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Contents

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Introduction

The papers in this volume were presented at the UCLA Conference on Historical Linguistics in the Perspective of Transformational Theory, February 1969. All but two deal with phonology. Although they are not equally concerned with linguistic change there is a common theme running through them—the need to remedy the excessive formalism in "classical generative phonology" as exemplified in Chapters 1-8 of *The Sound Pattern of English* by Noam Chomsky and Morris Halle.

The publication in 1968 of this long-awaited volume marked the end of one period in generative phonology and the beginning of another. The earlier period was characterized by a Saussurean separation of synchronic and diachronic description; in the later approach such a dichotomy has appeared increasingly impractical. Although both periods are represented in *SPE* it is the earlier approach which predominates until the last chapter. In Chapter 8 Chomsky and Halle state their basic methodological approach for the greater part of the book:

> First we develop a system of formal devices for expressing rules and a set of general conditions on how these rules are organized and how they apply. We postulate that only grammars meeting these conditions are "entertained as hypotheses" by the child who must acquire knowledge of a language. Secondly, we determine a procedure of evaluation that selects the highest valued of a set of hypotheses of the appropriate form, each of which meets a condition of compatibility with the primary linguistic data. [p. 331]

Then they add a word of caution on the idealization involved in this approach:

> We have been describing acquisition of language as if it were an instantaneous process. Obviously, this is not true. A more realistic model of language acquisition would consider the order in which primary linguistic data are used by the child and the effects of preliminary "hypotheses" developed in the earlier stages of learning on the interpretation of new, often more complex, data. To us it appears that this more realistic study is much too complex to be undertaken in any meaningful way today and that it will be far more fruitful to investigate in detail, as a first approximation, the idealized model outlined earlier, leaving refinements to a time when this idealization is better understood. The correctness of this guess, of course, will have to be judged by the long-range effectiveness of a research program of this sort, as compared with alternatives that might be imagined. In the meantime, this idealization must be kept in mind when we think about the problem of the "psychological reality" of the postulated mental structures. [p. 331]

It is clear, however, that the idealized model of language acquisition as an instantaneous process is less likely to be "fruitful" in diachronic linguistics since it excludes many possible explanations of linguistic change. It is therefore necessary in studying linguistic change to investigate somewhat less idealized approaches to the writing of grammar.

In Chapter 9 of *SPE* Chomsky and Halle acknowledge the limitations of the earlier approach:

> The entire discussion of phonology in this book suffers from a fundamental theoretical inadequacy. Although we do not know how to remedy it fully, we feel that the outlines of a solution can be sketched, at least in part. The problem is that our approach to features, to rules, and to evaluation has been overly formal. [p. 400]

Their proposed solution to this excessive formalism is the system of marking conventions and linking rules. In a sense, all the phonological papers in this volume are attempts to refine or improve on Chapter 9 of *SPE*. In particular, they are concerned with evaluative criteria that are not purely formal.

In quite different ways, Bach and Harms, Schane, and Labov all present evidence of the dangers of abbreviatory conventions for combining rules. If they are correct, then the evaluation metric cannot be a simple matter of counting symbols. Vennemann's argument that it is the *weight* of the marked features rather than the *sum* of such features that should count as a measure of complexity

has similar implications. It is therefore necessary to look for other than formal criteria in evaluative procedures.

One possibility is to replace marking conventions with a hierarchy of features. Foley and Vennemann present evidence in this direction from historically attested changes and Zwicky other evidence from allegro speech. Obviously, this is something which could be explored further in experimental situations as well as in comparative studies of the development of children's phonological systems. Jakobson's pioneering work in this area is now thirty years old, so there is justification for a re-examination of his conclusions in the light of the evidence that has accumulated since then.

A second possibility is to take a closer look at the evidence from phonetics. The relationship between phonology and phonetics has always been an uneasy one, for an obvious reason: The phonetician is likely to remind the phonologist that his idealizations are leading him rather a long way from the data of real speech. Labov's use of spectrographic evidence shows how impressionistic subjective judgments of vowel height may miss the point. The use of "naturalness" as a criterion demands that our knowledge of the phonetic facts be as accurate as possible. However, it is also important that the phonetician study speech in "natural" situations as well as in the laboratory, since stylistic variation may affect the results. Zwicky's study of allegro variants, for example, needs to be followed up by the collection of evidence on the actual use of such variants. This is particularly important since the speaker's intuitions are likely to be more accurate in reporting his formal speech than is the case with his informal speech.

A third area to be explored is the range of dialect variation that can exist within one language. Bailey's implicational scales show how dialect characteristics may be grouped into internally consistent sets. However, there is great need for more accurate information on dialect variation. Too often discussion of dialect differences is based on relatively crude distinctions or on subjective impressionistic judgments. Labov's work in New York City has demonstrated the feasibility of collecting accurate and representative data. We need comparable data for other dialects of English. Only then will it be possible to consider seriously the differences

between dialects that can be explained by rule ordering or by distinct underlying lexical representations. It may also be useful to investigate possible cases of unidirectional intelligibility in which the speaker of dialect A can easily understand the speaker of dialect B but not vice versa. Obviously, there are great problems in conducting empirical tests of such a situation, but the results might have important implications for a theory of naturalness.

There are no doubt many other ways in which naturalness could be investigated, but the main point to emphasize is that we need objective empirical support to supplement and correct the linguist's intuitions in this respect. In practice, this means taking a less idealized view of linguistic competence and being prepared to admit a wider range of data.

The study of natural rules and hierarchies is important if we are to understand one of the primary forces in linguistic change, namely, simplification. However, if all changes were of this kind languages would be a good deal more homogeneous than they actually are. Clearly, there are opposing forces which counteract this tendency. In Vennemann's terms, in addition to rules which decrease complexity there are rules which increase it. It is only in terms of the conflict of opposing forces that the dynamics of linguistic change can be understood. As Lakoff points out in her paper, the "drift" towards analyticity in Indo-European is paralleled by the contrary tendency in other language families. It would thus be wrong to assume that Lakoff's metacondition for "drift" in Indo-European ("segmentalize whenever possible") is a linguistic universal. However, it could belong to a set of universal metaconditions, any one of which may at a given time exert a powerful influence on linguistic change.

Bever and Langendoen claim that the conflict which produces linguistic change is "the historical competition between what makes a language easy to understand and what makes it easy to learn." While many people will be reluctant to accept Bever and Langendoen's view that a language with a greater variety of inflectional endings than modern English, especially with many irregularities, must be more difficult to learn, the possibility that such a claim could be correct is of great importance for a theory of linguistic change. We need empirical studies of "what makes a language easy

to understand" as well as of "what makes a language easy to learn." Such questions will not be simple to investigate, since preconceptions of the relative complexity of linguistic structures may be quite wrong. One source of useful information will be the evidence from the order in which children learn to use certain linguistic constructions and the kind of "incorrect hypotheses" which they set up on their way to the adult norms.

