Historical investigations in the framework of generative grammar have generally aimed at developing a theory of change which could hook up to the existing synchronic theory, so as to correctly characterize the possible forms of linguistic change, and the constraints to which they are subject. Much of the earlier work on this problem is summarized in King's book (1969). Since then, there has been much debate, for example, on the question of constraints on sound change: can rules be added to the middle of grammars (King 1970; Demers 1970)? Can the added rule be one that interchanges the values of a feature (Chomsky and Halle 1968; Matthews 1970)?

I will not try to cover this work here. I will limit myself to a parallel, and perhaps equally important line of research.
which attempts to deepen our understanding of linguistic change by removing the obstacles that presently lie on the synchronic side. We cannot simply take the theory of grammar for granted and hold it constant while we "apply" it to change. This is because many of the most central problems in historical linguistics, e.g., when does restructuring take place? or what determines the direction of analogical change? are really questions about language itself. However, the present state of linguistics is such that the synchronic theory is often rather indeterminate in exactly the respects that would be most relevant for historical linguistics. For this reason much progress in historical linguistics depends on sharpening the synchronic theory so that it will provide the right basis for diachronic explanation.

It is interesting that most work that has so far been carried out along these lines points to one general conclusion. This is that the function of the "evaluation measure" in linguistic theory is carried out by a series of substantive conditions in addition to (not instead of) the formal condition of simplicity. Chomsky originally suggested that the language learner has two sorts of things available to him:

1. formal devices for expressing rules
2. a way of picking the right analysis from the many analyses that these formal devices allow (an evaluation measure).

The usual claim (e.g., in Chomsky and Halle 1968) has been that the formal devices are very rich, and that the evaluation measure is very simple, viz. pick the shortest description, where length in phonology is defined in terms of the number of feature specifications in the grammatical description. It is especially the historical facts which show conclusively that this cannot be right, and that the "evaluation" of grammars involves their substantive properties. I will discuss these substantive properties under three headings:

1. Abstractness conditions
2. Paradigm conditions
3. Rule opacity.
1. ABSTRACTNESS CONDITIONS

The first modification of the theory that is suggested by historical facts is some constraint on abstractness. I discussed such a constraint in *How abstract is phonology?* (1968b), and it has been debated repeatedly in the recent literature. The type of theory that was proposed in the *Sound pattern of English* (henceforth: SPE), and on the basis of which analyses of other languages than English were proposed in the sixties, led to a number of analyses that many people find implausible. For example: the analysis of *boy* in English as being underlyingly [β] with an open, front, rounded vowel; the epsilon glide, i.e., the segment that is [-cns, -voc, -hi] that is claimed for the underlying forms of words like *tolerance*, or *menu*; or the distinction between the final vowels of *veto* and *motto* which in SPE is that between open o and mid o. All of these posited forms do not correspond in any direct way to anything that is present on the surface in any allomorphs of the words or morphemes that are analyzed as having these underlying segments. Simi-

larly, in Lightner's analysis of Russian, a word like [šum] has the analysis /xeumos/ with several abstract segments. Considerable methodological importance is, in fact, given to these analyses in SPE.

In many of these cases, arguments have subsequently been given within the theory of SPE that refute or at least weaken the evidence for the analysis that posits such abstract segments (McCawley and Stampe To appear; Harada and Imai 1970; Hoard To appear). Still, one might go on to ask whether there might not be some deeper, principled reason why these analyses are not right. Is there a substantive constraint limiting the relation between underlying and surface representations in some fashion? Such a constraint might say either that underlying representations which don't correspond directly to anything on the surface are hard to learn for a child or, more strongly, that such representations are impossible to learn. If such purely abstract segments in underlying representations are hard to learn, the theory will have to reflect this formally by making them expensive. There will be a
clause in the evaluation measure that places a high cost on them. The other possibility, that they are impossible to learn, would require setting up some absolute constraint in the theory that excludes such analyses completely. In How abstract is phonology? I considered both of those versions without really saying which one I preferred, the reason being that I was not sure myself.

Subsequently, there has been a lot of discussion of this proposal. Some modifications to it were suggested in Vetter (1968), Smith (1969), Davison (1971), and Piggott (1971). It was argued against in Hyman (1970a, b), Brame (1968), Kisseberth (1969), Kim (1970), Campbell (1969) and Crothers (1971) have reviewed and countered some of this criticism, favoring some sort of constraint on abstraction. A view similar (in this respect) to mine was also proposed independently by Andersen (1969).

The evidence given in How abstract is phonology? consisted of historical facts. These facts seemed to indicate that the rules and underlying representations which are postulated in certain kinds of abstract analyses differ from other rules and underlying representations postulated in generative phonologies in not functioning in any linguistic changes, and that there is hence some reason to doubt their existence. The analyses I was specifically concerned with were those involving absolute neutralization, i.e., the context-free merger of an underlying phonological contrast on the phonetic surface. I concluded that absolute neutralization either contributes a great amount of linguistic complexity to a grammar (the weak alternation condition) or is excluded outright (the strong alternation condition). This amounts to saying that the present evaluation measure, which is based on formal simplicity, is giving the wrong answers in some cases and needs to be revised by incorporating some version of an abstractness condition.

What some of the replies to How abstract is phonology? showed was that there are languages in which the (strong) alternation condition leads to more complex analyses than would otherwise be possible. But showing that introducing the alternation condition
can lead to more complex analyses cannot by itself refute the alternation condition, since the point at issue is precisely whether simplicity is the correct evaluation measure. The claim is that analyses involving absolute neutralization, however simple they may be, are still in some sense difficult or impossible for a child to learn; that simplicity is not the only criterion for evaluating a grammar and should be augmented by some substantive constraint on abstractness.

To avoid begging the question in investigations of this problem we must look for external evidence as to the correctness or incorrectness of specific analyses which are required or forbidden by the constraints at issue. The papers by Hyman (1970a, b) and Piggott (1971) went right to the heart of the matter. Hyman's papers are important in introducing another aspect of historical linguistics, viz. facts about borrowing, into discussion of phonological theory. However, as argued in Crothers (1971) and, in more detail, in Kiparsky (To appear a), both the internal and the external arguments for

Hyman's analysis are inconclusive. I should like to discuss Piggott's paper briefly here.

A major flaw in my paper was that it dealt mostly just with cases in which wholly abstract segments had been set up in order for one rule to have a phonological context. All the actual evidence given there pertains to such cases. True, the majority of cases in which wholly abstract segments had been used at that time were of this type, and it is these where the legitimacy of the device is most questionable. But obviously there exist cases in which wholly abstract segments are motivated by several distinct phonological processes, e.g., in the Spanish vowel system (Harris, 1969); a clear example is Newman (1968). As pointed out especially by Brame (1968) and Kisseberth (1969), who gave other such examples, the internal justification for abstract analyses is much stronger here. We might, accordingly, assume that wholly abstract segments are to be allowed when more than one rule refers to them crucially. From the viewpoint of language acquisition this would be very natural. We would expect, in general, the ease with which
absolute neutralization is learned to go up with the amount of evidence which the phonological processes of the language provides for the neutralized distinction. In effect, this amounts to the weak alternation condition, viz. that absolute neutralization adds complexity to the grammar, but is not categorically excluded.

