1. The Psychological Reality Problem

Suppose that someone succeeds in writing a grammar which correctly enumerates the sentences of a language and assigns them the right structural descriptions. Such a grammar would ipso facto correctly represent the substance of a fluent speaker's knowledge of this language. But it would not necessarily represent the form of this knowledge in the sense of actually corresponding to the system of rules which is internalized by the speaker and constitutes part of what enables him to produce and understand arbitrary utterances in the language. Similarly, the knowledge of someone who has learned arithmetic, that is, the infinite set of correct arithmetical computations, could be defined by many different systems of rules, including both psychologically incorrect ones, such as certain axioms of set theory, computer programs, and so on, and the psychologically correct one, namely whatever knowledge is actually used in arithmetical performance, such as the rules of school arithmetic and the multiplication table. How do we know that generative grammar is not psychologically as wrong a model of linguistic competence as set theory is of arithmetical competence?

The linguistic universals which linguistic theory specifies include fixed notations in which grammars are written and an evaluation measure, which together establish a hypothesis about which of the innumerable grammars that might characterize the sentences of a language possesses psychological reality in the sense of representing the form rather than just the substance of a fluent speaker's competence. From among the pile of generalizations that might be made about the sentences of a language they select certain ones as being linguistically significant and corresponding to the generalizations that a child hearing such utterances would actually arrive at in constructing his grammar. The question, then, is how the various aspects of this hypothesis are justified.

For many features of universal grammar there is justification enough in the fact that without them it would simply not be possible to write grammars that account for the sentences of a language. Particularly in syntax, as Chomsky has pointed out, the typical problem is not choosing the right one among various theories that work but finding even one that will work at all. But with conventions which are essentially abbreviatory in nature, such as braces and parentheses, among others, real problems of empirical justification can arise. A grammar can always be replaced by

---

I would like to thank J. R. Ross and W. G. Moulton for suggesting to me many improvements for this paper. But they do not necessarily agree with me, and the responsibility is mine alone.

---

1 This work was supported in part by The National Institutes of Health (Grant MII 13390-02).
another, descriptively equivalent one, in which any one of these abbreviatory notations is not made use of. There could not be a language whose sentences could be enumerated one way but not the other. Then what is the empirical force, if any, of such notational conventions?

For example, most linguists would agree that two rules of the form

\[ X \rightarrow Y \]
\[ Z \rightarrow Y \]

if not separated in the ordering by any other rule, should be combined by factoring out their common right hand side as follows:

\[ \{ X \} \rightarrow Y \]

We would say that the braces represent a linguistically significant generalization about these two rules. But how do we know that they do? How would we justify this convention to some linguist \( A \) who maintained that it was wrong and that the two rules should be kept separate? Or, to take a more likely contingency, how would we justify it to another linguist \( B \) who maintained that neither theory makes any verifiable truth claim as against the other, that since they are mutually convertible notational variants of each other, they represent equivalent hypotheses about the speaker's internalized grammar?

There are no conscious a priori ideas of generality that we can appeal to here in the way that we can appeal to intuitions that reflect features of structural descriptions, such as ambiguity and synonymy. The processes of normal language learning being unconscious, we have absolutely no ideas about the form of grammars, though we have clear ideas about the forms of sentences which grammars account for. It is true that the practicing linguist soon acquires ideas about the form of grammars and such concepts as generality. But these ideas are somehow the result of his work on languages, and we would like to know what the ideas are based on.

Nor is the fact that a generalization can be stated enough to show that it is real. All sorts of absurd notational conventions can easily be dreamed up which would express the kinds of spurious generalizations that we would want to exclude from grammars. It is necessary to justify conventions by showing that the generalizations they allow one to express do not hold accidentally. One might try to do this by arguing that a convention which can be used frequently in grammars cannot represent an accidental fact about language. But many presumably spurious conventions would come in handy very often in linguistic descriptions. For example, what about a linguist \( C \) who says that the brace notation should be extended to collapse rules of the form

\[ X \rightarrow Y \]
\[ Z \rightarrow X \]

into the form

\[ \{ X \} \rightarrow Y \]
\[ Z \rightarrow \{ X \} \]

He will be able to show us just as many cases where such braces could be used in grammars to group rules together. Or, to cite an actual linguist, Pāṇini often makes use of an abbreviatory convention which corresponds to the following kind of use of braces, ruled out in generative grammar:8

\[ A \rightarrow \{ B \} \]
\[ E \]
[\( C \)]
\[ D \rightarrow \{ F \} \]
[\( G \)]

What seems wrong about this is that it allows collapsing rules which represent heterogeneous processes. Of the rules combined here, two have absolutely nothing in common with each other: the top rule

\[ A \rightarrow B \]
[\( C \)]

and the bottom rule

\[ D \rightarrow F \]
[\( G \)]

But we would like the rules in a grammar to form blocks whose parts are related in some sense that goes beyond just partial identity. Ideally, the

8The reason is that abbreviatory conventions in the Indian grammatical tradition, originally an oral one, were not graphic devices such as braces, parentheses, or anything directly equivalent to them, but rather resembled the conjunction-reduction processes of natural language. In the following three sutras, for example, the bracketed words are omitted and understood as carried over from the previous sutra.

6.1.77 \( tko yan eei \) (high vowels become glides before vowels)
6.1.78 \( eco'vavyāvah \) (\( e, o, \ldots \rightarrow \) ay, au, \ldots before vowels)
6.1.79 \( [ceo] vānto yi pratyayē (o, au \rightarrow eu, āu before y) \)

It is hardly possible to collapse these three rules into one rule by any extensions of the conventions of generative grammar.
rules should be grouped into natural blocks whose parts represent different aspects of the same basic process.

Can psycholinguistics provide experimental evidence on the form of grammars? Recent psycholinguistic experiments designed to test the psychological reality of generative grammar have been concerned mainly with two questions. One group of experiments has sought behavioral correlates to the structural descriptions postulated by generative grammar. Bever, Fodor, and Garrett have, for example, carried out a series of experiments in which they found that the location at which a burst of sound is perceived in a synchronously presented sentence differs from its objective location in a way that can be predicted from the surface constituent structure of the sentence. The goal of another group of experiments was to find evidence bearing on the claim that a system of rules such as that postulated by generative grammar is involved in producing and understanding utterances. In contrast with the successful experiments concerned with the psychological reality of structural descriptions, those concerned with the psychological reality of grammars have on the whole been a failure (Fodor and Garrett, 1967). It is true that there was an initial spate of successes in which a clear relationship seemed to emerge between the grammatical complexity of a sentence, as measured by the number of rules of the grammar that contribute to its formation, and its perceptual complexity, as measured by various experimentally obtained performance parameters. But in recent experiments with more complex linguistic material this relationship has all but disappeared. It stands to reason that the utilization of the speaker's internalized grammatical rules is a highly complex process involving elaborate ways of tracking down the relevant rules and processing sentences in such a way that parameters which tap performance directly are not going to be related at all directly to such crude grammatical properties of sentences as the number of rules involved in their derivation. The fact that grammars are not performance models presumably means that the answer to the question of whether they are correct competence models is not likely to be forthcoming by any currently known experimental techniques until the contributions of competence can be separated out from the facts about performance.

What we really need is a window on the form of linguistic competence that is not obscured by factors like performance, about which next to nothing is known. In linguistic change we have precisely such a window.

