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FORMAL VS. FUNCTIONAL CONSIDERATIONS IN PHONOLOGY

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In his highly instructive paper "Functional Explanations in Generative Phonology" Michael Kenstowicz (to appear) distinguishes two kinds of explanation in phonology, "internal" and "external." According to Kenstowicz, "an internal" explanation attributes the presence of some (part of a) rule or other aspect of phonological structure to another independently motivated portion of the grammar, while an "external" explanation seeks motivation in terms of something that exists outside of and independently from the grammar." Appeals to simplicity, to feature counting or to the fact that certain rules are cyclically ordered would be examples of "internal" explanations for certain phenomena, whereas appeals to ease of articulation or to avoidance of ambiguity however defined might be viewed as examples of "external" explanations. It is the latter kind of explanations that traditionally have been labelled "functional." In what follows I examine a number of such "external" explanations and attempt to show that in each case an alternative "internal" explanation is not only available, but also preferable. While I do not wish to be understood as claiming that these examples prove that the search for "external" explanations should be abandoned forthwith, the discussion below does cast doubt on the claims that have been advanced by proponents of "functional" explanations.

It is an obvious fact that many languages have rules that neutralize contrasts between distinct lexical items or morphemes. Every beginning student has no doubt been told several times that Russian, German and a host of other languages have a rule that devices word-final obstruents. As a result, hearing the utterance [žénskij rôt], a Russian does not know whether the speaker is referring to a female's mouth (/rɔt/) or to feminine gender (/rɔd/). This ambiguity might in principle present difficulties for communication, and languages might, therefore, be expected to avoid rules that result in the neutralization of distinctive contrasts. As a matter of fact, neutralization rules are attested in practically every language and the phonetic ambiguities which they produce do not appear to have great practical effects. It might, therefore, appear that the ambiguity

resulting from neutralization of contrasts plays no role whatever in phonology.

This conclusion, however, does not seem altogether justified in the light of the interesting observations made by Jonathan Kaye (1975) that degree of ambiguity (or its complement "ease of recoverability") can explain certain otherwise quite puzzling phonological phenomena. I discuss here the simpler of the two examples analyzed by Kaye, that of Maori. In this language word final consonants were deleted in the alternations illustrated in (1):

(1)	Verb	Passive	Verb	Passive
	awhi	awhitia	mau	mauria
	aru	arumia	patu	patua etc.

Kaye observes that "since the consonants showing up in the passive forms are not predictable, the standard phonological analysis would posit underlying consonants for the verb stems which are deleted when not followed by a suffix" (p. 248-9). It has, however, been argued by Hale (1973) that this is not a correct analysis. Instead Hale proposes that in Maori all lexical morphemes end with a vowel and that the passive morpheme has eight allomorphs whose distribution is governed by an idiosyncratic property of the stem.

Kaye doubts the explanation advanced by Hale that an "underlying form in Maori with final consonant would violate a surface canonical constraint against final consonants..." because "many languages, e.g., Karok, Shawnee, Arapaho, Atsina, have rules that insert a prothetic consonant (typically ʔ or h) in front of (underlying) vowel initial words. In such languages no word begins with a vowel on the surface, yet ... there is no evidence of restructuring of the vowel initial words in any of these languages. A theory based on surface canonical forms cannot distinguish between the Maori case and the Karok case. Having vowel initial stems violates the surface constraints of Karok just as much as having consonant final stems does in Maori" (p. 249). What distinguishes the Karok case from that of Maori according to Kaye is that the consonant deletion rule of Maori produces multiply ambiguous strings, whereas the glide insertion rule of Karok does

not affect the ambiguity of lexical entries. Hence in Karok no restructuring is evident, whereas in Maori a very similar change resulted in restructuring.