Since there is evidence for some constraint on absolute neutralization, it can be fairly said that the burden of proof falls at least in part on one who wishes to argue for a limitation on this constraint. At any rate, as noted above, internal justification is not enough, because the theory itself is at issue.

We have to find external evidence which shows that the abstract analyses posited in certain cases do have psychological reality. Of course, this cannot and need not be done in every single example. Once we have provided external justification in a few clear ones, we can provisionally adjust phonological theory so that it will require the abstract analyses in these cases. The resulting theory will then lead to specific predictions about what the correct analysis is in many other cases, where external empirical evidence may or may not be available to test the theory.

The beginnings of this external empirical justification have been provided by Piggott (1971). Piggott analyzes a series of phonetic mergers in the history of the Algonquian languages. The cases which Piggott investigates involve segments which before the merger behave differently with respect to at least one phonological rule. For example, Proto-Algonquian */θ/ became */l/, thereby falling together with Proto-Algonquian */l/, which was not affected before */l/. Similarly, Proto-Algonquian */i/, which caused palatalization of certain preceding segments (including */θ/), merged with */ε/, which did not cause palatalization before it. Some of these phonetic mergers subsequently resulted in morphophonemic mergers. Changes took place which eliminated the need for making a morphophonemic distinction between the l from */θ/ (which turned to ə in the palatalizing environments) and the orig-
inal ́ (which was not subject to change).

The way this happened in some Algonquian languages was that all ́s, including those from */l/, started to become ̃ in the palatalizing environments. In Delaware, the opposite happened: the ́s from ð became like the original ́s and stopped being palatalized to ̃.

<table>
<thead>
<tr>
<th>Proto-Algonquian</th>
<th>basic</th>
<th>in palatalizing environment</th>
<th>basic</th>
<th>in palatalizing environment</th>
</tr>
</thead>
<tbody>
<tr>
<td>After merger</td>
<td>i</td>
<td>i</td>
<td>l</td>
<td>l</td>
</tr>
<tr>
<td>Usual reanalysis</td>
<td>i</td>
<td>i</td>
<td>l</td>
<td>l</td>
</tr>
<tr>
<td>Delaware reanalysis</td>
<td>i</td>
<td>i</td>
<td>l</td>
<td>l</td>
</tr>
</tbody>
</table>

We can interpret the reanalyses as the result of language learners failing to retain the underlying phonological distinction in their synchronic grammars, and instead setting up a rule ́ → ̃, which some ́s have to be marked as not undergoing. Subsequently, this mark is either removed from all ́s (i.e., all ́s become regular—the usual change) or the rule itself is eliminated (the Delaware change). I.e., the reanalysis proceed from a non-abstract synchronic anal-

ysis of the merged segments.

In other cases, such reanalysis does not take place. For example, modern Algonquian languages still show the distinction between original ́, which causes palatalization, and ́ from */e/, which does not cause palatalization. Piggott now observes the following correlation: phonetic merger leads to reanalysis where the distinction is relevant to the operation of just one rule; phonetic merger does not lead to reanalysis where the distinction is relevant to the operation of several rules. For example, palatalization is the only process that distinguishes original ́ and ́ from */o/. But the two ́s are distinguished by processes of vowel coalescence in addition to the palatalization rule. If the abstract analysis is permissible in this latter kind of case, then the preconditions for reanalysis are not met there. We then have an explanation for why these cases do not in fact undergo reanalysis.

If this idea proves to be generally valid (and it is certainly consistent with the examples known to me), then my claim in
How abstract is phonology? That absolute neutralization is unstable, must be qualified. The instability seems to show up mainly just where the neutralized distinction is weakly embedded in the grammar, making a difference for only one rule. This suggests a version of the alternation condition which has the effect of ruling out absolute neutralization under these conditions, but which allows absolute neutralization where the internal evidence involves several phonological processes of the language. There is no need to emphasize the lack of solid evidence as to the exact nature of the alternation condition, and the tentative nature of conclusions even as vague as those suggested here.

Another constraint on the relation between underlying and surface phonological representations has recently been proposed by Hale (1971), on the basis of ample synchronic and historical evidence from various Polynesian and Australian languages. Hale considers the type of morphological situation illustrated by the following data from Maori:

<table>
<thead>
<tr>
<th>verb</th>
<th>passive</th>
</tr>
</thead>
<tbody>
<tr>
<td>awhi</td>
<td>awhitia 'to embrace'</td>
</tr>
<tr>
<td>hopu</td>
<td>hopukia 'to catch'</td>
</tr>
<tr>
<td>aru</td>
<td>arumia 'to follow'</td>
</tr>
<tr>
<td>tohu</td>
<td>tohunga 'to point out'</td>
</tr>
<tr>
<td>mau</td>
<td>mauria 'to carry'</td>
</tr>
<tr>
<td>wero</td>
<td>weroha 'to stab'</td>
</tr>
<tr>
<td>patu</td>
<td>patua 'to strike, kill'</td>
</tr>
<tr>
<td>kitea</td>
<td>'to see, find'</td>
</tr>
</tbody>
</table>

If we wanted an "A" on our exam, we would, of course, say that the underlying forms are /awhit/, /hopuk/, /maur/, etc., and that the suffix is /ia/. We would then have a rule that consonants are deleted word-finally, but stay otherwise. Another rule would say that /ia/ turns to a after stems ending in a vowel, e.g., /patu + ia/ → patua. More support for this analysis would be the fact that the gerundive ending /ana/ also is preceded by the consonant postulated for the stem, e.g., awhitana, hopukana etc.

If someone were to say that the underlying forms are /awhi/, /hopu/, /mau/ etc., and that there are large numbers of different passive suffixes, /tia/, /xia/, /ria/ etc., he'd flunk. What Hale shows is that Maori
children learning their language flunk this "exam" and in fact set up underlying forms in which stems all end in different vowels and there are large numbers of different passive endings. There is strong evidence that the "clever" analysis is not psychologically correct. The psychologically correct grammar of Maori has /tia/ as the basic ending and /xia/, /ria/ etc., as a set of allomorphs used in verbs that have to be lexically marked as taking them. We have, in other words, a regular /tia/-conjugation and a number of subsidiary conjugations. In support of this analysis, Hale cites the following facts:

"(1) Stems which are basically nominal are often used verbally in spontaneous discourse; when they are so used, in the passive, they regularly take the ending /-tia/.
(2) Derived causatives (formed with the prefix /whaka-) take /-tia/ in the passive even if the basic verb stem takes another alternant when not in the causative. (3) There is a rule whereby certain adverbials are made to agree in voice with the verbs they modify; these adverbials take /-tia/ in the passive regardless of the shape of the passive ending which the verb itself takes.
(4) Borrowings from English, including un-assimilated consonant-final ones, take the ending /-tia/ in the passive. (5) Compound verbs derived by incorporating a noun from an adverbial phrase regularly form their passives in /-tia/. (6) In general, /-tia/ can be used when the conventional passive termination for a given verb is not remembered. These facts are entirely consistent with the suggested reanalysis. Only with difficulty can they be made consistent with the phonological alternative in the synchronic description of Maori. The situation is similar in other Polynesian languages—the extreme case of regularization is exemplified by Hawaiian, which now has a single passive ending /-tia/ (from *-xia, presumably)."