2. The Form of Linguistic Change

We can think of linguistic change in roughly the following terms. Grammars are subject to changes of two kinds: the addition of new rules to them and simplification of them. In phonology, the addition of rules corresponds roughly to the concept of ‘sound change’ (Halle, 1962; Postal, 1968). For example, the sound change whereby final obstruents in words became voiceless in German and many other Germanic languages is the addition of the rule

\[
\text{[+obstruent]} \rightarrow \text{[−voiced]}
\]

Through alternations such as [bunt];[bunde] (versus [bunt];[bunte]), in which this rule is reflected, it is learned anew as part of the language by each generation of speakers, and even in modern German the underlying representations of most words retain the medially pronounced voiced segment. Yet the addition of Rule 1 does not leave the lexicon entirely unaffected. Words like ab, ob, and weg, which never came to stand before an inflectional ending that would cause the reappearance of an underlying voiced obstruent, are never heard after the sound change with anything but a voiceless final obstruent; in these isolated forms, succeeding generations of speakers therefore have no reason to set up underlying forms with voiced obstruents. The change thus brings about a restructuring in a tiny corner of the vocabulary.

I hope that this use of the term generation will not convey the absurd picture of a society horizontally segmented into a number of discrete age groups, each with its own grammar. The point is simply that a language is not some gradually and imperceptibly changing object which smoothly floats through time and space, as historical linguistics based on philological material all too easily suggests. Rather, the transmission of language is discontinuous, and a language is recreated by each child on the basis of the speech data it hears. Nor should the term restructuring be understood as denoting a change of some speaker’s grammar into another grammar, for it refers just to a discontinuous linguistic change arising from the difference between the grammar constructed by a child and the grammar of those whose speech constituted his linguistic experience. In discussing linguistic change in these elementary terms we are, of course, missing a number of important factors which cannot in the long run be ignored. For example, as Jakobson has pointed out, metalinguistic information concerning such things as the social value of different speech forms is an important part of what a speaker knows, and Labov’s recent studies (1963, 1965) show its diachronic relevance very clearly. A conception of grammar in which these broader aspects of competence are explicitly accounted for will hopefully provide a general basis for the study of their role in linguistic change.

A sound change that I will frequently refer to is umlaut in German. By this rule, vowels were fronted before i (for example, Old High German
wurmi > wïrmi ‘worms’, täti > tâti ‘deeds’, nōti > nîti ‘needs’). Short a was not only fronted but also raised to e (for example, slagi > slēgi ‘strokes’, gasti > gêsti ‘guests’). The original umlaut rule, then, was the following:3

2. \[ V \rightarrow [-back] \] / \[ < -long > \] / \[ < -low > \] / \[ \rightarrow G \]

In modern German we encounter this rule in a somewhat different form. In the majority of dialects what we find as the productive umlaut of a is not e, as originally, but a e. For example, in the Low German dialect of Priegnitz (Mackel, 1905—1907) we have gast:gest, kraft:kreftig with a low front vowel in the unumlauted forms, rather than the expected gast:gest, kraft:kreftig. But the only e’s that have thus gone to a are those that were productively unumlauted from a. Phonemic e’s have remained unchanged. These include not only original Germanic e in words like nest but also e from historically unumlauted a in words like bet ‘bed’, net ‘net’ where e has become phonemic since there was no reason to derive it synchronically from an underlying a. Analogous facts hold true in Old English as well. In terms of the grammar, this widespread change is a simplification of the umlaut rule from its original form of 2 to the form in 3:

3. \[ V \rightarrow [-back] \] / ... 

(I leave open here the question of what exactly the environment of umlauting in modern German is, which is irrelevant for present purposes.)

The change from 2 to 3 is an instance of the second basic type of linguistic change, simplification.4 I shall merely illustrate this type for the moment but hope to justify it in more detail later. Simplification is a generalized and reinterpreted version of the traditional concept of analogy (Matthews, forthcoming; Kiparsky, 1965, 1967). This is particularly evident in its simplest form, namely morphological regularization as in changes of the type brought > bringed, which amount to loss of the special mark associated with lexical entries like bring that singles them out as morphological exceptions and specifies the nature of their exceptional behavior. Much more interesting in many respects are cases in which the simplification affects the rules of the grammar rather than the lexicon. Quite commonly, such simplification leads to the loss of parts of rules from the grammar, as in the change of the umlaut rule just cited, where what is lost is the part of the rule which raises a. The process may even lead to the loss of entire rules. For example, Rule 1, which devotes word-final obstruents and once was common to all dialects of German, has been lost in some dialects of Northern Switzerland as well as in some varieties of Yiddish. In place of bunt:bundes they have bund:bundes, with the morphophonemic distinction of voicing now again appearing phonetically in word-final position. We know that these languages once possessed Rule 1, as it has permanently affected the handful of isolated words like (a)vek ‘away’, ap (Yidd. op) ‘away’, which had a voiced final obstruent but lost it even morphophonemically after the phonetic devoicing took place because retention was not motivated by any inflected forms. Hence there was also no basis for reintroducing the voicing in these words once Rule 1 had dropped out of the language by simplification.

It is also evident that the order of rules in a grammar is subject to historical change. Later, I will try to show that this is a special case of simplification; right now a few examples will do. By a historically fairly old rule of Finnish, underlying long mid vowels are diphthongized, for example, vee > vie. Subsequently, the loss of certain medial voiced continuants introduces new long mid vowels, for example, teye > tee. In standard Finnish, these new long mid vowels stay, and the rule introducing them must therefore follow the diphthongization. That is, the order is

\[ a. \text{diphthongization} \]
\[ b. \text{loss of medial voiced continuants} \]

Yet in many dialects of Finnish the new long mid vowels have subsequently come to join in the diphthongization, for example teye > tee > tie. What this means is that the order of the rules has changed to

\[ a. \text{loss of medial voiced continuants} \]
\[ b. \text{diphthongization} \]

Notice also what it does not mean. It does not mean what anyone coming from traditional historical linguistics automatically tends to think it means, namely, that in standard Finnish, where tee from teye does not diphthongize, the diphthongization rule is not ‘productive’. On the con-
trary, it is perfectly productive since it must apply to underlying forms like *vee*, in which the underlying long mid vowel must be assumed because of morphophonemic rules such as those for past formation, for example, *vee + i > vei* like *saa + i > sai*, as McCawley (forthcoming) has shown. The difference between the two kinds of dialects has nothing to do with the productivity of the diphthongization rule but simply with its order with respect to the loss of medial voiced continuants.

An example of reordering which once again involves the uumlaut is the following. In the dialects of Northeastern Switzerland the back mid-vowel *o* becomes lowered to *ɔ* if it immediately precedes a dental or palatal (nongrave, or what Halle now calls a coronal) true consonant or *r*. Compare, in the Kanton of Schaffhausen (Wanner, 1941):

Retention of *o*:
- before *l*: foll, holts, gold
- before labials: grob, ops, hobal, xnopf, doba, ofa, xopf
- before velars: xoxxa, xnoxxa, roks, kflaga, boga.

Lowering to *ɔ*:
- before *r*: harn, torn, šara
- before dentals and palatals: rass, xrratta, lɔsa, ksatta, bɔda, pošt.

The distribution of allophones is given by the rule

4. $V_{-\text{high}} \rightarrow [+\text{low}]_{-\text{gravity}}$ $\rightarrow [+\text{consonantal}]_{-\text{lateral}}$

It is necessary to restrict 4 to the back vowels. The uumlauted variant *ő* of the vowel *o* is not lowered. The plurals of *bọga* and *bọ da* are *bọga* and *bọda*, both with a mid *ő*. Hence the relative order of 4 and uumlaut must be

a. Rule 3 (uumlaut)

b. Rule 4 (lowering)

This is the situation in some dialects on the northern fringe of Switzerland. Elsewhere a different state of affairs obtains.