I am not fully persuaded by Kaye's account. It seems to me that the ambiguity of a word such as /awhi/ is unchanged by the reanalysis. Whereas on the old account the word /awhi/ is multiply ambiguous -- at least, in principle -- by virtue of the fact that it could arise by word final consonant deletion from /awhit/ as well as from /awhim/, /awhit/, /awhik/ etc., /awhi/ is equally ambiguous on the new account because now the lexicon can include -- at least, in principle -- many items represented as /awhi/ which are distinguished one from another by the different passive suffixes that they require; /-mia/, /-ria/, /-kia/, etc.

There is, however, an alternative account available. Hale has pointed out to me in a recent conversation that the passive and the gerundive are the only forms in Maori where consonant alternations of the type illustrated in (1) are to be found. If we attempt to capture this fact with the help of a word final consonant deletion rule we are in effect implying that such alternations are the rule, rather than the narrowly circumscribed special cases that they in fact are. Moreover, this solution fails to capture yet another regularity of the language; namely it implies that morphemes can end either in vowels or in consonants; whereas if the passive and gerundive suffix are reanalyzed as having consonant initial allomorphs, all morphemes of the language end with a vowel. Thus, reanalysis does two things at once: it eliminates the word final consonant deletion rule from the phonological component and it restricts the canonical form of morphemes in the language. Arrayed against this is, of course, the additional cost of a complicated and ad hoc rule governing the distribution of the two suffixes. Although there does not exist at present a proposal of how to convert these considerations into a mechanical evaluation procedure, such an evaluation is surely not unimaginable and has implicitly been utilized quite widely. Consider, for example, the treatment of the verb to be in phonological descriptions of English. It is invariably treated as totally exceptional and attempts are not made to integrate it into the phonological and morphological rules of the language. Whatever simplicity considerations underlie this decision -- which to most linguists is so uncontroversial as not even to merit serious discussion -- will also correctly handle the case

of the Maori passive.

Thus, it would appear that the Maori case cannot be taken as evidence that functional explanations are to be sought in preference to formal ones. While this claim was not explicitly advanced by Kaye it is specifically made by Zimmer (1975) who draws attention to very interesting facts in Turkish phonology which according to him "highlight a kind of problem that the old simplicity measure signally failed to address itself to, and that is not solved by most recent proposals" (p. 556). I shall attempt to show here that Zimmer is mistaken in this assertion, that the old simplicity measure does not only address itself to this problem, but in fact forces the solution that Zimmer regards as the correct one. The facts of interest are as follows. Turkish has a rule devoicing stops and affricates in syllable final position:

(2) a.	absolute	plural	objective	
	ip	ip-ler	ip-i	"rope"
	dip	dip-ler	dib-i	"bottom"
	at	at-lar	at-ı	"horse"
	kanat	kanat-lar	kanad-ı	"wing"
	sac	sac-lar	sac-ı	"hair"
	aac	aac-lar	aaç-ı	"tree"

In the Istanbul dialect certain stem-final /k/'s alternate with \emptyset when followed by a vowel, while others do not:

(2) b.	i)	ayak	ayak-lar	aya-ı	"foot"
		inek	inek-ler	ine-i	"cow"
	ii)	ok	ok-lar	ok-u	"arrow"
		kök	kök-ler	kök-ü	"root"

Historically, these alternations between /k/ and \emptyset are due to the fact that in the Istanbul dialect /g/ was deleted intervocalically. A possible account for the facts -- and one adopted by Lees in his 1961 monograph -- is basically identical with the historical development. The grammar

includes a rule ordered after the syllable final devoicing rule which deletes intervocalic /g/:

(2) c.	$\left[\begin{array}{l} \text{-sonorant} \\ \text{-coronal} \\ \text{-labial} \\ \text{+voice} \end{array} \right] \rightarrow \emptyset / \text{[+syll]} \text{ ______ } + \text{[+syll]}$
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It will be noted that this analysis is abstract in the sense that it requires underlying representations in which a distinction is made between /k/'s that undergo rule (2c) -- these are represented as /g/, -- and /k/'s that do not undergo rule (2c) -- these are represented as /k/.