These facts are inexplicable under the "clever" analysis. For example, why should we have mau ~ mauria but causative whakamau ~ whakamautia? The analysis which sets up underlying /maur/ must say in addition that the causative prefix triggers a change of
the stem-final consonant to \( t \) (and, presumably, in vowel-final stems, epenthesis of \( t \)). Under the "stupid" analysis the causatives behave exactly as expected. The fact that we get whakamautia is just a special case of the general fact that derived words tend to be morphologically regular as compared to simple words. For example, causative verbs are weak in English and other Germanic languages, belong to the first a-conjugation in Sanskrit, etc.

Historically, there is no doubt at all that the consonants were indeed originally part of the stem, and were lost by the historical analog to the rule which the "clever" analysis postulates as a synchronic process. This sound change has taken place in the other Polynesian languages, too. Interestingly enough, they seem to have undergone the same kind of change as Maori, with the consonant being reanalyzed as part of the ending. But the fact that the various languages each pick a different consonant for the basic ending shows that the reanalysis has taken place independently in each of the languages. This is very strong evidence in support of Hale's claim.

What we must do now is to change linguistic theory so that this "wrong" solution will be the right solution. Hale notes that Polynesian languages have no final consonants on the surface, and ascribes the reanalysis to a tendency for the canonical shape of underlying representations to mimic the canonical shape of surface representations. Hale considers stating this as an absolute constraint:

\( (A') \) "An underlying phonological representation of stems is disallowed if it violates a universal (i.e., exceptional within the language) surface canonical pattern."

but suggests that it may be more correct to regard it as a relative constraint:

\( (A) \) "There is a tendency in the acquisition of a language for linguistic forms to be analyzed in a way which minimizes the necessity to postulate underlying phonological representations of morphemes which vio-
late the universal surface canonical patterns of the language."

This formulation brings out the relationship to the weak alternation condition very clearly. Just as the alternation condition says that underlying distinctions which do not directly correspond to surface distinctions are hard to learn, so Hale's principle says that underlying canonical forms which do not correspond to surface canonical forms are hard to learn. The motivation of both principles in the process of language acquisition is readily apparent.

2. PARADIGM CONDITIONS

Another type of substantive constraint that is needed to account for the historical facts has to do with the relation between the allomorphs in a paradigm (for more detailed discussion, see Kiparsky (To appear b); cf. also Miller (1971)). The first of these is essentially the traditional notion of "leveling". (See Harris (1970, To appear).)

A standard example of analogical change is the elimination of \( s \) in the inflection of certain Latin s-stems like honōs \( \sim \) honor. The stems that underwent this change were basically masculine and feminine polysyllables. In the oblique cases they were originally subject to the rule:

\[ s \rightarrow r / V \rightarrow r / V \]

The \( s/r \) alternation due to this rule was eliminated by generalizing \( r \) in the nominative:

- N. honōs honor
- G. honōris honoris
- A. honōrem honōrem

(The shortening of \( ō \) to \( o \) is due to a general rule of Latin which says that vowels are shortened before consonants word-finally.)

In my thesis (Kiparsky 1965) I said that the change was formally a reanalysis of the base form from \( /hōnōs/ \) to \( /hōnōr/ \). Thus the \( s \rightarrow r \) rule would no longer be needed in this derivation, although it continues to operate where the change did not take place, i.e., in monosyllables (flōs \( \sim \) flōris) and in neuters (genus \( \sim \) generis).

What I forgot was that some of the changing stems retain \( s \) in derivatives, e.g., honor \( \sim \) honestus. To account for this, some
ad hoc measures must be taken. Two come to mind as possibilities: (1) The underlying form is indeed restructured with /r/, but there is a special minor rule that sends r to s in derivatives like honestus. This cannot be a general process in the language and must be restricted to a fairly small number of words. (2) We do not have a restructuring of the underlying form after all, and /s/ is retained. Instead, there is a special additional clause on the s → r rule, which states that the rule must be also applied in the nominative (even though no vowel follows the s) in masculine and feminine polysyllables. Either way we have a complication of the grammar. And yet we would like to say that this is basically the same type of change that generally involves optimizing the grammar. How can we change the theory to make this a good thing to happen to a language, in spite of the formal complexity that results? The obvious way is to say that in the child's acquisition of language there is a cost attached to having alternations in a paradigm:

(B) Allophony tends to be functional.

a paradigm.
The existence of some such principle as (B) is shown by the fact that it can, in cases such as this, override formal considerations of simplicity. There are others in which it does not. Given that there is some such substantive principle as (B), it becomes our task to delimit the relative force of this substantive principle and the formal principle of simplicity in cases such as this where there is a conflict between them, so as to be able to predict which one will win out. Obviously we are not even close to being able to do this yet.

Principle (B) also solves some problems which arise in rule reordering. There are cases which do not fit the characterization of unmarked order in terms of the feeding and bleeding relations defined in Kiparsky (1968a). For example, in German the final devoicing rule

Devoicing: [+obstr] → [-voiced] /— \#

is ordered both ways, depending on the dialect, with respect to the rule that drops s after n.
$g$-deletion: $g \rightarrow \emptyset / [+\text{nasal}]$

Depending on the order, we get [lan$g$] or [lan$g$] for /lang/, as follows:

**Dialect group I**

1. Devoicing  
   lan$g$       --
2. $g$-deletion  
   --          lane

**Dialect group II**

1. $g$-deletion  
   lan$g$       lane
2. Devoicing  
   --          --

Historically, Group I seems to represent the original order, from which Group II has changed by reordering. (Cf. Vennemann (1969), for a detailed discussion of these rules.) However, the relation between the rules is one of mutual bleeding. Only one of the two rules can apply to any one input representation, and this is always whichever rule applies first. Yet this formally symmetrical ordering relation is not functionally symmetrical. The innovating order is in conformity with principle (B). This case of reordering therefore furnishes supporting evidence for minimization of allomorphy as an independent functional principle which is not reducible to formal properties of the
generative system of a language.

