procedure. Regarded as an analytic procedure, its goal is to provide the minimally redundant representation meeting the conditions of biuniqueness and local determinacy. We will show, however, that it is in general incapable of providing the minimally redundant analysis meeting these conditions, and furthermore, that it may even lead to a nonbiunique analysis.

We can formulate the principle in this way (following Harris, 1951a, chapter 7): Given a set of representations in terms of phones, let us define the distribution D(x) of the phone x as the set of (short-range) phonetic contexts in which x occurs. The relation of complementary distribution holds between phones x and y if D(x) and D(y) have no element in common. A tentative phoneme is a class of phones related pair-wise by the relation of complementary distribution. Some would require further that a defining phonetic property be associated with each tentative phoneme, marking each of its members and no other phone (the invariance condition).

A tentative phonemic system is a family of tentative phonemes meeting a condition of exhaustiveness. We find the phonemic system (or systems) by applying additional criteria of symmetry.

But consider the example of phonemic overlapping due to Bloch that was discussed above, namely, the case of a dialect with [D] as the realization of /t/ in "throw" and of /t/ in "Betty," where it contrasts with the [r] of "berry." The requirement of biuniqueness is preserved if we set up the phonemes /t/, with the allophone [D] in intervocalic, post-stress position, and /r/, with the allophone [D] after dental spirants. Given a phone in a phonetic context, we can now uniquely assign it to a phoneme; and given a phoneme in a phonemic context we can uniquely determine its phonetic realization (up to free variation). However, this solution, which is the only reasonable one (and the one Bloch accepted in his 1941 paper), is inconsistent with the principle of complementary distribution. In fact, the allophones [D] and [r] of /t/ are not in complementary distribution, since they both occur in the context [be-iy] ("Betty," "berry"). Hence complementary distribution is not a necessary condition for biuniqueness. Furthermore, the class of "tentative phonemic systems" as defined in the preceding paragraph will not include the optimal biunique system as a member, so that no supplementary criteria will suffice to select it from this class.

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This would be required by Troubetzkoy, Jakobson, and Bloch, but not by Harris (cf. 1951a, 72, note 28). He maintains that "any grouping of complementary segments may be called phonemic," and that further criteria have to do only with convenience, not with linguistic fact.

But now observe further that the class of tentative phonemic systems, as defined, will contain systems that fail the principle of biuniqueness. Thus, for example, [k] and [r] will be in complementary distribution in English (and, furthermore, share features shared by nothing else, e.g., in Jakobson's terms, the features Compact, Grave, Lax, Non-Flat). Hence they qualify as a tentative phoneme, and there is a tentative phonemic system in which they are identified as members of the same phoneme /K/. But in this phonemic system, "socked" [sakt] and "Scot" [skat] will both be represented phonemically as [sKt]. Similarly, [s] and [r] are in complementary distribution (and share defining features) and thus qualify as a potential phoneme. But if they are identified as variants of /R/, we will have "prevail" /pRRvśl/ [prevyel], "pervade" /pRvśd/ [pervyd], which is a violation of local determinacy, and of biuniqueness as generally construed. Consequently the principle of complementary distribution does not even provide a sufficient condition for biuniqueness. Since it provides neither a necessary nor a sufficient condition for biuniqueness, and, apparently, has no motivation except for its connection with biuniqueness, the principle of complementary distribution appears to be devoid of theoretical significance.

Related questions have been discussed by taxonomic phonemicists, but the general problem has apparently escaped attention. Troubetzkoy considers the example of English /r/ and /s/, and gives a rule (1935, Rule IV; 1939, Rule IV) that would prevent them from being assigned to the same phoneme in case the sequence [sr] is in contrast with [s]. This rule, as formulated, is not pertinent to the problem of preserving biuniqueness, and does not cover either of the examples of the preceding paragraph. It is, furthermore, entirely ad hoc, and thus simply serves to indicate a theoretical inadequacy of taxonomic phonemics.