This analysis of Lees is contested by Zimmer who argues that an observationally adequate description of the Istanbul dialect requires instead of (2c) the rule (2d), which deletes intervocalic velar obstruents in the third and subsequent syllables of the word:

(2) d.	[-son, -cor, -lab] \rightarrow \emptyset / [+syll][-syll] ₀ [+syll] ______ [+syll]
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In particular, Zimmer cites such Western loan words as:

(2) e.	bek bek-i "back" kartotek kartote-i "cardfile"
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and refers to experiments with nonsense words all of which show that deletion depends on position in the word rather than on underlying phonological representation. Zimmer views this result as evidence in favor of functional over formal considerations, and he is understood in this way also by Kenstowicz, who writes: "Turkish speakers have thus opted for a more concrete analysis of k-deletion. The reason appears to be that from the point of view of the language learner, it is easier to distinguish the two different kinds of k-final morphemes on the basis of an observable property (the number of syllables) rather than on the basis of a property (underlying /g/) whose existence follows from rather sophisticated reasoning."

I do not believe that this conclusion is justified. What Zimmer has shown is not that Actual Speakers, unlike Ideal Speakers or linguists, are

unable or unwilling to engage in "rather sophisticated reasoning." Instead, Zimmer has shown that the solution proposed by Lees is wrong, simply because it fails to characterize the data, especially the data in (2e). It hardly needs to be said that from an incorrect rule no theoretical conclusions whatever can be drawn.

In his paper Zimmer observes that the abstract analysis (one including rule (2c)) can be made to work by adding a morpheme structure condition that "post-vocalic morpheme-final /k/ occurs only in monosyllabic morphemes, and post-vocalic morpheme-final /g/ only in polysyllabic ones" (p. 561).

This condition, which must explicitly be added to the grammar, can be formally expressed as shown in (2f):

$$(2) f. \quad \begin{bmatrix} \text{-son} \\ \text{-cor} \\ \text{-lab} \end{bmatrix} \rightarrow \left\{ \begin{array}{l} \text{[-voice]} / \# \text{[-syll]} \text{[+syll]} \text{ ____ } + \\ \text{[+voice]} / \text{[+syll]} \text{[-syll]} \text{[+syll]} \text{ ____ } + \end{array} \right.$$

In other words, Lees' solution can be made observationally adequate if in addition to rule (2c) it contains also the morpheme structure condition (2f). It is this adequate solution which must be compared with Zimmer's

alternative; i.e., a solution in which deletion takes place in accordance with rule (2d). The latter solution will also require a morpheme structure condition which, however, will state a simpler fact than (2f), namely that all morpheme final post-vocalic velars are voiceless. We represent this condition formally as in (2g):

$$(2) g. \quad \text{[-son, -cor, -lab]} \rightarrow \text{[-voice]}' / \text{[+syll]} \text{ ____ } +$$

We thus have two alternative descriptions that cover the same range of facts: one including the rule (2c) and the morpheme structure condition (2f); and the other composed of rule (2d) and morpheme structure condition (2g). We observe that while rule (2c) is simpler than (2d) the difference amounts to just a single feature. On the other hand, morpheme structure condition (2f) is considerably more complex than its counterpart (2g). Thus, on purely formal grounds the solution advanced by Zimmer must be chosen over the empirically adequate version of the solution à la Lees.

In other words, the issue is not between a solution requiring no sophisticated thought and one that demands elaborate reasoning, but rather between two solutions that require the same amount of sophistication, where one is somewhat simpler than the other; simplicity being measured by the old feature-counting metric of SPE. I conclude that the Turkish example provides no evidence whatever for the proposition that purely formal considerations may be overridden by considerations of a functional sort.

It is important to be clear about the fact that simplicity considerations are relevant only in a restricted class of cases -- i.e., in cases like the Turkish velar deletion just discussed where the same theory admits two or more alternative formulations that cover the exact same body of facts.