We cannot conclude that the conference presented the sense of unity that the preceding summary of the problems that were faced might suggest. The participants shared a common theoretical orientation, and a common concern with the historical development of languages. At the end, many seemed to share also a sense of despair that perhaps transformational theory had less to contribute than we had imagined. But the worth of the work represented in these essays gives us reason to believe that the sense of despair was the result of sheer exhaustion after two hard days of endless discussion rather than from lack of accomplishment and promise. Lest the preceding summary give a wrong impression of unity, however, we present a brief summation of the major import of each paper, as we understand it, to enable the reader to select more readily the essays of greatest concern to his interests.

Bach and Harms's paper revolves around the attempt to find "tighter constraints on phonological systems" than were found in "classical generative phonology" (e.g., *The Sound Pattern of English,* Chapters 1-8). After illustrating how historical evidence may support the use of such abbreviatory notations as alpha-variables, they tackle the problem of the *plausibility* of phonological rules. In brief, their argument is that innovations should be "plausible rules," but that the simplification that occurs in transmission may lead to "crazy rules," i.e., rules which fail to satisfy the criterion of "plausibility":

> Thus, languages have rules which are plausible or which can be derived from plausible rules by a sequence of steps involving (among other things) simplification, but in the process rules can become highly implausible.

The most important part of this argument is that if strict "plausibility conditions" are set up for simplification then it is impossible to see how languages get "crazy rules."

Bach and Harms's point is clearly of great importance for a theory of linguistic change, since it would mean that two distinct processes are at work: innovation and simplification. This in itself is not new, but the notion that simplification can lead to "crazy rules" raises the question as to what psychological reality such rules could have. If it should turn out that "crazy rules" operate only where there is an idiosyncratic residue of certain historical changes, then it may be wrong to claim that such rules represent an essential part of any single native speaker's linguistic competence. In other words, there may be minor regularities observable in the *langue* which are unsystematically represented in the competences of native speakers. Another way of putting it is to question the validity of having underlying representations and phonological rules for processes which are no longer productive for the native speaker. The existence of "crazy rules" in the grammar may be more a reflection of the ingenuity of the linguist than of the actual linguistic system.

Bailey's paper focuses on the need for a notation to represent the fact that the native speaker can usually understand other dialects than the one he normally speaks. Bailey uses implicational scales to group together blocks of rules in which the presence or absence of one rule implies the presence or absence of certain other rules, and he shows how such blocks of rules characterize dialects and their relation to each other. Bailey is thus aiming at a "panlectal" grammar, that is, a grammar which can account for all the dialects in a language, rather than concentrating on a single dialect which is claimed to be representative of the whole language. Clearly, no theory of linguistic change can have any empirical support unless it starts from the assumption that "a language" is at least as complex as the situation Bailey describes. Moreover, since one of the possible causes of linguistic change is borrowing from another dialect, implicational scales of the kind Bailey sets up may help to explain the possibilities and direction of borrowing.

Bever and Langendoen's paper confronts the question of how a well-defined system (or at least a highly stable one), such as the syntactic rules which characterize the competence of a fluent speaker of a language, can change. The Chomsky-Halle hypothesis of change in phonology, with late rules added which bring about restructuring through simplification, has no clear analog in syntactic

change: A possible analog might be the addition of conditions on a transformational rule which become so cumbersome that it causes the rule to change. But Bever and Langengoen's hypothesis is a rather different one, arising from the view that there are strategies of perception—strategies that enable the speaker/hearer to put his competence to use. Their hypothesis is that these strategies may come into conflict with the rules of the competence model in such a way as to change those rules. In particular, when some set of signals utilizable by the perceptual strategies are eliminated from the language (for any reason: e.g., the loss of Old English inflections either because the noun inflection system became cumbersome with many small and irregular classes or because purely phonetic processes were at work), new perceptual strategies must arise which take advantage of different devices in the surface structure for the reconstruction of the deep structure and semantic interpretation. These strategies are relatively intolerant of ambiguities of a certain type: what Hockett has called "garden-path" ambiguities, as in

*The secretary discouraged the man wanted to see the boss.

where, lacking an accusative marker on *man* and lacking a relative pronoun introducing the clause *(who) wanted to see the boss,* there would be hesitation in the strategy that assigns the phrase *the man* to the relationship of object of *discourage* or subject of *wanted.* Such hesitation appears not to be easily tolerable, and if there is no obvious strategy to eliminate it, then the structures from which it arises must be modified. In the development of this hypothesis, Bever and Langendoen provide a rich analysis of the history of English relative clauses, which may well provide the beginning of new and more insightful research into historical syntax. And it should perhaps not go unremarked that the "strategies" they propose bear a close family resemblance to the kinds of analysis that were called "signals grammar" in the early 1950s.

The theory of markedness, or what is natural and unnatural in phonological systems, appears repeatedly in these papers. James Foley presented the most far-reaching, and the most vulnerable, set of proposals in this direction at the conference, but in these proceedings his contribution is represented only in summary form because his presentation was frankly programmatic and not yet

fully buttressed with detailed argumentation and exemplification. He believes that "historical changes . . . occur in groups definable by meta-rules." The nonphonetic distinctive features that he uses for the characterization of these changes are motivated by the same considerations that motivated notions of markedness (naturalness): Some changes simply strike us intuitively as more natural than others, and cry out for explanation. Whether one accepts, as a basis for further investigation, the particular proposals that Foley puts forward is irrelevant to the conviction that Foley is seeking "explanation" in some more basic sense than the standard works on historical change. His ideas are stimulating and controversial.

William Labov deals with the progress of a sound change through the lexicon, from the earliest stage, when the only manifestations of the sound change are in certain environments and under certain conditions, until the final stage, when the change is complete in all contexts, apart from sporadic exceptions. For the first stage Labov uses the notion of a "variable rule," which predicts the situations in which an optional rule is most likely to apply. This is an attempt to show that "free variation" is not a random process but is subject to certain constraints, which can be expressed in the formal notation of a variable rule. In his accounts of the centralization of /ay/ and /aw/ on Martha's Vineyard and the raising of "short a" in New York City, Labov is able to illustrate how the constraints on the rule are gradually removed until the original variable rule finally becomes a categorical rule. However, perhaps the most important part of Labov's paper is his account of the raising of "short a" before front nasals. Among older speakers the nasal environment inhibits raising, whereas for younger speakers the presence of a following nasal accelerates the raising. Labov shows how an alpha-switching rule can reflect this change but he claims that such a notation distorts the nature of the sound change by representing it as a discrete change instead of the continuous movement in one direction, as the spectrographic evidence seems to indicate.

Labov is thus arguing against the use of complex schema for combining separate rules if this is done solely on the grounds that "the same" processes are at work:

> But the structural economy thus achieved is not all gain. The parallels are not exact in most cases, and in such gross rule schema we find more and more vagueness as to detail, which moves us farther and farther away from the articulatory, acoustic, and distributional facts in which explanation may lie. The paradox is that the deeper we exercise our talent for linguistically significant generalization, the more we lose the formal detail on which linguistic generalizations are based.