I will take a dialect which in all other relevant respects is identical to that of the Schaffhausen area, namely that of Kesswil, in neighboring Oberthurgau (Enderlin, 1911). Rule 4 operates in unmodified form here too. All the vocabulary items cited above for the Schaffhausen dialects are found, with the same distribution of *o* and *ɔ*, in Kesswil. But the difference is that Kesswil, along with most of Northeastern Switzerland, has *ő* as the uumlauted form of *o*, but *ʒ* as the uumlauted form of *ʒ*. In these dialects the plural of *bọga* is *bọga*, but the plural of *bọda* is *bọda*.

The solution which first might come to mind is that the lowering rule in 4 was simplified to apply to rounded vowels regardless of whether they are front or back. But this fails since phonemic *ő* does not lower to *ʒ* in the environment of 4. The crucial cases are such forms as *plọtsli* and *frọsli* ‘frog’ (originally a plural form). The behavior of these isolated forms whose vowels are not lowered shows conclusively that we are in reality not dealing with a lowering of *ő* to *ʒ* at all, but rather with the uumlauting of *o* as well as of *o*. That is, the order of the rules has now become

a. Rule 4 (lowering)

b. Rule 3 (uumlaut)

Applying to the same underlying forms as before, these rules now produce the segment *ʒ*, which did not arise under the old ordering.

3. A Criterion for Psychological Reality

Returning after this brief survey of some main types of phonological change to the initial question about the justification for assuming the psychological reality of generative grammar, suppose that we now raise this question about some aspect of generative grammar, such as the requirement that grammars contain a certain level of representation, or that they be written with the use of certain notational conventions. The conception of linguistic change sketched out above, in which linguistic structure crucially figures at several points, suggests as one test for determining the answer that we ask the question: Do the levels, the kinds of rules, and so on, which are required by this theory ever play a role in linguistic change? Taking as our example again the simple case of the brace notation, we can ask: Do blocks of rules collapsed by braces form units of a kind which can undergo systematic change? If they do, this will be a powerful argument for this notation, and if not, we will have prima facie evidence that it is a spurious notation. On such questions, evidence of the following kind can be found.

In English, underlying long vowels, which are otherwise realized as diphthongs, are shortened in two main phonological environments: before two or more consonants (for example, *keep:kept*) and in the third syllable from the end of the word (for example, *vain:vanity, severe:severity*). The rules which bring these shortenings about are the following:
5'. $V \rightarrow [-long]/\_\_\_CC$
5''. $V \rightarrow [-long]/\_\_\_C \ldots V \ldots V$

The theory of generative grammar requires that 5' and 5'' be collapsed into a single rule as follows:

5. $V \rightarrow [-long]/\_\_\_C\{C \ldots V \ldots V$

It asserts that of the two descriptively equivalent grammars, one of which contains the two rules (5' and 5'') as separate processes, and the other as a single process combined into 5 by factoring out their common part and enclosing the remainder in braces, it is the latter which is the psychologically correct one.

Rule 5 arose in Early Middle English as a generalization of a much more restricted process of shortening. In Old English, vowels were shortened before three or more consonants (for example, godspell > godspell, bræmblas > bræmblas) and in the third syllable from the end provided they were followed by two consonants (for example, bleðsian > bleðsian). The corresponding rules were:

6'. $V \rightarrow [-long]/\_\_\_CCC$
6''. $V \rightarrow [-long]/\_\_\_CC \ldots V \ldots V$

Again, these rules must be collapsed as before:

6. $V \rightarrow [-long]/\_\_\_C\{C \ldots V \ldots V$

On comparing the Old English rule in 6 and the Early Middle English (and indeed Modern English) rule in 5 we see that the only difference between them is that the later rule (5) has lost one of the required consonants in its environment. It represents a simpler, more general form of the Old English vowel-shortening process. It will apply in all cases where 6 applied but also in cases where 6 would not have applied. Evidently the change from 6 to 5 is an instance of simplification, which we have seen to be one of the basic mechanisms of linguistic change. But in

---

4 Luick (1921, pp. 204, 352–353). In isolated words the Old English shortening also applied before geminates. But in these isolated words it led to restructuring, and since there was no shortening in derived words in Old English, the (synchronic) phonological rule of Old English was as stated. This rule covers all cases where there was actual alternation between long and short vowels in Old English.

---

a linguistic theory in which the brace notation plays no role, the relation between the Old English and the Early Middle English shortening processes is a different one. If the brace notation were not part of linguistic theory we would have two separate changes—namely, 6' > 5' and 6'' > 5'—on our hands and we would be faced with the very peculiar fact that two separate, unrelated rules have undergone an identical modification at the same point in the history of English. The linguistic theory on which traditional historical grammar was based is an instance of such a theory, and traditional historical grammar has in fact failed to see the regularity here and has treated the change as two separate processes.

In the same way, we can go on to ask whether rules of the form

$$
\{X \ Y\} \rightarrow Z
$$

can be added to grammars. On the assumption that sound changes are natural processes, and that the brace notation groups rules into natural blocks, we should predict that rules collapsed by braces should be capable of being added to grammars. There are of course numerous instances of this type of change. In fact, the addition of Rule 6 to the grammar of Old English is probably just such an instance. Similarly we should predict that rules collapsed by braces should participate in reordering as blocks.

The proposed test also has the virtue of rendering such notations eminently vulnerable to potential counterevidence from historical change. The counterclaim which would be made by the theory which excludes braces is that rules like 5' and 5'', or 6' and 6'', when found together in a grammar with no necessarily intervening rules forcing them apart, should be able to undergo simplification individually, in such a way that the resulting pair of rules could not subsequently be collapsed by braces. Such a change, which in this theory would be a legitimate simplification, would be neither a possible sound change nor a simplification in a theory which allows collapsing by braces, and it would therefore be excluded in the latter. If such changes could be found, they would be clear counterevidence against the brace notation and would suggest that the generalizations effected by means of braces are spurious ones. The position which excludes braces would also entail that a rule could be inserted between two rules collapsed by braces in such a way that they subsequently could no longer be so collapsed. And finally, it would also entail that the parts of rules collapsed in this way should be individually capable of reordering with other rules of the grammar. The fact that no such changes appear to exist is strong negative evidence which adds to the historical support for the essential correctness of this abbreviatory convention of generative grammar.
The aforementioned linguist C, who wanted to introduce abbreviations like

\[
\begin{align*}
X \rightarrow Y \\
Z \rightarrow \{ \}
\end{align*}
\]

and Pāpini, who supported other conventions which generative grammar does not countenance, now both get a real opportunity to prove their points by showing that the blocks of rules resulting from such conventions act as units in simplification (for example, by showing cases in which the joint environment \(X\) is simplified) or by showing that they are added as units to grammars, or reordered as units with respect to other rules. There is no evidence in sight that I know of to encourage them in this search.

One answer, then, to the question concerning the empirical basis for the notational conventions of linguistic theory is that these conventions are an essential part of any attempt to characterize what is a possible linguistic change and what is not a possible linguistic change. It involves in a sense only systematically drawing the conclusions from Halle’s idea (1962) that the class of possible sound changes (qua added rules) is the same as the class of possible phonological rules and bringing in the additional evidence of simplification, whose role in linguistic change Halle did not consider.