Appeal to the simplicity metric is irrelevant where alternative descriptions either cover different sets of facts or where they employ different theoretical devices. Thus, appeals to simplicity -- in the narrow technical sense of the term employed here -- cannot decide, for example, between a description that admits reference to metrical trees of the kind exemplified in Liberman and Prince's (1977) recent article as against one based on a theory like that of Chomsky and Halle (1968) that has no place for such theoretical constructs. The fact that simplicity considerations are irrelevant in such cases does not mean that there are no rational ways of choosing between alternative theories but only that this choice has to be made on grounds other than simplicity.

In several papers which deservedly have attracted wide attention Paul Kiparsky (1973) has suggested that certain limitations be imposed on the characterization of phonological facts. In the earliest of these papers "How Abstract is Phonology?" written in 1968, Kiparsky proposed that phonological theory should be so constrained as to rule out "the diacritic use of phonological features and the phonologic use of diacritic features." The latter use, according to Kiparsky, is exemplified by rules that "have the form of phonological rules but operate on diacritic features" (p. 16), and I have nothing further to say here about rules of this kind. The diacritic use of phonological features, on the other hand, is illustrated by cases where two forms are given different underlying representations "for the sole purpose of classifying segments into those that do and those that do not meet the structural description of a rule" (p. 15). Such

rules -- of which the /g/-deletion rule (2c) in Lees' account of Turkish is a typical example -- result in what Kiparsky terms "absolute neutralization," and in his 1968 paper Kiparsky proposed to exclude rules of absolute neutralization by imposing the so-called alternation condition which requires "that morphemes which are always phonetically identical must have the same underlying representations" (p. 18).

Kiparsky's condition is said to render grammars more easily learned; since the relationship between underlying and surface representations is more direct, the learning of the underlying representations requires much less of that "rather sophisticated reasoning" which many linguists appear to be uneasy about attributing to the average man or woman on the street. I should like to remark that while Kiparsky's condition renders grammars less abstract and hence, perhaps, more easily learned, it does not speak directly to the question of what role "formal" vs. "functional" considerations play in the functioning and development of languages.

Kiparsky's proposal is a proposal about the form of underlying representation of morphemes and is thus on a par with every other condition imposed on the form of grammars. What differentiates it from some of those -- e.g., from the conventions on rule ordering -- is that we have difficulty in thinking of a functional motivation for the latter, but can readily think of one for the alternation condition.

As is well known, Kiparsky's paper gave rise to an extended discussion, and as a result of this discussion as well as of additional independent work, Kiparsky has modified his views to a considerable extent. The modified position is outlined in the paper "Abstractness, Opacity and Global Rules" (1973). Its most important feature is that whereas previously "the Alternation Condition (had) been formulated ... as a limitation on underlying forms," the new proposal reformulates it "as a limitation on the application of phonological rules" (p. 65). Specifically, Kiparsky proposes the requirement (3):

- (3) Nonautomatic neutralization processes apply only to derived forms.

One of the most important facts that led Kiparsky to revise the Alternation Condition in the above manner was his discovery that there

exists a class of examples where absolute neutralization is blocked in certain contexts and admitted in others. In particular, rules of this class apply only in "derived" contexts; i.e., either across morpheme boundary, or morpheme-internally, if a crucial part of the context triggering the rule is itself derived by an earlier rule. They do not apply morpheme-internally where the triggering context is present already in the underlying representations. I regard the discovery of the existence of rules of this kind as one of the most significant empirical finds in modern phonology.