Labov is thus also arguing against "unnatural" or "crazy" rules and illustrating the kind of empirical evidence that may help to identify such rules. It is important to realize, however, that Labov is not claiming that there are no discrete sound changes, but in this particular case the data suggest that it would be a mistake to represent the change as discrete in spite of the fact that it is possible to describe the situation by means of an alpha-switching rule.

Robin Lakoff's paper considers a number of syntactic and morphological changes which have occurred widely and repeatedly in Indo-European languages as manifestations of a single general principle of change: "If there is a choice between a rule and a lexical item to produce a surface structure containing independent segments, as opposed to one containing morphologically bound forms, pick the former." She takes this as a metacondition on change, perhaps not expressible formally, but nonetheless influential within a specifiable range of languages in a specifiable period of time. The challenge of her paper is to find a way to represent such a condition, if it is real. And if it is not real, then, as she says, "all the Indo-European languages have been subject to an overwhelming series of coincidences." Her paper is thus part of the trend in linguistics today to seek out and confirm the broadest and deepest kinds of universals, universals which clearly exist and for which we have no satisfactory explanations yet.

Sanford Schane is concerned not so much with "crazy rules" as with "unnatural rules," e.g., the nasalization of final vowels in a dialect of German. He shows that in this particular situation an apparently unnatural rule may be the result of a sequence of natural rules, in the example cited the nasalization being caused by a final nasal consonant which is subsequently deleted. The question Schane then raises is how we can categorize "natural rules." He gives three examples of general processes or categories of natural rules: assimilation, preferred syllable structure, and maximum differentiation. As Schane points out, it is not enough to state that such processes

are at work; it is also necessary to have a precise statement of the conditions under which these processes can take place. Schane conjectures that assimilative processes may be largely due to the physiological properties of the articulatory mechanism, whereas dissimilation is more likely to be a consequence of perceptual strategies, i.e., psychological in origin. He also suggests that linguistic change may come about through conflict between the naturalness condition on segments in lexical representations and the uniqueness criterion which requires a single underlying representation for each lexical morpheme. His argument runs as follows: Because natural phonological rules guarantee that derived structures will conform to naturalness conditions, they may have the effect of violating the uniqueness condition on morphemes. Perceptual strategies may then cause restructuring of the underlying representation in order to restore the uniqueness condition. Although Schane's remarks on linguistic change are put forward tentatively, they draw attention to a problem generally ignored by the other participants at the conference, namely the motivation for a particular underlying representation and the effects of a change in such a representation. Finally, Schane goes on to suggest an evaluation metric in which the metatheory favors natural rules because they apply generally whereas less natural rules require restrictions placed on them. His conclusion: "Natural rules, which are context-free rules, are universal; unnatural rules, which are context-sensitive rules, are language-specific."

Theo Vennemann's paper proposes and elaborates extensive revisions in the theory of markedness, and its relevance to historical sound change. He argues that previous work has underestimated the possibilities inherent in the markedness notation, and develops a theory of marking in which there are two levels of representation, "a descriptive level, on which features are specified for what they are materially, independent of the context in which they occur, and an interpretative level, on which such material specifications are characterized as more or less predictable (more or less natural) in their . . . contexts." Within this framework he explains "a number of . . . apparently unrelated consonant changes as consequences of a single change" of a type that increases the complexity of all affected segments. The paper presents a wealth of detailed evidence

for this view and appears to represent a substantive and permanent enrichment of the Chomsky-Halle markedness conventions (Chapter 9 of *SPE*).

Arnold Zwicky, rather than dealing directly with some aspect of historical change as most of the other authors do, is concerned with "some aspects of English phonology which supply evidence about the content of grammatical theory and thus, derivatively, about linguistic change." In particular, he is concerned with the view that there are hierarchies among the segments of phonological systems, that one such hierarchy is the sequence "vowels, glides, [r], [l], [n], [m], [ŋ], fricatives, stops," and that evidence for such a hierarchy is to be found in the variants that are produced by the rules of allegro speech. He presents evidence concerning the nature and functioning of such rules as a prolegomenon to work in historical linguistics that remains to be done.

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1.

How Do Languages Get Crazy Rules?

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It has been proposed that one of the major internal forces in linguistic change is rule simplification (Bach 1968, Harms 1966, Kiparsky 1965, 1968). This hypothesis is a corollary to the more general hypothesis that the language learner acquires his language by constructing the simplest grammar compatible with the data presented him. If the data were equally complete for every learner and if grammar construction were completely determinate, the language would never change (assuming no external influences). But the data made available to the learner or noticed by him is, as we all know, highly fragmentary and skewed. Hence, other things being equal, the new grammar will be simpler (more general) than the original grammar. Since one way in which one grammar may be simpler than another is in the form of a particular rule, we would expect rule simplification to occur with more than random frequency. Of course other things never are equal and many other forces impinge on the development of a language.

Two kinds of empirical support could be sought for such conjectures. On the one hand, we can look to studies of language acquisition, and we could even devise simple experiments to test the hypothesis. Let us teach a group of children an invented language, which contains the rule "make vowels lax before /p/ and /t/ but not before /k/" (where these are the only voiceless stops in the language). Now let us systematically exclude from the sentences

taught the children all items containing postvocalic /k/. Our prediction will be that the children's language will contain the rule "make vowels lax before voiceless stops."

Second, we can look to historical change to see if, in fact, rule simplifications have taken place. If we find many examples of simplification and no examples of complication, then our hypothesis is confirmed. We can also make predictions about the direction of spread for adjacent dialects that show differences in the form of a rule. Suppose that Dialect A has a rule R, and Dialect B a rule R′, which is in the obvious sense a counterpart to R but not as simple. The hypothesis about simplification implies that the rule was transmitted (if it was transmitted at all and not independently developed) from Dialect B to Dialect A. This then is a factual consequence of the hypothesis and we can look for independent evidence to see if it is correct. We can actually observe the process of rule transmission (see various results reported by Labov, e.g., 1965), and it behooves us to examine afresh the evidence about the chronology and geographical diffusion of well-known changes like the High German consonant shift.

Notions like simplification are not, as we are periodically reminded, given to us somehow in advance of linguistic investigation. The evaluation metric with its associated set of notational conventions represents a hypothesis about language, subject to falsification or confirmation. Moreover, as Paul Kiparsky 1968 has neatly demonstrated, the study of linguistic change provides an important testing ground for hypotheses about the nature of grammars. Let us consider a new example of the way in which inferences about linguistic theory can be drawn from historical change.

The logic of the argument is as follows: We are interested in a notational convention made available by a particular formulation of the evaluation metric. We need to know whether the convention represents linguistically significant generalizations, such as are incorporated into a real grammar in someone's head. We find a particular historical change where at Stage II a rule exists which is related to a rule at Stage I in exactly the way expressed by the convention in question. On the assumption that language change proceeds by simplification of rules, that is, by incorporating linguistically significant generalizations, we conclude that the hypothesis is confirmed in some small measure. We find a further confirmation of the linguistic reality of the convention in the occurrence of a later change in which exactly the rule schema in question is lost.