Apparently only Harris has considered a special case of this problem explicitly. He points out (1951a, 62, note 10) that we might have phonetic representations [tRAY], [kRAY] for "try," "cry," where /-k/ and /-r/ are in complementary distribution. But if we were to set up a tentative phonemic system in the manner described above, we could have a phoneme /T/ with allophones [T] before [r] and [k] before [r], and a phoneme /R/ with allophones [R], [R]. But now both "try" and "cry" would be represented /TRAY/. To avoid this, Harris suggests that we first group [s] and [r] into /t/, and then redefine distributions in terms of the newly specified contexts, in which [s] and [k] now contrast before /t/. This procedure will avoid the difficulty in the particular case of "try," "cry," but not in the cases described above. Furthermore, the same procedure could just as well be used to group [s] and [k] into /T/, keeping [s] and [r] phonemically distinct (in further justification, we could point out that this regularizes distributions, since now /t/ occurs neither before /r/ or /l/, instead of, asymmetrically, only before /s/). Hence, as in the case of the procedures discussed above, it fails to distinguish permissible from impermissible applications. Finally, the procedure as stated is inconsistent with Harris' general requirement on the set of linguistic procedures (1951a, 7), namely, that operations must be "carried out for all the elements simultaneously" without any "arbitrary point of departure." In fact, this requirement was what made it possible for Harris to avoid Bloomfield's use of descriptive order (cf. note 35, above). It is violated by the procedure just discussed.

4.4 Criteria for systematic phonemics. Systematic phonemics in the sense of
part: (68). There is no necessity for demonstrating "psychological reality"
Harris seems to take a similar position in his
laboratory generalizations will not be expressible and descriptive adequacy cannot be achieved.
whether or not this is true, we have now, in Section 4.3, accumulated evidence showing that if a level meeting the conditions associated with taxonomic phonemics is incorporated in a grammar, then many generalizations will not be expressible and descriptive adequacy cannot be achieved. It is important, then, to see whether there is some way of justifying the assumption that a level of taxonomic phonemics actually constitutes a part of linguistic structure.

4.5 The motivation for taxonomic phonemics. We are now concerned with the question: Why should it be assumed that a grammar must generate representations meeting the conditions (32), as part of the structural descriptions of utterances? What, in other words, is the justification for the theory of taxonomic phonemics, in any of its modern varieties?

Many linguists would perhaps take a position of the sort expressed by Twaddell (1935). In opposition to the "mentalistic" approach of Sapir (that is, the approach that is concerned with descriptive and explanatory adequacy), he proposes a method of phonemic analysis for which the following is "the only defense that may be offered": "this procedure . . . appears to be characterized by a minimum of the
in this sense. In any event, there is surely no reason why the linguist must necessarily limit himself to "the study of phenomena and their correlations," avoiding any attempt to gain insight into such data by means of an explanatory theory of language, a theory which, of course, "mentalistic," in that it deals with the character of mental activity rather than with its physical basis.

If one is unwilling to settle for just consistency and convertibility, what further justification can be offered for taxonomic phonemics? I have tried to show above that the internal linguistic evidence does not support taxonomic phonemics. Taxonomic phonemic representations do not contribute to the simplicity or generality of a grammar, but, in fact, have just the opposite effect. Therefore one must search for external evidence. In particular, it is important to ask whether reasonable requirements for a perceptual model (lbs of Section 1.3) or a learning or discovery model (lbs of Section 1.3) have any bearing on the validity of taxonomic phonemics. Considerations of this sort may actually have been at the core of some theoretical and methodological studies.

