Chapter 8

The Role of the Evaluation Metric in the Acquisition of Phonology

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The title of this contribution could more properly refer to "an evaluation metric," since two empirical questions are really at issue here: whether linguistic theory should provide a device for evaluating postulated grammars and what the characteristics of that device ought to be. Responses to both these questions are offered: first, that the need for an evaluation metric is demonstrated by the existence of phonological and morphological projection puzzles (Baker, chapter 10), instances of rule learning despite demonstrably inadequate primary data; and second, that the evaluation metric has essentially the form envisioned in Chomsky and Halle (1968, pp. 330–335), though with some adjustments for later modifications of this general phonological framework.

These two theses cannot be separated in practice, however: any evidence that appears to support the general necessity for an evaluation metric can be interpreted only in the light of a particular form of that metric, and conversely. This essay will, therefore, be chiefly structured around examples that illustrate both points. I present four cases, each involving a projection paradox that can be solved by selecting a grammar with simpler rules and representations, in the technical sense. Two general points are discussed before these more detailed studies. The first deals with the overall formal character of the acquisition of phonology and morphology, the second with some earlier investigations of the evaluation metric as well as the specific form of the metric proposed here.

Two problems will not figure significantly in this treatment, although they may be directly relevant to determining the form of the evaluation metric. First, I will make no attempt to discuss incorporation of a theory of phonetic substance into the phonology. This is often known as the problem of rule naturalness, although that term is by no means

used consistently. Several interesting proposals for dealing with this problem formally have been discussed in the literature, such as those by Bach (1968) and Chen (1973). It has also motivated substantial revisions of the overall structure of phonological theory, either along the lines of Chomsky and Halle (1968, pp. 400–435) and Kean (1974) or of Stampe (1972) and Hooper (1976). Second, I have nothing to offer to the debate on natural rule ordering, which can be found in a number of articles that have appeared since Kiparsky (1965). If some observations about natural orderings are correct as claims about synchronic grammars, then clearly an account must be made of them in the evaluation metric. One useful suggestion along these lines is implicit in work by Anderson (1974). In any case, these issues, although they may have import for the form of the evaluation metric, are distinguishable from the problems I discuss here. No considerations of phonological naturalness or of natural rule interactions appear to play any significant part in the following examples.

The Basis of Learning Phonology

Consider the familiar diagram in (1), in which the terms for levels of adequacy are used in the sense of Chomsky (1964a):

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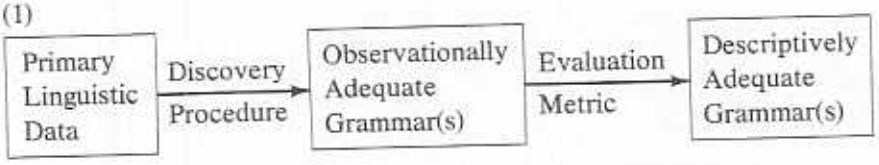

The observationally adequate grammars, which are induced from the primary data by some as yet poorly understood discovery procedure, are all compatible with the primary data. The evaluation metric, or some equivalent device, selects the observationally adequate grammar that most closely models the knowledge actually acquired by speakers. It is possible that more than one descriptively adequate grammar might emerge, but no conceivable linguistic behavior would enable us to distinguish between them. This is, therefore, not an empirical question.

One important characteristic of the problem of the acquisition of phonological and morphological grammars is that, in principle, there could be nothing of interest to learn. One might imagine a model of acquisition in which some set of purely phonetic representations—essentially digitized versions of the corresponding articulatory and acous-

tic events—is merely memorized by each speaker of a language. A certain amount of computable deviation from this set will be needed to handle the obvious problem of perception despite differences between speakers, but the essential idea remains that something akin to the surface phonetic representation might be the greatest abstraction speakers are capable of. The model of acquisition presupposed by this theory is clearly a simple one: the mapping designated as the discovery procedure in (1) will be the analog to digital function, and the evaluation metric will be a simple isomorphism. In effect, all observationally adequate grammars will be descriptively adequate, so this distinction becomes unnecessary.

It is, of course, well known that the equivalent theory is inadequate to the demands of natural language syntax because recursion in the base leads to potentially infinite sentences. This argument does not carry over to phonology and morphology, though. An upper bound could be placed on the length of the phonological phrase (minimally, there is a physiological limit), which, since the lexicon is apparently finite, would ensure the existence of only a finite number of such phrases.