A second paradigm constraint has to do with preservation of semantic information on the surface (Kiparsky To appear b). For example, in Labov's data on Black English (Labov et al. 1968:158), final $g$ is deleted much more frequently in genitives and third person singulars than in plurals. This fact has echoes in historical linguistics. In German, $-a$ drops optionally in the singular dative but not in the plural. In Middle High German, the rule was optional in both cases but was applied more frequently in the dative. In Middle English, $-n$ dropped more frequently when it was a case marker than when it was a plural marker. It seems to be a general phenomenon, then, that plural endings are more resistant to deletion than case and agreement endings. One would like to relate this to the fact that plural is selected in the deep structure and has a direct semantic interpretation, whereas (surface) case and agreement are inserted by transformational rules and have no direct semantic interpretation. Loss of the plural distinction hence impedes meaning more than
loss of the other categories. A consideration of analogical change supports this point. When lost, plurals are more likely to be reintroduced by analogical change than are cases or agreement morphemes. We can then formulate the following principle:

(C) **Morphological material which is predictable on the surface tends to be more susceptible to loss than morphological material which is not predictable on the surface.**

In the light of this last point it is worth reconsidering the significance of the kinds of facts that Labov and his collaborators have been unearthing. They found that if you measure the frequencies with which optional rules are applied, you get very systematic results. The relationship between the frequencies in different cases is fairly constant. If one speaker deletes case more often than plural, that will be the case for any other speaker, or for the same speaker one day later. There is a real question as to how such facts are to be accounted for in linguistic theory. There are basically two ways one could see this done. Labov's proposal is to devise a new sort of linguistic rule, the variable rule, in which the frequencies of rule application, or at least something from which these frequencies can be deduced, are attached to the rule. This amounts to saying that the frequencies have to be learned by the child, and are simply part of the linguistic information you have to acquire when you learn your phonology.

An alternative, which is suggested by the cross-linguistic validity of the factors that control the frequency of application, is the following: the frequencies are not learned by the child, but predictable from exactly the kinds of substantive constraints we have been considering here. This may well be too strong. There are intermediate possibilities, for example, that the child must learn the "basic strength" of an optional rule, i.e., its overall frequency of application, whereas the variations in frequency of application in specific environments follow from general functional considerations such as those discussed here.

The paradigmatic factors interact with
phonotactic factors that have to do with maximizing the naturalness of the output sequences. For example, when consonants are deleted optionally at the end of a word, they are dropped more frequently when the next word begins with a consonant, less frequently when it begins with a vowel, i.e., the frequencies of deletion are such as to favor CVCV sequences in the output.

In addition, such factors as style, social class and tempo play a role. It will be useful to distinguish these as non-grammatical conditioning factors from the grammatical factors, i.e., phonotactics and paradigm structure. The justification for making this distinction is that the grammatical factors play a role elsewhere in grammar, for example in characterizing the possible targets of phonological "conspiracies".

The alternative hypothesis, then, is:

(D) Grammatically conditioned variability in the application of optional rules favors optimal outputs.

where optimality should be characterizable independently of any particular language in terms of paradigmatic and phonotactic properties of the output. For example, we would not expect to find a dialect of English in which plural -s drops more often than genitive or third singular -s. More generally, no language should show a bias in favor of deleting a deep structure category rather than a transformationally introduced category. No language should drop final consonants more often when the next word begins with a vowel, or drop final vowels more often when the next word begins with a consonant, etc. So far the available facts seem to be compatible with this hypothesis.

More unclear is the question whether we can also predict the relative force of the different factors that jointly condition variability. For example, could two dialects differ in that paradigmatic factors were more important in one dialect but phonotactic factors were more important in the other? The answer may be no. There is some reason to believe that the strength of the factors is itself predictably variable. Conditioning by grammatical category becomes relatively more important in more formal speech (Labov
et al. 1968). This suggests that the strength of the conditioning factors is dependent on the functional requirements of the speech situation.

The closest I have seen to a counterexample is the tensing and raising of /m/ in New York, on which Labov (To appear) has reported (cf. Bailey 1970) for further discussion of this example). This happens before voiced non-nasal stops (bad, bag, cab), voiceless fricatives (laugh, bath, pass, cast), and front nasals (man, ham, land). The raising is to some extent variable in all environments. For speakers over about seventy, there is more raising before fricatives and voiced stops than before nasals (i.e., the vowel is higher on the average in pass, bad than in man); but for younger speakers the situation is reversed: there is more raising before nasals than before fricatives and voiced stops. In addition, there is some less clear fluctuation in the relative strength of the fricative and voiced stop conditions. Before concluding that there is a genuine reversal of the nasal constraint, one should bear in mind the additional facts which are probably involved:

(1) The tensing before fricatives in pass, last etc., is a historically older process which is clearly related to the British English development of [a:] in these words (Luick 1964:704-9). It is evidently a separate rule from the other tensing. In the speech of seventy years ago this was the primary process of tensing which operated.
(2) Presumably the increasingly strong raising before nasals parallels the increasing nasalization and concomitant lengthening and tensing of vowels before nasals in American English. Evidently, then, there is more going on here than a simple reversal in the strength of the conditioning factors. The raising of /m/ actually involves several separate processes.

Hypothesis (D) leads to a reexamination of the theory of sound change. Labov's work on sound change indicates that it typically proceeds in two stages. The first is a stage in which the new rule is optional and the frequency of application in particular cases is governed by a complicated interaction of different grammatical paradigmas.
tic and phonological and non-grammatical social and stylistic factors. In the second stage, the rule becomes obligatory in some cases, in which it was frequently applied at the previous, optional stage, and it becomes inapplicable in other cases, in which it was less frequently applied before. The conditioning now becomes relatively simple, generally with either paradigmatic, or phonological, or social, or stylistic factors being selected as the single conditioning factor of the rule. I suggest the following interpretation of this phenomenon: in the first stage, the rule is optional and is subject to the systematic and predictable variability of optional rules in general. The complex interaction of conditioning factors at this stage is not learned as part of the grammar of the language, but follows from the nature of optional rules. In the second stage, the conditioning factors become part of the acquired grammar of the language. But the variability is too complex to learn as such. The changeover necessitates a polarization and simplification of the conditioning, some conditioning factors are eliminated and for the others the high and low frequencies get sorted out as respectively obligatory and inapplicable cases of the new rule.

Generally the conditioning factor that wins out is the phonological one. This gives the regular case of phonologically conditioned sound change. When the paradigmatic, social, and stylistic conditioning factors win out we get cases which neogrammarian theory handles variously as "analogy" or "dialect mixture". It has long been noted that sound changes can depend on grammatical categories. We can add that particular kinds of grammatical categories are going to block sound changes from taking place. This strengthens the theory of sound change considerably by subjecting it to the same constraints that hold on rule variability. In the same way, lexical splits should reflect factors of style or emphasis. Examples of stylistic split are the split between lengthening in everyday words like grass, glass, ass, versus short vowel in special words like crass, lass, gas; the earlier dropping of /k/ in everyday words like ass, cuss, bust.
versus the retention of \( r \) in special words like *parsley*, *hoarse* (note the doublets *cuss/curse*, *ass/arise*, *bust/burst*). The *ardite/vulgar* splits known from many languages belong here. Split of emphatic versus non-emphatic is e.g., the lengthening (reported by Ferguson (1971) for Philadelphia) of the affective words *mad*, *bad*, *glad*, versus *lad*, *add*, *ad*, *Brad*, *Tad*, *fad*, *cad*, with *sad* varying for the two Philadelphia speakers I have checked with. Closely related are also splits along major versus minor category such as the English voicing of \( \theta \) to \( \ddot{\alpha} \) in pronouns.