We choose for an example the Greek letter variable convention for abbreviating pairs of rules related in the well-known way (in this instance for expressing agreement in feature specifications). The reason for our choice is that it is this convention which will figure crucially in our later argument about the role of phonetic plausibility in sound change. (Part of the following is alluded to briefly in Bach 1968.) The facts involved are summarized as follows. In the history of German a rather remarkable sequence of changes occurred:

$$\text{I. } a > e \,/\, \_i$$
$$\text{II. } a > o \,/\, \_u$$
$$\text{III. } \begin{matrix} e > a \,/\, \_i \\ o > a \,/\, \_u \end{matrix}$$

The explanation of these parallel changes in the German diphthongs, where a sound change was precisely reversed, provides a particularly striking confirmation of the view of historical change as change in grammars (and in particular, grammars provided by a specific set of assumptions made in generative phonology).

In the eighth century in Old High German a rule was added which changed *ai* into *ei*. The following evidence justifies the claim that the rule remained part of the synchronic grammar of Old High German and was not just a change in underlying representation. In Ablaut classes II–V the form of the first and third singular preterit indicative shows a replacement of the nuclear vowel of the stem (a front vowel) by *a*. Thus we have

	Base form	Pret. sg. 1/3 ind.
II.	fleug-	flaug
III.	rinn-	ran
	werf-	warf
IV.	stel-	stal
V.	geb-	gab

In Class I we have (after the change of *ai* to *ei*)

rīt- reit

We posit an underlying representation for *rīt-* as *reit-*. There is independent support for this representation in the other forms, where the underlying nuclear vowel *e* is deleted to give *ritum* like *flugum* (from *fleug-*). The independence of the rule changing *ai* to *ei* (as against an addition to the rule giving the preterit singular form) is shown by the fact that we would need such a rule anyway at some level to account for the fact that after the change we have *ei* in all cases. That is, the rule applies not only to diphthongs derived by the Ablaut rule but to underlying diphthongs as well. Thus we posit

$$\text{Rule O}': \begin{bmatrix} V \\ +\text{low} \end{bmatrix} \rightarrow \begin{bmatrix} -\text{back} \\ -\text{low} \end{bmatrix} / - \begin{bmatrix} +\text{high} \\ -\text{back} \\ -\text{cons} \end{bmatrix}$$

Slightly later (toward the end of the eighth century, completed by about 850) we find that forms like the preterit of *fleogan* assume the form *floug* (and again all *au* become *ou*). Evidently the rule has been extended in just the way expressed by changing the specification [-back] to [α back]:

$$\text{Rule O}: \begin{bmatrix} V \\ +\text{low} \end{bmatrix} \rightarrow \begin{bmatrix} \alpha\ \text{back} \\ -\text{low} \end{bmatrix} / - \begin{bmatrix} +\text{high} \\ \alpha\ \text{back} \\ -\text{cons} \end{bmatrix}$$

Language history is full of examples of this kind of generalization (for the OHG facts see the relevant sections of Braune/Mitzka 1963). Thus we have rather strong historical support for the hypothesis that the alpha notation expresses linguistically significant generalizations.

Rule O remains operative throughout the MHG period. But, as we have noted, in the period between then and the modern language the diphthongs revert to their original form. Thus, just as with the laxing rule of English, which was used by Kiparsky 1968 as support for the brace notation, we find strong confirmation of the linguistic (i.e., psychological) reality of the rule schema incorporating the abbreviatory convention in question. Without the alpha notation we would have two completely disconnected changes happening twice in the history of the language. Without the notion of linguistic change as rule addition, simplification, and loss we would have no explanation for the initial change and the reversion of the forms to their original form. Incidentally, this sequence of changes is an especially bad case for versions of autonomous phonemics with an invariance condition. Throughout the period in question there are *o* and *e* phonemes to which the first members of the diphthongs *ou* and *ei* must be assigned. If sound change must be stated in terms of autonomous phonemes, then we have two mysterious facts to explain: first, a very unnatural shift (lowering and centralizing of both *o* and *e* before high vowels); second, the fact that this implausible shift is an exact reversal of a change that the language has just undergone.

So far we have done nothing more than consider a paradigm case for the role of simplification as generalization in sound change, and we have illustrated the way in which historical evidence can be brought to bear on questions of synchronic theory. Such arguments are cogent to the extent that we can place them in the context of relatively specific hypotheses about theoretical constructs like "rule," "evaluation metric," "alpha-variable," and the like. This discussion as well as those referred to above have been carried out largely under the assumptions of what might be called classical generative phonology (as represented in Chapters 1-8 of Chomsky and Halle 1968). A number of linguists have begun to emphasize the serious limitations in this account of phonological theory. The main criticism has been directed at the lack of substantive assumptions about the content of rules, the particular generalizations that are available for the human (as opposed to the dolphin) communicator and so on. Under the headings of "markedness," "naturalness," or "archetypal rules" linguists have begun to search for tighter constraints on phonological systems and rules.[1] Such discussions frequently take the following form: Linguistic theory should define a class of possible grammars. Under Theory X it is possible to construct a grammar with such and such a rule. We agree that the rule is inadmissible. Therefore Theory X is wrong.[2] Or as a variation: Theory Y does not distinguish between two rules in terms of cost, naturalness, expectedness, and the like. We agree

that there is a difference which should somehow be reflected in the evaluation measure (to be tested in the usual highly indirect way). Therefore, Theory Y is inadequate.

We can illustrate this point by looking again at the OHG rules O′ and O. Given only the formal measure available in classical theory, Rule Z would be an equiprobable simplification:

$$\text{Rule Z: } \begin{bmatrix} \text{V} \\ +\text{low} \end{bmatrix} \rightarrow \begin{bmatrix} \alpha\,\text{back} \\ -\text{low} \end{bmatrix} \; / - \begin{bmatrix} +\text{high} \\ -\text{back} \\ \alpha\,\text{cons} \end{bmatrix}$$

that is, $a > e$ before i, and $a > o$ before palatals, palatalized dentals, and labial consonants.

In order to preserve neutrality toward specific revisions we refer to these various attempts to remedy such defects in phonological theory as attempts to come to terms with the requirement of phonological *plausibility* (whether in underlying systems or in rules—we shall be concerned mainly with the latter). We are fully in agreement with the intent of such revisions. In the rest of this paper, however, we shall argue against one particular way in which some linguists have suggested bringing plausibility considerations into linguistic theory.

What we are concerned with is the suggestion that plausibility constraints should be reflected directly in the evaluation metric.[3] Our argument is based on the fact that such an attempt runs into insuperable difficulties when we try to understand historical changes of the sort just considered. Further, we shall suggest that plausibility and simplicity play quite different roles in language change, the former bearing largely on innovation, the latter on transmission. The central fact that needs to be explained is that although some rather strong plausibility conditions seem to play a crucial part in determining what rules a language can initiate, these same conditions do not seem to bear any relation to changes that take place in rules.