This does not leave us without recourse, however. There is an alternative, which serves as the basis of much recent work that seeks to validate or falsify the claims of generative phonology on so-called external grounds. Substantial effort in articles too numerous to cite has been directed toward demonstrating the existence or the nature of the generalizations that speakers express in their grammars by adducing evidence not normally available to language learners, although this criterion has rarely been made explicit. The idea is that such evidence reveals aspects of the speaker's linguistic knowledge that could not possibly have been directly memorized, since only the primary linguistic data are input to the discovery procedure. In sum, this method makes it possible to determine which of several observationally adequate grammars is, in fact, descriptively adequate. On the basis of this determination, one can construct a linguistic theory that will more highly value the descriptively adequate grammar or else rule out the other observationally adequate grammars entirely.

What I propose here is a similar sort of investigation. Several observationally adequate grammars, all of which appear to be compatible with the axioms of a plausible phonological theory, will be offered for each body of primary linguistic data. From consideration of data not accessible to language learners it will emerge that only one of these

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grammars is descriptively adequate, and I will argue that an evaluation metric based on simplicity is able to make this choice. The nonprimary data adduced here are of relatively uncontroversial interpretation, chiefly involving some characteristics of loan words and the like. But in the first example, the process of Expletive Infixation, I will show that there are essentially no primary data at all and so the study focuses entirely on adult speakers' intuitions.

The Evaluation Metric

The fundamental idea behind the phonological evaluation metric is that it should count the linguistically significant stipulations made by different grammars and then select the grammar that makes the smallest number of them, all other things being equal. The definition of a linguistically significant stipulation is provided by the theory; in general, stipulations will be units of the system of formalization for rules and representations. The evaluation metric of a generative phonology is therefore distinct (contra Chen, 1973) from Hjelmslev's (1961) principle of simplicity, which is offered independently of any such theory.

The particular form of the evaluation metric adopted here has the following characteristics. Like the familiar device of Chomsky and Halle (1968), it values competing systems of phonological rules inversely according to the number of phonological features appearing in each system. A few natural elaborations of this procedure are also needed in response to gaps in its applicability.

Recent work on metrical phonology (Liberman and Prince, 1977) suggests that some rules and representations—particularly those referring to stress or syllabification—make use of formal devices, metrical trees, that are rather different from phonological features. Although study in this area is still at a very early stage, certain regularities in the construction of metrical trees and their application to segmental strings can be observed. When these regularities are cast in terms of a formal metrical theory, they can be said to represent a base that can be deviated from only with additional stipulations, although the exact cost of such deviation is unknown. This problem will become somewhat clearer in the discussion of the relevant example from Cairene Arabic.

The mode of evaluating readjustment rules is never described by Chomsky and Halle (1968), although this issue is of some significance. These rules, also known as morpholexical rules (Anderson, 1975) or

allomorphy rules (Aronoff, 1976), chiefly account for fairly restricted segmental alternations in morphological terms. But it is possible to give any mildly abstract phonological process such a formulation if only a finite number of morphemes display the alternation. Therefore some procedure is needed to determine when a phonological and when a morphological formulation is appropriate. I will argue that this decision is made by the evaluation metric, based on the following consideration: in a readjustment rule, reference to a single morpheme or a single morphologically defined class of morphemes requires the equivalent of one phonological feature. In other words, an essentially morphological process like a readjustment rule can manipulate a particular morpheme by making a single stipulation. This proposal is in contrast to the occasional descriptive practice of referring to morphemes by mentioning enough segmental information to define them uniquely. The claim is that, all other things being equal, a readjustment rule will be superior to a phonological rule if and only if the process under consideration depends on some single morpheme or natural morphological class.

A final point concerns the extension of the evaluation metric to the lexicon as well as to the rules. Chomsky and Halle (1968) wrestle with this issue inconclusively, dealing primarily with the difficult problem of specious morpheme structure rules. Other questions are involved, however. Suppose, for example, that only rules are evaluated, so the values of the lexicons in competing grammars are not considered by the formal evaluation metric. Under this assumption, regularities not reflected in alternations will never be expressed by phonological rules. If such regularities are left unexpressed, they complicate only the lexicon; but if they are extracted from the lexicon, they complicate the phonological rules. I will make the converse assumption, the one ultimately adopted by Chomsky and Halle (1968): the value of a grammar is inversely related to the number of features in its rules and in its lexicon. This proposal may require considerable elaboration in the light of recent studies in lexical structure (Aronoff, 1976; Lieber, 1980) and of some points raised by Phelps (1979), but it will suffice for the example discussed here.