Bhat's (1969) observations on Indian caste dialects are also naturally explicable in these terms. Bhat compared geographic and social (caste) dialect differentiation in Tulu, a Dravidian language. He noted that, whereas regional isoglosses are formed by both regular and sporadic sound changes, all caste isoglosses are formed by regular sound changes. Bhat proposes to explain this by the assumption that regular sound changes are carried out by children (who rarely associate with children of other castes) whereas sporadic sound changes take place in adult speakers (who do interact with members of other castes). However, probably a simpler way of explaining Bhat's observation is that polarization of speech differences between social classes is naturally a powerful factor in a society where the boundaries between the classes are drawn very sharply.

Further work will have to show whether this is the right approach. It remains to be seen, for example, whether this framework can account for the facts that have led Wang and his collaborators to postulate the "diffusion" of sound changes through the vocabulary, for such cases of grammatical conditioning of sound change as that cited by Postal (1968) from Mohawk, and many others. And even if it does turn out to be the right approach, there is no doubt at all that it will have to be greatly enriched and elaborated before it is nearly adequate to account for the complexities of synchronic variability and diachronic change.
3. RULE OPACITY AND REORDERING

The reordering conditions proposed in Kiparsky (1968a), that feeding order tends to be maximized, and bleeding order tends to be minimized, in most cases correctly predict the direction of reordering. The unmarked status of feeding order is not subject to any serious doubt. Still, a number of examples have turned up where these conditions are inadequate. There are three sorts of cases like this: (1) reordering contrary to the conditions; (2) reordering where the concepts of feeding and bleeding are inapplicable; (3) reordering in cases of "mutual bleeding". Some examples have been collected in Kenstowicz and Kisselthor (1971).

In this section I will review the examples known to me where the previously proposed conditions do not work. We shall see that they reveal an inadequacy in the concept of bleeding order. I will tentatively suggest a reformulation of the conditions which accounts for the problematic cases as well as for those which the old conditions handle. This reformulation will make use of the concept of rule opacity, which I will

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...has an important role to play elsewhere in linguistic theory as well.

I will begin with the syntactic example analyzed in Klima (1964). Klima wrote this paper before it had been shown that reordering of rules is a primary form of linguistic change. His analysis is therefore formulated solely in terms of rule addition and subsequent restructuring. However, it is now easy to recognize that the historical process Klima describes is essentially a series of downward shifts in the order of case marking which makes case in English dependent on increasingly superficial configurations. For example, the shift from whom did you see? to who did you see? is a shift from a grammar in which case marking applies before Wh-movement (i.e., to a representation of the form you past see Wh+PRO) to a grammar in which case marking applies after Wh-movement (i.e., to something like Wh+PRO you past see). From the drift-like progression of this reorder of case marking we can conclude that it constitutes a natural development in the grammar of English. This raises the question, what general principle lies behind the direction
ility of the drift. The ordering asymmetries of feeding and bleeding order (Biparzely 1968) do not help us here. If anything, Klac's analysis shows a drift towards less application of the case marking rule.

We can make an initial approximation to the required principle in the following way. Let us make a distinction between reordering transformations, which move constituents around, and feature changing transformations, which add some morphologically realized mark onto a constituent. Consider tentatively a principle which says:

(E') Feature changing transformations preferably follow reordering transformations.

This implies that the evaluation measure assigns a higher value to a grammar in which a feature changing rule follows a reordering rule than to the otherwise identical grammar in which the two rules apply in the reverse order. This should, if correct, have the usual consequences in terms of directionality of change, language acquisition (children should make mistakes in the direction of the preferred order but not in the reverse direction), and frequency in the world's languages.

The study by Hale (1970) of the ergative and passive in Australian languages gives some support of principle (E'). On the basis of a comparative diachronic analysis of several languages Hale tentatively concludes that the ordering:

Pronominalization Passive

is unstable. He shows that languages having this order tend either to reorder the rules or to make the passive structure obligatory and basic. It is possible, as Hale suggests, to attribute the reordering to the anti-feeding nature of the unstable order. However, perhaps a more natural way of looking at the change is that the drift is toward making pronominalization, a feature-changing rule, follow the passive transformation.

There is a phonological analog to this type of asymmetry in the order of transformations. In recent papers, Kisaeberth and Kenstowicz (1971) and Kaye (1971) have discussed cases in which the unmarked order of phonological rules is not characterized by
the bleeding or feeding relations. The examples of Kenstowicz and Kisselberth are of the following sort. In Yokuts, vowels are shortened in closed syllables:

\[ j \rightarrow \text{[-long]} / \_\_\_ \_C \{ \# \} \_\_\_C \]

e.g., *dojihôl* 'night report' but *dos-hin* 'report'. This rule is critically ordered with respect to an epenthesis rule:

\[ j \rightarrow i / \_\_\_ \_C \{ \# \} \_\_\_C \]

which breaks up clusters and thereby turns closed syllables into open ones. Epenthesis precedes shortening, but as Kenstowicz and Kisselberth note, the bleeding order:

1. Epenthesis
2. Shortening

in which the rules actually apply is quite natural, and the one which one would expect them to apply. In at least some cases, then, bleeding order seems to be unmarked.

Kenstowicz and Kisselberth do not actually give evidence that bleeding order really is unmarked in such cases. However, there are cases of historical change which support their conjecture. Wayne O'Neil (personal communication) has noted that Paroese has two rules:

1. Intervocalic spirantization
2. Vowel syncope

which originally apply in the given order, e.g., *heidnir* → *heïnir* → *heïnir* 'heathen (pl.)', and have been reordered into a bleeding order:

1. Vowel syncope
2. Intervocalic spirantization
giving *heïnir*, where spirantization no longer applies.

Another possible example where diachronic evidence may point to the unmarked status of a bleeding order is discussed briefly in Hurford (To appear). Cockney English drops initial h. According to Hurford, there is an older dialect which says a 'cuse, whereas younger speakers say an 'couse. We might say, then, that the non-bleeding order:

1. an → a / \_\_\_ C
2. h → ∅ / \# \_\_\_ 

has changed into the bleeding order:

1. h → ∅ / \# \_\_\_ 
2. an → a / \_\_\_ C
There are a couple of ways out. In the first place, we might say that the second dialect actually has no underlying \( h \), but that words formerly ending in \( h \) have been restructured with an initial vowel. Secondly, we might argue that the rule for the indefinite article reads:

\[
a \rightarrow an /\_\_\_V
\]

in which case we no longer have a bleeding relationship. But it seems to me that these objections are really beside the point.

Suppose that it is possible to motivate the underlying representations which Hurlford assumes for the second dialect. Clearly this is a possible analysis. The change from \textit{a house} to \textit{an house} should be predicted under this analysis as well as under the other possible analysis. That this is the case is a flaw in the theory.

Kenstowicz and Kisselberth discuss two distinct ways in which the theory of phonology might be amended to characterize the markedness of ordering relations in the correct way. The first is that:

(\textit{E''}) Where one rule \textit{A} stands in a bleeding relation with another

\textit{rule B} by virtue of \textit{A}'s altering a structure so that it no longer satisfies the environmental conditions of \textit{B}, bleeding order is unmarked.

