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Some controversial questions in phonological theory

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In the first issue of this journal, Fred W. Householder discussed two papers of ours[1] which he found defective in various respects. We feel that the issues involved are important and deserve the fullest clarification. We will therefore discuss Householder's objections and the underlying issues in some detail, reiterating points that have been made in the aforementioned papers and elsewhere and making no attempt to avoid redundancy if this can contribute to clarity.

In the two papers under discussion, we were concerned with the phonological component of a generative grammar. A generative grammar contains a

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[1] Delays in publication as well as other factors have led to what may be a rather confusing situation, with regard to the paper by Chomsky to which Householder refers. It appeared at approximately the same time in three different versions under two different titles. The *Preprints* paper (Chomsky, 1962) appears in the *Proceedings of the Ninth Congress* in a revised version submitted for publication in 1962; its title is that of the session at which it was presented (Chomsky, 1964*a*). A revised and expanded version was submitted for publication in 1963 and has now been published (Chomsky, 1964*b*) in Fodor and Katz (1964). A still later and further revised and expanded version was submitted for publication in late 1963 and has now appeared as a separate monograph (Chomsky, 1964*c*). We will give page references in a double form, referring to the *Proceedings* and the separate monograph. The latter page reference is only for the convenience of the reader; the various versions do not differ in any way relevant to this discussion.

Halle's paper (1962) also appears (Halle, 1964*b*) in a revised version in Fodor and Katz (1964). This anthology also contains a revised and expanded version of an earlier article (Halle, 1959*a*–1964*a*). For the convenience of the reader we will give page references to Halle's paper also in double form (1962 and 1964*b*).

system of rules that assign structural descriptions to the sentences of a particular language and thereby express a fundamental aspect of what the speaker of this language knows. The phonological component of a generative grammar relates the phonetic representation of an utterance to its syntactic structure. This syntactic structure consists of a string of formatives (minimal syntactically functioning elements) with a Phrase-marker determining the Immediate Constituent structure (the labelled bracketing) of this string. Applying the rules of the phonological component to a syntactic structure in a fixed and predetermined order, we derive the phonetic representation corresponding to this structure. In studying the organization and functioning of this system of rules, we found that we were able to isolate two levels of representation, corresponding to what Sapir called phonological and phonetic representation. We suggested the names SYSTEMATIC PHONEMIC and SYSTEMATIC PHONETIC for these levels of representation. The systematic phonemic representation includes in addition to purely phonological elements also information about the morphological and syntactic structure of the utterance. The rules that relate the systematic phonemic representation to sound are TRANSFORMATIONAL in that they take into account the Phrase-marker of the string. 'Higher-level' considerations play an important role in determining the choice of this system, which is selected in such a way as to contribute to an optimal generative grammar of the language. The systematic phonetic representation utilizes a universal phonetic alphabet, based on a theory of distinctive features. For this purpose, we have adopted with some modifications Jakobson's theory of distinctive features, since this seems to us to be by far the best theory of universal phonetics now available. We showed that for a wide range of linguistic data, which otherwise would have to be treated as isolated fact without systematic import, we could offer partial explanations if we consistently formulated all rules and representations (both systematic phonemic and systematic phonetic) strictly in terms of features. Systematic reliance on features thus permits a deepening of insight into the nature of linguistic competence and makes possible an analysis of the notion 'linguistically significant generalization', a notion which underlies all descriptive practice but has, so far, resisted clear and general formulation. We also discussed some of the consequences of this phonological theory for historical linguistics, and showed how certain otherwise knotty problems can be handled in what appears to be a natural and effective way.

In addition, we studied various theories of sound structure that have been developed since Sapir. We called these TAXONOMIC PHONEMIC THEORIES so as to bring out their reliance on procedures of segmentation and classification and their essential independence of 'higher levels'. Taxonomic phonemics, in one or another form, has been almost universally regarded as either supplanting phonology in Sapir's sense or as constituting a new and fundamental level

intermediate between systematic phonemics (often called 'morphophonemics') and systematic phonetics. We tried to show that neither claim is justified – that taxonomic phonemics is an artifact and does not constitute a level of linguistic structure. That is, a taxonomic phonemic representation simply cannot be incorporated into a fully explicit grammar that expresses phonological processes in full generality, and therefore cannot be justified on internal linguistic grounds. We also argued that it has no support on external grounds (of perception or language use or acquisition), and that the procedures of analysis that have been proposed (in particular, insofar as they rely on 'complementary distribution') are seriously defective and ineffective as analytic devices. Consequently, we concluded that taxonomic phonemics has no place in a grammar that describes the competence of the speaker or in a model of language use. Its sole justification is that it may allow sentences to be read or written in an economical notation by someone who does not understand the language; and, whatever the utility of such a system may be, there is no reason to assume that it plays any role in language use or has a place in grammar.

This, in brief, is the content of the articles that Householder is discussing. Our comments will refer directly to his discussion of these points, and are intended to be read in conjunction with his paper. We shall classify Householder's major criticisms under the following seven headings: (i) the goals of linguistic theory; (ii) evaluation procedures; (iii) independence of phonology; (iv) the status of phonemes vs. features; (v) the adequacy of Jakobson's theory with respect to natural classes; (vi) the validity of procedures of taxonomic linguistics; (vii) the phonetic basis of phonemic analysis. We will discuss these in turn. In each case, we will first briefly restate our position, as given in Halle (1962 & 1964*b*) and Chomsky (1964*a* & *c*). We will next try to state Householder's version of our position. Unfortunately, this version often has little relation to what we have stated and, correspondingly, is unsupported by reference or quotation. We will then discuss his criticisms in the cases where the criticism does relate to some position we have taken.

I. THE GOALS OF LINGUISTIC THEORY: In Chomsky (1964*a* & *c*) three roughly delimited goals are suggested as a framework for the discussion. We say that a grammar meets the level of OBSERVATIONAL ADEQUACY if it correctly describes the data on which it is based and nothing more – if, in other words, it gives a compact one-one organization of this data. We say that a grammar meets the level of DESCRIPTIVE ADEQUACY to the extent that it gives a correct account of the speaker's 'tacit knowledge'.[2] We say that a linguistic

[2] We assume, with no further discussion, the distinction of LANGUE-PAROLE (except that we do not accept the Saussurian limitation of LANGUE to 'system of elements', but regard it also as a system of rules – for discussion, cf. Chomsky (1964*a*:914–923 & 1964*c*:7–27). That is, we consider here only such linguistic grammars as attempt to

—*Continued on following page*

theory (n.b., not a grammar) meets the level of EXPLANATORY ADEQUACY to the extent that it provides a principled basis for the selection of descriptively adequate grammars. We remarked that these three levels of adequacy can be clarified by describing them in terms of the problem of developing an acquisition model for language. An acquisition model can be regarded as a device AM:

primary linguistic data → [AM] → grammar

This device takes as input the kind of data used by the child who acquires the language and gives as output the grammar which is internally represented by the child in some way, and which expresses his implicit knowledge. A linguistic description meets the level of observational adequacy if it gives a correct account of the input to AM, and meets the level of descriptive adequacy if it gives a correct account of the output of AM. A linguistic theory meets the level of explanatory adequacy insofar as it succeeds in describing the internal structure of AM and thus shows how the descriptively adequate grammar arises from the primary linguistic data. Such a linguistic theory is explanatory in that it accounts for the linguistic intuition (the underlying competence, tacit knowledge, LANGUE) of the speaker on the basis of a certain assumption about the form of language (i.e., about the internal structure of AM) and about the data that was available to the speaker. Clearly the assumption about the form of language embodied in an explanatory theory of this sort must be universal – that is, we take it for granted that a child is not genetically predisposed to learn one human language rather than another. Consequently a strong assumption about the device AM is easily falsifiable if incorrect (and thus is an interesting claim) by a demonstration that it fails to give a descriptively adequate grammar for some new language.

Thus the levels of observational adequacy, descriptive adequacy, and explanatory adequacy relate, respectively, to the input, output and internal structure of AM. Clearly language is acquired (i.e., a grammar is developed) by an organism with fixed initial constraints that delimit the class of possible

Footnote 2 continued—
reveal the principles (which are, obviously, in general, unconscious and may even be beyond the level of possible consciousness) that determine how utterances are interpreted in the language, and what structures are assigned to them by the speaker-hearer, idealizing away from the various factors irrelevant to the system of grammar (lapses of attention, etc.) that interact with this tacit knowledge to determine actual performance. Notice that there is no 'moral imperative' in our concern with grammars that describe LANGUE in this sense. One who has no interest in these traditional questions will simply not be concerned with DESCRIPTIVE ADEQUACY, in the sense in which we are using this term, but only with what we have called OBSERVATIONAL ADEQUACY. Such a linguist (if he exists) would find our concerns (as those of Saussure, Sapir, and others) rather exotic, as we would find his overly limited. There is no question of right or wrong here, but only of goals and interests.

grammars. It is acquired on the basis of some sort of data; therefore, interest in the data, the acquired grammar, and the fixed initial structure (that is, interest in observational, descriptive and explanatory adequacy, respectively) is surely legitimate. There is, in particular, a right answer to the question: what is the basis for the acquisition of linguistic competence? A linguist may choose to disregard these questions, but there is surely no conceivable reason for him to object to the attempt of others to investigate them.

For concreteness, let us consider an actual example of linguistic material analyzed at these three levels of adequacy. The simplest example, and the one that Householder takes as central to his critical discussion, is the case of 'possible, non-existent forms' in a particular language. Thus in English there is a form *brick* (/brik/), but no /blik/ or /bnik/. Nevertheless, a speaker of English knows that /blik/ is an admissible form in a sense in which /bnik/ is not. This distinction is, furthermore, not a matter of universal phonetics. These are the facts of the matter. So far as we know, no one challenges them; certainly Householder seems to accept the distinctions in question.

A description of English will achieve the level of observational adequacy, in this case, if it distinguishes /brik/, as an occurring form, from /blik/ and /bnik/, as non-occurring forms. Thus a lexicon – a list of all occurring forms – meets the level of observational adequacy. It correctly presents the data available to the child, the input to the acquisition model AM. The description will meet the level of descriptive adequacy if it distinguishes /brik/ and /blik/, as admissible forms, from /bnik/, as an inadmissible form. In this case, it will state what the speaker knows (unconsciously – cf. note 2) to be true; it gives correctly the output of AM. To meet the level of explanatory adequacy, a linguistic theory must justify the descriptively adequate grammar on internal grounds. That is, it must show on what basis the device AM (or the linguist) selects a grammar admitting /blik/ and excluding /bnik/. It must, in other words, justify the inclusion in a grammar of English of the rule (1) but not the rule (2):

(1) Consonantal Segment→Liquid in the context: # Stop – Vowel
(2) Consonantal Segment→/r/ in the context: # /b – ik/

Both rules (1) and (2) are true of English, at the level of observation. That is, both correctly state facts about occurrence and nonoccurrence. Rule (1) excludes /bnik/ as inadmissible, but permits /blik/. Rule (2) excludes both /bnik/ and /blik/ as inadmissible. To achieve the level of explanatory adequacy, a linguistic theory must present some general grounds for requiring the grammar to include rule (1) but not allowing it to include rule (2), despite the correctness of (2) at the level of observation. One natural way to achieve this result is to devise a measure for evaluating grammars within general linguistic theory in such a way that rule (1) will contribute to the value of English grammar, as so defined, but rule (2) will detract from the value of

English grammar. This problem has never, to our knowledge, been raised in traditional or modern taxonomic phonology. The only attempt that we know of to achieve the level of explanatory adequacy is the proposal, discussed in Halle (1962 & 1964*b*) and Chomsky (1964*a* & *c*) and earlier publications, to construct an evaluation procedure based on systematic feature analysis. We return to the details in section 2 of our discussion. For the moment, we are only concerned to distinguish the three levels of adequacy, in this particular case.

As noted in Chomsky (1964*a* & *c*), almost all linguists, in their actual practice, are concerned with descriptive adequacy. But taxonomic linguistic THEORY has generally limited itself to observational adequacy. That is, it has been concerned to develop procedures that rearrange and organize the data of a corpus in various ways, for economy and perspicuity. This has often been stated explicitly (cf., e.g., Harris, 1951:1, 3, 12, 361, 366); often it is implicit in the definition of procedures. There have been some attempts to develop procedures that go beyond organization of data in a corpus.[3] However, these fail badly to give grammars that achieve the level of descriptive adequacy.

To summarize, we are concerned only with grammars that attempt to meet the level of descriptive adequacy and with linguistic theories that attempt to meet the level of explanatory adequacy. Only such grammars and theories are empirically significant in the sense that evidence can be brought for or against them, in that they can be right or wrong. Hence our decision to limit attention to these topics seems quite justifiable.

[3] For example, see Harris (1951:chapter 15); Chomsky (1953); or, consider the various attempts to develop procedures of morphemic analysis during the forties and early fifties. These attempts presuppose intuitive knowledge of the correct morphemic analysis, and are concerned to develop procedures that give the intuitively correct analysis. Thus these are attempts to raise linguistic theory to the level of explanatory adequacy. However, these attempts met with little success. Invariably, it is necessary to apply the procedures 'intelligently', in some sense, if they are to give the desired (intuitively correct) results. Hence the procedures, as formulated, do not succeed in capturing the notion 'morpheme', in the desired intuitive sense. And the same is true of the explicit procedures designed to isolate other linguistically significant units.