One might try to justify the conditions (32) by arguing that speech perception involves two successive stages: the hearer first uses only local phonetic cues to identify the invariant criterial attributes that determine the successive taxonomic phonemes; and he then goes on to determine the deeper structure of the utterance (in particular, its systematic phonemic representation and its syntactic structure). This clearly seems to be the view of Jakobson (cf. Jakobson, Fant and Halle, 1952) and of Joos (1957, 92), among others. However, there is no real basis for this account, and it is scarcely in accord with what little is known about complex perceptual processes, or, for that matter, about speech perception. Thus it is well known that intelligibility is preserved under gross phonetic distortion, which may be completely unnoticed when grammatical constraints are met; and brief exposure to an unfamiliar dialect is often sufficient to overcome unintelligibility or even an impression of strangeness (note that related dialects may differ greatly, sentence by sentence, in phonetic and taxonomic phonemic representations, though perhaps hardly at all on the level of systematic phonemics—cf. in this connection Halle, 1962; also Chomsky, 1959, for an analysis of some of the data presented by Sledd, 1955, 1958, from this point of view). Sapir is the only linguist to have presented careful observations of native perceptual responses relevant to this question, in his classic paper on psychological reality (1933), and his reports are directly counter to the taxonomic account of speech perception. Surely one would expect that in identifying an utterance, the hearer will bring to bear the full grammatical apparatus that determines the space of possibilities from which this utterance is drawn and the nature and interrelations of these objects. That is, one would naturally expect that, as in the case of other perceptual processes, the hearer's knowledge will provide a complex schema within which the actual signal is interpreted. To the extent that this is true, the "atomatic" view of the taxonomic phonologists will be in error. In any event, presently available evidence does not support the taxonomic model given above as an adequate general account of speech perception.

It remains to consider the status of taxonomic phonemics with respect to a model of acquisition of language. There is, in fact, an approach to the question on these grounds.

Suppose that we impose on the acquisition model the condition of separation of levels, which we can interpret as requiring that the level of systematic phonetic representation must be "rationalized" and converted to a level of taxonomic phonemic representation without reference to any morphological or syntactic information. Observe that this condition is not to be confused with the conditions of biuniqueness and local determinacy. These (as all of the conditions (32)) pertain to the "perceptual model"; they assert that the phonemic correspondent to a given phonetic sequence must be determinable by operations involving only neighboring sounds, once the phonemic system is fixed. But the condition of separation of levels is not a formal condition on a phonemic system and on the rules that relate it to sound; it is a methodological condition on information relevant to determining the correct choice of a phonemic system. It thus pertains to an acquisition model such as (lb), rather than to a perceptual model such as (la).

Nevertheless, there is a connection between the condition of separation of levels and the conditions of biuniqueness and local determinacy. If no higher-level information is relevant to determining what is the taxonomic phonemic system, it is natural to require that once the taxonomic phonemic system is fixed, on purely phonetic grounds, no higher-level information should be relevant to determining what is the sequence of taxonomic phonemes corresponding to a given sequence of phones. Consequently, an argument in support of the condition of separation of levels would, indirectly, provide a motivation for imposing the conditions of biuniqueness and local determinacy on the perceptual model as formal conditions on the notion "phoneme."

This is apparently the line of reasoning that has been followed insofar as justification for the conditions of biuniqueness and local determinacy has actually been provided. Thus, for example, Hockett gives only one argument in support of these conditions in the review cited above (Hockett, 1951), namely, that given these conditions "one knows definitely to what level each fact applies." Otherwise, we have a "hodge-podge arrangement." He is concerned here with the context of discovery, not perception, and is offering an argument in support of the condition of separation of levels rather than in support of the biuniqueness and local determinacy conditions directly. Similarly, in his important paper on phonemic overlapping (1941), Bloch 51"For further discussion, see Halle and Stevens (1961), Miller and Chomsky (1963) and references there cited. For discussion in a similar vein on the syntactic level, see Matthews (1961).

52One or another form of this is implicit in all substantive discussions of linguistic procedures that I have been able to locate. Some linguists (e.g., Pike and Harris) would allow restricted use of certain higher-level information in phonology, where this can be obtained by "cyclic" or "spiral" procedures (cf. Pike, 1947, 1952; Harris, 1951a), but many American linguists insist on strict separation. Glossemicists also mention successive and intricately interwoven procedures of analysis and synthesis (Diderichsen, 1958). The kinds of procedures they have in mind also allow for some sort of interdependence of levels, but the reference to procedures is too vague for the extent of permitted interdependence to be determinable, in this case.
offers only one argument (an argument that Joos, in his comments, 1957, considers conducive) to show why the biuniqueness condition must be maintained, namely, this: “Suppose that we are studying a new and unfamiliar dialect of English, and that we have succeeded in pairing the stressed and the unstressed vowels of such words as at, them, could, will, so, and the like; if we now hear a phrase like out of town, with the unstressed vowel of the second word perceptually the same as those which we have already identified with various stressed alternants, how are we to treat this? We must defer the phonemic analysis until we chance to hear a stressed form of the same word, which may not occur at the dialect we are studying, or which, if it does occur, we may fail to recognize as ‘the same word.’”