That is, a rule of the form:

\[
\text{K} \rightarrow \text{L} / \text{M} \_\_\_ \text{N}
\]

is preferably bled (i.e., preceded) by a rule which destroys \textit{M} or \textit{N} (the environment), although it is preferably \textit{not} bled (i.e., followed) by a rule which destroys \textit{K} (the input proper).

However, they note that \textit{(E'')} does not take care of the following sort of case. In some Slavic languages there is a vowel copy rule of the form:

\[
(C) \text{V R C} \rightarrow 1 \ 2 \ 3 \ 2 \ 4 \\
1 \ 2 \ 3 \ 4
\]

This rule, known to be a dialectal innovation of East Slavic, has to apply before an old rule which accents the initial vowel of certain words, as shown by the following derivations:
It seems evident that this is indeed the expected ordering of vowel copy and accent insertion. But here bleeding order is completely irrelevant. Both rules will apply to underlying /golov-u/ in either order, but with different results.

In view of this example, Kenstowicz and Kisselberth suggest an alternative principle \( C_2 \), in terms of the substantive effect of the rules. I formulate it here as follows:

\[ (E') \] Rules which affect syllabic structure (e.g., metathesis, epenthesis, deletion) preferably apply before rules that refer to syllabic structure (e.g., assimilation).

It is clear that Kenstowicz and Kisselberth are generally on the right track. However, neither \( (E') \) nor \( (E'') \) can be correct. This is shown by Kaye's Ojibwa example.

Kaye discusses the following two rules in Ojibwa:

\[ \text{vowel copy} \quad /\nu+ov/ \quad /\text{golv}+u/ \]
\[ \text{accent insertion} \quad /\nu+ov/ \quad /\text{golv}+u/ \]
\[ \text{later rules} \quad \text{vorónu} \quad \text{gólóvu} \]

\[ \text{vowel coalescence} \quad \text{aw}+i \rightarrow \begin{cases} \text{a} / \_k \\ \text{o} / \_n \end{cases} \]
\[ \text{n-assimilation} \quad n \rightarrow k / \_k \]

He presents evidence that certain Ojibwa dialects have gone from the above order to the reverse order. Underlying /nōntaw-in+k/ has thereby changed from nōntōkk to nōntākk.

Again, the bleeding (and feeding) relations are inapplicable, so that principle \( (E'') \) cannot be invoked. However, principle \( (E'') \) also gives the wrong result, since this reordering puts an assimilation rule before a rule that changes syllable structure, whereas \( (E'') \) says that assimilation rules preferably go after rules that change syllable structure.

In place of these principles, which are still somewhat unsatisfactory, I would like to put forward tentatively a principle which accounts for all the examples, including the syntactic ones. Define the concept opacity of a rule as follows:

**Definition.** A rule \( A \rightarrow B / C \rightarrow D \) is opaque to the extent that there are...
surface representations of the form

(1) A in environment C ___ D
or (ii) B in environment other than C ___ D.

(The first of these two conditions has been referred to by structuralists as an alternation being "non-automatic" and by Stampe (1969) as a rule being "contradicted on the surface"). Note that both cases can arise in several ways. For example, CAD might arise through a rule E + C /___ED, or a rule

F + A / C ___ D. B might appear in environments other than C ___ D either through some process that changes CBD to EBD, or through a process that introduces B in other environments, etc. Of course, exceptions add opacity to a rule according to the above definition. The definition can be extended to syntax in the obvious way. Opacity as here defined is a matter of degree, although I have no suggestions as to how to quantify it formally. I suspect that the concept will ultimately turn out to be more complicated than the above definition indicates.

Let us refer to the converse of opacity

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The hypothesis which I want to propose is that opacity of rules adds to the cost of a grammar; more concretely, that opacity is a property of rules that makes them, and the underlying forms to which they apply, harder to learn. In particular, I conjecture that transparent rules are learned first, and that the acquisition of phonological rules proceeds roughly in the order of increasing opacity.

As regards unmarked ordering, we can now derive, as a special case, the following principle:

(B) Rules tend to be ordered so as to become maximally transparent.

This principle seems to cover the hitherto recalcitrant examples of ordering asymmetry, capturing what is correct about the three principles (B'), (B''), and (B').

For example, in Yukuts the preferred, unmarked order is that in which shortening applies to the superficial, more phonetic representations reached after epenthesis, rather than the deeper, more abstract representations that the derivation shows before epenthesis. If shortening came before epen-
thesis; it would be a more opaque rule by
DEF., Case (ii). In Slavic, the rule "accent
the first vowel in words of the goly class"
would be opaque if it preceded copying,
since copying would introduce a stressed
vowel on the second syllable of these words
as well (DEF., Case (ii)). In Ojibwa, the
aw + å /ān rule is more opaque in the or-
dering that gives nöntökk than in the innov-
vating ordering that gives nöntökk (DEF.,
Case (ii)). Similarly, in Klima's syntac-
tic example the drift is towards making case
marking an increasingly transparent process.

The reader will have noticed that these
examples all involve Case (ii) of the defini-
tion of opacity. The same is true of the
syntactic examples of Klima and Hale, and of
the other examples discussed in the paper by
Kenstowicz and Kisseberth. What about Case
(i), then? This is simply a characterization
of non-feeding order; the consequence that
feeding order tends to be maximized is given
by including Case (i) in the definition of
opacity.

For example, suppose we have the two
rules:

1. γ + φ / VV
2. [V
-10] + [+hi] / V

Now the order given is the unmarked feeding
order, in which we get derivations like
eye + ee + ie. This is also the order in
which both rules are maximally transparent.
Reversing the order would result in an out-
put ee for the input eye, making the vowel
raising rule opaque by Case (i).

Our revised characterization of the or-
dering asymmetries now takes care of feeding
order, one type of bleeding order, and cer-
tain cases where the concepts of feeding
and bleeding are not applicable. Then what
about cases of marked bleeding order of the
type discussed in Kiparsky (1968a), namely
those in which two rules potentially apply
in the same segment? For example, in Swiss
German there is a reordering from

1. Umlaut (e.g., in env_1) /bode + Sg./
2. o + o /+[coronal]
 to
bode + Sg. /bode + Pl./

1. o + o /+[coronal]
2. Umlaut (e.g., in env_1)
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But these are exactly the cases in which the paradigm condition that allomorphy tends to be minimized, which we have seen to be needed on independent grounds, will give the correct result! In the Swiss German example, the innovating order preserves the stem shape between singular and plural. A review of the examples given in Kiparsky (1968a); King (1969), and elsewhere, shows that the cases which have been cited as reordering from bleeding into non-bleeding order are all of this type.

The proposed reformulation seems necessary in order to account for the examples brought up by Kaye and by Kenstowicz and Haase, and the syntactic cases discussed by Klima and Hale. It has at least as clear a functional basis in language acquisition as the notion of maximal applicability which it replaces. In addition, it has some important further advantages which I will now mention briefly.

The notion of rule transparency enables us to establish a profound relationship between the two historical phenomena of rule reordering and rule loss.