It seems to us that the repeated failures of attempts to develop adequate procedures in taxonomic linguistics reflect an inherent limitation of this approach, namely, its adherence to the Saussurian view that repeated segmentation and classification exhausts the domain of linguistic structure. This is an empirical claim about the nature of language, which, in fact, seems clearly to be false. It seems that the underlying base forms and the linguistically significant abstract structures cannot be revealed by segmentation and classification but are, rather, related to phenomena only by intricate sets of ordered rules that violate the formal conditions presupposed in taxonomic methodology. We in no way disparage these attempts; rather, they are notable in that they are serious attempts to achieve explanatory adequacy. But their failure, and the reasons for this failure, suggest that it is necessary to approach the problem of developing an adequate linguistic theory in an entirely different way; and our purpose in Halle (1962 & 1964*b*) and Chomsky (1964*a* & *c*) and elsewhere has been to outline a different and, we feel, much more hopeful direction for the development of a linguistic theory that can approach the level of explanatory adequacy.

One of the most common objections to the programme just outlined (and one that reappears explicitly in Householder's paper) has to do with the statement that a descriptively adequate grammar must give a correct account of the tacit knowledge (linguistic intuition) of the native speaker. We have discussed these objections at various times in the past, but as they recur in Householder's paper, it may not be out of place to repeat once again that which seems to us entirely obvious. Without reference to this tacit knowledge there is no such subject as descriptive linguistics. There is nothing for its descriptive statements to be right or wrong about. Taxonomic linguistics has been unhappy with this state of affairs. Since Bloomfield conferred upon linguistics the first place among the social sciences for having freed itself of the yoke of the 'elusive spiritistic-teleologic words of our tribal speech' (Bloomfield, 1927), it has been thought undignified to pay attention to such a 'mentalistic horror' as linguistic intuition. Nevertheless, tacit knowledge of the speaker's language is at every stage and every point precisely what the linguist is dealing with. No one, surely, is content simply to rearrange the data in a corpus. Every linguistic description attempts, at least, to extract 'patterns' or 'regularities' from a corpus, or to abstract from it principles that will apply to other linguistic material as well. But statements of 'patterns', 'regularities', and 'underlying principles' go beyond the data. They are based on some assumption about the nature of linguistic patterns or regularities. Without such assumptions (which, unfortunately, are rarely made explicit), innumerable 'patterns' and 'regularities' can be found in any data, all mutually conflicting, and most of them, for some reason, quite ridiculous. All linguistic work is, obviously, guided by certain assumptions about the nature of linguistic structure and linguistic patterns; and such assumptions, which are the heart of linguistic theory, can be tested for adequacy in only one way, namely, by determining whether the descriptions to which they lead are in accord with tacit knowledge concerning the language. Whatever may have been said in methodological discussions, this ultimate reference to the speaker's tacit knowledge of his language is quite apparent in all actual linguistic work.

Consider again the successive attempts to develop procedures of analysis in taxonomic linguistics (cf. note 3). These were, obviously, guided by assumptions about what is the correct result – intuitively correct, that is. Otherwise, the successive attempts to develop refinements and improvements would be quite unintelligible. Without reliance on tacit knowledge as an ultimate criterion, there could be no possible objection to a 'phonological grammar' (e.g., a chart of the type given by Whorf, 1940) that excludes /blik/ along with /bnik/, just as there could be no objection to the rigorous definition of 'morpheme' as 'sequence of three phonemes', or the rigorous definition of 'juncture' as 'boundary inserted before a voiced stop', etc. Even operational tests, were they available, would not eliminate the reliance on linguistic intuition. On the

contrary, an operational test must be tested for conformity to linguistic intuition before it becomes acceptable. Otherwise there would be no objection to some operational test that characterizes 'morpheme', let us say, in terms of GSR fluctuation (averaged, perhaps, to obtain uniformity).

There is only one way to avoid complete reliance on linguistic intuition (i.e., tacit knowledge) in descriptive practice. This is to develop operational tests and explanatory linguistic theories that correctly characterize linguistic intuition in a mass of clear cases, so that we then have some basis for relying on their results in new or unclear cases.

In any event, there is no basis for disagreement between Householder and us insofar as reliance on linguistic intuition or concern for correctness is concerned. His belief that he somehow escapes this necessity is a simple consequence of his willingness to use such terms as 'systematic redundancy', 'economy', 'generality', etc., as if they were clear and well-understood in advance. But this is quite false. These terms have no clear meaning. One cannot rely on them to escape the responsibility of facing up to the problems of descriptive and explanatory adequacy. To characterize these notions correctly is, on the contrary, one of the major problems of linguistic theory. There is no necessity for everyone to be interested in linguistic theory, of course, but one must remain aware of the fact that he is solving no problems by uncritical use of just those notions that the theory of language is concerned to analyse and clarify.

Householder further objects that we assume the existence of a unique correct grammar. This strikes us more as a quibble than as a serious issue. It is logically possible that for English, for example, two different grammars will some day be discovered, neither of which is falsifiable on any conceivable empirical grounds. It is logically possible that two linguistic theories will be constructed that are perfectly consistent with all obtainable empirical evidence, and that fully justify, with equal effectiveness, all descriptively adequate grammars. Similarly, it is logically possible that several incompatible theories of motion, or light, will some day be developed, none of which is falsifiable on any empirical grounds. We agree, then, that to state that the goal of linguistic theory is to determine the choice of 'THE descriptively adequate grammar' (or that the goal of linguistics is to discover 'THE theory of linguistic form') is as misleading as it is to describe physics as concerned with discovering 'THE theory of motion', 'THE theory of light', 'THE theory of the chemical bond', etc. Having stated this, we propose to return to the use of these locutions, and we assume that the physicist, biologist, etc., will continue to do so as well, as they invariably have in the past.

In fact, even to raise these questions is to betray a misconception of the problems that actually face the linguist, or, for that matter, the physical scientist. The real problem for the linguist is to find a theory (actually, a

small part of a theory) that will come somewhere near accounting for some domain of linguistic fact; the problem for the grammarian is to develop some small fragment of a grammar that is adequate for some part of the language that concerns him. Where two equally effective fragments can be constructed, the grammarian will attempt to choose between them by enriching the domain of relevant fact or deepening linguistic theory; where two linguistic theories are equally adequate, he will attempt to adjudicate between them by bringing additional facts to bear, facts which can be accounted for by one but not by the other. This is the only way in which the grammatical descriptions of particular languages or the general theory of linguistic structure can progress. Those who prefer to contemplate the abstract possibility that at some time in the future it will be impossible to make any further progress (because all theories will be so perfect as to be unfalsifiable) are welcome to do so.

To conclude the discussion of this point, we should like to comment on Householder's notion of 'correctness'. On p.16 of his paper, he gives the following summary of his views:

> 'I am more inclined to the view that two inconsistent and irreconcilable descriptions of a language may each convey some important "intuition" about the language which cannot be conveyed by the other, NOR BOTH BY ANY THIRD. I do not think we should assume that there is always one point of vantage from which we can equally well see the front and back, the inside and the outside, the left and the right. Maybe there is, but I'm against assuming this.' (Our emphasis.)

This is the most general expression that he gives to views that apparently dominate his paper throughout, and seem to be distressingly common in recent methodological discussion by descriptive linguists.

We may begin by eliminating the false analogy presented in the second sentence of the quotation. A view from the front is not inconsistent with a view from the back, etc. The importance of giving a full description of an object from many vantage points has never been at issue, and does not bear on Householder's view expressed here and elsewhere; namely, that there is no reason to 'assume' the possibility for a consistent description of a language.

Consider now the first sentence of the quotation. Once again, there is no issue about the question of whether two inconsistent descriptions may each convey some important 'intuition' about the object under investigation. It is significant that there are no known examples of this in the case of language (and Householder cites no linguistic evidence to justify his beliefs), but it is certainly possible in principle that each of two inconsistent descriptions may, in some vague sense, shed some light on the actual character of what is being described, whether this is a language or any other object of study and research. In any serious field of investigation, the discovery of two mutually inconsistent descriptions, each somehow suggestive, would be taken as posing a problem for

research, a challenge to the investigator to be resolved by showing how the two inconsistent 'suggestive' descriptions can be replaced by a single, consistent description that goes beyond mere suggestiveness and actually expresses a precise view about the nature of the object of study. But this is because in all serious intellectual pursuits, whether those of the historian, the psychologist, the biologist, the physicist, the literary scholar, etc., it is taken for granted that the goal of research is to discover the truth about the object under investigation. This truism is rarely discussed; it is a deeply-engrained assumption that one finds no need to emphasize because it has not been challenged for centuries, within scholarship and science.

But Householder seems to take a different view. If the comment we have singled out for emphasis, in the passage from his article quoted above, reflects his actual views, he believes that there is no truth to be discovered. If the optimal description of a language consists of two mutually inconsistent parts, then this description cannot be proposed as a significant and verifiable assumption concerning the language – that is, no claim to empirical truth can be made for the description that is presented, and no evidence can conceivably be relevant for or against what the linguist does. In place of a proposal with content concerning the language he is studying, Householder's linguist has to be satisfied with the following assertion: 'I have a description D_1 and a description D_2, each of which is somehow suggestive and conveys some intuition; the two are inconsistent, but this is no problem, since in any event there is no reason to try to find a consistent description of a language, or to assume that one exists – language, in other words, may be an undescribable object, beyond the possibility of rational discussion'.

Notice that this question can arise at the level of either descriptive or explanatory adequacy. In the case of the former, the linguist may or may not be concerned with giving a correct account of the rules of the language that he is describing and of the way in which sentences are formed and understood, on the basis of this system of rules, by speakers of this language. In the case of the latter, he may or may not be interested in giving a correct account of the nature of language, the FACULTÉ DE LANGAGE that underlies the acquisition of linguistic competence, the linguistic universals that define the general form of human language. A linguist, who, like Householder, is willing to accept inconsistent accounts – in fact, claims that such inconsistency is ineliminable – has disavowed any concern for the topic of descriptive or explanatory adequacy. He has simply given up the attempt to find out the facts about particular languages or about language in general. His work is immune to criticism, of course, as an automatic consequence of his tolerance of inconsistency.

II. EVALUATION PROCEDURES: Our approach to the question of evaluation procedures is, briefly, this. Suppose that we are concerned to

develop a linguistic theory that meets the level of explanatory adequacy. It seems that a two-pronged attack on this problem offers some hope of success. In the first place, we attempt to enrich the structure of linguistic theory so as to restrict the class of grammars compatible with the data given – in other words, we attempt to make the strongest legitimate universal claim about the structure of language. Secondly, we attempt to construct an evaluation procedure for selecting one among the various grammars permitted by the proposed linguistic theory and compatible with the given data. The narrower the class of grammars permitted (as possible hypotheses), the easier it will be to select among them by an evaluation procedure. The broader the class of permitted grammars, the more difficult it will be to construct an effective procedure of evaluation.

Let us return, for concreteness, to the linguistic example discussed above. The child is presented with /brik/ but not with /blik/ or /bnik/. He somehow constructs a grammar that contains rule (1) but not rule (2), so that he then knows (see note 2) that /blik/ is admissible but /bnik/ is not. It is this fact that the linguist is attempting to explain. He can try to do so by making a strong assumption about the form of grammar – about the class of hypotheses that the child is free to sample – and developing an evaluation procedure for selecting among them. In Halle (1962 & 1964*b*) and elsewhere two specific proposals are made, relating to this question. The first concerns the form of grammar; it states in particular, that phonological rules should be stated completely in terms of features. The second concerns the evaluation measure; it states that a grammar should be evaluated by minimizing the total number of features specified in the lexicon and in the phonological rules.[4] It is then pointed out that these two assumptions jointly lead to the selection of the descriptively adequate grammar and exclude the descriptively inadequate grammar in a variety of interesting cases. These proposals thus stand as an empirical hypothesis concerning general linguistic theory – concerning the language acquisition model AM discussed above – a hypothesis which (like any empirical assumption) is to be tested by its consequences.

It is furthermore pointed out by Halle that various other proposals will not work. For example, the evaluation procedure cannot be stated in terms of minimization (or maximization) if the theory of grammatical form permits rules to be stated in terms of phonemes rather than (or along with) features.

[4] This was a first approximation. We now know of various ways in which it can be refined and improved. For further discussion of this and other related questions, see Chomsky and Halle (to appear). In general, it is an extremely difficult problem to construct an evaluation procedure that gives the desired results, and it is a pity that so little attention has been paid to the problem of justifying correct descriptions. We emphasize once again that the uncritical use of such notions as 'systematic redundancy', 'economy' 'general rules', etc., is largely responsible for this failure on the part of Householder and others to face this and other problems of linguistic theory.

This observation is offered as an argument in favour of the hypothesis that features, rather than phonemes, are the linguistically significant elements. To meet this objection to the use of phonemic notation in grammar, a linguist who doubts our empirical assumption must develop a theory of the form of grammar permitting phonemic notation and an associated evaluation procedure which, jointly, have the same effect as the proposals in terms of features. We do not maintain that this is A PRIORI impossible. However, it must be done in a serious way. Numerology of the sort to which Householder treats us is entirely irrelevant.[5]

We stress once more that choice of an evaluation measure is an empirical matter. The problem of discovering an evaluation measure is much like that of evaluating a physical constant. We are given a certain pairing of empirical facts: primary linguistic data D_1 leads to descriptively adequate grammar G_1, D_2 leads to G_2, etc. One can no more give an A PRIORI argument for a particular evaluation measure than one can for a particular value of the gravitational constant.