Kiparsky illustrates how such rules operate with the help of the following two rules of Finnish phonology:

- (4) a. $e \rightarrow i / \text{---} \#$
 b. $t \rightarrow s / \text{---} i$
 c. i) /haltt+i/ ($\vec{4}b$) /halt+i/ "wanted"
 ii) /vete/ ($\vec{4}a$) /veti/ ($\vec{4}b$) /vesi/ "water"
 iii) /tila/ "place" /itikka/ "mosquito" /neiti/ "Miss"

It should be noted here that in Kiparsky's formulation restriction (3) does not apply to all phonological rules but only to phonological rules which are neutralizing and which, moreover, are non-automatic, i.e., which admit specific exceptions. In their recent book on phonological theory, Kenstowicz and Kisseberth (1977) raise cogent questions about this restriction: "Why should automatic neutralization rules apply in both derived and nonderived contexts, but non-automatic neutralization rules just in derived contexts? ... Suppose that there are one or two items that are exceptions to (a) rule of neutralization ... Why should the existence of such random exceptions have the consequence that "an alternation ... cannot be accounted for" by appealing to a rule of absolute neutralization (p. 212). As a matter of fact, Kenstowicz and Kisseberth adduce two examples where a non-automatic neutralization rule must be permitted to apply in nonderived contexts. I briefly sketch one of these examples. In the Yawelmani dialect of Yokuts vowels are shortened in closed syllables. This shortening rule is a neutralizing process since it results in the

merger of long vowels into their short counterparts, which otherwise are distinct phonemes in the language. The shortening rule is also non-automatic: it does not apply before the causative-repetitive suffix /isa/. In view of this, restriction (3) should block the application of the shortening rule in nonderived contexts. This, however, is not the case. Kenstowicz and Kisseberth cite the form [ʔa:ml + a:] "might help" which must derive from an underlying [ʔa:ml + a:]. Thus shortening, which is a nonautomatic neutralizing rule, must be allowed to apply in a nonderived context, in violation of (3)¹. Moreover, in his dissertation, Mascaró (1976) develops 22 rules for Catalan phonology: he shows that 14 of these 22 rules apply only in derived contexts (he states that they are cyclic rules, but as we shall argue below, cyclic rules are restricted to applying in derived contexts only). Each of these 14 rules is neutralizing, and several of these are automatic (see pp. 17-18). Thus, (3) cannot be understood as a biconditional for there exist automatic processes that apply in derived contexts exclusively.

In the light of the preceding, one is strongly tempted to accept Kenstowicz and Kisseberth's pessimistic conclusion that "it is not immediately clear that there is a way to predict which rules will apply only in derived contexts and which rules will apply in nonderived contexts as well. If no general principle is at work, the structural descriptions of particular rules would have to be global in nature" (p. 214). Fortunately, however, there is an interesting alternative to this. It has been argued by Mascaró (1976) that given independently motivated constraints cyclic rules will be limited to applying in derived contexts only. In order to see this we must examine what is meant by the assertion that a grammar contains rules which apply cyclically. As Mascaró remarks, at a minimum this requires that such rules obey convention (5):

- (5) Given the nested constituents
 $[_n \dots [_{n-1} \dots [{}_1 \dots [{}_1 \dots] \dots] \dots] \dots]$
 a set of cyclically ordered rules C will apply to the domain
 $[_j \dots]$ only after having applied to the domain $[_{j-1} \dots]$.

Convention (5), however, is not sufficiently restrictive for it allows for types of rule interactions that are much more complex than those actually observed. For example, convention (5) as stated admits a rule that applies in even-numbered constituents but not in odd-numbered constituents. Perhaps more interesting is the fact that convention (5) allows a feature switching rule to reapply on each successive constituent. Thus, it is conceivable that in a language with a feature switching rule like the vowel shift of modern English, a word consisting of a single constituent with the stem vowel /i/ would appear on the surface as [ay]; if the same stem were part of a word consisting of two nested constituents, its vowel would be manifested as [ey]; if it were part of a word consisting of three nested constituents, its vowel would be manifested as [iy] on the surface. Moreover, the same series of outputs would be repeated in words consisting of four, five, six nested constituents, etc.².