Thus, languages have rules which are plausible or which can be derived from plausible rules by a sequence of steps involving (among other things) simplification, but in the process rules can become highly implausible. In short, languages have crazy rules. This fact is inexplicable if simplification is constrained too much by plausibility conditions.

First we would like to establish that we are not just setting up a straw opponent. Unfortunately, although there is a great deal in the air and even some in print about the general considerations we have just sketched, there is relatively little in the way of specific proposals toward the position we are attacking. At the very least, then, we can hope to raise some questions and perhaps make some negative suggestions about future work.

We can, however, cite a few examples. In Chapter 9 of *The Sound Pattern of English,* Chomsky and Halle introduce the notion of linking. Two mechanisms for blocking the application of linking rules are also suggested, both connected with questions of rule simplification.

Suppose a language has a rule fronting high vowels in some environment. According to linking theory the result will be an unrounded front vowel unless the rule is complicated by a specification for rounding. Thus the phonological rule P will change *u* to *i* by linking:

$$\text{Rule P: } [+\text{high}] \rightarrow [-\text{back}]$$

In order to preserve rounding for vowels like *ü,* which might result from such a rule, it will be necessary to complicate the rule:

$$\text{Rule P′: } \begin{bmatrix} +\text{high} \\ +\text{round} \end{bmatrix} \rightarrow \begin{bmatrix} -\text{back} \\ +\text{round} \end{bmatrix}$$

At first blush, this seems to be an attractive idea. We can now interpret certain sound changes as simplifications of rules like P′. So, for instance, Ancient Greek *υ* is usually interpreted as a front rounded vowel. This vowel later becomes unrounded to *ι*. Obviously, this is exactly the change from P′ to P.[4]

On second thought, however, certain embarrassments arise. Note first that in order to overcome a certain technical defect in the proposal of Chomsky and Halle for blocking linkage we have had to introduce a specification of the nonoptimal feature "+round" in both the structural change and structural analysis parts of the rule. A rule like (56) in Chomsky and Halle 1968: 433:

$$[+\text{high}] \rightarrow \begin{bmatrix} -\text{back} \\ +\text{round} \end{bmatrix} \; / \text{ in certain contexts}$$

will have the unwanted effect of changing not only the *u* to *ü*, as desired, but also *i* to *ü*, unless we add [+round] to the structural analysis, as we have done. But now we have a unique kind of rule in phonological theory, namely, one in which we are forced to mention a feature specification which is unchanged by the rule in both the analysis and the change parts.[5] What is particularly objectionable is that the notation suggests that such a rule has exactly the same status with respect to a simplicity metric as a rule like the following:

$$\text{Rule Q: } \begin{bmatrix} +\text{high} \\ +\text{round} \end{bmatrix} \rightarrow \begin{bmatrix} -\text{back} \\ -\text{tense} \end{bmatrix}$$

that is, *u, ū* > *ü*, whereby two features select the natural class [u ū] and two features are altered by application of the rule.

A second, somewhat weaker, argument against the proposed blocking device and the concomitant idea that ordinary simplification of such a rule leads to changes like the one in Modern Greek is that three possible changes could actually take place with respect to a rule like P′. Besides P we could also have had P″

$$\text{Rule P''}: [+\text{high}] \rightarrow \begin{bmatrix} -\text{back} \\ +\text{round} \end{bmatrix}$$

Now P″ suggests a highly unlikely development (we know of no language that has only *ü* in the high vowel range, and we know of no cases of such a change even when some other vowels come to fill the *i* and *u* places by other rules). (A rule P‴ produced by deletion of "+round" in the change would automatically merge with P.) There could be even wilder results from P′, say a rule fronting all rounded vowels, or rounding all high vowels, but these are troubles shared by all accounts of simplification, as with Rule Z above. The basic trouble here is that the format of Rule P′ suggests that it differs from Rule P by two independent properties, whereas all of us would probably agree that it really differs in only one—its behavior with respect to linking or some similar device for specifying values of features connected to the new specifications in the rule.

Third, the proposed explanation of the change from P′ to P by simplification and linking provides no way to account for the fact that in every case that we know of, rules such as P are not automatically accompanied by the further shift of [ü] to [i] suggested

by linking. Where we have documentary evidence we find that this further shift occurs later, and rather significantly, it generally occurs by context-free rule; for example, Greek, Estonian, and Swiss German and Yiddish dialects (in Estonian only the first /ü/ of the stem is excepted). To take another example, the [r] which develops from /z/ by rhotacism in Germanic is distinguished in Runic Norse from original /r/. Thus, the moving force behind such changes seems to be a tendency to decrease the markedness of lexical items rather than a tendency toward natural rules. Note that the interpretation of later shifts like *ü*>*i* as the result of rule addition as opposed to a linking convention leads to testable predictions. By linking theory we should expect a development such as rule (a) u → ü to rule (b) u → i to result in a differentiation of rule-derived segments and underlying representations: If a language has an [ü] from an underlying /ü/ and an [ü] derived by a rule like an Umlaut rule, then the change from a system with rule (a) to one with rule (b) will result in a split of the relevant segments into [ü] (from lexical /ü/) and [i] (from /u/ via rule (b)). The fact that we have no instances of this split is strong evidence against the linking explanation.

By linking we should also expect the unrounding of front vowels, say *ü ö* to *i e*, to occur as a single historical process. Yet, even in the case of English fronting, the only case cited in support of unrounding by linking (see below), the shift [ö] > [e] was historically prior to [ü] > [i].

We encounter similar difficulties in the all-or-none condition posited by Chomsky and Halle 1968:43 as a second hypothesis about blocking of linking: "A linking rule applies either to all or to none of the segments formed by a given rule." We have here a rather different connection between the evaluation metric and marking theory. In the cases just considered rule simplification leads to linking and the creation of less highly marked segments. But by the all-or-none principle rule simplification is supposed to lead to blocking and the creation of more highly marked segments. This result in itself might lead us to suspect that the attempt to incorporate notions of rule plausibility directly into the evaluation metric is misconceived. But we can cite direct evidence against the all-or-none principle.

From the all-or-none principle and the simplification hypothesis we can derive the prediction that generalization of a rule from seg-

ments that are linked by a particular convention to segments that are not will lead to a rather drastic change. Suppose for instance that there is a rule which spirantizes *p* or *t* (or both). By linking we will get *f* and *s*. If the rule is extended to include *k*, linking will be blocked and the results will now be ϕ, θ, and *x*, that is $f, s > \phi, \theta$. Note further that if the generalization is in the opposite direction, we must predict that the marked labial and dental spirants will be the direct result and that *f* and *s* will only arise by later addition of rules. The High German Sound Shift is a typical example of a rule that was spread by generalization. Needless to say there is not a shred of evidence that either of these results took place.