An evaluation measure is probably best regarded as a procedure for assigning an integer to a grammar, the smaller integer corresponding to the higher value. It should be designed in such a way that the greater the degree of linguistically significant generalization achieved in a grammar, the higher the value and the smaller the assigned integer. It should, in other words, be a measure of the degree of linguistically significant generalization achieved. As a first approximation, it seems reasonable to take the measure to be length. But

[5] In a supplementary footnote added after his article was written, Householder proposes that 'the reciprocal of the numbers of phoneme symbols in each rule should give substantially the same rank-order as Halle's method'. This very proposal has been considered and refuted by Halle. Halle considered the following three rules (the numbering is that of Halle, 1962:56–57; cf. also Halle, 1964*b*:336):

/a/→/æ/ in the env. /i/ – – – /i/ (3′)
/a/→/æ/ in the env. – – – /i/ (2′)
/a/→/æ/ in the env. – – – { /i/ /e/ /æ/ } (4′)

He noted that in the sense of generality that is commonly accepted in linguistics, (3′) is less general than (2′), which in turn is less general than (4′). Halle showed then that this rank ordering will be obtained by using the number of features as an evaluation measure, but that if phonemes are used in place of features an evaluation measure based on counting symbols will give results that fail to correspond to the correct rank order. For example, following Householder, let us calculate 'the reciprocal of the numbers of phoneme symbols in each rule'. Our results are:

(3′) . . . $\frac{1}{4}$
(2′) . . . $\frac{1}{3}$
(4′) . . . $\frac{1}{5}$

This clearly does NOT 'give substantially the same rank order as Halle's method'.

The rest of the supplementary footnote, which purports to summarize the main points of Chomsky's lectures at Bloomington in 1964, is as inaccurate as the part examined directly above. Since little purpose is served by correcting these additional inaccuracies we refrain from further comment.

if this is the selected measure, then the theory of grammatical form must permit only such notations as convert considerations of generality into considerations of length. (If notations are invented freely, any desired grammar can be made the shorter and the measure collapses.) This, in fact, is the motivation for the particular decisions that have been made concerning notations in the work in generative grammar in the past ten or fifteen years. Notations were selected in such a way that what seemed to be linguistically significant generalizations gave rise to shorter, hence higher valued grammars. The proposal to write grammars in terms of features rather than phonemes also has just this effect, and hence fits in rather nicely with what has been, so far, a moderately successful approach to the problem of explanatory adequacy. The notations that have been selected constitute an empirical hypothesis as to what is a significant generalization, a hypothesis which can be falsified on grounds of descriptive inadequacy, if it is incorrect.[6]

The notion of 'simplicity' with which we have been concerned is internal to linguistic theory. That is, a simplicity measure (evaluation measure) is an empirical hypothesis about the form of grammar, ultimately, about the native intellectual capacity ('faculté de langage') that makes language acquisition possible and, correspondingly, that justifies the linguist's attempt to describe what it achieves (to construct a descriptively adequate grammar). In this usage, 'simplicity' is a technical term of linguistic theory, on a par with 'phoneme', 'grammatical transformation', 'distinctive feature', etc. There is another sense in which one talks of the simplicity, elegance, etc., of theories (relativistic physics is more elegant – simpler – than Newtonian physics, Copernican astronomy is simpler than Ptolmaic, etc.). We do not deny the interest of this notion, but we have nothing to say about it. It is a concept that

[6] Householder's notion of 'evaluation' is rather different from ours, so far as we can make out from his comments. He objects (1965:19–20) to the evaluation measure presented in Halle (1962 & 1964*b*) because the form of grammar on which it is based, with systematic use of features, is clumsy, difficult for linguists to read, wasteful of printer's ink, burdensome to the memory and attention. If we may assume that Householder is using our terms as we do, then we are forced to attribute to Householder the following empirical hypothesis: a descriptively adequate grammar is selected (by the child, or linguist – i.e., by the acquisition model AM, above) on the basis of an evaluation measure involving amount of printer's ink, ease of reading, etc. Of course, he intended no such absurd claim. We conclude, then, that his criticism of our notion of evaluation is not based on what we have stated.

In fact, it is not difficult to see where Householder has gone astray in his criticism. An evaluation measure is often called by the technical term SIMPLICITY MEASURE. As we noted above, Householder (along with many others) has fallen into the unfortunate habit of using the terms 'simplicity', 'economy', 'generality', etc., as if they are somehow clear in advance and require no analysis. Hence when he sees the term 'simplicity measure' used in the technical sense described above, he concludes that what is intended is the vague, intuitive notion that he has in mind, and he is naturally taken aback to see that what we call the simplest (highest-valued) grammar does not meet his A PRIORI conception of 'simplicity'.

belongs not to linguistics, nor to any particular scientific discipline, but rather to general epistemology or philosophy of science. Our concern, as linguists, is to clarify the status and the precise content of the evaluation measure (simplicity measure, in the technical terminology that has now become familiar) that is internal to linguistics and that plays a role in the determination of a descriptively adequate grammar. We emphasize, once again, that such a measure is relative to a particular form of grammatical statement. It is not of the slightest importance to us that the simplest grammar, in our sense, may be difficult for some linguists to read, or that it may be wasteful of printer's ink. We do assume that a grammar has a physical representation in the speaker's brain, but obviously neither we nor anyone else has the slightest idea about this. But evidence about this, if it will some day be forthcoming, is the only relevant evidence relating to physical representation of grammars.

Householder is not alone in assuming some absolute notion of 'simplicity' that can be relied upon to choose among linguistic theories. As another example, consider the discussion of ordering of rules in Lamb (1964). Lamb compares two theories of generative grammar, each of which contains rewriting rules of the form: $A \rightarrow X/Z - Y$. The two systems differ in the way in which these rules are organized. The first theory is the one we have been considering (for this discussion, we will refer to the grammars it permits as MUTATION GRAMMARS, following Lamb's terminology). In a mutation grammar the rewriting rules are linearly ordered, and apply in the given sequence. The second theory requires that the rewriting rules be unordered, except that the rules that rewrite the same symbol form a linearly ordered bloc, and are to be applied in the given order with the additional constraint that only one may apply. The rules of such a grammar (following Lamb, we call it a REALIZATION GRAMMAR) convert a representation on a 'higher level' to one on a lower level; each rule applies to the higher level representation, not to the representation as it appears after application of previously selected rules. Except for the condition on ordering of subrules, Lamb's realization grammars are identical to those considered by Harris (1951: Appendix to 14.32), in his demonstration of how one of Bloomfield's examples of descriptive order can be restated with a different condition on rule-application, not involving ordering.[7]

How can one decide between mutation systems and realization systems? Obviously, one must turn to questions of empirical import. One must try to discover phenomena that can be adequately described by one of these systems but not the other. For example, it has been pointed out by Chomsky that although certain examples of descriptive order can be handled by realization

[7] In fact, Lamb's 'hypothetical morphophonemic example', invented to illustrate this property of realization grammars, is identical, except for choice of symbols, with the Menomini example of Bloomfield's discussed by Harris in his illustration of the same point, and Lamb's treatment of this example is identical with Harris's.

systems (cf. 1964*a*:946, n.34 & *c*:70, n.8), such systems are inadequate for slightly more complex examples of a sort easily found, and illustrated there. Lamb does not try to show how these defects can be overcome (in fact, he makes no reference to the earlier discussion of realization systems); nor does he show how certain phenomena can be handled by realization systems that are beyond the scope of mutation systems. In short, he does not attempt to deal with the question of empirical import. Instead, he argues for realization systems on two grounds: one, on the ground that mutation systems are appropriate only for diachronic linguistics; two, on grounds of absolute simplicity. The first statement we shall not discuss. It is simply an ex cathedra pronouncement, for which no justification is offered. The second argument is the one that concerns us here. Lamb claims that some theory-independent absolute notion of simplicity favours the organization of rules allowed by realization grammars over that of mutation grammars. Furthermore, he is quite explicit about some of the details of this universal measure. For example, each rule in a mutation system must be supplied with exactly one extra symbol, for this universal measure to be applied (viz., an integer indicating its position in the ordering); the statement of a negative context (i.e., a context of the form 'except for . . .') is exactly as complex as the statement of the corresponding positive context (cf. Lamb, 1964:114); the absolute number of symbols in a mutation grammar is commensurable with the absolute number in a realization grammar; etc. Needless to say, he makes no attempt to validate this absolute measure nor to develop it in any systematic way; nor does he show any awareness that such justification is necessary, that these particular arbitrary decisions are not obvious A PRIORI truths.[8]

We can perhaps clarify this question still further by considering the one actual linguistic example that Lamb presents. He considers a language (Monachi) in which both /w/ and /k^w/ are realized as [q^w] in the context $V_1h–V_2$ (where V_1 and V_2 are two classes of Vowels, the membership of which is of no further relevance here), /w/ being realized as [k^w] elsewhere after /h/. In a mutation grammar, the facts are stated in the form (3); in a realization grammar, in the form (4).

[8] Notice that even if this absolute notion of 'simplicity' could somehow be justified, this would have little relevance to the problem of choosing among linguistic theories. Suppose it were true that a grammar that derives Y from X by a sequence of rules, each applying to the last step of the derivation (i.e., a mutation grammar), is more 'complex', in some sense, than a grammar that derives Y from X by an unordered set of rules, each available at every stage of the derivation and each applying to X rather than to the last step of the derivation so far produced (a realization grammar). This conclusion would still leave open the question whether the system used in natural language is 'maximally simple' in this absolute sense. There is not the slightest reason to expect natural languages to be 'maximally simple', assuming that some content can be given to this curious notion. The relevant constraints are those of physical realizability, not 'absolute simplicity', whatever this may mean.

(3) $w \rightarrow k^w / h-$ (i)
$k^w \rightarrow q^w / V_1 h - V_2$ (ii)

(4) $w \rightarrow \left\{ \begin{array}{l} q^w / V_1 h - V_2 \\ k^w / h- \end{array} \right\}$ (i) (ii)
$k^w \rightarrow q^w / V_1 h - V_2$ (iii)

In the grammar (4), the only requirement on order of application of rules is that (i) must precede (ii). In the grammar (3), the rule (i) must precede (ii).

In the mutation grammar (3), the vocalic environment determining [q^w] is stated once; in the realization grammar, it is stated twice. But, Lamb argues, the relatively greater economy of the mutation grammar is only apparent, since 'this economy is achieved at the cost of making these rules ordered'. (1964:119).[9] We agree with Lamb in denying that (3) is 'simpler' than (4), but not for his reasons. It is not that an extra symbol must be added to each rule in (3), to be counted in measuring 'economy' along with the symbols in the rules themselves; rather, the point is that comparison of (3) and (4) in 'economy' is entirely meaningless until some cross-theoretical absolute notion of 'economy' is produced, and no such measure is known.

There is a real difference between (3) and (4). It is not a matter of 'economy', however, but of empirical import. The theory of mutation grammar claims, in effect, that there is a linguistically significant generalization underlying the phenomena described by (3) and (4); it claims, in other words, that there is a significant relationship between the realization of /w/ and /k^w/ as [q^w]. The theory of realization grammar takes this to be a mere accident, of no linguistic significance. This difference between the two theories becomes obvious if we consider a language identical to the one just discussed except for the fact that /w/ is realized as [x^w] after /h/ except in the context of (4i), and/k^w/ is realized as [q^w] not in the context of (3ii) and (4iii), but in some entirely new context V_3h-V_4, where $V_1 \neq V_3$ and $V_2 \neq V_4$. For this language, both the mutation grammar and the realization grammar would have the form (5):

(5) $w \rightarrow \left\{ \begin{array}{l} q^w / V_1 h - V_2 \\ x^w / h- \end{array} \right\}$ (i) (ii)
$k^w \rightarrow q^w / V_3 h - V_4$ (iii)

Assume now that each of the theories under discussion has some reasonable evaluation measure internal to it (e.g., symbol counting under specified notational transformations, as described above). Then the mutation grammar (5) is more complex than the mutation grammar (3), but the realization grammar (5) is identical in complexity to the realization grammar (4). This comparison brings out one of the empirically significant differences between mutation grammar and realization grammar. For the former, the facts described

[9] Actually, Lamb's conclusion is incorrect even on his own grounds, since exactly one statement of relative order is required in both (3) and (4). However, this inconsistency is unimportant, in the light of the meaninglessness of his attempted comparison.

by (3) exhibit a systematic arrangement, in comparison with those of (5). For realization grammar, neither set of facts falls into any systematic arrangement – the actual language of Lamb's example and the invented language are identical, with respect to the possibility of finding significant generalizations.[10]

The crucial point, once again, is that the issue between the alternative theories is an empirical one. In the particular case that Lamb discusses, the issue is whether there is a real generalization underlying the phenomena described by (3) and (4), as compared with the unsystematic arrangement of (5). There is no question of the 'relative complexity' of (3) and (4). There is no known absolute measure of complexity that can be called upon to make such a comparison among linguistic theories. If each of two theories has an evaluation measure INTERNAL TO IT, then the question of empirical consequences can be raised and fruitfully discussed. We can, in particular, determine what sorts of configurations of data count as 'systematic' in terms of the competing theories, and, to the quite considerable extent to which independent agreement is possible on this matter, we can adjudicate between these theories. We can, for example, reject the theory of realization grammar on the grounds of its inability to distinguish (4) from (5). But in the present state of our knowledge, a comparison of two theories in terms of their 'absolute complexity' or 'economy', in the sense of Lamb and Householder, is entirely without significance.