In many crucial respects this criterion for rule naturalness lends support to the assumptions which are currently made in the theory of generative grammar. But accepting the equivalence of possible sound change and possible rule commits one to placing many restrictions on the notations of grammatical descriptions which are not at present acknowledged, and on the other hand, it suggests the need for many new conventions and new extensions of notations which should be incorporated into linguistic theory. For example, by saying that braces are needed we have only told half the story. We would like to limit the use of braces in such a way as to combine only processes which are indeed in some sense related and can jointly produce a sound change. Suppose, for example, that we found a language with three phonological processes that all applied before vowels and that did not have to be separated by other rules:

1. voiced stops become continuants
2. \(s\) becomes \(h\)
3. vowels drop

For all three processes to take place before vowels is quite natural, and examples for each of them could be cited from dozens of languages. Yet there would be something wrong about combining all three by virtue of their shared environment. It is evident that \(a\) and \(b\) are more closely related than either of them is to \(c\), and that an adequate theory should require the combining of \(a\) and \(b\) but not \(c\). The basis for this feeling is, I think, nothing but the fact that \(a\) and \(b\) characteristically occur together in linguistic change and thus form a natural block of phonological processes. In fact, their relatedness has really nothing to do with the fact that they share a common environment but follows from an essential kinship of the phonetic processes involved. Thus they should be grouped together in a grammar even if they both were context free. To determine the natural groupings of rules was a goal of traditional historical linguistics which has been abandoned to a large extent in structuralism, at least in America. For example, \(a\) and \(b\) would have been considered weakenings. Probably phonology would profit by attempting to develop further and to make precise such concepts, which traditional grammars use to introduce an organization into their treatments of diachronic phonology.

4. Diachronic Evidence Concerning Phonological Levels

The psychological reality of levels of representation which emerge in different linguistic theories is subject to verification and falsification by diachronic evidence along the same lines. A question to be asked whenever some level of representation is proposed as linguistically relevant is whether this level functions in linguistic change. For example, it would be a striking and, to my mind, conclusive piece of evidence for the reality of autonomous (‘taxonomic’) phonemics if it could be shown that there were sound changes whose conditioning environment could be stated naturally only at precisely this level. It should be made clear just what such a demonstration would involve. It would involve showing both that the environments of this sound change were not morphophenemic and (the crucial part) that they could not be reformulated in terms of the phonetic level without restating exactly the rules that relate the phonetic and phonemic level. Of course, it is always by definition possible to re-formulate a phonemic environment in terms of phonetic representations, and what would have to be shown is therefore that such a restatement of the conditioning environment of a sound change would lose a significant generalization. A hypothetical example of what to look for would be a change in some Russian dialect which affected all voiced obstruents except [\(\text{[s]}\)] and [\(\text{[z]}\)], the two voiced obstruents in Russian which are not phonemic but always come about only by automatic voicing assimilation...
of /ɛ/ and /ɛ/. As far as I know, no one has ever presented any instance of this kind, and there is therefore no basis for the claim that the facts of sound change somehow support a level of autonomous phonemics. And as has been repeatedly argued (Halle, 1962; Chomsky and Halle, 1968, Postal, 1968, Kiparsky, 1965, 1967), the facts of sound change do provide clear evidence for a deeper level of representation in phonology.

The contention has often been made that the level of autonomous phonemics is relevant to sound change in a somewhat different way. The suggestion is that the direction of sound change is determined by tendencies toward a symmetry of phonological units. What is important for our present discussion is that these units are often held to be specifically autonomous phonemes. Many of the comments again apply: if the level in question were demonstrably the relevant one here, and the tendencies in question could really be shown to exist, then this would decide the question of the existence. But once again, the necessary proof has to my knowledge, never been provided.

Moult had studied the vowel systems of Swiss dialects with the purpose of testing these concepts of ‘phonological space’. He maintained (1961) that Rule 4—the lowering of ɔ to ɔ before dentals, palatals, and r, whose relation to umlauting we discussed as an example of reordering —was caused by a drive towards symmetry through filling the ‘empty slot’ in the systems which Moult supposes that these dialects possessed before the lowering took place:

\[
(A) \quad \begin{array}{l}
i \\
e \\
\varepsilon \\
\varepsilon \\
\varepsilon
\end{array} \quad (B) \quad \begin{array}{l}
i \\
e \\
\varepsilon \\
\varepsilon \\
\varepsilon
\end{array}
\]

But what is the justification for assuming that System B had this asymmetrical structure rather than the symmetrical structure, C, which one would have normally supposed it to have?

\[
(C) \quad \begin{array}{l}
i \\
e \\
\varepsilon \\
\varepsilon
\end{array}
\]

Why did Moult not assign a to the back vowels in these dialects as he did in the A dialects? Moult has discussed the reason for his choice in another article (Moult, 1960, p. 174), where the justification given for the asymmetrical System B is that these dialects underwent the lowering by Rule 4: ‘The fact that the subsequent development of the vowel system of the North was parallel not to that of the West and Center but to that of the East confirms the belief that arrangement [B above] represents linguistic reality more faithfully, and suggests that arrangement [C above] would indeed be only a playful manipulation of symbols on paper.’ In other words, these dialects had an asymmetrical system because they underwent lowering of ɔ to ɔ and they underwent lowering of ɔ to ɔ because they had an asymmetrical system!

In sum, one prediction to which such theories lead is that certain phonological changes should be determined by whether or not pairs of certain sounds are contrastive in some phonetic environment and hence that isoglosses formed by phonological changes should characteristically be coextensive with boundaries between different autonomous phonemic systems. Other predictions are certainly also entailed, and the cases I have mentioned by no means constitute a full or even representative illustration of the range of predictions made, nor of the kind of evidence that is available to test them. But the never show how this theory and related ones do have very specific consequences which can be tested fully on historical material. I would guess that when this is done it will turn out that real enough tendencies towards phonological symmetry exist, but that they have nothing to do with the autonomous phonemic level for which they are often claimed. Rather they are probably brought about by simplificatory phonological changes such as rule simplification and rule reordering, and the symmetry they result in is phonetic rather than phonemic symmetry. This at any rate is what the Swiss German dialect material recently investigated by Moult suggests.

5. Diachronic Evidence Concerning Features and Underlying Representations

The particular Swiss German example that I have talked about also raises a nest of further problems unrelated to that of the reality of the autonomous phonemic level, but highly relevant to the general topic of the relevance of linguistic change to linguistic universals. It will have been noticed that phonemic System A above, with four distinctive vowel heights, is a clear counterexample to Jakobson’s distinctive feature system, which allows only three phonemic degrees of vowel height to exist in a language. First of all, the four degrees clearly contrast in simple, underived words and cannot be predicted by any general rules from some system with only three heights in any way that would not be ad hoc. For example,
In addition these dialects have a phonemic a which is quite distinct from all of these front vowels. Evidently, then, Jakobson's features compact and diffuse (low and high) should be replaced by two other features which allow four distinctive degrees of vowel height. A natural one would be the following:

\[
\begin{array}{c|cccc}
 & æ & ε & e & i \\
\hline
\text{High} & - & - & + & + \\
\text{Mid} & - & + & + & - \\
\end{array}
\]

Yet if we shift our point of view somewhat and regard impossible systems simply as the end points of increasing scales of markedness, the proposed change to allow four heights is a relatively minor one. In a sense, these dialects, particularly if the historical evidence is brought in, support Jakobson's thesis in the modified form that vowel systems with four heights are complex, that is, highly marked systems, in the technical sense. For historically, a four-height system of this kind had to arise in all High German dialects. However, everywhere, with the exception of some tiny Swiss areas in Appenzell and Toggenburg, the four heights have been reduced to three by mergers either of the mid vowels or of the two low vowels. These mergers have taken place quite independently in numerous dialects and thus have the character of drift or simplification rather than of normal sound changes. What this seems to indicate is that systems of four vowel heights are unstable because of their complexity, a conclusion which is indicated in any case by the rarity of such systems in the languages of the world.

The particular way in which these four-height systems have merged to three in the various dialects is itself a small piece of historical support for the feature system which I have proposed. The other possible alternative of characterizing four vowel heights by two features would be this:

\[
\begin{array}{c|cccc}
 & æ & ε & e & i \\
\hline
\text{High} & - & - & + & + \\
\text{Raised} & - & + & - & + \\
\end{array}
\]

There would be no natural way of formulating the merger of mid vowels here since mid vowels do not make up a natural class under these features.