As a matter of fact the HG sound shift provides some particularly telling evidence against the attempt to use linking to explain changes from marked to unmarked segments. At the beginning of the OHG period the shifted segments remain distinct not only from the results of the Germanic sound shift but also from the inherited /s/ of Indo-European. That is, we have the voiceless obstruents: /p^f t^s k f_1 θ h f_2 s_2 x s_1/ (for Franconian dialects, no particular claims about underlying forms or assignment to phonemes is implied). Of these /f_1 θ h/ and /f_2 s_2 x/ result from the first and second sound shifts, respectively. There is quite firm evidence for the distinctness of these segments. Ultimately in German we get a system in which some of these distinctions are wiped out. In particular, /f_1 f_2/, /s_1 s_2/, /h x/ merge in varying ways, respectively, and /θ/ disappears completely. If we had records for only the first and last stages of this development we would probably consider it to be a particularly clear instance of the way in which linking operates to reduce the number of highly marked segments. But although the development as a whole is strong evidence that historical change tends toward this goal, we can show by direct documentary evidence that in case after case linking plays not the slightest role. For instance, /θ/ disappears as a result of merger with /d/, and we can follow the course of this merger as a generalization of a conditioned sound change (i.e., rule addition). The merger of /s_1 s_2/ is the result not of a change in the segment produced by a rule but a change in the underlying IE /s_1/. Transcriptions of Slavic materials in Bavaria show that the IE /s_1/ was in the [š] range, while the shifted segment was [s]. Obviously, if change of a rule to incorporate linking is to be invoked to explain a change from marked to less marked segments, it must be the segments resulting from the rule that change and not, as here, the underlying segments.[6]

The only real evidence given in any detail by Chomsky and Halle to support the all-or-none principle is the difference between English fronting and German Umlaut. Since the English fronting rule does not affect the low vowels, while the German Umlaut rule does, the linking convention for rounding operates in the first but not the second case. Hence, we have English alternations like *mouse/mice,* but German ones like *Hut/Hüte.* But this "explanation" is only possible if we are willing to swallow a completely unsupported assumption about German Umlaut. As noted (in Chomsky and Halle 1968:433, fn. 19) the result of the Umlaut rule in German for /a/ is not a low front vowel, but /e/. The simplest formulation of the facts is to incorporate the specification [-low] in the structural change of the German rule. But now the all-or-none principle should affect the fronted vowels in exactly the same way in German as in English. There are many German dialects in which the results of the Umlaut rule are front unrounded vowels, but there is absolutely no connection between this fact and the treatment of low vowels. Note further that in Old English (when the Umlaut rule affected all vowels just as in German, but retained a low front long /æ/) the derounding of the front vowels was already in operation. The all-or-none principle has no more explanatory force for this example than for the facts of the HG Sound Shift.

In fact, a moderate sampling of changes in various languages which might bear on the correctness of the principle leads to the conclusion that whatever might have been explained by the principle must be explained in some other way. Note that in each case where the principle fails we are forced either to posit extra rules or to complicate the rule in question. It would seem that the burden of justification for such rules or complications must fall on those who want to maintain the principle that necessitates them.

In the Japanese dialect of Nagoya /oi/ and /ui/ go to [ö] and [ü], respectively (Wenck 1954:154). Since the low vowel is not affected we should expect linking to produce unrounded vowels. In Spanish, voiced stops are spirantized after vowels. Since the rule affects all three stops, the principle correctly predicts that bilabial

and interdental spirants will remain as such. In Amharic, there is a similar rule which affects only /b/ (as in *Addis Ababa*); the result is not [v] but a bilabial spirant. In both the first and second Sound Shifts of German /p/ goes to /f/ against the prediction. In Japanese, initial /p/ goes to a bilabial spirant, again against the prediction.

Let us reiterate that we are not arguing against marking theory as such, or the general intent of putting more substance into phonological theory. What we are arguing against is the particular way in which linking theory has been used to build plausibility directly into the evaluation metric.[7]

We have shown that the mechanisms of linking and its associated blocking devices lead to great difficulties when we try to understand historical change. Aside from the arguments against the particular devices discussed, however, we can make a more general argument against all such attempts. If simplification is the major force in internal language change, and if plausibility enters directly into the evaluation metric, then rules should in general tend to become more plausible. But rule change does not seem to lead to more plausible rules; therefore we must conclude that one of the premises is false. Since there seems to be a lot of evidence in favor of the first premise, there must be something drastically wrong with the second. Let us now examine two cases which support this reasoning. In each case we exhibit a relatively implausible rule claimed to exist in a language, and then seek an explanation for its presence as a result of rule simplification which goes its way, blind to plausibility constraints.

Basic to the argument, of course, is the assumption that the rules in question are present in the languages. It will not do to argue that the rules are incorrect because they are implausible. If we allow ourselves to construct an ad hoc sequence of plausible rules with appropriate changes in underlying forms just to avoid apparently unnatural rules, we will completely trivialize any hypothesis about plausibility constraints in phonology. Moreover, in the two situations we shall consider, it appears that we could construct such ad hoc plausible rules only at the cost of introducing other unmotivated and just as unnatural rules. To take a simple hypothetical example, suppose we find a language in which underlying /tu/ and /ti/ are realized phonetically as [ču] and [ti], respectively—clearly an unexpected and implausible result. A much more plausible rule would be one taking /t/ to [č] before /i/. But we can only do this by positing an interchange between /i/ and /u/. Now, the worst case will be one in which we have some independent motivation for the original assignments of the phones to underlying vowels, say paradigms like the following:

Form a	Form b
aku	aki
apu	api
aču	ati

In this instance, we will have to posit a rule interchanging /i/ and /u/ exactly after /t/, and then change just these vowels back to their underlying forms in just the context where they have done their well-behaved phonological job. At best there will be no evidence for the underlying character of the vowels, and we will have to have an implausible context-free interchange of vowels.[8]

There is a well-known alternation in Japanese between dental stops, affricates, and spirants. The synchronic evidence for the phonological rules is very good, having to do with quite regular paradigmatic changes in verbs, and a widespread voicing of initial segments in compounds. We can illustrate by citing possible syllable types (letting *a* represent not only itself but the other non-high vowels *e* and *o* as well):

ta	či	cu
da	ǰi	
sa	ši	su
za		zu

Note the neutralization of the voiced obstruents /z/ and /d/. We can formulate a rule to account for these phonetic results as follows:

$$\text{Rule J:} \begin{bmatrix} -\text{sonorant} \\ +\text{coronal} \\ \langle +\text{voice} \rangle \end{bmatrix} \rightarrow \begin{bmatrix} +\text{delrel} \\ +\text{strident} \\ \alpha\ \text{anterior} \\ \langle \alpha\ \text{continuant} \rangle \end{bmatrix} / - \begin{bmatrix} \text{V} \\ +\text{high} \\ \alpha\ \text{back} \end{bmatrix}$$

Under any feature system that we can think of the rule exhibits interconnections that do not seem to have any particular phonetic

or phonological plausibility. The appearance of /t d s/ as [č ǰ š] before /i/ is a result that can be matched by rules in many languages. But the affrication of /t/ before /u/ is not. A change of [d^z] to [z] seems quite plausible (and can again be matched in many languages), but a change of [ž] to [ǰ] does not. Finally, the neutralization of the voiced segments in favor of an affricate before /i/ but a continuant before /u/ seems highly haphazard.