It has been pointed out by Stampe (1969) and Andersen (1969) (cf. also Darden (1970)) that rules tend to be lost from grammars through historical change only under certain specific conditions. Basically, we can say that rules are susceptible to loss if they are hard to learn. And one of the major factors that makes rules hard to learn (though not the only one) is precisely opacity as defined above.

Consider the first condition that leads to opacity (Case (1) in the DEFINITION). A rule is opaque if representations of the sort which it eliminates exist on the surface. Loss under this condition is common (cf. the Algonquian example cited from Piggott (1971) in § 1 of this paper). To give just one more case, a similar situation arose in early Iranian. The Iranian languages once had Bartholomae's Law, by which voiceless stops were subject to progressive assimilation of aspiration and voicing after voiced aspirates, e.g., /drugh+ta/ → *druqha.

Voiced unaspirated stops did not cause progressive assimilation, and were themselves subject to regressive assimilation of voicing
(with velars in addition being spirantized), e.g., /bhag-ta/ → *bhaxta. Subsequently the voiced aspirates were deaspirated. The resulting situation is one in which some voiced stops (the old aspirates) cause progressive voicing assimilation, but others (the original unaspirated stops) do not, e.g., (assuming rephonemicization) /drug+ta/ → dhuxta, but /bag+ta/ → baxta. This stage essentially survives in older Avestan. In later Avestan, however, the now opaque progressive assimilation rule is simply lost, giving /drug+ta/ → dhuxta like /bag+ta/ → haxta. Bertholomae’s Law is lost as a rule and survives only in isolated words in lexicalized form, e.g., in azda ‘thus’. Other examples are given in Andersen (1969).

We would also expect the second type of opacity (Case (ii)) to lead to rule loss. There is not a great deal of evidence for this being so. But a tentative analysis by Erteschik (1971) is, if correct, exactly such a case. The Hebrew spirantization rule, apparently applicable originally to all stops, has been limited to p, b, and k. Erteschik suggests that p, b, and k, wot. *

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the time when the rule became restricted in this way, exactly the stops whose spirantized cognates were not phonemic, the system being:

\[
\begin{array}{ccc}
\text{p} & \text{t} & \text{k} \\
\text{b} & \text{d} & \text{g} \\
\text{s} & \text{X} & \text{z} \\
\text{r} & \\
\end{array}
\]

In support of this conjecture she notes what happens when children learn spirantization in modern Hebrew. Here X has lost its pharyngealization, thereby merging with k, the spirantized form of k. This means that spirantization of k has become opaque with respect to Case (ii), since the output of spirantization of k now has another source in the grammar. Spirantization of p, b, on the other hand, remains transparent. According to Erteschik, children make mistakes with spirantizing velars (but not, apparently, with labials). For lexabes ~ kvisa she has observed children saying both lexabes ~ kvisa and lexabes ~ kvisa. That is, children find it harder to learn the opaque part of the modern Hebrew spirantization rule (and the underlying forms insofar as they
are subject to this rule) than the transparent part of the same rule.

Stampe has suggested other factors which make a rule susceptible to loss, in particular the unnatural status of a rule. For example, certain natural processes of vowel lengthening or shortening may be unnatural as the corresponding tensing or laxing processes when the length opposition changes into a tenseness opposition in the course of historical change. Such rules might also be susceptible to loss. Therefore not all cases of loss will necessarily involve rules which are opaque in the sense defined here. Opacity will, however, be one of the factors that bring about the loss of rules from a grammar by historical change.

Rule opacity also plays a role in paradigm changes like those discussed above. Strictly speaking, Principle (B) is inadequate by itself to deal with an example like the change of honōs to honor. The ξ which is reintroduced in the nominative causes shortening of the preceding vowel by a general rule of Latin. As a result we have actually just traded in the θ/θ alternation of honōs/honoria for the θ/θ alternation of honor/honorōs. Still, everyone would grant that there is a real levelling here. It is obvious that the new paradigm in some sense shows "less allomorphy" than the old one. But what exactly does this mean formally? I would like to suggest as the crucial difference that the shortening rule, which produces the θ/θ alternation, is transparent with respect to Case (i), whereas the rhotacic rule, which produces the s/r alternation, is opaque with respect to Case (i). That is, there are no words ending in -śr in Latin, but there are many words with intervocalic s (partly because of exceptions to the rule, e.g., miser 'miserable', positūs 'put (pp.)', including loans like basis, asinus 'donkey', and partly because of θ from other underlying sources, e.g., /cād·tus/ → cāsus 'fallen').

Finally, I should like to suggest that the concept of rule opacity may prove to be useful in the theory of exceptions. It is hard to find a clearcut division of phonological rules into those that may and those that cannot allow exceptions. However, it
is possible to say that certain kinds of rules are much less likely to have exceptions than others. As a first approximation to the needed criterion I propose opacity. The more opaque a rule, the more likely it is to develop exceptions. More specifically, opacity by Case (i) leads to input exceptions, whereas opacity by Case (ii) leads to environment exceptions (see Kisseberth (1970) and Coats (1970) for these concepts). As an example of the first case, consider Finnish consonant gradation. This is a weakening affecting consonants in the environment

\[ \text{VC} \{ \# \}

The rule applies to single stops and geminate stops in the following way:

- **Weakening of simple stops**: 
  - \( t \rightarrow d \) (e.g., \textit{maton} \rightarrow \textit{madon} 'worm's')
  - \( p \rightarrow v \)
  - \( k \rightarrow s, v \)

- **Degemination**: 
  - \( tt \rightarrow t \) (e.g., \textit{matton} \rightarrow \textit{maton} 'carpet's')
  - \( pp \rightarrow p \)
  - \( kk \rightarrow k \)

Degemination has virtually no exceptions (only certain very foreign names might not be subjected to the rule, e.g., gen. \textit{Giuseppen}, provided they are not assimilated in any way, including stress). On the other hand, the weakening of simple stops normally fails to apply in loans (\textit{auton} 'car's'), as well as in many native personal names (\textit{Lempi} 'Lempi's'), brand names (\textit{Upon} 'of Upo'), slang and affective words (\textit{räkän} 'of snot'), etc.

It seems reasonable to correlate this with the fact that degemination is nearly completely transparent with respect to Case (i), whereas the weakening of simple stops is rather opaque with respect to that case. That is, the sorts of input representations that are destroyed by degemination exist on the surface only in the rare exceptions, and in a few cases where strong boundaries block the application of gradation, e.g., \textit{Kekkosta} 'Kekkonen (partitive)' (Karttunen 1970).

But the sorts of input representations destroyed by the weakening of single stops are quite frequent on the surface; indeed, they arise with every application of degemination. Thus, we might suppose that the weakening of simple stops, and the underlying forms subject to it, are harder to learn than the
degeneration rule, and the underlying forms subject to it. (This should be readily testable in child language). The greater proneness of the weakening of simple stops to develop exceptions would, then, be a consequence of its greater opacity.