In general, the absolute 'size' of a grammar is a matter of no linguistic importance, and, in the abstract, it makes no difference how it is measured. If we decide to use length, under certain 'notational transformations', as an evaluation measure, then relative length becomes important. A decision concerning notational transformations (e.g., a convention for the use of brackets, parentheses, etc.) will be of some interest if it affects different systems of rules in different ways; otherwise, it will be of no interest. For example, the now common use of brackets is an important empirical hypothesis about the nature

[10] Of course, Lamb is no more willing to accept this absurd conclusion than any other linguist would be, and so he then goes on to propose a further modification of realization grammar, suggesting new mechanisms that permit organization of rules beyond the ordering allowed in the system described above. No doubt he would claim that these new principles of organization are still 'simpler' in his absolute sense than the kind of organization permitted in linearly ordered mutation grammars. If so, this claim would be as meaningless as the claim he makes for realization grammar as opposed to mutation grammar. In any event, it would serve no purpose to undertake an investigation of the further principles of organization he proposed. They are designed, AD HOC, to account for the inability of realization grammars to express the distinction between the facts of (4) and those of (5), and they do not overcome the defects of realization grammars that have been discussed in the literature. Cf. p.110–111. Once again, since Lamb does not attempt to deal with these difficulties and suggests no compensating advantage for the new mechanisms he proposes, there is little point in discussing them any further.

of language, because it shortens a grammar and hence assigns it a higher value if this grammar contains successive rules which are similar in form in a certain well-defined way, but does not shorten a grammar containing similar rules that are not successive, or successive rules that are not similar in the way which is implicitly defined by these conventions. Thus conventions for use of brackets amount to the assumption that certain formal properties of a grammar (in this case, a certain sort of similarity among successive rules) count in its favour, and that an acquisition model will prefer grammars with these properties – will select grammars with these properties over others that are also compatible, on the level of observation, with the data presented to it. There is no A PRIORI reason why this assumption should be correct – it is, in other words, a non-trivial assumption. Similarly, the proposal to minimize the number of features in the lexicon and redundancy rules is an interesting hypothesis, since it selects certain grammars (in particular, the grammar containing rule (1)) over others (in particular, the grammar containing both rule (1) and rule (2)) which are equally compatible with the data, and it thus contributes directly to explanatory adequacy, as defined above. It is important to avoid mere arithmetical fetishism, and to bear in mind that an evaluation procedure is important only insofar as it contributes to an understanding of the notion 'linguistically significant generalization' and to the extent that it distinguishes among grammars that provide different hypotheses as to linguistic competence.

III. INDEPENDENCE OF PHONOLOGY: The next point raised by Householder (1965:17–20) concerns the 'independence of phonology'. The high point of this discussion is a six step deduction (p.19) which is Householder's reconstruction of an argument which he attributes to Halle (with no specific reference), purporting to show that phonology cannot be independent of grammar. We agree with Householder in finding the argument inconclusive. Furthermore, we find its separate steps largely unintelligible. We observe, however, that the argument as presented by Householder is totally unrelated to anything we have ever proposed or would ever think of proposing. We do not recognize it or any of its steps. We therefore omit all further discussion of it. Instead we briefly outline our position as stated in Halle (1962 & 1964*b*) and Chomsky (1964*a* & *c*) on the questions of how the phonological component of the grammar relates to the syntactic component, and whether a 'phonological grammar' is needed as a new and independent part of a full grammar.

We assume that the phonological component of the grammar is purely interpretive. The syntactic component generates a 'surface structure' which consists of a string of formatives (minimal syntactically functioning elements) and a Phrase-marker (labelled bracketing) of this string. The formatives are represented completely in terms of features. The phonological rules fall into

three basic types. The redundancy rules (morpheme structure rules) state general properties of formatives, and thus make it possible to eliminate redundant feature specifications from lexical entries. The transformational phonological rules apply in a cycle, as determined by the Phrase-marker (see Chomsky (1964*a* & *c*) and references cited there for details). The nontransformational phonological rules apply, in sequence, just once – namely, at the stage when word boundaries are reached in the transformational cycle. Since the Phrase-marker determines the functioning of the phonological rules, we would say that phonology is not independent of syntax, but we have no idea whether we agree or disagree with Householder about this. In particular, when he attributes to Halle the view that ' "phonology is wholly dependent on grammar" ' (p.18) (with no citation or reference despite his use of quotes), we are quite surprised. Taken literally, this presumably means that all phonological rules are transformational, in our sense. This we deny. It is obviously false, and we have never even hinted at such an absurd claim.[11] Perhaps Householder has in mind something else, but once again we have no way of knowing. On the other hand, when he attributes to us (and our 'young admirers') the claim to have established 'the unity of grammar with phonology', we accept this attribution, if it means that transformational rules involving the Phrase-marker of a string are a fundamental part of the phonological component (we do not know what else it might mean). This seems to us to be established beyond reasonable doubt.[12] Furthermore, as argued in detail in Chomsky (1964*a* & *c*), there seems to be no systematic linguistic level intermediate between systematic phonemic (see above, pp.97–99) and systematic phonetic, the former (but not the latter) requiring transformational (i.e., syntax-dependent) rules to relate it to physical fact. If this implies that 'phonology is united with syntax', in Householder's sense of this phrase, then again we accept this as a fair statement of our present view, a view, furthermore, that we feel that we have supported by strong and valid arguments.

Consider now the need for an independent 'phonological grammar'. Perhaps it will clarify matters to trace the brief history of the notion. In his review of *Syntactic Structures*, Lees pointed out (1957), correctly, that grammars of the type considered there failed to achieve descriptive adequacy in that they made no distinction between admissible and inadmissible phonological forms. To

[11] The identical mistake was made by Ferguson (1962) in his review of Halle (1959*b*). Like Householder, Ferguson mistakenly assumes that a denial of the autonomy of phonology from the rest of the grammar implies the claim that all of phonology is totally dependent on grammar; cf. Chomsky (1964*a*:31 & 1964*c*:105–7).

[12] Of course, we can claim no originality in this respect. The fact that phonological processes must be stated in terms of syntactic structure (for example, lexical category) was a commonplace until challenged by modern structural linguistics. Cf., for example, Postal (1964).

remedy this, he proposed that the grammar contain a separate system of rules to generate the admissible forms.[13] With such additional rules, the grammar would achieve descriptive adequacy. This suggestion has now been repeated by Householder in several places, although he has made no attempt, to our knowledge, to go beyond Lees' suggestion by developing either some sort of specific theory of rules of this type, or some concrete linguistic example of them.[14]

Shortly after Lees' review appeared, Halle pointed out (1959a, 1962, & 1964*b*) that a grammar of the *Syntactic Structures* type can achieve descriptive adequacy in this respect even without the addition of a new and AD HOC phonological grammar. If we state the phonological rules strictly in terms of features and include among them redundancy rules (morpheme structure rules) that extract regularities from individual lexical matrices, then the distinction of admissible versus inadmissible is already made by the grammar which generates just the occurring forms. For example, if the lexicon lists only occurring forms (e.g., /brik/) and the grammar contains rule (1) as a redundancy rule, then this grammar distinguishes the non-occurring but admissible /blik/ from the non-occurring but inadmissible /bnik/ in the following way: there exists a possible but non-occurring distinctive feature matrix which is converted by the phonological rules into /blik/, but there is no possible matrix that is converted by these rules into /bnik/. We can therefore define the admissible forms as those that can be generated by the rules from some (possibly non-occurring) matrix. Consequently, the rules that generate the occurring forms also make the distinction between admissible and inadmissible, and it is unnecessary to add a new system of rules (an independent phonological grammar) for the grammar to achieve descriptive adequacy, in this respect.

This observation alone is sufficient to rule out the suggestion that the grammar must be supplemented by an independent phonological grammar. But actually, the approach we have adopted goes well beyond this. It is possible to achieve not only descriptive, but (in part) also explanatory adequacy in this way, as is pointed out explicitly in Halle (1962 & 1964*b*) and elsewhere. Suppose, in fact, that the grammar is stated strictly in feature notation, and that the minimization procedure mentioned above is taken as an evaluation measure. If we add rule (1) to the grammar, as a redundancy rule,

[13] In the same place, Lees also suggested that an independent sub-grammar might be necessary to account for non-transformational morphology. This suggestion Householder has also adopted without, however, elaborating, extending, or exemplifying it in any way. As distinct from the question of 'phonological grammar', this problem seems to us very much open. For some inconclusive discussion see Chomsky (1965: chapter 4).

[14] The problem of developing Lees' suggestion has been taken up by Contreras and Saporta (1960). However, in the light of the remarks that we present directly, we feel that there is no necessity for an independent phonological grammar in the first place.

we may delete from the lexical entry of each item of the form/Consonant+Liquid+. . ./ the feature (namely, [+Vocalic]) that indicates that the second segment is a Liquid rather than a true Consonant. In this way, many features are saved in the lexicon, many more than are added by the rule (1), stated in feature notation. On the other hand, if we were to add rule (2) to the grammar we would save one feature specification for each entry /brik . . ./ (more generally, in fact, every entry /bri+Velar . . ./), since the second segment would not have to be lexically distinguished from /l/, but this would not amount to the number of features added by rule (2) itself. Consequently, the grammar that contains rule (1) is higher-valued than the grammar not containing this redundancy rule; but a grammar that contains rule (2) is lower-valued than the grammar not containing this rule. In this way, we can account for the selection of a grammar containing rule (1) and excluding rule (2) – we can account, in other words, for the intuitive knowledge that /blik/ has a different status from /bnik/. In short, the distinction between admissible and inadmissible forms results automatically from the attempt to extract all possible generalizations from the lexicon and to state these as redundancy rules, UNDER A HIGHLY SPECIFIC DEFINITION OF 'GENERALIZATION', given jointly (and implicitly) by the decision to use strict feature notation in grammar and to count features in evaluating grammars. It is also pointed out in Halle (1962 & 1964*b*) and elsewhere that these assumptions have many other merits as well.

Incidentally, it goes without saying that our assumptions about the form of grammar and about an appropriate evaluation measure are surely defective in many ways, and will require refinement and modification as additional empirical evidence is obtained (see notes 4 and 15). This is a first approximation, a first step towards the problem of defining 'linguistically significant generalization'. But it seems to us a promising first step, which already has some achievements to its credit. So far as is now known, the suggestion that an independent phonological grammar be added to the grammar takes no step at all towards a solution of this problem, and is, therefore, to be rejected.

Our claim, then, is the following. If we construct a grammar to generate the occurring forms (more precisely, to generate the well-formed sentences involving attested lexical items), then this grammar already makes a distinction between admissible and inadmissible. Furthermore, by applying a rather well-motivated evaluation measure to grammars of the type to which we restrict ourselves, we can even achieve partial explanatory adequacy; that is, we can offer a general explanation for the intuition of the native speaker that certain forms are admissible and others, inadmissible. We can thus take certain preliminary steps towards establishing a hypothesis about the internal structure of the acquisition model AM discussed above – about the general character of the distinction in question. In the light of this, we see no point

in adding to the already descriptively adequate grammar a new, unjustified, AD HOC system of rules that simply repeats what this grammar already establishes, and does so in a way that permits of no explanation or justification, so far as is known. In short, we see no need for an independent phonological grammar.

To justify an independent phonological grammar it would be necessary to show one of two things: either that it can achieve descriptive adequacy in a way in which the grammar to which it is added cannot, or that it is possible to motivate its rules in a way that is more far-reaching than is possible in the case of phonological redundancy rules. The second question is beyond Householder's concerns altogether, since he nowhere indicates any interest in explanatory adequacy. As to the first, he does suggest that a phonological grammar will be superior to the kind of system that we have described in detail. On p.19 he cites two examples of rules that can be handled more easily in a phonological grammar than by means of 'the considerations Halle drags in'. The first of these involves 'rules concerning the number of segments in a morph (since this must already be specified in the lexicon)'. We have no idea what this comment means, and therefore make no attempt to discuss it. The second concerns a hypothetical case in which 'feature a in segment A is solidary with feature b in segment B, so that either may be used to predict the other, but for each individual morph one or the other has to be specified'. Such a case is certainly logically possible; whether it exists is not so clear. In real cases, there are usually many considerations that determine the choice one way or another. However, let us assume that such an example can be found. Then a grammar of the sort we have described would (by hypothesis) have to make an arbitrary decision, and from our point of view this would be a defect in our theory.[15] Householder assures us that in this case, the arbitrary decision 'can be avoided in a separate phonological grammar' of the sort he has in mind. Since he has never presented an example of such a separate phonological grammar, nor described its properties in any way, we have no idea whether his claim is justified. If it is, and if real examples of this sort of

[15] Why it should be a defect from Householder's point of view is unclear. This is a case in which there are (by hypothesis) two equivalent grammars, indistinguishable on empirical grounds, and this is what he takes to be the normal situation (see the discussion of uniqueness of grammar above, and on p.16 of his paper).

Cases of this sort are interesting, and we have considered them several times in our various attempts to develop an adequate phonological theory, but without arriving at a satisfactory proposal. One feature of redundancy rules that seems to us somewhat questionable is precisely their 'directionality', that is, the fact that they select one position as independent and characterize the feature composition of other segments in terms of it. A possible modification of our theory would be to replace these 'directional' rules by schemata that simultaneously fill in unspecified features in various positions in a phonological matrix. There are various ways to realize this proposal, but we have no strong empirical examples that motivate one or another decision, and prefer therefore to leave the question open.

case exist, then this would be an argument in favour of the theory that he has in mind. Obviously such discussion of hypothetical examples and how they would be handled by still unformulated theories has little bearing on real issues and can not outweigh the empirical evidence we have presented above. We persist, therefore, in our belief that a separate phonological grammar is superfluous.