Both Bloch and Hockett are proposing that the condition of biuniqueness must be imposed on the notion “phoneme” because the model for acquisition must meet the condition of separation of levels. But it is important to observe that both of them are presenting an argument that is methodological rather than substantive. They do not suggest that an accurate model of the process of acquisition of language must incorporate the condition of separation of levels—that this is a fact about the design of language and about the intrinsic characteristics of an organism capable of learning a language under the empirically given conditions of time and access. They are considering rather the problems of gathering and organizing data, and thus their indirect argument for the conditions of biuniqueness and local determinacy at most shows that it would be convenient for the linguist if there were a level of representation meeting these conditions, but it does not bear on the question of the existence of this level as a part of linguistic structure.

Let us turn to the question of separation of levels as a substantive issue. As in the case of the conditions (32), two kinds of considerations are relevant: external considerations pertaining, in this case, to language acquisition rather than perception; and purely internal linguistic considerations. As to the former, Hockett has in fact suggested in various places (e.g., 1948) that the successive steps of the analyst should in some way parallel those of the language learner. But clearly the child does not master the phonology before proceeding to the syntax, and there is no possible justification for the principle of separation of levels from considerations of this sort.

It remains then to ask whether this condition can be justified (thus indirectly providing a justification for the biuniqueness and local determinacy conditions) on internal linguistic grounds, that is, by a demonstration that it contributes to the clarity, generality, or coherence of a full grammar. But it seems clear that this principle has rather the effect of detracting significantly from these qualities, and, in fact, that adherence to this principle makes it impossible to attain the levels of descriptive or explanatory adequacy. Consequently, the principle seems to be entirely superfluous, in either its stronger or weaker forms (see note 51).

The effects of strict application of a principle of separation of levels have often been discussed. The matter of word boundary that Hockett cites in his invented example discussed above illustrates the problems that arise when it is adopted. It has long been recognized that a phonemic system is quite unacceptable if no junctures are recognized. Consequently, linguists who adopt the principle of partial or complete separation of levels have attempted to devise analytic procedures that would make it possible to place junctures in appropriate places on the basis of phonetic evidence alone. These procedures make use of phonetic features that appear at utterance boundary to determine the position of junctures medially in utterances. Thus a juncture would be marked in “night rate” because it contains an utterance-final allophone of /t/ followed by an utterance-initial allophone of /r/. Apart from the counterexamples that have already been offered to this principle (and that remain unanswered—e.g., Leopold, 1948; Harris, 1951a, 87; Chomsky, Halle, Lukoff, 1956, Section 2) it is clear that it cannot succeed because of examples of the following kind. In many dialects of English, /t/ has the allophone [D] in word final position after a weak stress and before a main stress—thus we have [iADetj] (“at Ed’s”), [<iADetj] (“at our”), [<iADetj] (“that at”), contrasting with [iADetj] (“attend”) [ltrek] (“attack,” “a tack”) and with [iADetj] (“adapt”). But [D] occurs only medially, never finally. Thus any consideration involving utterance boundary will place junctures in exactly the wrong places. Alternatively, if no junctures are placed, [D] must be taken as a third alveolar stop, giving an equally unacceptable phonemic analysis. We must conclude, then, that there is no known method for assigning junctures in terms of phonetic evidence alone. Present methods do not distinguish permissible from impermissible applications, and, consequently, are useless as they stand. It seems unlikely that this difficulty can be remedied, and unless it is, the principle of separation of levels is entirely untenable.

As a second example, consider the much debated subject of English vocalic nuclei. According to a view that is widely held among American structuralists, these are to be analyzed as short vowels plus one of the glides /r/, /w/ or /h/. On the purely phonetic grounds on which the question must be discussed by those who accept the principle of separation of levels, this is a very neat and well-motivated description. In particular, the post-vocalic /h/, representing a centering glide, can be used to account for such contrasts as “real” /rihl/, “really” /rihli/, versus “reel” /riyl/, “Greeley” /grilik/, etc.