On the other hand, this alternative suggests mergers such as æ and ĕ or e and i which certainly do not occur. Vowel shifts of the type i > æi, which are common in many languages, would also be expressed more simply in the system I have proposed. However, an alternative which may be even preferable and should in any case not be counted out yet is that vowel height is not broken down into two binary dimensions at all but forms a single dimension expressed by a feature which in underlying representations can assume at most four values (and must assume at least two).

Against the analysis which posits four heights of vowels in these dialects one might try to carry the argument that this analysis is implausible because closely related dialects have only three heights, and one would expect closely related dialects to differ not in their underlying phonemic system, but only in the rules which relate phonemic representations to phonetic representations. This would be a complete non sequitur. It is an empirical observation that related dialects often have the same phonemic system, but it is not a theoretical condition on related dialects that this should be the case. To say otherwise would be to credit children with historical or dialectological knowledge which they cannot possibly possess. The fact that the children of each generation in learning their language take a fresh look at the facts means that there is reason for underlying representations to be transmitted only when the synchronic facts of the language warrant it. The argument is just as irrelevant, and for just the same reason, as it would be to maintain that language L must have rule R in its grammar because R was a sound change in L.

A more difficult objection is based on the fact that æ in these dialects is the productive umlaut of a. To account for morphological umlaut in a language like German it is necessary to set up some abstract conditioning environment which will be a property of certain endings, such as plural -er. Whether this is a feature [±umlaut] as proposed by Zwicky (1967) or some phonological property of underlying representations will not matter here. Whatever this abstract environment is, generative phonology at present allows—and indeed probably requires—the trick of making it an obligatory part of isolated words like schön, plötzlich, Türk, which have umlaut vowels that correspond to no back vowels in any related forms. These words are then entered with underlying back vowels which undergo obligatory umlauting by virtue of this property of their underlying representations. The effect is to do away altogether with umlaut vowels in the phonemic system. In our case, then, æ would never be treated as phonemic and there would be only three phonemic vowel heights to worry about.

It is again the historical evidence which shows that this trick is wrong and that words like schön, plötzlich, Türk must have phonemic umlaut...
vowels. To see this let us go back to the example of reordering involving Rules 3 and 4 in Northeastern Switzerland. It will be recalled that as a result of the reordering, derived ő as in plural bödza became ʒ but phonemic ː as in plötli was not changed. There would be no way of accounting for a change like this (by no means an atypical case) in a theory which asserted that all umlaut vowels are underlying back vowels, for then we would have no natural way of telling apart those that are really so derived and do undergo lowering from those which are only fictitiously so derived and do not undergo lowering. This linguistic change cannot be accounted for unless phonological theory is tightened up in some way to exclude tricks of such a kind. It is interesting to note that whatever exactly the right way to do this turns out to be, it will bring the underlying representations of generative phonology a step closer to Sapir’s descriptive practice (McCawley, 1967). And once this necessary move is made, the existence of systems with four vowel heights cannot be argued away.¹

This last conclusion has the peculiar status of at present resting entirely on historical evidence, and of a fairly indirect kind at that. Whether or not we draw it depends on what we consider the subject matter of linguistics to be. We could not draw it if we regarded a grammar simply as a theory of the sentences of a language, and a linguistic theory as a theory of grammars. For this position would entail that linguistic change is no concern of linguistic theory, although it might of course be a pleasant bonus if linguistic theory could be usefully ‘applied’ to questions of linguistic change. But it would not cause us to demand of a linguistic theory that it must (in conjunction with a theory of linguistic change) provide an explanation of the linguistic regularities of diachrony. It is a very different matter if we regard a grammar as a theory of linguistic competence, and the field of linguistics as the study of universal grammar. On this view, which forms the topic of this conference and which I share, the facts of linguistic change assume a new relevance as empirical evidence on the nature of language. We must be prepared to allow them to bear on even purely synchronic questions and, for example, to let the fact that some phonological change is explainable by one linguistic theory but not by another carry weight in the choice between these two theories. The application of linguistic change to linguistic theory now becomes at least as important as the converse process.

The above rather scattered observations illustrate various types of inferences that can be made about grammatical form from the ways in

¹ David Stampe points out to me that the naturalness condition he proposes (at this same conference) requires exactly the underlying representations which we have seen to be justified on historical grounds.

which it shapes linguistic change. The reason I have dealt with phonological changes and not syntactic ones is partly that I know more about phonology, but also that the historical facts are here much easier to come by and the evidence they give is more needed in phonology than in syntax. I have been concerned not so much with establishing the virtues and faults of specific notations, levels, and so forth—much more evidence would be needed for that in almost every kind of problem dealt with above—as with making a case for the legitimacy and potential fruitfulness of certain general patterns of inference from linguistic change to the nature of grammar. In no case have the conclusions depended on very specific or controversial assumptions about linguistic change. The basic assumption from which these conclusions follow has been the very tame one that where grammar is involved in linguistic change it is involved in terms of its natural components and rules.

It is not so with another kind of inference from linguistic change to grammatical form to which I should now like to turn. This inference is based so heavily on the existence of grammatical simplification as a form of linguistic change that before proceeding to it I should like to outline the justification for assuming the existence of such a form of linguistic change.

6. Formal Justification for Simplification

The conclusion that such changes as simplification and reordering must exist does not and could not rest just on the fact that we observe related dialects to differ in the ordering of their rules, or to show minor differences in the details of essentially shared rules. That such differences are typical isologues is true but compatible with the position that addition of rules is the only form of phonological change. For as long as we look at dialects without knowledge of their historical origin we could explain any rule-ordering difference between them in a wave-theory fashion. For example, a spreading rule might be adopted at one position in the sequence of rules in one dialect and at some other position in another. There is another wave-theory effect which can cause pairs of rules to be differently ordered in different dialects. If Rule A spreads from West to East and Rule B spreads from East to West across some dialect area, then, if the two rules are critically ordered with respect to each other, the Western area will end up with the order A, B and the Eastern area with the order B, A. Undoubtedly these are both, in fact, quite common causes of ordering differences between dialects. Small differences in the form of rules can well occur in the course of their diffusion from one
dialect to another. It has been observed that in such borrowing a narrowing down in the scope of rules often takes place. Thus the diphthongization of the long high vowels of Middle High German during its spread southward in Swiss territory was restricted to word-final position at a certain point before it stopped spreading altogether. Compare also the gradual curtailment of the High German consonant shift in the so-called Rhenish Fan.7

However, we find just the same types of minor differences in the form of rules and in their ordering when we compare successive stages of the same dialect rather than geographically adjacent dialects, and here the wave-theory and imperfect borrowing explanations are excluded. Furthermore, in such cases the form of rules almost always changes in the direction of greater simplicity. Can such changes be accounted for on the assumption that addition of rules is the only form of phonological change?

Consider the Finnish example cited in Section 2, in which the diphthongization rule was dialectally shifted down to follow loss of medial voiced continuants so as to apply to the long vowels which arose by this historically later rule (for example, teve > tee > tie). Technically, it is not impossible to account for this change by means of added rules. There are even two ways of doing it. One is to assume that a rule of loss of medial voiced continuants, identical with the original one, was entered before diphthongization, causing the original one to become vacuous and to be dropped. The other is to assume that a diphthongization rule, identical with the original diphthongization rule, was entered after loss of medial voiced continuants. The optimal grammar for the resulting output would once again be the desired one. The unfortunate aspect of this is the arbitrariness of the choice between the two descriptions. It is hard to see how the distinction between them could correspond to any linguistic difference. The two distinct grammars containing an identical rule at two different points which are required as virtual intermediaries seem to be mere artifacts of a theory which excludes reordering as a mechanism of change and therefore must make an inappropriate extension of rule adding to account for a quite different kind of process.