IV. THE STATUS OF PHONEMES VERSUS FEATURES: This 'remaining philosophical point is a real puzzler' to Householder. He cannot understand why we propose that phonological rules be stated completely in terms of features, rather than in terms of phonemes or in a mixed notation. The only possible explanation that he can concoct for this strange perversity of ours is that we are entangled in some sort of phenomenalist metaphysics, and therefore believe that physical objects are really sets of properties (1965:20–23). We omit completely any discussion of this digression.

The explanation for our conclusion that so puzzles Householder is given in a clear and straightforward fashion in Halle (1962 & 1964*b*), and we have repeated it above. To repeat once again, if we state rules strictly in terms of features, then we can propose an effective evaluation procedure which distinguishes true generalizations in terms of natural classes (in the sense in which linguists have always intuitively relied on these notions) from linguistically nonsignificant pseudo-generalizations, and which makes the distinction between admissible and inadmissible forms in an interesting class of cases. With a mixed notation, or a purely segmental notation without features, we know of no way to construct an evaluation procedure that will have these properties. For detailed argument, see Halle (1962 & 1964*b*). Furthermore, the grammar must obviously somewhere contain a specification of elements in terms of features. As has frequently been observed (see note 24 below), all phonology breaks down if we do not assume analysis on the phonetic level in terms of universal phonetic features.

We conclude, therefore, that only feature notation has linguistic significance, and that segments are simply to be regarded as conventional abbreviations, utilized to cope with the exigencies of printing but having no linguistic significance in themselves. The arguments for this position are purely linguistic, and are based neither on metaphysical commitments nor analogies to elementary particle theory, as Householder supposes (p.22). To refute them, it will be necessary to show how equally good or better results can be achieved by a different theory that allows separate status to 'phonemes' or other segmental units. This Householder does not even attempt. His only argument for such units is that they save ink or 'physical bulk' in printing, and are easier for him to read. We fail to see the linguistic significance of these comments, the correctness of which we certainly do not challenge.

V. THE ADEQUACY OF JAKOBSON'S THEORY WITH RESPECT TO NATURAL CLASSES: The next section of Householder's paper (1965:23–26) concerns the use of distinctive features (in Jakobson's sense) to specify the classes of elements that play a role in phonological[16] rules. This is an interesting and important topic, but unfortunately, in Householder's treatment of it few of the serious questions are even touched.

It is important to be clear about the various roles played by distinctive features in our adaptation of Jakobson's theory.[17] In the first place, they provide a universal phonetic theory that determines all possible 'outputs' of the phonological component of any generative grammar. That is, the feature system determines the class of 'possible sentences' from which the sentences of any human language are drawn, and, furthermore, imposes an intrinsic classification, in terms of feature composition, on the sounds that constitute utterances. Hence the theory provides an empirical hypothesis about the phonetic constitution of any human language and the organization and structure of the system of sounds and of possible phonetic contrasts that a human language can utilize.

In the second place, the features indirectly determine the classification of lexical entries.[18] Each lexical entry must be classified in terms of a variety of

[16] The reader will recall that our use of the word 'phonological' (='systematic phonemic') is that of Sapir, not that of post-Bloomfieldian taxonomic phonemics. Thus it incorporates what is now designated 'morphophonemics', in some of the many and various senses of this loose cover term. Our decision to return to more traditional terminology is motivated by what we take to be an empirical finding, namely, that only two levels of representation (systematic phonemic and systematic phonetic – see above, p.97–8, and references cited there) can be isolated in the phonological component. The new term 'morphophonemic' is justified if there is a third level, intermediate between systematic phonemic and systematic phonetic, to which the term 'phonemic' might be applied. But since the existence of any such intermediate level has not been demonstrated, so far as we can determine, we see no justification for the terminological innovation and therefore suggest a return to earlier usage.

[17] To simplify the discussion, we will not raise the question of how our adaptation of Jakobson's theory conforms to Jakobson's views. In fact, we feel that our position is in no way different from his in any respect relevant to this discussion. But since we limit ourselves here to Householder's critique of our papers, we will not discuss this further question.

[18] Householder proposes (1965:17) that as an alternative to feature analysis, phonological rules be presented in terms of 'non-terminal symbols' analogous to the non-terminal symbols Noun, Verb, Noun Phrase, etc., of syntax. Thus we will have such rules as Vowel→V-Front, V-Front→ [i, e, æ], etc., in a pure generative phonology, analogous to the rules Verb→VerbTransitive, VerbTransitive→*eat, read, see*, etc., in syntax. Actually, precisely the opposite decision must obviously be made: that is, the syntactic rules used as an analogon must be restated in terms of features (see Chomsky, 1965, for discussion of this point which summarizes and generalizes the many proposals that have been made during the last five or six years for remedying the defect of grammatical theory illustrated by such rules as Verb→VerbTransitive, etc.). The reason is that cross-classification of the sort that is typical on the phonological level (as on the lexical level, in syntax) is inexpressible in terms of rewriting rules. Thus if we want to introduce both Frontness and Height, let us say, which intersect in [i], it is impossible to use the device of rewriting rules with non-terminal symbols, in the way that Householder suggests.

syntactic, semantic, and phonological categories. We consider here only the latter. For example, the lexical entry *bee*, in English, must be so represented in the lexicon that the phonological rules will assign to it the phonetic representation [bīy].[19] Thus, *bee* must be assigned to the category of forms that have an initial Voiced nonContinuant, the category of forms that have a Diffuse, nonGrave Vowel following the initial Consonant, and so on. The natural way to represent this categorization of *bee* (and, in general, of all lexical items) is by a matrix in which columns stand for 'segments' and rows correspond to features. Each feature corresponds to a pair of opposed categories, membership in one being indicated by a + and membership in the other being indicated by a —. These paired categories are mutually exclusive but not necessarily exhaustive. Thus certain entries may be blank, indicating that no information is given as to the membership of the segment in question with respect to the paired categories corresponding to the row in question. The phonological rules then fill in the blanks, modify entries where necessary, and replace the +'s and —'s by integers indicating degree, where the features correspond to phonetic scales. Thus the lexical entry *bee* might be represented by the phonological matrix (6) (we modify this below), which is converted by phonological rules to the fully specified feature matrix conventionally abbreviated as [bīy].

(6)

Vocalic	—	+
Diffuse	+	+
Grave	+	—
Nasal	—	
Continuant	—	
Voice	+	

The representation (6) is a lexical categorization; the representation [bīy] (in its full, matrix form) is a phonetic specification of the corresponding item. In this unusually simple case, it may seem that the phonological rules simply contribute further specification to (6); for example, one rule assigns the feature [+Tense] to the Vowel because of the fact that it is in final position, a second rule of equal generality (THE DIPHTHONGIZATION RULE, to which we return below) adds a Diffuse Glide to the Tense Vowel, the Glide sharing the categorization of the Vowel with respect to Gravity (thus [w] is added to Grave Vowels and [y] to nonGrave Vowels), and so on. But in general, phonological rules will also modify entries, as, for example, when an unstressed Vowel, in various contexts, is reduced to [i], in English. It is, in fact,

[19] In citing examples, we will omit phonetic detail irrelevant to the discussion. Our discussion of phonological rules will also omit details and refinements that have no bearing on the issues being raised. The reader will recall that the phonetic symbols, in our view, are nothing other than conventional abbreviations for feature complexes. Furthermore, we maintain that in practice this is everyone else's view too, and has been for many centuries.

quite common for the lexical categorization of a segment to appear in none of the corresponding phonetic matrices, without change. For example, for reasons that we cannot detail here, the second Vowel of *demon* must be entered lexically as /ɔ/ (nonTense, Grave, Rounded, Compact) although it appears phonetically, in most American English dialects only as [i] (non-Compact, nonRounded) or /ā/ (Tense, nonRounded), as in *demon*, *demonic*, respectively. In fact, we will see below that even in the case of *bee*, the phonological rules modify as well as expand feature content.

Notice, incidentally, that there is no need to incorporate in a grammar rules that convert phonological to phonetic features (the rules discussed on pp.16–17 of Householder's paper). Rather, we may think of the phonetic features as inducing a lexical categorization, in the sense described above. Notice further that lexical items may be assigned to additional categories beyond those provided by the phonetic features. Thus, for example, the word *see* in English must be assigned a categorial (phonological) representation that indicates that it takes irregular inflections, etc.

Implicit in Jakobson's theory is another interesting and non-trivial empirical assumption, namely, that the lexical categorization induced by the phonetic theory will be 'natural', from the point of view of the functioning of phonological rules; that is, that the rules will apply to classes of segments which can, in general, be easily and simply specified in terms of feature composition. There is no A PRIORI reason why this should be true. The framework we have just described presupposes that it is true in general; otherwise, it would be quite unmotivated and pointless.

Householder challenges this empirical assumption, and suggests that very often the classes of elements that are referred to in phonological rules are not natural in the sense just outlined. He also draws some curious conclusions concerning the universality of a feature system from this purported demonstration of the inadequacy of the Jakobsonian framework. We return to his conclusion below, but first investigate the six examples that he offers to show that the classes that appear in rules are not natural. We shall see that none of his examples has any bearing on this quite interesting question – in most cases, because he has simply paid little attention to the linguistic facts.

His first example is the rule that converts /k/ and /t/ to [s] before high front Vowels (e.g., *democrat – democracy*, *electric – electricity*). But /k/ and /t/ differ in Diffuseness and Gravity. Therefore the rule does not apply to a natural class.

Notice, however, that in the positions where /k/ goes to [s], which is Diffuse and nonGrave, /g/ generally goes to [ǰ] (e.g., *pedagogue – pedagogic*), which is nonDiffuse and nonGrave. It seems to us that the simplest way to describe these alterations is by a sequence of rules: the first applies to [k, g] (that is, the archisegment consisting of the features common to [k] and [g]) in certain contexts, changing the Gravity of the consonant to nonGrave; a second rule

raises the nonGrave variant of /k/ to Diffuse; a third rule converts all of the nonGrave stops to Strident Continuants, in certain contexts. This brief account omits details, but, so far as we have been able to discover, it extracts whatever generality there is in this set of consonant alternations. (For details, see our *Sound Pattern of English*, forthcoming.) But, in fact, each rule involves natural classes. We see no improvement that can be effected by adding new, AD HOC categorization to the principled categorization induced by distinctive feature theory, in this case. Furthermore, Householder suggests no way in which this set of alternations can be described more simply by using AD HOC features. Hence this example is apparently quite irrelevant to his claims.

Notice, incidentally, that to refute Jakobson's theory (or our version of it), it is not sufficient to point to some complicated phenomenon. It is also necessary to show that certain generalizations that are linguistically significant cannot be extracted if we restrict ourselves to the categories provided by distinctive feature theory. This Householder makes no attempt to do in this case or, as we shall see, any of the others he cites.

Householder's second example involves Vowel length in Latin, before final Consonants. The rules, in this case, seem to be the following:

(5) $V \rightarrow [-\text{long}]$ in the context: $\left\{ \begin{array}{ll} VC - - \begin{bmatrix} +\text{cons} \\ -\text{strid} \end{bmatrix} & \text{(i)} \\ \#C - - \begin{bmatrix} -\text{strid} \\ +\text{diff} \\ -\text{voc} \end{bmatrix} & \text{(ii)} \end{array} \right\} \#$

Case (i) applies to polysyllables, and asserts that Vowels shorten in this case before nonStrident Consonants or Liquids. Case (ii) asserts that in monosyllables, the Vowel shortens before a nonStrident Diffuse true Consonant (i.e., before Liquids there is no shortening, in this case). Probably, Diffuseness need not be specified in case (ii), since the examples with final Velars (which are, in any event, sparse) have an elided final -e, and proper ordering of rules can thus eliminate these cases. Furthermore, it does not seem difficult to extend the rules to final clusters.[20] In any event, the rule is quite simple and

[20] It seems that before double Consonant, the Vowel is Long if one of the Consonants is Strident; otherwise Short. In particular, then, before Nasal plus Strident, the Vowel will be Long (e.g., *mōns*, *mēns*). The Nasal then is dropped in this position (possibly, post-classically), giving a Long, presumably Nasalized Vowel before the single Strident Consonant. We have not discussed the problem of shortening before /d/, as in Ablative and Accusative of personal pronouns, where the /d/ drops finally. It may be that dropping of the final /d/ precedes (synchronically) the shortening rule. Note, incidentally, that shortening before /t/ is attested late.

As in any real linguistic example, there are numerous facts to consider before one arrives at a hypothesis as to the grammatical description. We have made no attempt to exhaust the problem of Classical Latin shortening, obviously. But we see, at the moment, not the slightest relevance of this phenomenon to the question of inadequacy of the Jakobsonian framework, and are therefore mystified by Householder's reference to it.

involves only natural classes. It must therefore be regarded as an example supporting the distinctive feature framework and it is entirely unclear why Householder brings up this example at all.

Householder's third and fourth examples we quote in full: 'A rapid check of Sanskrit Sandhi rules indicates about five (out of some 25 or so) classes of this sort [i.e., of the sort that involve unavoidable disjunctive classes: NC/MH]; one set of rules for Latin nouns requires four out of ten'. Since one cannot refute a serious theory by a 'rapid check of . . . rules', or by mentioning 'one set of rules' (which may be the wrong set) we shall not discuss these examples. We cannot resist observing that such unwillingness to deal with linguistic data ill becomes one who takes it upon himself to lecture others on their disregard for facts (cf. Householder, 1965:14).