If, however, we are concerned with selecting a phonemic system that will be compatible with a fully descriptively adequate grammar, this analysis becomes quite unacceptable. Thus observe that on the level of systematic phonemics, the words “real,” “really” will be represented /riel/, /riel + i/ (because of “reality”), just as “total,” “totally” are represented /tɔtʌl/, /tɔtʌl + i/ because of “totality,” and “mobile” is represented /mɔbil/ because of “mobility.” Furthermore, the glide of “real,” “really” is not to be distinguished on the level of systematic phonetics from that of “total,” “totally,” “mobile” (or, for that matter, “dial,” “betrayal,” “refusal,” “science,” etc.), namely, [i]. Hence in all of these cases the systematic phonetic representation can be derived from the systematic phonemic by the very general rule of English phonology that:

\[ \text{Vowel} \rightarrow \text{i when unstressed} \]

52For an account of its background, see Gleason (1961, chapter 19). An important critique is presented in Siedd (1955).

53This rule is of course incorrect as stated (cf. “relaxation” [rikakesa’i], “condensation” [kan’daensai], etc.) if it is one of a set of unordered rules of a taxonomic grammar. But it is correct if it is embedded into a transformational cycle of the kind discussed above. Cf. references of note 6 for details.
If, however, we wish to provide the taxonomic phonemic representations /rihl/, /rihyl/, /towili/, /towilily/, /mowibil/, /daylkit/, /bytreyil/, etc., as an intermediate stage of formal description, we must replace the general rule (42) by the three rules:

\[
\begin{align*}
\text{Vowels} & \rightarrow i \text{ post-consonantally, when unstressed} \\
\text{Vowels} & \rightarrow h \text{ post-vocally, when unstressed} \\
\text{h} & \rightarrow i \text{ post-vocally, }
\end{align*}
\]

where the first two relate "morphophonemic" and "phonemic" representations, and the third relates "phonemic" and phonetic representations. Thus again we find that what may very well be the optimal taxonomic phonemic system is not incorporable into a descriptively adequate grammar. The failure to achieve descriptive adequacy in this case, is traceable to the requirement of separation of levels in the underlying theory.

In his review of Halle (1959b), Ferguson (1962) criticizes Halle for his rejection of the biuniqueness and local determinacy conditions (condition (3a) in Halle's presentation), and offers a defense of these conditions. But he presents the issue incorrectly, and as a result neither his critique of Halle's position nor his arguments in support of biuniqueness and local determinacy are to the point. Since Ferguson's is the only recent discussion of this issue from the point of view of taxonomic phonemics, it is important to trace the argument with some care. Ferguson argues for what he calls "the autonomy of phonology," that is, the view that phonology is entirely independent of syntax and morphology, and that biuniqueness and local determinacy conditions are thus reasonable. Halle's position—and the one that I have advocated here—is the direct contradictory of this, namely, the view that some phonetic processes depend on syntactic and morphological structure so that phonology as a whole cannot be studied, without distortion, in total independence of higher level structure. Let us call this the view that phonology is "nonautonomous." A third possible position we may call the assumption of "inseparability of phonology," that is, the view that all phonetic processes depend essentially on syntactic and morphological structure. This view has certainly never been advocated by anyone, and it is unnecessary to refute it. But it is the assumption of inseparability of phonology, not the assumption of nonautonomy of phonology, that Ferguson imputes to Halle, and against which he presents a series of arguments (to which we return directly). These arguments against the inseparability of phonology have no bearing on the question of autonomy of phonology. This failure to observe the distinction between inseparability of phonology and nonautonomy of phonology in fact vitiates Ferguson's argument entirely.