The difficulties become considerable in such a case as the loss of word-final devoicing in Swiss German and Yiddish. We cannot, clearly, simply suppose that a late rule which made final obstruents voiced was added. Such a rule could not distinguish between morphophonemically voiced and voiceless stops and would wrongly turn into bunde not only the bunte that is related to bunte but also the bunt that is related to bunte. In desperation we would take recourse to an ad hoc rule which somehow

7 E. Bach has pointed out to me that these examples are not certain. If, as he suggests, rules are never narrowed down in borrowing, the case for simplification becomes even stronger.

would provide morphophonemically voiced stops with a diacritic feature before they got devoiced and later would use this diacritic feature as an environment for devoicing, after which the diacritic feature could be deleted again. Obviously this bears not the faintest resemblance to what actually happened, and no one would want to salvage a theory at the price of such an absurd analysis.

Chomsky and Halle (1968) discuss a convention for handling exceptions to rules which might be used in this particular example. The idea is that grammars can contain rules of the form

\[ X \to [\text{next rule}] \]

where \( X \) is a specification of the special cases in which some rule must not apply. Then it would be possible to say that a rule

\[ [ \ ] \to [\text{next rule}] \]

was placed directly before the devoicing of word-final obstruents, thus preventing everything from undergoing it. The inoperative devoicing rule would then simply not be incorporated into the grammars of the next generation.

The difference here is not merely notational. The exception-rule solution generalizes neither to the reordering example that was just cited nor to cases like the simplification of the umlaut rule from 2 to 3 which was mentioned earlier. Since what was deleted here was part of a rule and the Chomsky-Halle convention for handling exceptions does not allow items to be exceptions to parts of rules, the solution which the convention made possible in the previous case is not available here. The best we can do is to say that the change consists of two separate but simultaneous events: first, the rendering inapplicable of the old umlaut rule (2) by the placement of a Chomsky-Halle exception rule before it, and second, the entering of the new umlaut rule (3) in its stead. That is, we are forced to treat this event as a composite product of two simultaneous changes, one of which alone would have far more spectacular consequences than the two have together. This leaves us completely in the dark as to why so many dialects (quite independently of each other, as is clear from the geographical distribution) should have undergone such a complicated pair of changes.

We see that to account for such examples by added rules, we would be forced to relax the proposed restriction that a sound change is the addition of a rule to the grammar to the extent of allowing a single historical change to involve the addition of two rules. In that case all arguments like those in Section 3 about sound change as a criterion for rule
naturalness at once go out the window. And if this is done we also prepare a welcome for innumerable absurd descriptions of other changes. For example, in the case of the Finnish reordering of diphthongization and loss of medial voiced continuants (see Section 2 above) there are now two further alternatives which add to the general arbitrariness: the change might consist of simultaneously making diphthongization inapplicable and adding an exact replica of it after the loss of medial voiced continuants, or of simultaneously making loss of medial voiced continuants inapplicable and adding an exact replica of it before diphthongization.

Also, it is now just as easy to express the reverse change, that is, a change as a result of which the order

a. loss of medial voiced continuants
b. diphthongization

changes into

a. diphthongization
b. loss of medial voiced continuants

The effect of this would be that all ie diphthongs derived by way of ee from eve would revert to their intermediate representation ee, while the ie diphthongs derived from basic ee would stay unchanged. There is no doubt that a theory of linguistic change should either completely exclude the possibility of such a change or at least reflect the obvious fact that it would be a far more complex and unlikely historical event than what actually happened. But the version to which the theory that rule addition is the only form of linguistic change has been driven at this point is completely incapable of doing so. As the brute necessity of somehow accommodating one set of data has forced it to be relaxed and extended more and more, it has lost the capacity of expressing the facts about sound change that originally motivated it.

7. Simplification and Language Acquisition

To avoid this hopeless mess, the concept of simplification would be necessary even if we were concerned merely with characterizing the possible ways in which successive stages of a language could differ (which would be enough for purposes of linguistic reconstruction). But we also would like to find an explanation for why languages can change in the ways that they do. In that case, the reasons for assuming that simplification is a form of linguistic change become more compelling still. We cannot, then, close our eyes to the fact that the kind of driftlike changes which rule addition fails to handle without the special acrobatics of which samples were performed in Section 6 result in just the kind of grammars that appear spontaneously as intermediate stages in the course of the child’s language-learning process.

I am not thinking just of the fact that instances of morphological analogy (oxes, bringed) are as characteristic of child language as they are of historical change, although this is perhaps the most evident instance of the correspondence. The parallelism goes deeper than that. For example, there is in many languages a drift toward multiple negation, as in substandard English ‘I don’t see nothing nowhere’. Such multiple negation has developed in the Romance languages and elsewhere in Europe too. Jespersen tried to attribute this drift to some vague tendency toward redundancy which he thought governed the direction of linguistic change. But this can hardly be true, for in other languages, such as Finnish, no comparable drift toward multiple negation is observed. Then it cannot be true that multiple negation is simply a general target in the direction of which all languages develop. In fact I think it is true that multiple negation appears only in those languages that have the equivalent of Klima’s neg-incorporation rule which produces negative quantifiers such as in English nobody, nothing and French rien. Surely this is related to the facts about the development of negation in child language found by Bellugi (cited from McNeill, 1966). She discovered that at the point at which the child’s sentences like

I didn’t see something.
You don’t want some supper.

give way to sentences with negative quantifiers like nobody, nothing, no supper, a period of multiple negation at first sets in. As the child first formulates his neg-incorporation rule, it has not the form of standard English but of substandard English (which he very well may never have heard); and instead of producing the ‘normal’ sentences like

I saw nothing.
Nobody likes me.

he at first comes out with

*Finnish has indefinite pronouns such as kukaan, mikään, corresponding to English anybody, anything, but a negative cannot be incorporated into them to form any equivalents of nobody, nothing.
I didn't see nothing.
Nobody don't like me.

Thus some relationship between 'substandard' neg-incorporation and 'standard' neg-incorporation may be responsible for the fact that the former is the natural predecessor of the latter in the development of a child's linguistic system and also the natural result of the latter by linguistic change.

These facts begin to add up when we think of language acquisition as a process in which the child arrives at adult grammar gradually by attempting to match to the speech it hears a succession of hypotheses of an increasing order of complexity (in the linguistic sense of complexity) as these increasingly complex hypotheses become available to the child through maturational change. For phonology this was clearly shown by Jakobson's spectacular discovery that the child learns phonemes in a largely fixed order, which is determined not externally by the order or frequency with which they are heard, but internally by their relative linguistic complexity, as reflected also in the rules governing the possible phonemic systems of the languages of the world (Jakobson, 1942). Thus the child first produces the maximally unmarked, unvoiced, unaspirated stops, even if these, as in English, happen not to occur (except in some special environments) and only then splits up this first stop series into two series. In phonology, then, the order in which a child incorporates a particular piece of data into his internalized grammar is determined not by frequency or order of presentation, but by the readiness of the child to assimilate the kind of structure that underlies it. If we assume that the order in which the syntactic rules of the child unfold is internally determined in the same way, we can think of the child's multiple negation as analogous to his unvoiced, unaspirated stop in the sense that both are necessary prior structures which can be discarded only after the full structure develops. This is reasonable in view of the fact that multiple negation is produced by a version of neg-incorporation which is in two respects simpler than the adult version of this transformation. In the first place, the adult rule not only adds a neg to the quantifier, but it also deletes the original neg after the tense; this additional operation of deletion is absent from the child's first version of the rule. Secondly, the adult rule adds a neg to just one single quantifier in the sentence, whereas the child spreads the neg over all quantifiers that appear in the sentence, producing such specimens as the following:

I can't do nothing with no string.