Householder's fifth example involves a hypothetical rule that applies only to i, e, and æ. Just why he gives this example is rather unclear, since these constitute a natural class (nonGrave, nonRounded), as noted incidentally in Halle (1962 & 1964*b*) (see also fn.5 above).

Householder's final example is the most interesting of all. He discusses the Vowels that occur before final [ŋ], and notes that they do not form a natural class. That is, we have the Diffuse nonGrave Vowel [i] (as in *sing*) and all the Compact Vowels ([æ], as in *sang*; [ʌ], as in *sung*; [ɔ] as in *song*).[21] Thus we have the class: Compact, or Diffuse nonGrave. And this is not a natural class.

There is, however, not the slightest reason why the phonetic data should fall into natural classes. The theory presumes only that phonological rules will apply to natural classes. Furthermore, one would expect that underlying phonological (systematic phonemic) forms will be symmetrically arranged, in general. In this case, one would expect the natural class /i, u, æ, ɔ/ (i.e., the class $\left\{\begin{matrix}[+\text{Diffuse}]\\[+\text{Compact}]\end{matrix}\right\}$) instead of the nonnatural class /i, ʌ, æ, ɔ/, which appears in the phonetic representations. And, in fact, if we look at the situation more closely, this is exactly what we find.[22]

[21] There is a dialectal variation here which we will overlook, since it is irrelevant to the main point. The main point is the departure from naturalness in the phonetic data, and this concerns only the fact that [u] does not appear.

[22] For our speech, the situation is even more simple, since only /e/ is excluded (phonologically) before [ŋ] (=/ng/) in final position. Thus *long* (phonetically, [lɔŋ]) is phonologically /long/, as we can see by the alternant *length* (involving the Gravity shift rule that we mention below). The lowering of phonological /o/ to phonetic /ɔ/ in this and various other positions is automatic and fairly general. Phonological /ɔ/ also appears, as in *thong* (phonetic [θāŋ]), where /ɔ/ goes to [ā] by the general rule noted above, p.122). Phonological /u/ also appears, as we shall see directly.

Notice that there is no Tense-Lax contrast before [ŋ#] (as there is before [n#], [m#]), but this is regular. There is a general redundancy rule specifying Vowels as Lax before Consonant clusters (more precisely, before clusters that are not fully dental or palatal). Since [ŋ#] is phonologically /Ng#/, where N is the archi-segment [+Nasal],

—*Continued on following page*

Notice first that phonetic [ŋ] is clearly phonological /ng/, (or, more properly, /Ng/). The reasons are well-known, and we will not take the space to discuss them here.

Notice further that the problem that Householder mentions is not unique to final /ng/, but involves ALL final Consonant clusters. That is, in the context —CC#, we have phonetic [ʌ] instead of the expected [u] and thus fail to have a natural class. Hence the gap has nothing to do with [ŋ], but is a matter that involves all final phonological clusters. Thus instead of [luŋ] we have [lʌŋ], instead of [bump] we have [bʌmp], instead of [tusk] we have [tʌsk], instead of [dukt] we have [dʌkt], etc. In each case, the phonetic data fails to form a natural class because of the absence of phonetic [u].

This observation suggests that we consider adding to the phonology of English a rule

(8) u→ʌ

applying in the context: —CC# (and, in fact, in various other contexts – for details, see our forthcoming *Sound Pattern of English*; the historical basis for this synchronic rule is quite well known). If this rule can be justified, then the underlying phonological representations will have the symmetries expected in terms of Jakobsonian features, though the phonetic facts will not.

But, in fact, rule (8)is extremely well motivated. First, observe that it allows us to remove /ʌ/ completely from phonetic representations. The Lax Vowels will be simply /i, e, æ, u, o, ɔ/, and Rounding will now be entirely redundant for them (it will coincide with Gravity). Second, notice that this rule will immediately account for simple Vowel alternations, e.g., *reduce – reduction*. The Grave Diffuse Vowel of *reduce* becomes Lax [u] in *reduction* by general rules that we need not discuss here, and then is converted to phonetic [ʌ] by rule (8).

Similarly, we can extract subregularities from many irregular Verb alternations, by means of rule (8). Thus the rule (9), which shifts Gravity, is of fairly wide applicability in English 'irregular' constructions:

(9) [α Grave] → [− α Grave]. (α = + or −)

This rule will convert /sing/ to /sung/, which is converted by rule (8) to [sʌng], finally, [sʌŋ], and it applies similarly in other cases.

But most important is the fact that rule (8) is really a special case of a very general rule in English that is a synchronic reflection of the Great Vowel Shift. This rule converts Diffuse Vowels to Compact Vowels (and has various other

Footnote 22 continued—

this rule applies to the Vowel preceding it, eliminating the contrast. Hence the ONLY gap before [ŋ#] is /e/. Here, too, the situation is of course slightly more general, since we have the same gap before [ŋk#]. Thus the gap is before Nasal+Velar.

Summarizing, no restrictions need be stated on the distribution of vowels before phonological /Ng/ except for the rule that /e/ does not occur before N+Velar. All other distributional limitations are accounted for by general rules.

effects that we will not discuss here in detail).[23] Together with the Diphthongization rule (see p.121 above), it converts phonological /ī/ to [āy] (through intermediate [īy], by Diphthongization) and converts phonological /ū/ to [āw] (through intermediate [ūw], by Diphthongization). The reader will observe that rule (8) is simply the special case of this extremely general rule where the Vowel in question is Lax, Diffuse. The rule applies to all Tense Vowels, and to the Lax Diffuse Grave Vowel (i.e., /u/) and, in certain cases, to the Lax Diffuse nonGrave Vowel as well. Its primary motivation is to explain such Vowel alternations as *divine – divinity*, *decide – decision*, etc., and their well-known analogues for the other Vowels. Thus *divine* is phonologically /divīn/ and becomes /divāyn/ by Diphthongization and Vowel Shift, while *divinity* derives from underlying /divīn+ity/ by automatic Laxing before an unstressed Vowel in a nonfinal syllable. In the same way, we can account for the other alternations. But notice that this also allows us to account automatically for such alternations as *profound – profundity* from underlying /pro+fūnd/ (which becomes [profāwnd] by Diphthongization and Vowel Shift, and [profʌnd] before *-ity* by automatic Laxing and Vowel Shift, in this case, rule (8)).

We give no more details here than this, but these few comments and hints are sufficient to indicate that rule (8) is actually a special case of a very general phonological process, which is quite central to the synchronic phonology of English (and also, as is usual in such cases, reflects a historical 'sound change'). Hence there is a very strong justification for the assumption that the underlying phonological representations form a natural class.

We have gone into this degree of detail to illustrate a very important though perhaps obvious point. Nothing significant can be learned from superficial phonetic observation alone. In particular, from the observation that the phonetic distribution of Vowels before [ŋ#] is defective, nothing whatsoever follows. This defective distribution may have any number of causes. A slightly more serious look at the question shows that the defective distribution (the nonnaturalness of the class) has nothing particular to do with [ŋ], but is a property of all (phonological) final clusters. A deeper investigation of the facts of English shows that the underlying systematic phonemic forms do constitute precisely a natural class, in the Jakobsonian sense, because of the centrality of the Vowel Shift rule (and its special case (8)) in the phonology of contemporary English. In this case, Householder's conclusion about the inadequacy of the Jakobsonian framework result simply from his failure to pay attention to the facts of English structure.

[23] In particular, it converts nonDiffuse nonCompact Vowels to Diffuse, and Compact to nonCompact. Thus the phonological representation for *bee* cited above is actually incorrect, and should be replaced by /bE/, where /E/ here is the archi-segment [nonGrave, nonCompact, nonDiffuse Vowel]. The Vowel Shift rule is the central rule in the synchronic nontransformational phonology of English. We discuss it in detail in *Sound Pattern of English*.

Having shown that none of the examples that Householder adduces has any relevance to his purpose, let us now return to the conclusion that he draws from them. In commenting on the difficulties that he believes himself to have discovered, he states (p.24):

> 'In most cases this could be avoided by a willingness to abandon Jakobsonian features, but (though a few daring souls may ADD on extra features to the "systematic phonemic" set) no one seems to be anxious to construct features from scratch for each language, specifically to be maximally useful in the description of that language. There is a kind of orthodoxy which no one dares violate because no one understands the reason for it . . .'

This is a remarkable comment. Notice first that he has not shown a single deficiency in the distinctive feature system. Notice secondly that even where he thinks he has found a difficulty (because of his inattention to linguistic facts), he has not even suggested a way to avoid these presumed difficulties by abandoning Jakobsonian features. Finally, Householder not only overlooks the rich literature and serious linguistic studies that have been concerned with justifying and improving the distinctive feature system, simply asserting, casually, that this system is a mere orthodoxy which no one dares violate but for which no support has been proposed; he also confesses to inability to conceive of a reason why 'no one seems to be anxious to construct features from scratch for each language',[24] thus, in effect, he insists that there must be a new linguistic theory for each language.

[24] As has often been noted – cf., e.g., Chomsky (1964*a*:944–73 & *c*:65–110) – the assumption of a universal feature structure is made (often only implicitly) in every approach to phonology that is known, and clearly cannot be avoided. What is at issue only is the choice of features, not their universality. To repeat the obvious once again, suppose that we were to approach, let us say, English with no assumption at all about universal features (i.e., suppose we were to follow Householder's proposal). Consider only monosyllables of the form CVC. We find certain phonetic elements in initial position and others (phonetically distinct from them) in final position. Phonemic analysis of any sort requires that we somehow identify initial and final phones. With no assumption about feature structure, we can do this in any way we like. No way is more 'useful' than any other, or more 'simple', in any abstract sense. For example, if we wish to show that the maximally simple system is to identify initial p with final t and initial t with final p, it is only necessary to invent a physical feature (there are innumerably many of these) which is common to initial p and final t, and excludes initial t and final p. Call this the feature A. Following Householder, there is no reason to prefer, e.g., the feature Labial to the feature A. And furthermore, having now (à la Householder) accepted A for our analysis, we can immediately demonstrate the absurdity of using the feature Labial for the analysis of English. For consider the complexity of the feature Labial. Thus a phone is Labial only if it is in initial position and has the feature A or in final position and has the feature non-A. Obviously such a disjunctive feature does not contribute to the 'simplicity' or 'usefulness' of linguistic description (in the vague sense of these terms that Householder apparently has in mind).

It is for such reasons as this that one does not 'construct features from scratch for each language'.

VI. THE VALIDITY OF PROCEDURES OF TAXONOMIC LINGUISTICS: In Chomsky (1964*a* & *c*) it is pointed out that the notion of 'complementary distribution' is of no theoretical significance (it permits entirely unacceptable results, including even analyses that violate biuniqueness, and excludes optimal analyses) and that the other conditions that define 'taxonomic phonemics'[25] are also unacceptable. Householder discusses only the criticism of 'complementary distribution' but, because of an equivocation in his argument, his discussion is quite beside the point.

To clarify the issue, let us state precisely what is involved. We assume the notion 'contrast' defined on utterances (i.e., *pin* contrasts with *bin*, *lighthouse keeper* with *light housekeeper*, etc.). All approaches to phonology with which we are familiar require of a phonemic notation that it meet condition (10):

(10) if X contrasts with Y, then the phonemic representation of X differs from the phonemic representation of Y,

where X and Y are utterances (let us assume, phonetically transcribed). Taxonomic linguistics adds various other conditions (e.g., the converse of (10)), but this does not concern us here (our feeling is, as argued in Halle (1962 & 1964*b*), Chomsky (1964*a* & *c*), and elsewhere, that many of these additional conditions are incorrect – that is, if imposed, they define a system of representation which is not a part of linguistic structure).

A major concern of taxonomic linguistics has been to develop procedures of analysis which will guarantee that condition (10) and others will be met by a phonemic system. Interesting and valuable accounts of such procedures have been presented by Trubetzkoy, Harris, Bloch and others, and it is these that are discussed in Chomsky (1964*a* & *c*). One notion that is central to many of these procedural accounts is the notion of COMPLEMENTARY DISTRIBUTION. The 'distribution' of a phone is the class of phonetic contexts in which it occurs (these being of some reasonable length). Two phones are said to be in 'complementary distribution' if their distributions are complementary, i.e., nonoverlapping. A TENTATIVE PHONEME is a class of phones which are pair-wise in complementary distribution. The PHONEMIC SYSTEM of the language is a family of tentative phonemes that meets certain other conditions. This approach can be

[25] The conditions discussed are LINEARITY, INVARIANCE, BIUNIQUENESS, and LOCAL DETERMINACY. Householder finds the first three clear, but states that 'Chomsky says so little about [. . . local determinacy . . .] that I can't quite make out how it differs from linearity or perhaps a combination of linearity with biuniqueness'. A more careful reading of Chomsky (1964*a* & *c*) would have explained to Householder the source of his difficulty. As shown there in detail, linguists who refer freely to the 'biuniqueness principle' apparently are not using 'biuniqueness' in the technical sense of this word. It appears, from the examples they give, that what they have in mind is another principle, called 'local determinacy' in Chomsky (1964*a* & *c*) to distinguish it from 'biuniqueness', which, after all, has a clear technical meaning. But this principle has never been clearly formulated. The unclarity is, then, the fault of taxonomic linguistics and not of the attempt in Chomsky (1964*a* & *c*) to evaluate it.

and has been refined in various ways, and is intended as a procedure of phonemic analysis, a procedure which will, in particular, guarantee that (10) will be met.