Such repetitive reapplications of rules could be blocked by a restriction that would prevent rules from returning "to earlier stages of the cycle after the derivation has moved on to larger, more inclusive domains" (Chomsky (1973)) p. 243). Chomsky, who encountered a similar problem in his work in syntax proposed the restriction (6a):

- (6) a. No (cyclic-MH) rule can apply to a domain dominated by a cyclic node A in such a way as to affect solely a proper subdomain of A dominated by a node B which is also a cyclic node.

The consequences of this restriction for phonology were explored by M.-L. Kean (1974). Although one of Kean's two examples was subsequently shown by M. Allen (1975) to be amenable to a simpler solution that did not require recourse to restriction (6a), the paper was immensely instructive in that it demonstrated in detail how general abstract restrictions on the applicability of rules such as (6a) can serve to account for rather subtle surface facts. To illustrate what is involved consider the effects of the rules (1) and (4) on the strings (11) in (6b) below:

- (6) b. i) X → Y/____Z...] ii) P → Q/____Y...]
 iii) [...[PX]Z...] [...[PY]Z...] [...[P]YZ...]

i) -- --
 ii) -- qY

i) PYZ -- --
 ii) blocked by (6a) -- qYZ

Convention (6a) blocks the application of rule (6b ii) to the string [[PX]Z] on the second pass through the cyclic rules because the substrings PY (from PX) is a proper subdomain of the external constituent entirely contained within the internal constituent. The same condition does not hold in the case of the substring PY in the derivation from the input [[P]YZ].

It will be readily observed that rules (6b i) and (6b ii) are structurally identical with the Finnish rules (4a) and (4b) respectively.

Suppose that we assumed, therefore, that the Finnish rules are cyclic and attempted to apply them to the strings in (6c) in accordance with constraint (6a):

- (6) c. i) [#vete]# i) [#neiti]# i) [#halut+ti]#

4a. --- -- neisi

4b. --- --

4a. veti

4b. blocked by (6a) halust+ti

Output *veti *neisi halusi

As shown in (6c) we get incorrect outputs in two out of the three examples.

The reason for the incorrect outputs is not the rules, but rather the constraint (6a) which determines under what conditions the rules will or will not apply. As will be shown immediately below, if the constraint on cyclic rule application is modified along lines suggested below the two rules will produce the correct output. The version of the constraint on cyclic rule application that I propose below is a combination of certain suggestions made by Kiparsky (1973) p. 60 with others due to Mascaro (1976) p. 9:

- (7) A cyclic rule R applies properly on cycle j only if either a) or b) is satisfied:

a. R makes specific use of information, part of which is available on a prior pass through the cyclic rules, and part of which becomes first available on cycle j. There are three separate cases subsumed under a): R refers specifically to some A and B in:

i) [_jXAY...[_j-1]...B...][Z];

ii) [_jZ[_j-1]...B...][XAY];

iii) [_jX[_j-1]...A...][Y[_j-1]...B...][Z]

b. R makes specific use of information assigned on cycle j by a rule applying before R.

These conventions (7) generate the derivations in (8):

- (8) i) [#vete]# i) [#neiti]# i) [#halut+ti]#

4a. --- -- blocked by (7)

4b. --- --

4a. veti blocked by (7)

4b. vesti blocked by (7) halust+ti

Output vesti neiti halusi

Like in derivation (6c) the string [#vete]# is turned into [#veti]# by rule (4a) but unlike in (6c) this string is now subject to rule (4b) by virtue of convention (7b). The rule (4b) applies to the string [#halut+ti]# by virtue of convention (7a). The derivation of /neiti/ is the most interesting of all for it illustrates the important fact that given the convention (7) no cyclic rule can apply to the innermost constituent of a string. We note that (7a) requires that material from two distinct constituents be specifically mentioned in the structural analysis of a cyclic rule. Convention (7b) does not require this, but it must be preceded by a rule which applies by virtue of (7a) and which, therefore, cannot apply to the innermost constituent of a string. Thus, since neither of the conditions