Normally these oversimplified intermediate grammars which the child constructs on its way to adult language eventually give way to the full complexity of the adult system. The linguistic change of simplification takes place on those relatively rare occasions when some feature of these intermediate grammars survives into adulthood and becomes adopted by the speech community as a new linguistic norm. See Jakobson's remark (p. 332 of the 1962 reprinting):

'Die Sprachveränderung ist kein äusserer Beitrag, den die Kinder dem Sprachgebilde aufwirigen, sondern sie antizipieren dessen innerlich vorherbestimmte, sozusagen in der Luft schwebende Umwandlungen.'

That such survival is possible is not quite so surprising when we consider the extreme imperviousness of children to adult correction of their speech, as illustrated for multiple negation by the following dialogue (McNeill, 1966, p. 69):

CHILD: Nobody don't like me.
MOTHER: No, say 'nobody likes me'.
CHILD: Nobody don't like me.

(eight repetitions of this dialogue)

MOTHER: No, now listen carefully; say 'nobody likes me'.
CHILD: Oh! Nobody don't likes me.

Thus we can relate the concepts of rule addition and simplification to adult and child language, respectively. The typical form of rule addition is the borrowing of rules among adults; simplification typically occurs in the learning of language by children. An interesting consequence of this is that isoglosses formed by the spread of rules over a speech territory should form large, coherent dialect areas, whereas those formed by simplification should be characteristically discontinuous because of independent development of the same change in several speech communities. The historically interesting isoglosses, therefore, should be based on the presence versus absence of rules, and not on differences in the form and order of shared rules. Indeed, this is what dialectologists have always implicitly assumed. The boundaries between the major dialect areas of Germany are drawn according to the rules they have, such as the consonant shifts. The isogloss between the two forms of the umlaut rule, 2 and $\exists$ (that is, between $\epsilon$ and $\alpha$ as the productive umlaut of $a$), would form a useless patchwork of no historical significance. Nor would anyone suppose a historical relationship between Yiddish and Swiss German on the grounds that they share the loss of the word-final devoicing rule. Very
schematically, the two types of isoglosses would look like this (shaded areas are the innovating ones):

8. Reordering as Simplification

Reordering resembles simplification both in the negative property that rule addition miserably fails to do justice to it and in the positive property of its driftlike character. I shall now claim that reordering is in fact a special case of simplification, and that the direction of reordering is predicted by general principles which assign certain types of order a higher value than others. If this can be established, then current phonological theory, which does not distinguish different kinds of linear order, is wrong and must be revised to account for this asymmetry.

To be convinced that reordering is a one-way affair, much as other simplification is, it is enough to examine the individual examples. For instance, many Swiss dialects have put the umlaut rule (3) after o > o (Rule 4), but none of these have made the reverse switch, and we could not easily imagine it taking place. And a dialect of Finnish in which tie from terce > see becomes see again, but vie from vee retains the diphthong, that is, a dialect in which diphthongization reverts to its original position before the loss of medial voiced continuants (which I will now call γ > 0 for short) is inconceivable. The question is how this asymmetry, intuitively evident enough in each particular case, can be given a general characterization.

Of the various functional relationships that can hold between rules, two are of relevance here. One way in which two rules, A and B, can be functionally related is that the application of A creates representations to which B is applicable. That is, the application of A converts forms to which B cannot apply into forms to which B can apply; schematically:

A. [ ] > [γ]
B. [γ] > [ ]

Such a relationship holds for example between γ > 0 (terce > see) and diphthongization (tee > tie) in our Finnish example. If the rules are applied in that order, γ > 0 supplies a set of new cases (namely those derived from eve) to which diphthongization can apply. In such a situation, call A a feeding rule relative to B (for example, γ > 0 is a feeding rule relative to diphthongization). Call this relationship between rules a feeding relationship (for example, γ > 0 and diphthongization are in a feeding relationship) and the linear order in which the feeding rule precedes a feeding order (for example, 1. γ > 0, 2. diphthongization is a feeding order). Then one of the principles that determine the direction of reordering is

1. Feeding order tends to be maximized.

Schematically:

A. [ ] > [γ]
B. [γ] > [ ]

A further example of I involves the several palatalizations in Slavic. By the so-called first palatalization, k and g became ć and ǵ, respectively, before front vowels and γ, for example, *kito > čito 'what', *givū *țivū 'alive'.

7. [+consonantal] → [−grave]
   [−diffuse] → [−strident] / [−back]
   [−consonantal] / [−back]

But the resulting voiced affricate ǵ has become a continue ć in all Slavic languages by the rule

8. [% voiced] → [% continuant]

For example, *țiru > țirū.

Subsequently new front vowels came to stand after velars by the rule

9. ai → ę

By the so-called second palatalization k and g, derived from k and g by

* Other aspects of the Slavic palatalizations are dealt with by Halle and Lightner in a forthcoming study. My knowledge of the rules is based entirely on their work. I state the rule here with the Jakobsonian features rather than any of the recent alternative proposals which have greatly improved the system.
an earlier rule) became e and 3 before these new front vowels, for example, *kēna > cēna ‘price’, *gēlo > zēlo ‘very’:

\[
10. \begin{bmatrix} +\text{obstruent} \\
-\text{grave} \\
-\text{strident} \\
-\text{diffuse} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{strident} \\
+\text{diffuse} \end{bmatrix}
\]

The resulting affricate ฐ, unlike the earlier ฐ, is retained in Old Church Slavic and in modern Polish. The grammars of these languages have Rules 7–10 as phonological rules in an order that matches their relative chronology. But elsewhere in Slavic, ฐ also has been replaced by its corresponding continuant, namely з, for example, зēlo > зело. These languages have the same four rules, but 8 must here follow 10, in order to apply to the affricate produced by the second palatalization as well. It is these two rules between which the feeding relationship obtains. Rule 10 is the feeding rule and the reordering establishes a feeding order between 10 and 8.

It should be noted that this relationship is a matter of the function and not of the form of the rules. In the Slavic example there is, as is often the case elsewhere too, a formal similarity between the related rules in that they mention some of the same features, and so on. But it would not be possible to define the correct relationship on the basis of the form of the rules. The two Finnish rules previously cited have very little in common, and the relationship is simply based on properties of the derivations the language has.

Another possible functional relationship between two rules is that A removes representations to which B would otherwise apply:

\[
A. \ [\ ] > [\sim q] \\
B. \ [q] > [\ ]
\]

Such a relationship holds for example between umlaut (A) and o > ə (B) in the example of Section 2. Thus the application of umlaut turns o into ə, a front vowel to which the lowering rule is no longer applicable. If the lowering rule comes first in the ordering, it applies, turning o to ə, and umlaut can then still apply. In the terms of the Indian grammatical tradition, umlaut is here the nītya or ‘constant’ rule. Call A a bleeding rule relative to B, the relationship between A and B a bleeding relationship, and the ordering in which A precedes B a bleeding order. The principle which underlies the asymmetry of order in this case is the following:

II. Bleeding order tends to be minimized.

\[
A. \ [\ ] > [\sim q] \text{ B. } [q] > [\ ] \\
B. [q] > [\ ] \text{ A. } [\ ] > [\sim q]
\]

In this way the original order, in which umlaut preceded lowering, became switched around into the new order, in which the bleeding did not take place.