In Chomsky (1964*a* & *c*) it is pointed out that the procedure, in any of its known forms, does not guarantee that (10) will be met. For example, in English the unaspirated allophone of /k/ is in complementary distribution with Lax [a] but if the two are combined in a phoneme /K/, then both *socked* and *Scot* will be represented /sKKt/, violating condition (10). This illustrates the fact that the 'principle of complementary distribution' unfortunately is not sufficiently strong to guarantee that (10) will be met. But this is its major theoretical justification (it is also pointed out in Chomsky (1964*a* & *c*) that this principle fails to permit optimal 'biunique' systems, so that its other theoretical justification fails). Consequently, the principle is apparently of no theoretical significance, and should be dropped from linguistic theory altogether.

Householder proposes to counter this argument against the definition of 'phoneme' in terms of complementary distribution by giving condition (10) the new name 'the principle of complementary distribution', and, presumably, dropping the principle of complementary distribution as defined by Harris, Bloch and others and restated above.[26] With this terminological change, he is

[26] He points out that the version of the principle of complementary distribution given in Chomsky (1964*a* & *c*) (and restated, briefly above) 'is adapted from Harris, and would perhaps not be universally accepted'. He does not go on to point out that the principle is given in the same form by everyone else who has defined the notion, and he also does not observe that in his own review of Harris (Householder, 1952) he found no difficulty with Harris' formulation of what is, after all, the fundamental notion in procedural taxonomic linguistics.

Householder also states that 'the link Chomsky makes with biuniqueness is quite puzzling to me'. This comment is quite puzzling to us, particularly in the light of the fact that he proposes to use the term 'principle of complementary distribution' for (10), which (with its converse) is just the principle of biuniqueness.

To be precise, we should point out that Householder assigns the new name 'principle of complementary distribution' not to the familiar condition (10), but rather to a stronger variant of it, namely, the principle that two phonetic segments (of reasonable length) must be phonemically distinct if they EVER contrast. This is much too strong. For example, it rules out phonemic overlapping, in most cases. Thus, take Bloch's example of a dialect with alveolar flap [D] as the variant of /r/ after [θ] (as in *throw*), and assume, as is widely true, that intervocalic post-stress /t/ has the variant [D]. Most formulations of taxonomic principles will permit assignment of [D] to /r/ after [θ] and to /t/ intervocalically, as is obviously correct. This leads to no violation of (10); nor, for that matter, does it even necessarily lead to a violation of biuniqueness. But it does violate Householder's stronger variant of (10) (assuming that phonemes are 'of reasonable length'). Since [r] and [D] sometimes contrast (namely, *berry* – *Betty*), they cannot be considered allophones of the same phoneme. Hence the post- [θ] phone [D] must be assigned to /t/ (giving /θtow/ as the phonemic representation of *throw*, counter both to intuition and to otherwise valid rules of morpheme structure).

Householder's formulation of (10) also has a consequence which he considered an absurdity in his review (1952) of Harris. Thus he objected strenuously to Harris' treatment of q and g in Moroccan Arabic as 'at all times distinct phonemes', on the grounds that they sometimes contrast, a conclusion which he finds 'staggering' (1952: 263–4). But it is just this conclusion that is required by the principle he has now proposed.

now able to avoid the conclusion that the principle of complementary distribution leads to a violation of (10), i.e., to a violation of itself. He also can conclude that the 'principle of complementary distribution', as newly defined, excludes the *socked – Scot* example. This is quite correct. Since this example was specifically constructed so as to violate (10), it follows that the principle (10) excludes it. With this ingenious defence, the term 'principle of complementary distribution' is saved, though of course the notion 'complementary distribution' and the procedural definition of the phoneme based on it are entirely given up.

It is interesting, incidentally, to note that well before the principle of complementary distribution (for the remainder of our discussion, we return to the pre-Householder usage of terms) was formulated as a basis for phonemic analysis, Jakobson gave an example of exactly the *socked – Scot* type which shows the inadequacy of any such principle (Jakobson, 1931). He pointed out that in Czech, although [i] and [j] do not contrast, [ij] contrasts with [ji]; and he proposed the following subsidiary principle of phonemic analysis:

(11) if AB contrasts with BA, then A and B cannot be assigned to the same phoneme.

This rules out the Czech case, and also the *socked – Scot* case, but it is still ineffective. Thus in English, the pre-[r] allophone of /ī/ is apparently in complementary distribution with [d], and their assignment to the same phoneme is not ruled out by (11), but if we assign them to the same phoneme D, then *dreary* will be /DrDri/ and (*two*)-*eared* will be /DrD/, violating biuniqueness (in its usual interpretation, as local determinacy). Other similar examples are not difficult to construct.

In fact, no such subsidiary principle as (11) is necessary OR sufficient. What is needed, as a defining principle for the notion 'phoneme', is simply the condition (10) itself rather than any of its special cases (such as (11)). Of course, having recognized this fact, all hope of a 'procedural' definition of 'phoneme', in the usual sense of these terms, quite disappears.

Returning to Householder's discussion, we should like to comment on several new principles of phonemic analysis that he proposes. He suggests (p.27) the principle

(12) ' "If a sequence of two segments contrasts with either of the two segments separately or with the two in reverse order, then they contrast with each other." I.e., [AB] may contrast with [BA], or [B] or [A], and all these are instances of a B versus A contrast.'

Householder proposes (12) as 'his statement' of a principle of Trubetzkoy's which was found wanting in Chomsky (1964*a* & *c*), but the relation of (12) to Trubetzkoy's principle seems obscure. In fact, while Trubetzkoy's principle was correct so far as it went, but inadequate to its purpose, Householder's 'restatement' of it is quite absurd. Thus (12) rules out the possibility of

analyzing geminates as double consonants or long vowels as double vowels, in the usual cases where this is done. For example, the lengthened /l/ of English *solely* cannot be analyzed as /ll/, because it contrasts with the single /l/ of *holy*; or to take an example from Householder himself (1952:265), his double /t/ in *thirteen* cannot be analyzed as double /t/, as he proposed, because it contrasts with the single /t/ of *thirty*. In all such cases, the usual analysis would give a sequence of two segments contrasting with one of the members of the sequence, and (12) requires that the two segments in question must be assigned to different phonemes in this case.

Householder also suggests (p.28) such additional rules of phonemic analysis as the following:

(13) 'If [XABY] is much more frequent than [XAY] or [XBY] for all values of X and Y, then the advantages of treating AB as a unit phoneme should be considered.'

The only emendation we would suggest is that the advantages be considered under any other circumstances as well. As to the other rule that he discusses (p.28), we have nothing to say. It is far too vague to discuss, and we ask only why he calls it a restatement of a rather clear (though AD HOC) rule of Trubetzkoy's.

In a recent paper, Vachek (1964) offers a defence of Prague phonological theory against the criticisms that we have just been discussing. This defence, however, is in reality merely an acknowledgement of the justice of the criticisms. Thus Vachek admits that the principles of Prague phonology are in fact inconsistent with the facts cited as counter-examples, but he suggests that this is not important because these principles are valid for all cases except those of a certain specific category, namely, the category of cases where the language is in a 'state of flux.' Accepting this view, we must then ask what the defining criterion is for this category of cases. The answer is that this is the category of cases for which Prague phonological principles fail. There is no independent way of identifying this class. Conclusion: the theory in question works for all cases except those for which it fails; Vachek is claiming no more than this.

There is no disagreement between Vachek and us as to the correctness of particular analyses. Clearly, then, we share some notion of descriptive adequacy, a notion that is not captured by Prague phonological theory, and is in fact, in conflict with the assumptions of this theory in the cases mentioned. For this reason, the theory must be revised, and it is just such a revision that we have been proposing.

VII. THE PHONETIC BASIS OF PHONEMIC ANALYSIS: The linguistic examples used in Chomsky (1964*a* & *c*) are, for the most part, simply taken from the authors whose principles of phonemic analysis are discussed. But Householder doubts the phonetic accuracy of the analyses

given by Bloch, Harris and others, which are accepted in Chomsky (1964*a* & *c*) as a basis for discussion of their principles. For example, he objects to Bloch's example of overlapping in the case of the flaps of *throw* and *Betty* (*r*:*tt*), claiming that the 'basis for phonetic identity' is unknown to him, in such cases. He goes on to state (p.28): 'I have never yet seen a good example of phonemic overlap where this escape route [namely, the assumption that the phones in question are physically distinguishable] was not open'. Thus all examples of overlapping can be eliminated, by a sufficiently fine phonetic analysis. And the same is true of all the other examples given in Chomsky (1964*a* & *c*). That is, all of these examples are based on certain assumptions about phonetic identity of physically distinct occurrences, and it is always possible to insist upon a finer and more detailed phonetic representation in which these distinct occurrences are phonetically distinguished. In other words Householder is making the point that distinct physical events can (with sufficiently fine measurement) be distinguished from one another, and that certainly, two phones in different phonetic contexts can generally be shown to be physically distinct in some respect.

How can it be, then, that Bloch, Harris and others overlooked this insight in presenting and discussing their examples? The answer is not difficult to find. They knew perfectly well, of course, that it is possible to give a phonetic analysis so fine that phones in different contexts are differently represented – so that, in particular, the flaps of *throw* and *Betty* can be distinguished. But they were also aware of the fact that no phonemic analysis can be based on a phonetic transcription so fine. This truism Householder seems to have overlooked. Obviously a very narrow phonetic transcription makes any distributional phonemics impossible, since distributional procedures will lead nowhere unless phones and contexts repeat sufficiently often. Knowing this, Bloch, Harris, and others who are concerned with developing phonemic theory base their analyses on a phonetic theory which is not so narrow as to exclude sufficient repetition, and, at the same time, gives rise to the cases of overlapping, etc., which they discuss. The dilemma that Householder has placed himself in is this: true, he can insist on a phonetic analysis so narrow as to eliminate the examples of Chomsky (1964*a* & *c*), in which case distributional phonemics of the sort he wishes to defend is also ruled out as vacuous; or he may require only a phonetic analysis of the type assumed by Harris, Bloch, etc., in which case distributional procedures will apply nonvacuously, but the counter-examples discussed in Chomsky (1964*a* & *c*) will also arise.[27]

[27] In the same connection, Householder discusses Harris' example of an English dialect in which the phonetic distinction of *writer* – *rider* is length of the Vowel. This seems to him an unacceptable example, and he gives an irrelevant disquisition on instrumental phonetics to illustrate the difficulties of defining length. The problem that disturbs him is, apparently, that some occurrences of the 'shorter' phone may be physically longer

—*Continued on following page*

Householder concludes this discussion (p.29) by pointing out that the difficulties presented in Chomsky (1962*a* & *c*) exemplify precisely 'the morass' of problems 'that the phonemic principle was intended to extricate us from long ago'. We agree, but we add that as shown in Halle (1962 & 1964*b*), Chomsky (1964*a* & *c*) and elsewhere, the phonemic principle, as developed in modern taxonomic phonemics, apparently fails in this attempt and simply introduces a rash of new problems of its own, although what we called 'systematic phonemics', a phonological theory based heavily on earlier ideas of Sapir's combined with a version of Jakobson's distinctive feature theory and various other new notions (e.g., the transformational cycle), does seem to extricate us effectively from this morass. These arguments Householder does not deal with at all.

This completes our discussion of the main issues raised by Householder. Turning now to his summary, we state his major conclusions and, briefly, the result of our analysis of them.

Householder's first conclusion (p.32) is this: 'Halle appears to offer a demonstration that feature matrix notation is more economical than phonemic notation. This is seen to be clearly false.' What is false is that anyone has even attempted this absurdity. In fact, we have no idea what it might mean to show that matrix notation is more economical than phonemic notation, or the opposite. What we are interested in is an entirely different question, as discussed above.

His second conclusion is that the feature system does not provide natural classes. As we have shown, he has not given a single argument to support this conclusion.

His third conclusion is that we have given no argument to justify the incorporation of morpheme structure rules in the grammar. This conclusion is based entirely on a complete and thorough misinterpretation of what we say about evaluation procedures, and is therefore worthless. In fact, the argument

Footnote 27 continued—

than some occurences of the 'longer' phone. This is surely true. But surely it is unnecessary to repeat here the introductory lecture to an elementary phonetics class in which it is pointed out that phonetic representation is based on normalization (with respect to speed of utterance, loudness, etc.), and that phonetic features are relative rather than absolute.

In any event, the whole discussion is quite beside the point. The very same argument could have been given for the dialect in which the distinction is one of quality rather than length (i.e., with *writer*=[rʌyDr]=/raytr/ and *rider*=[rayDr]=/raydr/).

Householder comments (fn.14) that 'Halle uses a similar example in which the difference is qualitative (1962:63) [a] vs [ə]. This is more realistic.' But this is surely a slip of the pen, for we do not doubt that he is familiar with the fact that in certain dialects of English the difference between the two variants of the diphthong is one of length (/a:/ vs /a/) whereas in others it is one of tenseness (/a/ vs /ə/) (cf., Kurath & McDavid, 1961: maps 26–27). If indeed Householder means what he seems to be saying and regards the latter dialects as 'more realistic' than the former, then he must also regard German as more realistic than English or Latvian as more realistic than Igbo, a conclusion that seems to us as unbelievable as it is unavoidable.

for incorporating such rules, as given in Halle (1962 & 1964*b*) and Chomsky (1964*a* & *c*), and again above, is quite simple and straightforward.