We suggest that this rule is to be explained as a series of simplifications and amalgamations of more plausible rules. Assume that some dialect adds a quite plausible rule palatalizing the dentals before /i/:

$$\text{Rule J1: } \begin{bmatrix} -\text{sonorant} \\ +\text{coronal} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{delrel} \\ +\text{strident} \\ -\text{anterior} \end{bmatrix} / - \begin{bmatrix} \text{V} \\ +\text{high} \\ -\text{back} \end{bmatrix}$$

Now suppose that the rule was generalized in some dialects by alpha-generalization over anterior and back:

$$\text{Rule J2: } \begin{bmatrix} -\text{sonorant} \\ +\text{coronal} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{delrel} \\ +\text{strident} \\ \alpha\ \text{anterior} \end{bmatrix} / - \begin{bmatrix} \text{V} \\ +\text{high} \\ \alpha\ \text{back} \end{bmatrix}$$

We would now have the alternations that would occur if we filled in the missing members of the table above:

ta	či	cu
da	ǰi	d^zu
sa	ši	su
za	žǐ	zu

This is the state of affairs that appears to have existed in the seventeenth century and which is fixed in the *kana*-orthography (on this whole question, see Wenck 1959: 351-78). It is also the situation in the present-day Tosa dialect (Wenck 1954: 170). (Note that there are also dialects that apparently generalized J1 in a different way, by deletion of "+high," thus palatalizing before all front vowels.[9])

Now all that we need to explain is the neutralization of the voiced segments in the peculiar way noted. Once again we can posit the addition of a very plausible rule in some dialects, taking [d^z] to [z]:

$$\text{Rule J3: } \begin{bmatrix} -\text{sonorant} \\ +\text{strident} \\ +\text{voice} \\ +\text{anterior} \end{bmatrix} \rightarrow [+\text{continuant}]$$

We suppose that this rule was also generalized:

$$\text{Rule J4: } \begin{bmatrix} -\text{sonorant} \\ +\text{strident} \\ +\text{voice} \\ \alpha\ \text{anterior} \end{bmatrix} \rightarrow [\alpha\ \text{continuant}]$$

This gives exactly the change needed: [ǰ] and [z] collapse to [ǰ]; [d^z] and [z] to [z]. Now we have two rules dealing in large part with the same sets of features and involved in related paradigms:

mata-	mači-	macu
hanasa-	hanaši	hanasu
cuyo ~	zuyo	
šima ~	ǰima	

They must all be collapsed into the unholy Rule J.

Note that the hypothesis about linking provides no way to get beyond the first rule, which would presumably be something like this:

$$\text{Rule F: } \begin{bmatrix} -\text{sonorant} \\ +\text{coronal} \end{bmatrix} \rightarrow [+\text{high}] / - \begin{bmatrix} \text{V} \\ +\text{high} \\ -\text{back} \end{bmatrix}$$

Various linking conventions would then have the same effect as our Rule J. But now there is no way to proceed to explain the affrication of dentals before high back vowels. (Note that deletion of "–back" would lead to [č] here as well.) This example illustrates the major defect of the linking proposals. It makes no sense to talk about the generalization of a linking convention. The germ of truth contained in linking theory must be captured by a theory about changes in *rules*. Only if we actually end up with a rule like J1 can we use the explanatory power of rule simplification to account for the changes posited above.

The objection will no doubt be raised that Rule J is an artifact, that we have illegitimately lumped together into one schema three

disparate rules, say Palatalization (ti > či), Affrication (tu > cu), and Neutralization of Voiced Stridents. There are three answers to this objection. The first is that such a solution would fail to capture the generalization that the three rules affect the same natural class of obstruents. The second is that splitting the rule in three fails to account for the fact that the distribution of the rules in Japanese dialects is not independent (the last rule, of course, depends on the other two). There are dialects with Palatalization, and dialects with both Palatalization and Affrication, but as far as we know none with just Affrication (*ti > ci) (Wenck 1954: 139-77) except as a result of the special development noted below. The third answer is that no dialect shows a separation of the putative rules by some other rule, and in the two cases we found that involve apparent reordering, the reordering takes place around all three. On Hachijō-jima (Wenck 1954: 152), initial /r/ becomes [d] and the resulting segment undergoes all the results of Rule J. We must assume that the rule was either introduced directly before Rule J or reordered. Many Japanese dialects have low-level rules which tend to minimize the difference between /i/ and /u/ (normally only weakly rounded) after spirants and affricates, including the spirant allophones of *h* from underlying /p/. In some, the rule has apparently been reinterpreted as a rule backing /i/ after dentals and reordered before Rule J, so that we have a complete collapse of syllables with stridents followed by high vowels (in favor of the forms found in other dialects before /u/: [cï, sï, zï]).

The Russian Oboyan dialect is known for its strange rule which dissimilates pretonic nonhigh vowels after palatalized consonants: They become [i] if the following stressed vowel is low, and [a] if the stressed vowel is not low (discussed in Harms 1968):

$$\text{Rule R: } \begin{bmatrix} V \\ -\text{high} \end{bmatrix} \rightarrow \begin{bmatrix} \alpha\,\text{high} \\ -\alpha\,\text{low} \\ -\alpha\,\text{back} \end{bmatrix} / \text{ C}' \_\_ \text{ C}_0 \begin{bmatrix} V \\ +\text{stress} \\ \alpha\,\text{low} \end{bmatrix}$$

Thus, at the stage in a derivation at which Rule R applies the following changes take place:

$$[\text{e}\ \epsilon\ \text{o}\ \text{ɔ}\ \text{a}] \rightarrow \left\{ \begin{array}{l} \text{i when [}\epsilon\ \text{ɔ a] follow} \\ \text{a when some other vowel follows} \end{array} \right\}$$

A partial explanation for this unlikely set of changes is suggested by the subrule expansions of R:

$$\text{R-a } \begin{bmatrix} V \\ -\text{high} \end{bmatrix} \rightarrow \begin{bmatrix} +\text{high} \\ -\text{low} \\ -\text{back} \end{bmatrix} / \text{ C}' \_\_ \text{ C}_0 \begin{bmatrix} V \\ +\text{stress} \\ +\text{low} \end{bmatrix}$$

$$\text{R-b } \begin{bmatrix} V \\ -\text{high} \end{bmatrix} \rightarrow \begin{bmatrix} -\text{high} \\ +\text{low} \\ +\text{back} \end{bmatrix} / \text{ C}' \_\_ \text{ C}_0 \begin{bmatrix} V \\ +\text{stress} \\ -\text{low} \end{bmatrix}$$

Expansion a is considerably more reasonable than expansion b. In fact, expansion b can be explained as an alpha-variable extension of an earlier rule equal to R-a.