in (7) can be met by the innermost constituent of a string, no cyclic rule can apply to the innermost constituent. It follows from this that if a particular phonetic property -- e.g., stress -- is assigned by a cyclic rule, the rule cannot assign it to root words; instead stress on root words will have to be supplied directly in the lexical representation. It is, therefore, of considerable interest that Mascaro³ was able to show that in Catalan, where stress is assigned by cyclic rules, the attempts to extend these rules so that they would also supply stress to root words in their lexical representations have been failures and that stress must be supplied directly in lexical representations.

Upon reading an earlier version of this paper G. N. Clements has suggested to me that convention (7) might be replaced by the following:

- (9) a. A cyclic rule R applies properly on cycle j if and only if there is an immediately preceding cycle j-1 such that R must make specific use of information not present in the string at the end of that cycle.

This convention differs from (7) in that it requires cyclic rules to apply in the following two types of underlying strings which are ruled out by (7):

- (9) b.i. $[_jZ[_{j-1}\dots]XABY]$
 ii. $[_jXA[_{j-1}\dots]BZ]$

I note that since application of rule R to the string AB would be ruled out if AB were part of the innermost constituent, it would seem plausible that the rule R should not be allowed to apply to the string AB when the string first appears on the jth cycle.

The only difference between the strings (9b i) and (9b ii) is that the units A and B to which the cyclic rule applies are adjacent to one another in the first, and nonadjacent in the second. I would guess that this difference between adjacent and nonadjacent constituents should not make an essential difference for a convention on rule applicability such as (7) or alternative (9). I, therefore, prefer (7) to the alternative (9). Unfortunately, neither Clements nor I have been able to think of actual

examples of strings of the forms above. The difference between the formulations (7) and (9) must, therefore, remain moot at this time.

To conclude it would appear that the special conditions on rules discovered by Kiparsky are unconnected with the automatic or nonautomatic character of the rule. Their relationship to the abstractness of lexical representations is indirect: since cyclic rules cannot apply to innermost constituents they cannot be the source of phonetic properties within root morphemes, such properties will in many instances have to be directly present in the lexical representation. What is crucial about the conditions discovered by Kiparsky is that they are perfectly natural conditions on the application of the class of rules that make reference to the internal constituent structure of words and phrases. Such rules have received somewhat less attention in phonology than they deserve, especially in recent years. Mascaro's dissertation is particularly instructive in this respect for it demonstrates in detail the important role that cyclic rules play in the segmental phonology of Catalan.

I have examined here three typical examples in which it was claimed that "functional" rather than "formal" considerations must be invoked in order to account for the facts, and have shown that in each case the standard "formal" considerations provide an account that is not only adequate, but also superior to the other alternative. It would be hasty to conclude from this that "functional" considerations are totally irrelevant in phonological research. Apart from the fact that negative results do not warrant such an inference, there is no question that "functional" factors play a role in linguistics. If their influence is often hard to discern clearly, this is due to the fact that they are just one type among a host of factors that determine the course of linguistic evolution. By disregarding them in our research we, therefore, run the risk of missing potentially promising and enlightening lines of inquiry. The search for these factors, however, does not warrant a disregard of other -- in particular, formal -- factors in phonology.

NOTES

- 1 Kiparsky (personal communication) contests Kenstowicz and Kisseberth's analysis of Yawelmani, in particular their rule of /l/-epenthesis on which the above example crucially depends.
- 2 That cyclic rules might function in this fashion was suggested by T. Bever and T. Langendoen (1963), who attempted in this manner to account for the e/o ablaut in Greek and other Indo-European languages. This ingenious idea unfortunately failed to be confirmed by the facts and has, therefore, been abandoned.

3 The above raises questions about the SPE analysis of English stress. It should be noted that the only cases where stress needs to be supplied to the innermost constituent are the deverbal nouns of the type condensation.

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