Expletive Infixation

The first example is quite compelling by virtue of the extreme paucity of the primary data, the clarity of the phenomenon, and the simple mode of application of the evaluation metric. One of the most produc-

tive rules of English morphology, yet the one for which the language learner has by far the least data available, is the process of Expletive Infixation. Any word, subject to some phonological conditions, can have inserted into it an expletive like *fuckin* with a kind of vague emphatic force. A fuller discussion of the phonology of this process can be found in McCarthy (forthcoming); here I will somewhat simplify the problem.

First, let us consider this phenomenon in its sociolinguistic aspect. Until recently it has been, in many social groups, taboo to utter words like *fuckin* in the presence of children at any age when they might be in the process of language acquisition. The significance of this fact should not be underestimated: this is an example in the acquisition of phonology of extremely degenerate primary data, with the environment providing almost no evidence on which to base the formulation of a rule. Casual observation suggests that many (possibly most) speakers learn this process on the basis of the single exemplar *fan-fuckin-tastic*, which may be heard in childhood.

In contrast to this lack of primary data, a process emerges that is extremely productive, usually subject to fluent production and rapid perception. Judgments of well-formedness are normally quite robust for individual speakers and remarkably consistent across speakers. All of these facts are incompatible with any sort of true adult learning or with metalinguistic activities like language games. (Compare English-speakers' control of learned morphology or of pig Latin to Expletive Infixation.) I conclude, then, that Expletive Infixation is a genuine (albeit marginal) part of English morphology and that there is a serious problem in determining how it could possibly be acquired.

I will begin from the assumptions that some trivial initial stimulus, like *fan-fuckin-tastic*, demonstrates to the language learner that expletives can in fact be infixed, and that the entire learning process must be based on this unique form, and perhaps a few others. This example will constitute an extreme test of the evaluation metric and of the concomitant phonological and morphological theory, inasmuch as virtually the whole of the acquisition process for this rule must be a computation by the learner rather than an approximation to grammars of greater observational adequacy.

In fact, phonological theory does provide a partial answer to the question of where an expletive may be infixed. Consider the contrast in the examples in (2):

(2)
a. *fa-fuckin-ntastic
   *fant-fuckin-astic
b. *fanta-fuckin-stic
   *fantas-fuckin-tic
c. fan-fuckin-tastic

The data in (2) represent clearly uncontroversial judgments. The forms in (2a) are ungrammatical because the expletive has failed to lodge at a syllable boundary. The forms in (2b), on the other hand, show that the syllable following the expletive must bear stress. Although the following syllable has primary stress in (2c), this is not essential. Compare (2c) with the equally well-formed *anticipa-fuckin-tory* or *antici-fuckin-pate*, where the following syllable has only secondary stress.

In McCarthy (forthcoming) I demonstrate that these two conditions on Expletive Infixation can be subsumed under a single rubric—an expletive may fall only at the boundary of a metrical foot. The prosodic category foot in English can be defined as the string composed of a stressed syllable and any immediately following unstressed syllables (Liberman and Prince, 1977). Moreover, this condition on the rule of Expletive Infixation need not be stipulated. Rather, it follows from general considerations of the well-formedness of prosodic structures. Since *fuckin* and any other expletive like *bloody* are themselves metrical feet, infixation would involve inserting a foot inside another foot. This would yield an improper bracketing with one foot containing another, distinct foot, a situation that can be ruled out by hypothesis in metrical phonological theory.

The conclusion, then, is that it is a necessary condition for Expletive Infixation at any position in a word that that position be a foot boundary, and this condition follows from universal principles inherently available to the language learner. On the other hand, one must ask whether this is a sufficient condition as well.

The language learner, when presented with the stimulus *fan-fuckin-tastic*, can posit a very large number of possible additional conditions on Expletive Infixation that are consistent with this form. For example, one could hypothesize that only the sequence *nt* may be split by infixation, that the infix may precede only voiceless stops, that only consonant clusters can host the expletive, that only morphologically complex words can have an infix, or that the preceding syllable must be stressed as well. All of these hypotheses are compatible with the given datum,

as are many others. Even if the learner were presented with a somewhat richer body of primary data, it would surely be small enough to permit the extraction of some set of similar conditions. In fact, none of these additions is correct, and the universal principle permitting expletives only at a foot boundary is both a necessary and a sufficient condition for Expletive Infixation.