Yet, even taking R-a as the source for rule R is not entirely satisfactory. It is perfectly natural for an unstressed vowel to become [i] following a palatalized consonant, but it is difficult to understand that this happens only when the following vowel is low.

The history of two of the above Oboyan low vowels shows that they were originally high lax vowels, corresponding to IE */i u/, and deeper synchronic analysis shows that even in the modern Russian dialects they are best treated as underlying high vowels (cf. Lightner 1965). In so-called strong position they become low; in weak positions they are lost (e.g., Rus. [rɔt] < /rutu/ "mouth," [rta] < /ruta/ "mouth (gen.)"). If we now restate rule R-a in terms of [ɛ ɔ] as underlying high vowels and consider the rule not to apply before [a], the "low stressed vowel" of the environment is now replaced by "high lax stressed vowel," and the result is a significant shift in the direction of greater plausibility. The rule now becomes an instance of phonetically motivated assimilation: (a) nonbackness is determined by the preceding consonant and (b) highness is determined by the position between palatalized consonant and high vowel. After the shift of /i u/ to low vowels the transfer of this rule could also be based on the alternation before stressed [ɛ ɔ], and the most general phonetically based rule would specify the environment as "low stressed vowel," thus pulling in [a] as well. Once the environment is generalized to include [a], there is little point in trying to state it in terms of underlying high vowels and it becomes dissimilatory. It is this process of dissimilation which is generalized to give the modern Oboyan rule.

In the original rule, in addition to being high and front, the vowel affected was also made tense, thus [ī], the primary source of modern Russian [i]. Otherwise it would be subject to the same lowering and deletion rules as lax [i]. The reason that the change originally occurred only before the *lax* high vowels may be related to their status as reduced vowels in Slavic and the vowel-lengthening type of stress which developed in Russian. The stress-induced quantity was thus shifted to the pretonic vowel, which became tense. We note, however, that in general Russian pretonic vowels are less strongly reduced than the other unstressed vowels.

The case for the Oboyan example rests in part on the assumption that certain types of dissimilation generally involve adjustments of a primarily prosodic nature, such as Grassmann's Law or backness dissimilation of high vowels after homorganic glides.

In conclusion, the fact that languages have plausible rules is, in our opinion, the result of strong naturalness constraints on the initiation of phonetic rules. These constraints are essentially diachronic and should not be incorporated into the simplicity metric. Although little is known of the nature of these constraints, we find the notion of archetypal rule as developed in Foley 1968 an attractive basis for their formulation. The apparent historical striving toward more optimal segments is most likely to find explanation as a consequence of some kind of marking theory, although attempts to build marking into rule economy (linking) seem ill-founded.

The existence of implausible rules can be shown to result in large part from the transmission and simplification of plausible rules. There are obviously substantive constraints on the kinds of simplification that can take place, such as limitations on the features which can be related by Greek letter variables. We do not consider simplifications like those leading to Rule Z or Rule P″ to be possible. Yet these constraints must be much weaker than those restricting rule initiation, perhaps identical with the universal conditions needed to distinguish permissible from impermissible uses of diacritics suggested by Chomsky and Halle 1968: 170, fn. 7.

NOTES

1. Among the linguists who have been discussing these questions are Chomsky and Halle 1968, Cairns 1969, Lightner 1968, Foley 1968, and Stampe (in several papers and lectures).

2. Such arguments involve a tacit and probably unwarranted assumption that the class of possible grammars for natural languages is derivable from theories of linguistic competence alone. In fact, theories about competence might provide a class of grammars from which the class of possible actual grammars is selected by other theories about language acquisition, performance, neurophysiology, etc.

3. Note the following typical passages:

Chomsky and Halle 1968: 427: "To give a general solution to the problem in these terms, we would have to extend the theory of rule plausibility so that it would automatically provide a 'simplest interpretation' for each possible case."

Foley 1968: 6: "More generally, I make the requirement that every putative first order rule [i.e., rule in an individual grammar EB RTH] must be an interpretation of a second order rule."

Lightner 1968: 199: "If assimilation is a natural and favored process in the physical realization of utterances, it seems correct to say that a natural and favored way of writing abstract phonological rules is in terms of assimilation."

4. Note that the linking explanation presupposes the existence of a rule fronting /u/ in Ancient Greek. We assume such a rule for the sake of the argument without attempting to support it. If there is no synchronic evidence for such a rule at the time when the further change to *i* is supposed to have taken place, then the linking explanation for the further development obviously has no basis whatsoever.

5. One could circumvent this objection by adding "+back" in place of "+round" on the left. Obviously this is just a trick, possible only on the assumption, unstated above, that the language has no high back unrounded vowels. Under the type of linking suggested by Postal 1968, "+round" on the right would be replaced by "M round." In any case the following arguments apply equally to these suggestions.

6. There is no evidence that either of the *f* sounds was bilabial. The exact nature of the difference is unknown. The development of the velar spirant is somewhat different. In medieval German /h/ and /x/ came to be positional variants of one phoneme.

7. It might be suggested that linking should be constrained so as to distinguish between systems in which certain contrasts exist at the systematic phonemic level and those in which they are non-distinctive. (Compare also Postal 1968: 185: "It seems then that here a difference between two different kinds of [*u* Voice] may be showing up. [*u* Voice] appears to behave one way in a

context X__Y if there is a contrast in X__Y, but another way if there is not.") For example, in a language with an underlying system /i ü u/ and Rule P, the change u (> ü) > i/__ü is clearly unnatural. Equally unlikely is the motivation for the linking-motivated shift ü (from underlying /u/) > i while systematic /ü/ remains unaffected. Neither of these types of change is known from historical change. The evidence presented above, however, appears to justify the stronger conclusion that linking is not a property of synchronic grammars. The phenomena which seem to favor linking are generally illusory, resulting from constraints on rule initiation and a tendency toward less highly marked lexical representations.

8. The procedure of our hypothetical example is abundantly illustrated in Foley 1968. For example, to account for Javanese alternation of the type (1a) p— (1b) m— and (2a) b— (2b) mb— considerations of rule plausibility (in terms of archetypal rules) lead Foley to posit underlying forms /mb–/ and /mp–/ for (1b) and (2b), respectively. Two phonological rules then operate on:

	/mb–/	/mp–/
(1) assimilating the stop by one feature	mm–	mb–
(2) reducing the geminate cluster	m–	mb–

Foley does not discuss the fact that this solution also entails rules of questionable naturalness which simultaneously voice initial voiceless stops and devoice initial voiced stops.

9. A different explanation for the development of J2 might be given, as an amalgamation of two rules, one a palatalization rule, and the other a deaffrication rule, and there is even an interpretation of the history of the Japanese dental obstruents which would favor this explanation. Against it we can put the fact noted that there are no dialects with only an affrication (or deaffrication rule). In any event, should the latter explanation prove to be correct, it will simply change the details of our argument without affecting its conclusion: that simplification can lead to unnatural rules.

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