Specifically, Ferguson cites in favor of his position the undeniable fact that syntactic and morphological structure are not involved in certain sound changes and in certain aspects of language learning and dialectal variation. This observation is irrelevant to the issue of autonomy or nonautonomy of phonology (though it successfully demolishes the absurd thesis of inseparability of phonology). It is also apparent that morphology and syntax play an important role in specifying the range and character of certain sound changes (cf. much of Kuryłowicz' recent work, or e.g., Twaddell, 1935, p. 79), of certain aspects of phonological development in language learning, and of certain aspects of phonological dialectal variation. Consequently, to the extent that considerations of the sort that Ferguson adduces are relevant, they show nothing more than the untenability of the thesis of autonomy of phonology. It is true that in plotting isoglosses, "it is often quite clear that subareas of different phonological systems do not coincide well with subareas of grammatical systems and lexical inventories" (Ferguson, 290), just as it is clear that isoglosses drawn for vocalic systems often do not coincide with those drawn for consonantal systems. The argument from this to autonomy is equally apposite in both cases. Similarly, in the case of Ferguson's other examples.

Finally, I should like to comment on Ferguson's assertion that Halle's theory (as also the theory of the present paper) does not provide machinery for describing phonetic data that is accounted for adequately by his autonomous phonology. He cites, e.g., the word *Audrey* with the cluster /dr/ as compared with *bedrock* with /d + r/ and *bedroom* with variation between /dr/ and /d + r/. In this case, a "nonautonomous" generative grammar would give rules stating that in *bedroom* the morpheme boundary sometimes does and sometimes does not become a phonetic juncture (depending on dialect or style, as the facts indicate). It would, on the other hand, make no such statement about *Audrey* (with no boundary) or *bedrock* (where the boundary always becomes phonetic juncture). I do not see what is the problem here, or how an autonomous phonology of type that Ferguson proposes would handle the situation any differently. Ferguson's example simply shows the absurdity of the claim that every morphemic boundary is a phonetic juncture, but surely no one has ever maintained this. What has been maintained is that syntactic and morphological considerations must be taken into account in determining when to handle phonetic facts by placement of junctures, and when to handle them by postulation of new phonemes, and Ferguson's remarks have no bearing on this question.

Summarizing, then, it seems that if we are concerned with descriptive and explanatory adequacy, only two levels of representation can be justified in structural descriptions provided by the phonological component, namely, the levels of systematic phonemics and systematic phonetics. The level of taxonomic phonemics is not incorporable into a descriptively adequate grammar. As noted in Section 4.2, this conclusion is close to the position of de Saussure and Sapir, and is close to Bloomfield's practice, though perhaps not his theory.

It is interesting to consider the kinds of criticism that have been offered by taxonomic linguists against de Saussure, Sapir, and Bloomfield. Wells (1947) criticizes de Saussure for not making use of the principle of complementary distribution with respect to a particular language in his "phonologie" (but only the analogous principle with respect to all languages). In his long review of Sapir's collected papers (1951), Harris devotes very little attention to Sapir's fundamental theoretical papers on phonology (Sapir, 1925; 1933), and remarks only (293) that they confuse
V. MODELS OF PERCEPTION AND ACQUISITION

A concern with perception and acquisition of language has played a significant role in determining the course of development of linguistic theory, as it should if this theory is ever to have broader scientific significance. But I have tried to show that the basic point of view regarding both perception and acquisition has been much too particularistic and concrete. It has failed totally to come to grips with the “creative” aspect of language use, that is, the ability to form and understand previously unheard sentences. It has, in general, failed to appreciate the degree of internal organization and the intricacy of the system of abstract structures that has been mastered by the learner, and that is brought to bear in understanding, or even identifying utterances. With regard to perceptual models, these limitations reveal themselves in such conditions as linearity, invariance, and biuniqueness; with regard to models of acquisition, in such methodological conditions as the principle of separation of levels, the attempt to define grammatical relations in terms of co-occurrence, and, in general, in the emphasis on elementary procedures of segmentation and classification that has dominated modern linguistic theory.55