The task, then, is to show why speakers, with great uniformity, do not acquire a version of Expletive Infixation that is restricted to some properties of the lexical item *fantastic* or the like. This observation follows directly from the evaluation metric, since the metric prohibits unjustified complication of this rule. To see how the metric works in detail, let us look in particular at the possible conditions on the stressing of the syllable preceding the expletive.

Two observationally adequate analyses could be proposed that are in agreement with the form *fan-fuckin-tastic*. The first would incorporate into the structural description of the infixation rule a partial environment of the following form: $\underset{[+\text{stress}]}{\sigma}$ ____. That is, the infix appears only when it has a stressed syllable to its immediate left. The second analysis would be identical except for the lack of this stipulation. Both analyses offer observationally adequate accounts of the primary data.

Since this process is extremely productive, it is not difficult to find nonprimary data to select the descriptively adequate grammar. Many examples show free infixation after unstressed syllables: *Kĕn-fuckin-tucky, Nĕ-fuckin-braska, ĭm-fuckin-portant, air cŏn-bloody-ditioner*. The last two examples are actually attested; the first two reflect strong judgments. The obvious conclusion is that it is incorrect to require that the syllable preceding the infix be stressed.

Application of the evaluation metric to these two fragmentary analyses yields the same conclusion. Since there is no evidence in the primary data showing that the preceding syllable must be stressed, the first analysis is needlessly complicated by virtue of the stipulation $\underset{[+\text{stress}]}{\sigma}$ with respect to the second. The second is therefore more highly valued and consequently is the one incorporated into a descriptively adequate grammar.

Although this is an almost absurdly simple comparison of two competing rules by the evaluation metric, it is nevertheless important. Because of the degeneracy of the primary data, one must take the problem of acquisition very seriously here. There are many inherently plausible, observationally adequate analyses of this tiny corpus of data, yet

further evidence of the kind presented shows that nevertheless the data are mapped onto a single process that is remarkably clear and consistent in adult grammars. This phenomenon is, then, a valuable test of the evaluation metric's role in acquisition.

Cairene Arabic Stress

Elsewhere I have presented a metrical theory of the role of syllable weight in stress assignment and an analysis of the accentual phenomena of the Arabic dialect spoken in Cairo (McCarthy, 1979a; 1979b; 1980). What follows is partly abstracted from those treatments, with certain complications suppressed that are not relevant to the argument.

The basis of stress assignment in Cairene Arabic is the division of syllables into two weights. Heavy syllables contain a long vowel (CVV) or a postvocalic consonant (CVC). Light syllables have neither (CV). Heavy syllables will be represented metrically by a branching node, light syllables by a nonbranching node. These geometric characteristics, which correspond to the familiar bimoraic/monomoraic syllable distinction, may be referred to by rules of stress assignment. I will further assume that all final syllables are represented formally as light, with a nonbranching node, though the full story is somewhat more complex.

The role of heavy and light syllables in accentuation can be seen in the data from Cairene Arabic in (3):

(3)

a. Heavy penult:

maʕáaki	'with you (f. sing.)'
ʕamálti	'you (f. sing.) did'

b. Heavy antepenult and light penult:

martába	'mattress'
šuftáha	'I saw her'

c. Light antepenult and light penult:

búxala	'misers'
muxtálifa	'different (f. sing.)'

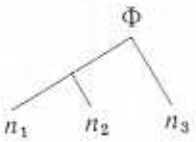
The forms in (3a) and (3c) represent phenomena that are often paired with one another—compare them with the results of the Romance Stress rule in English *Amánda* or *aróma* versus *América*, or to the related Classical Latin stress rule. What is unusual is the pattern of stress in (3b). For words with syllables of this type the Romance Stress

rule gives antepenultimate stress (*ársenal, kínkajou, rádio*), as do many other stress rules, so the Cairene penultimate stress is surprising.

I will present different formal metrical analyses of the data in (3), the differences centering around the treatment of (3b). It will emerge that one of these analyses is clearly preferred by the evaluation metric and that this preference is confirmed by the accentuation of Classical Arabic words according to the Cairene pattern.