His fourth conclusion (p.32) is that 'phonology has a structure which is independent of the syntactic structure of a language'. If this means that no phonological rules refer to syntactic structure, we have given abundant (and unchallenged) evidence that he is wrong. If it means that there is a taxonomic phonemic level, independent or relatively independent (in the Pike or Harris sense) of syntax, we have, again, given so far uncontroverted arguments that this is false. If it means that a grammar must contain an independent 'phonological grammar', we have argued (a) that this is superfluous, since the new addition simply repeats what is already fully accounted for by the grammar to which it is added, and (b) that it leads to a loss of explanatory adequacy, and this argument, too, is so far uncontroverted. If it means (Householder, 1965: 19) that 'phonological structure . . . can be changed completely without altering any of the syntactic structure of a language', then we agree, but fail to see what this remark has to do with anything we have ever said, or why it is of any interest, or why it implies that (in the usual sense of these words) phonology is independent of syntax (it seems to us, rather, that it implies that syntax is independent of phonology). What else this comment might mean we do not know.

His fifth conclusion is that taxonomic phonemics must be part of grammar. But he has given no justification for this claim and has not dealt with our counter-arguments in Halle (1962 & 1964*b*), Chomsky (1964*a* & *c*), and elsewhere. We therefore see no reason to discuss this claim.

His sixth conclusion is that distinctive feature theory is clumsy and must be revised or abandoned. No argument has been offered for this assertion, which is, therefore, simply to be disregarded.

His seventh conclusion is that Halle seems to overlook the distinction between systematic phonemics and systematic phonetics (though Chomsky is not guilty of this oversight). True, this distinction is not discussed explicitly in Halle (1962 & 1964*b*), not being germane to the issues raised there. By the same logic, he can show that Chomsky overlooks the existence of Elizabethan English (though Halle seems aware of it (1962 & 1964*b*)), and that both of us overlook the existence of elephants.

His eighth conclusion is that there are several different types of features. This is surely true. We do not see the relevance of this familiar observation here, and are unable to discern any new contribution to this interesting question in Householder's paper.

His final conclusion is that our approach somehow blocks the path of inquiry. We would be disturbed, naturally, if this were true, but see little reason to believe it. We think that we have presented good reasons for the belief that taxonomic linguistics of the sort that Householder defends has been

far too limited in its interests, and that these limitations have, in fact, led to the wide adoption of incorrect assumptions about the nature of language. A return to many traditional questions seems to us very much warranted; and, in fact, some of the traditional answers (in particular, Sapir's views with respect to phonology) seem to us to provide a very good basis for further progress. Taxonomic linguistics has provided a wealth of new data, higher standards of rigor, and many useful ideas. But we have tried to show that the framework within which it operated was fundamentally wrong, in many serious ways. We have tried to show that it is quite possible to transcend these limitations and to face problems of descriptive and explanatory adequacy in new and promising ways. We see no respect in which the path to inquiry is blocked either by our critique of taxonomic phonemics or by our suggestions regarding what we have tried to show is a better supported and more effective theory of sound structure.

APPENDIX

In the course of his discussion of the major issues outlined above, Householder makes many incidental remarks and observations that we would like to comment on briefly, even though they do not touch on questions of general linguistic interest.

In the first place, the reader familiar with Householder's paper will recall that much of it is concerned with defending Householder's own notions of the organization of grammar, the nature of phonemes, and so on, and with the relation of his conceptions to ours. He is especially concerned to defend his ideas from our critique of taxonomic phonemics. We have not discussed this aspect of Householder's paper above, since there is no explicit reference to any of Householder's views in our critique of taxonomic phonemics. As noted in Chomsky (1964a:951, n.40 & *c*:75, n.13) the critique was limited to positions that are sufficiently clear and well-formulated so that critical analysis might be useful and productive. Neither in his paper (1965) nor elsewhere, to our knowledge, has Householder given an account of his notion of 'phoneme' for example, which meets this condition. Since many clear and careful formulations do appear in print, we saw no reason earlier and see no reason now to discuss positions such as Householder's which are only vaguely hinted at. In particular, we can suggest no answer to his question (1965:31) as to how phonemes, as he thinks of them, fit into the phonological component, and we can offer no opinion as to whether the notion he has in mind is, as he claims, immune to objections that we have raised to taxonomic phonemics. There is no way to determine whether this is true, and the question cannot be raised seriously until his views are presented with sufficient explicitness and clarity to make critical scrutiny worthwhile.

Householder's discussion of the question of levels of adequacy leaves us somewhat in the dark as to what his actual views may be. His objections to the three levels of adequacy proposed in Chomsky (1964*a* & *c*) are contained in two consecutive paragraphs (1965: 14–15). In the first paragraph, Householder declares that of the three levels of adequacy, only ' " observational adequacy" is intelligible (at least to me)' and he takes Chomsky to task for viewing observational adequacy as uninteresting. In the next paragraph Householder goes over the same ground again, this time however, coming to the conclusion that observational adequacy 'is surely not an acceptable goal to any linguist'; i.e., precisely the view that in the preceding paragraph he found so unacceptable when expressed by Chomsky. Moreover, observational adequacy is no longer the sole intelligible goal. According to the second paragraph, descriptive adequacy is not only intelligible, but also quite 'old hat', in fact; something on which 'the Firthians were keen . . . long before Halle'.

Householder points out (1965:3) that it is necessary to distinguish those occurrences of /t/ and /k/ that undergo softening to /s/ from others that do not, by a separate categorization in the lexicon. This is quite correct. Thus the lexicon must distinguish Romance words from words of Greek origin (e.g., *monarchy*, cited by Householder), and both sets

from the Germanic part of the vocabulary. Obviously, these subparts of the lexicon behave rather differently with respect to phonological processes and this fact must be indicated by a lexical categorization. Unfortunately, the other example that Householder gives to illustrate this point has nothing to do with it. He states that the /t/ of *rackety* must be distinguished from the /t/ of *democrat*, because the former does not undergo softening before *-y* in *rackety*, while the latter does, in *democracy*. But the *-y* affix, in the case of *rackety* has no connection with the *-y* affix of *democracy* or *monarchy*. In fact, before the Adjective-forming *-y* of *rackety*, the t-s alternation never takes place, because this affix is one of those which has an automatic # boundary before it (as distinct from the other, Noun-forming *-y* affix). We see this from the fact that it is neutral with respect to stress, leaves final clusters in word-final phonetic form, etc.

Referring to Halle (1962:71 & 1964*b*:351), Householder asserts (p.29) that 'Halle achieves the remarkable position of specifying distinctions which are present (apparently) only in the brains of speakers'.[28] We see nothing strange in the conclusion that perceptual distinctions may be heavily determined by SET and need not correspond in any simple way to physical stimuli. This is a commonplace of perceptual psychology and we fail to understand why anyone would think it remarkable that speech perception shares the properties of other perceptual processes. In the case of language, the speaker's 'set' is largely a matter of his knowledge (see note 2) of the language, and this may (in fact, surely does) lead him to make perceptual judgments that are not simply related to physical fact. For example, speakers of English may be trained to record stress contours of great complexity with fair consistency, and we have no doubt that what they record is an accurate account of what they 'hear'. But we doubt very much that these stress contours are physically present in a degree of detail that even begins to approach what they 'perceive' in the utterances that they are transcribing. Since stress contours are, apparently, largely a reflection of syntactic structure, it is sufficient, to account for the perception, that the hearer understand the utterances. Similarly, we see nothing strange about the conclusion that an underlying base form may be 'perceived' (or internally represented in some way in the process of interpreting an utterance) although it corresponds to no identifiable physical part of the sound stimulus. And we see no difficulty in accepting the hypothesis, which seems so outlandish to Householder, that a systematic phonemic distinction can persist (because of its role in the system of phonological processes) despite the fact that there is no one-one correlation of the systematic phonemes to identifiable physical phones.

Nor is this hypothesis new in linguistics. Surely every student in an elementary linguistics course is made to read Sapir's 'Psychological Reality of Phonemes' with its classical discussion of instances where naive speakers heard speech not in terms of actual physical sounds, but in terms of an underlying abstract form, which may correspond, in fact, to an etymological reconstruction (Sapir, 1933:49). Hence Halle's 'remarkable position' is actually a classical one within linguistics, and one that is not only supported by linguistic evidence but consistent with whatever else is known about complex perceptual processes.

Householder argues (1965:30) against the claim in Chomsky (1964*a* & *c*) that there is

[28] The comment is based on the following remark in Halle (1962:71 & 1964*b*:351) 'No difficulties are experienced in accounting for the change if we postulate that, for reasons of the kind discussed in §11, /a/ and /æ/ remained distinct entities even though every /a/ was actualized phonetically as /æ/.' In quoting this, Householder leaves out the phrase 'for reasons of the kind discussed in §11 . . .'. What is discussed in §11 is a Russian example that shows how 'simplicity considerations force us to maintain distinct representations of segment types that never contrast phonetically', despite the fact that, superficially, 'it seems pointless to use different feature complexes to represent segment types that are never distinguished phonetically'. In fact there are many different considerations that might lead to the conclusion that segment types must be distinguished phonologically despite the absence of any phonetic contrast between these feature sets.

Householder does not attempt to show some error in the analysis presented by Halle but simply implies that the conclusion that he affords is, for some reason, intolerable or absurd.

no level intermediate between systematic phonetic and systematic phonemic, on the basis of the fact that there are two different kinds of rules in the phonological component. But in fact, as noted by Chomsky, there are not just two but many different kinds of rules, of varying generality and function. What is pointed out by Chomsky is that there is no particular point in derivations where it seems possible to extract a new and independent linguistically significant level. Various arguments are given for this, and these Householder makes no attempt to deal with.

Householder asks (1965:31–32) how we arrive at our systematic phonemic representation.[29] He makes no reference to the discussion of this question in Chomsky (1964*a*:547–48 & *c*:95–96), and simply asserts, with no shred of evidence or argument, that in practice, the procedure is first to construct a taxonomic phonemic representation ('making stout use of complementary distribution', and other taxonomic procedures), and then, presumably, to go on from this to the systematic phonemic representation. This claim raises two questions: (1) How does a taxonomic phonemic representation, once available, facilitate the discovery of the systematic phonemic representation, in particular, since this preliminary taxonomic representation apparently has no linguistic status? (2) How does Householder know that Sapir, Bloomfield, and other linguists who presented systematic phonemic representations actually went through the intermediate step that he claims to be essential, in the light of the fact that they left no evidence of this and, furthermore, that taxonomic phonemics had not been invented at the time when they were working?

The fact of the matter is that the necessity of working through a preliminary taxonomic phonemic analysis is, so far as is known, a complete myth. A variety of arguments have been offered to show that taxonomic phonemics has no justification on INTERNAL linguistic grounds (quite the contrary, it can be incorporated in a grammar only at the cost of loss of generalizations) and that the principles on which it is based are faulty. No answer has been offered to these arguments. It has also been pointed out that there are no known EXTERNAL grounds (i.e., in terms of what is known concerning use of language or language-acquisition) for the assumption that taxonomic phonemics constitutes a significant linguistic level. Again, no answer is proposed to these arguments. What we are left with is a totally unsupported claim that somehow taxonomic linguistics is necessary for the practicing linguist analyzing a new language. As far as we can see, this claim is not worth discussing until some argument is given for it. Returning to the two questions raised above, we see not the slightest reason to believe that the discovery of a grammar will be facilitated by a prior careful taxonomic phonemic analysis, constructed in complete or (with Pike and Harris) partial independence of any investigation of syntactic or morphological structure and, furthermore, playing no role in the grammar, once this is constructed. And we see not the slightest reason to believe that Sapir, for example, followed this curious procedure in his field work.

In Householder's discussion of our proposed evaluation procedure, he attributes a certain general principle to Halle in a passage which we quote in full:

> '[Halle] does not there state the principle explicitly but I should word it thus:
>
> "No symbol-system may be judged economical unless it is always the case that the more general statement, when expressed in it, is the shorter".
>
> Where Halle got the idea for this principle, I cannot discover. My friends in various other fields (including philosophy of science) have not heard of it, and obvious counter-examples come readily to mind.' (1965:16)

[29] And also, how we arrive at a set of features. But the two questions are not at all comparable. The set of features is provided by our general linguistic theory (along with our other notions concerning the form of grammar). The systematic phonemic representation for a particular language, on the other hand, has to be discovered by the grammarian analyzing this language.

To the question how one arrives at the hypothesis (in general linguistic theory) that a certain set of features constitutes the universal phonetic framework for language, we can offer no helpful answer, just as we can offer no suggestion as to how one discovers the proper notion of 'transformation', 'morpheme', etc.

We are as ignorant and surprised as Householder as to the sources of this completely absurd principle. We are able to find nothing in Halle (1962 & 1964*b*) that could lead any reader to conclude that such an absurd principle was being advocated, either explicitly or implicitly. In fact, the principle is a complete invention of Householder's.

In the light of the preceding discussion, it is perhaps understandable that we are unimpressed with Householder's repeated admonitions to us for leading our 'followers' astray by imbuing them with disrespect for the facts. We wish only to note in this connection that here, once again, Householder cites no evidence in substantiation of his charge that linguists who have been interested in or influenced by our work have no regard for linguistic fact and fail to meet common standards of accuracy and seriousness.

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