These taxonomic models of acquisition are not far removed from the extremely limited paradigms of learning and concept formation, based exclusively on some notion of matching or similarity or possession of a common property from some fixed set of available properties, that are to be found in recent cognitive psychology. But it does not seem plausible that the kind of generative grammar that seems to be descriptively adequate might be acquired in a reasonably brief time (if at all) by an organism that brings to the learning task only a “quality space” and a “distance measure” along these dimensions. Evidence of the kind discussed above suggests that each natural language is a simple and highly systematic realization of a complex and intricate underlying model, with highly special and unique properties. To the extent that this observation can be substantiated, it suggests that the structure of the grammar internalized by the learner may be, to a presently quite unexpected degree, a reflection of the general character of his learning capacity rather than the particular course of his experience. It seems not unlikely that the organism brings, as its contribution to acquisition of a particular language, a highly restrictive characterization of a class of generative systems (potential theories) from which the grammar of its language is selected on the basis of the presented linguistic data. There is no a priori reason to expect that these potential theories are of the highly simple taxonomic variety with which modern linguistics has been preoccupied, and the linguistic evidence seems to show, in fact, that they are not.

In the case of perception of language, as noted above in Section 4.4, the step-by-step analytic models of taxonomic linguistics are not in the least convincing. The process of coming to understand a presented utterance can be quite naturally described, in part, as a process of constructing an internal representation (a “percept”) of its full structural description. There is little reason to doubt that the full apparatus of the

55One might cite de Saussure as a source for this preoccupation with inventory and with taxonomic procedures. Cf. (1916, 154).
generative grammar that represents the hearer’s linguistic competence is brought to bear immediately in carrying out this task. In particular, much of the perceived phonetic shape of an utterance (e.g., in English, the complex arrangements of reduced and unreduced vowels and stress contours) is a reflection of its syntactic structure. It would not be surprising to find that what the hearer (or the phonetician) perceives is an ideal pattern, not incompatible with the signal that actually reaches his ears, that is projected by the phonological component of his grammar from the syntactic description that he has assigned to this signal (cf. references of note 50).

In part, these questions belong to theoretical psychology. But purely linguistic research can play a fundamental role in adding substance to these speculations. A perceptual model that does not incorporate a descriptively adequate generative grammar cannot be taken very seriously. Similarly, the construction of a model of acquisition (whether a model of learning, or a linguistic procedure for discovery of grammars) cannot be seriously undertaken without a clear understanding of the nature of the descriptively adequate grammars that it must provide as output, on the basis of primary linguistic data (cf. Section 1.3). It presupposes, in other words, a general linguistic theory that achieves the level of explanatory adequacy. It is clear that we have descriptively adequate grammars, and underlying theories that reach the level of explanatory adequacy, only for a rather narrow range of linguistic phenomena in a small number of languages. It seems to me that present theories of transformational generative grammar provide a basis for extending and deepening our understanding of linguistic structure. In any event, whether or not this hope is ultimately justified, it seems clear that to pursue the goals of Section I in any serious way, it is necessary to go far beyond the restricted framework of modern taxonomic linguistics and the narrowly-conceived empiricism from which it springs.

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**On the Notion “Rule of Grammar”**

Noam Chomsky

### 1 General desiderata for grammatical theory.

The traditional aim of a grammar is to specify the class of properly formed sentences and to assign to each what we may call a structural description that is, an account of the units of which the sentence is composed, the manner of their combination, the formal relations of the sentence to other sentences, and so on. If we hope to go beyond traditional grammar in some significant way, it is essential to give a precise formulation of the notion structural description of a sentence and a precise account of the manner in which structural descriptions are assigned to sentences by grammatical rules. The rules contained in a traditional grammar are of widely diversified kinds, and there is no clear indication of what is to be the exact nature of a structural description. Modern linguistics has devoted a great deal of attention to clarifying the latter question, but has not considered with any seriousness the notion grammatical rule. Intention to the process by which structural descriptions are generated and assigned to sentences leaves a serious gap in linguistic theory, however, and leaves open to serious doubt particular decisions about the inventory of elements in actual descriptive studies, since clearly such choices should not be independent of the complexity of the system of rules by which the structural description of each sentence is specified. In any event, it seems that a really insightful formulation of linguistic theory will have to begin by a determination of the kinds of permitted grammatical rules and an exact specification of their form and the manner in which they impose structural descriptions on each of an infinite set of grammatical sentences.

By a grammar of the language L I will mean a device of some sort (that is, a set of rules) that provides, at least, a complete specification of an infinite set of structural descriptions of sentences.