As another illustration of the effect of II, consider the relation of two rules pertaining to voiced obstruents in German. One of them, which is historically the older, is the devoicing of obstruents in word-final position (for example, bunde > bundt, tąd > tük). This is Rule 1, which has come up in the discussion several times already. The other, found only in a certain group of dialects (Schirmunkski, 1962, p. 302), is the spirantization of postvocalic voiced stops, for example, tąg > tągə, sągt > sągəl (>sąxt). Originally, devoicing preceded postvocalic spirantization. Since, with this order, morphophonemic final voiced stops lost their voicing before spirantization applied, they remained stops and the contrast of ták:tągə resulted. This bleeding order, in which word-final devoicing deprives spirantization of some of the voiced stops to which it would otherwise apply, is still retained in some Alsatian, Bavarian, and Middle German dialects. More frequently the reverse ordering is found, with final voiced stops undergoing first spirantization (tąd > tąg) and then devoicing (tąg > tąx.) This order is widespread and especially common in the Low German dialects. We know that this order is a secondary development because some words like (a)weh (Standard German weh), where the voicing of the stop had no morphophonemic support, failed to spirantize even in the reordering dialects. This would be inexplicable unless we suppose that the devoicing was historically earlier even in these dialects in spite of the fact that it is synchronically later.

Another example can be cited from this same familiar area. A very widespread sound change in German dialects (Schirmunkski, 1962, p. 212) is the rounding of ə to ə. As æ, the umlaut of ā, is unaffected by this change, it brings about alternations between ə and æ such as swän ’swan’. swän ’Pl.’, szept ’late’; spekt ’later’. Hence there is a bleeding order between the rules

\[
a. \text{ umlaut} \\
b. \ ə > ə
\]

Many modern German dialects have just this system (see Rabeler, 1911, and Hotzenkümperle, 1934, for a Low German and Swiss German dialect,
respectively). In others (for example, Wanner, 1941) the system has changed in that the un laut form of ą is ą̄, for example, ąwąn, ąństęter. The grammatical difference is that un laut now applies after rather than before the rounding of ą. As phonemic ą̄ in words like tąść ‘tough’ and ląr ‘empty’ stays unrounded (more proof of the correctness of the argument is in Section 5) it is clear that the possibility of a simplification of the rounding rule to all long compact vowels is excluded and we are again faced with a case of reordering, which conforms perfectly to Principle II.

There is a more general principle underlying the two reordering tendencies (I and II) which combines them under a single wider concept of fuller utilization and makes their nature intuitively much clearer:

III. Rules tend to shift into the order which allows their fullest utilization in the grammar.

If I am right that such a principle determines the direction in which reordering proceeds, then it follows that the order toward which rules gravitate in this way is linguistically simpler than its opposite. It is hard to see what other explanation there could be for such a consistent tendency toward a specific kind of order in linguistic change. As a convenient designation for the order types which are shunned and preferred according to Principles I–III, I suggest marked and unmarked order, respectively. It may well be that marking conventions analogous to those which assign the unmarked feature values in segmental phonology are the appropriate device for reflecting the asymmetry of ordering relations as well.

9. Leveling and Extension

As further justification for my assertion that unidirectional reordering tendencies exist and that they obey Principles I–III, I want to adduce an unexpected parallelism which obtains between reordering, if so constrained, and rule simplification. We can begin with a distinction drawn in traditional and structural historical grammar between two types of analogy, one called leveling and the other called polarization or extension. By leveling was meant roughly that existing alternations are either curtailed or eliminated altogether, with the result that allomorphs of some morphemes become more similar to each other or merge completely. Thus the change of bunt:bunden to bunt:bunde would have been regarded as a leveling of the alternation of voiced and voiceless stops in word-final position. The simplification of the umlaut rule (2) to its other version (3), which replaced kraft:kreftig by kraft:kraftig would have been regarded as a leveling of the height alternation in favor of the low vowel throughout the paradigm.

Polarization, or extension, on the other hand, refers to a type of analogical change in which existing alternations spread to new instances. Here linguistic contrasts come to be more fully implemented than before, whereas leveling has precisely the opposite effect. We would presumably be dealing with extension if, for example, the alternation of medial voicing and final voicelessness in obstruents as in tąć:tąćk, bunt:bund, instead of being eliminated altogether, had become extended beyond its original domain to the sonorants, as has in fact happened in Icelandic. The change of the limited Old English vowel shortening rule (6) to its present more general form (5) is another instance of extension.

This distinction, implicit in traditional historical studies, though rarely drawn systematically (but see Hoenigswald, 1960, pp. 63, 108), is a useful one, partly for reasons that have to do with linguistic reconstruction. Leveling will often be recoverable by historical reconstruction, because of the relic forms which reflect older linguistic stages that leveling leaves behind. Extension, however, will in general not be so recoverable because, with certain very interesting exceptions, it cannot leave relic forms behind. The difference between these two types of analogy can be defined in terms of the formal differences of two kinds of rule simplification in a very straightforward manner. Rules consist of two parts, a structural analysis, which specifies to what forms the rule applies, and a structural change, which says what happens to these forms. In the customary notation for phonological rules, the structural change is the part between the arrow and the slash and the structural analysis is everything else. Then any rule simplification which modifies the structural change of a rule (whether or not it also modifies the structural analysis) is a leveling, and any rule simplification which does not modify the structural change of a rule is an extension. Thus the loss of final devoicing (Rule 1) and the simplification of Rule 2 to Rule 3 affect the structural change of the rule and are hence levelings, but the change of the shortening rule in English did not affect its structural change and is hence an extension.

It is a fairly surprising fact that the two kinds of reorderings we have found, namely those governed by I and II, correspond pairwise to these two kinds of rule simplifications and in turn to the traditional distinction between extension and leveling. Reordering by II results in leveling and this corresponds to simplification in the structural change of a rule. For example, the effect of placing umlaut after ą is that the height alternation in bąd:bańd and innumerable similar cases is leveled and the resulting forms, bąd:bąńd, retain the low vowel through-
out the paradigm. So, too, the reordering of spirantization and word-
final devoicing results in the dropping of a two-feature alternation,
\textit{tak:ta} (with change of both voicing and continuance), in favor of a
simpler one-feature alternation, \textit{tak:ta} (with a continuant throughout
the paradigm), that is, again in leveling. In their effect on surface forms
and on the relation of surface forms, leveling by simplification in the
structural change of rules and leveling by reordering in accordance with
Principle II have similar effects in that they make more alike the different
shapes in which morphemes appear. But they bring this effect about in
different ways because leveling by rule simplification brings the forms
closer to the base forms, whereas leveling by reordering takes forms farther
away from their base forms. But both types share the property that they
can leave behind relic forms which make the recovery of these processes by
linguistic reconstruction a possibility. What guarantees us the earlier
grammar in each of these cases are the forms like \textit{weg} (in the case where
the devoicing rule is lost and in the case in which it is reordered with
spirantization), \textit{plötsli} (in reordering of umlaut and \( o > \alpha \)), and so on.

On the other hand, reordering by I results in extension (polarization)
and so corresponds to simplification which affects only the structural
analysis of rules. In the case of the Slavic palatalizations (see Section 8),
for example, the voiced stop:voiced affricate alternation is polarized into
a voiced stop:voiced continuant alternation. It is clear that in this case
any forms which undergo the old form of the rules are also going to
undergo them after the reordering, so that relic forms which would al-
low reconstruction of the change could not be created.

These relationships are summarized in the following table.

<table>
<thead>
<tr>
<th>Reordering</th>
<th>Corresponds to</th>
<th>Reconstructible</th>
<th>Surface</th>
</tr>
</thead>
<tbody>
<tr>
<td>by I</td>
<td>Structural analysis only</td>
<td>No</td>
<td>Extension (polarization)</td>
</tr>
<tr>
<td>by II</td>
<td>Structural change</td>
<td>Yes</td>
<td>Leveling</td>
</tr>
</tbody>
</table>