Examples of the second type (opacity by Case (ii) leading to environment exceptions) are commonplace. This case is simply the initial stage of the process of morphologization, by which rules lose their phonological conditioning and begin to be dependent on abstract features in the lexicon. The paradigm case is Germanic umlaut. The elimination of the conditioning i and ı turned the umlaut rule opaque by Case (ii). At some point after this took place, umlaut started to be reanalyzed as a morphologically conditioned process.

Footnotes

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DISCUSSION

JANICE REDISH: In regard to your Latin example, do you want to end up keeping the base form in s or in r?

PAUL KIPARSKY (M.I.T.): I don't really know. I don't think it is important for my point to have made up my mind on that question. I suspect that you probably keep the s and have a rule that s turns to r in the nominative of masculine or feminine polysyllables. The reason I think so is that in comparatives, whose stem ends in s, you have e.g., masc. audacior, neuter audaciōs. Again, the rule does not apply in the neuter. But in these comparatives, you obviously couldn't just fiddle around with the underlying form. You have to have some rule that changes s to r in a weird grammatical context. This is the solution which Martti Nyman has proposed in an unpublished paper on Latin rhotacism.

JANICE REDISH: In general, this is a much larger problem: the question of how to weight the choice of a base form that will keep a paradigm regular but will mess up the derivational forms. In talking about the complexity to the child of having allomorphs in a paradigm, is it possible to weigh the derivational section differently from the paradigm? What I am talking about is weighing the relationship between forms like sane and sanitas on a different level than that between forms like honor and honoris.

PAUL KIPARSKY (M.I.T.): Yes. I think you're
right. By paradigm, I mean the inflectional stuff not the derivational stuff. It is very clear that one must distinguish between derivation and inflection in dealing with the phonology of a language.

In connection with this, I might mention the work of Dick Stanley on Navaho. [Cf. Richard Stanley. 1969. The phonology of the Navaho verb. Unpublished Ph.D. dissertation. Cambridge: M.I.T.] In this language there is a hierarchy of boundaries that increase in strength as you go out from the root. The generalization which comes out from Navaho and which fits very nicely with Finnish, Sanskrit and some other languages is that: if you have AB (where A is a root and B is a suffix) undergoing some process which crosses the boundary between A and B, and you have AC and the boundary between A and C blocks that process, then you can predict the order of the suffixes B and C if they're both present. You always have ABC; never, ACB. The way you might think of representing that formally is by abandoning the boundaries that Chomsky and Halle have with features like [+stem boundary], [morphe boundary], etc. and replacing them by a system of bracketing that predicts the strength of boundaries. Given [[[A][B]][C]], we would have [[A][B]], but [[[A]][C]], with a stronger boundary in the latter case. This system would predict that the richness of the morphology will essentially give you the number of potential boundaries that are operative in a language.

PAUL KIPARSKY (M.I.T.): If something is learned, it has to be in the grammar; if something is universally predictable, it is not learned and can be taken out of the grammar, i.e., it can be made to follow from some general principle about language with a capital L. What I am conjecturing is that Labov's data can be taken out of the grammar of English, the grammar of German, Spanish, etc., and derived from a theory about optional rules in general. This theory says that optional rules are applied in such a way as to optimize the output with respect to things like syllabic structure, distinctness of categories, etc. Labov's percentages actually reflect what anybody's going to do given that a rule is optional. The percentages are not something you have to actually tabulate in your mind and look up every time you're going to speak.

PATRICIA M. WOLFE (U. of British Columbia): In regard to what you were saying about distinctness, the interesting thing in English is that you have as many completely uninflected plurals left like fish, sheep, deer etc. as you do forms like oxen, children, brethren.
More interestingly, the place you really get the n ending left is where it carries no information at all and is redundant, viz. on past participles where you already have an auxiliary verb marker; furthermore, the weak verbs manage without any kind of marker there whatsoever. The point here is that when you get n preserved as an inflectional ending in English, you get it preserved many, many times more where it doesn't carry any information than where it does. Without quarreling with the general idea that languages obviously preserve distinctness, I think that in some of the data you cited you're going to find not only no support for your claim but actually an awful lot of counter-evidence.

PAUL KIPARSKY (M.I.T.): Yes. There are a lot of things I don't understand about this question. Another problem is the feminine -e in German, which, for some strange reason, is very strong. I don't understand why that should be the case either. Sometimes you can make sense of apparent counterexamples: in languages that drop personal pronouns, agreement endings on the verb are strong and don't drop; in languages which have enclitic pronouns, they are weak. If you don't succeed in explaining all of the things we've been discussing, you're going to have to wind up having arbitrarily variable rules and arbitrary morphological conditioning in historical changes. That's essentially throwing in the towel. I prefer, at this point, not to throw in the towel and keep looking for reasons for the variability.

WALBURGA VON RAPFLER ENGEL (Vanderbilt University): I think we are basically in agreement that there are two types of simplicity: psychological simplicity and linguistic simplicity. If the two are in conflict, then the psychological simplicity prevails. Don't you agree with me?

PAUL KIPARSKY (M.I.T.): I think that perhaps we just have a different way of saying things. What you call linguistic simplicity is what I call (formal) simplicity and what you call psychological simplicity is what I call substantive conditions. The reason I don't like your terminology, although I agree with what you mean, is that I don't think there is anything linguistic which is not psychological.

WALBURGA VON RAPFLER ENGEL (Vanderbilt University): In regard to Professor Wolfe's point in substandard English, you have I have shook, I have took. The entire past participle drops out which would give you a point.

LLOYD B. ANDERSON (U. of North Carolina): I have a possible explanation for the data from Maori. If there is anything like an analysis-by-synthesis routine, where, when you listen, you try to match the morphemes you're hearing, then your matching routine might state that if you think, for semantic reasons, this root ought to be there, produce that root in your matching system. You don't want to produce more than is going to be there. You therefore produce any rather than just as your basic form. Only if you're under further conditions, can you produce
The m is like a stem-vowel in Latin a-stems, say, where it is part of the root, but it's not part of the root. It's controlled by the root, because the root arbitrarily causes you to produce it, but it's not there in the minimal form. Because you have to match that might be a reason why you would pick the shortest form that occurs in all forms of the paradigm as your minimal form to match or as your most basic form. I wonder if you have any ideas on whether that would work?

PAUL KIPARSKY (M.I.T.): I'm not sure whether that would cover all the cases that one would like to see handled by Ken Hale's constraint. For example, Hale's constraint would say, as a special case, that the segment types that exist in underlying representations tend to match those that occur on the surface.

HARRY A. WHITTAKER (U. of Rochester): I don't have a question—just a comment. Harold Goodglass did some work which is reported in the volume by Rosenberg and Koplin [S. Rosenberg and James H. Koplin (eds.). 1968. Developments in applied psycholinguistic research. N.Y.: The MacMillan Co.] about the differences between the plural markers and some of the other kinds of affixes. He noted that there was quite a differential behavior between these two in aphasic breakdown. Unfortunately Goodglass's data is a little bit tricky because of his failure to control the population samples and as a result he also got differences in the other direction between production and reception disorders. Perhaps with a little more analysis, this work might lead some very interesting