In the first putative grammar of Cairene stress, stress assignment is envisaged as a two stage process. First, stress is assigned according to the Romance Stress rule model; then a stress shift rule moves the stress one syllable to the right from a heavy antepenultimate syllable. A metrical formalization of this analysis first maps the basic prosodic unit foot, represented by the label Φ, onto a word from the right boundary toward the left. This foot will maximally be of the form in (4).

(4)
Foot Structure:



The terminal nodes n_1, n_2, and n_3 are mapped onto the terminal nodes of the branching and nonbranching trees associated with heavy syllables and light syllables respectively. For some representative examples, this mapping yields the result in (5):

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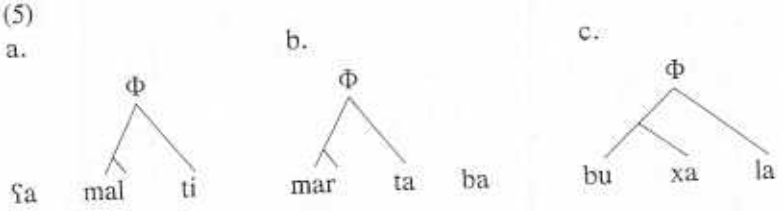

Metrical theory provides another level of tree structure whose terminal nodes are the roots of feet and any syllables not yet incorporated into feet. This word-level tree consolidates all feet and stray syllables in a word into a single metrical structure. I will assume that the word-level structure of Cairene Arabic is represented by a right-branching tree,

although this would show up overtly only in forms somewhat longer than those in (5).

Finally, the grammar must specify the labeling of the metrical tree. It indicates a relation of relative prominence defined by complementary *s* (strong) and *w* (weak) labels on nonroot nodes of the tree. In this case, a labeling rule identical to that proposed for English by Liberman and Prince (1977) will operate throughout the tree.

(6)
Labeling Rule:
Label the right node strong (*s*) if and only if it branches.

The final result of foot assignment, construction of a word-level tree, and labeling is the set of representations in (7).

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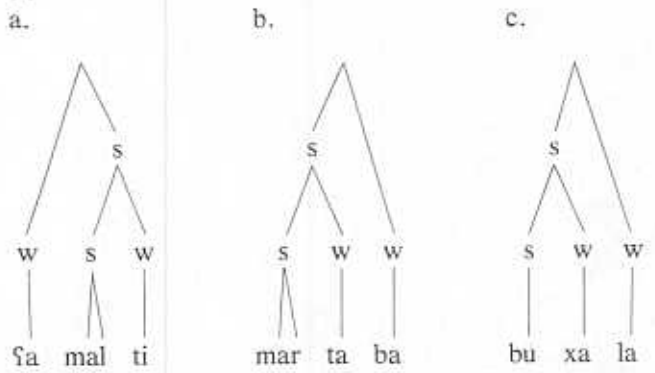

For (7a) and (7c), the correct stress relations are represented. But (7b) incorrectly shows antepenultimate stress instead of penultimate.

Up to this point the analysis of Cairene Arabic has included all of the characteristics needed to generate stress in Latin or Damascene Arabic, for example. It now must diverge to include a means of assigning the correct stress in forms like *martába*, given the intermediate representation (7b). At least two possible directions can be taken, both at least partly precedented in other fairly well understood metrical systems.

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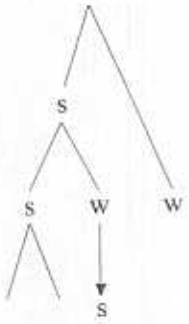
Relabeling Rule:



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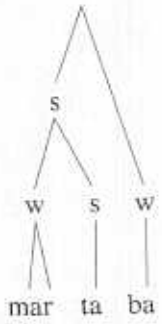

The second possibilit
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One possibility is to write a relabeling transformation that will move the stress from the antepenult to the penult in (7b). This rule is formulated in (8):

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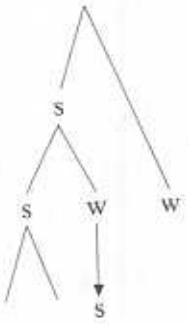
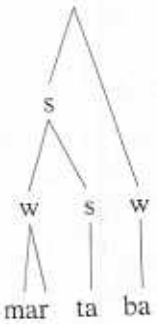
Relabeling Rule:



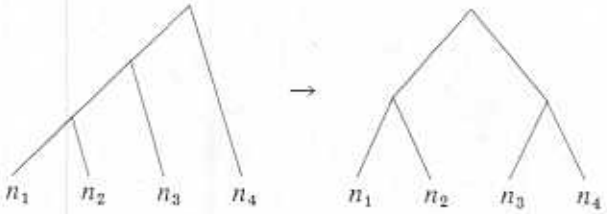
Since the labels *s* and *w* on sister nodes always have complementary values, it suffices to indicate a change in just one sister, as in (8). The application of this relabeling rule to the intermediate structure in (7b) will yield the derived structure in (9), which correctly shows penultimate stress:

(9)



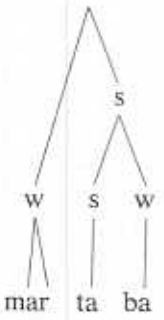
The second possibility is to add a rule of tree formation to the one already formulated in (4). This rule will apply to the tree in (7b) to split it into two feet, as in (10):

(10)
Restructuring Rule:



I assume that this rule applies after assignment of word-level structure but before the labeling rule. Its output, therefore, will be subject to the usual labeling, producing the derived structure in (11).

(11)



So this alternative also will correctly assign penultimate stress.

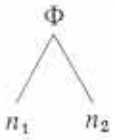
This analysis is therefore adequate to handle the facts of Cairene Arabic stress. Moreover, on several counts—Foot Structure (4) and Labeling (6)—it is formally identical to the stress systems of several other languages. It does, however, require the added complication of either the Relabeling rule (8) or the Restructuring rule (10). Both rules are of types that apparently must be countenanced by linguistic theory. Rules of relabeling have been justified in many languages, with some, like the English Rhythm rule, conditioned by stress clashes, but with others, like those of Tiberian Hebrew (McCarthy, 1979a) and Yidin^y (Hayes, 1980), conditioned by morphological or syllabic contexts. On the other hand, the Restructuring rule (10) is formally almost identical

to an English process applicable in words like *obligatory* (Liberman and Prince, 1977, p. 296). In sum, it seems that metrical theory must provide apparatus that would make either (8) or (10) a possible rule in an analysis of Cairene stress.

An alternative treatment of Cairene stress is based on a very different basic rule of stress assignment. Suppose that stress is assigned by a left-to-right procedure that counts pairs of light syllables. The first syllable in the last such pair will then be the one to bear the main stress. This is somewhat easier to visualize in the metrical formalization.

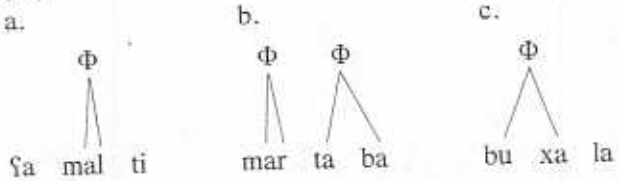
The foot, assigned from left to right, has the form in (12).

(12)
Foot Structure:



Therefore the foot contains only two moras, taken either from two light syllables or a single heavy syllable. This is actually a reasonably common foot type; one of the clearest cases is in the accentual system of Creek (McCarthy, 1979b). Application of this foot structure to some representative examples yields the results in (13).

(13)

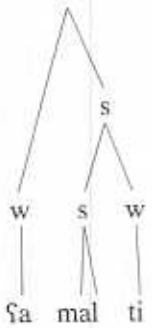


Note that the left-to-right mapping of (12) onto words ensures that the first two and not the last two syllables in (13c) are paired into sisters.

As in the first analysis, provision is made for a right-branching word-level tree and for labeling of structures in accordance with (6). The final output of the stress rules under this proposal will be the set of representations in (14):

(14)

a.



b.

c.

These trees, like those derived by the first analysis, indicate the correct accentuation of these forms.

What is interesting about Cairene Arabic is that two such very different grammars are both observationally adequate. A moment's reflection will show that these analyses make different predictions only in words that end in a string of more than three light syllables. Such words are ordinarily impossible in this language for historical reasons. The sole exception, a class of words with four light syllables, belongs to the only major type of morphologically governed stress assignment in Cairene, so it must be discounted. Therefore, no evidence in the primary data would choose between the two analyses purely on grounds of observational adequacy.

The question of which of these observationally adequate analyses is more highly valued is not difficult to answer, even with our current lack of certainty on many details of the metrical formalism. All the rules invoked are possible within the theory and many are even common. But the first proposal, based on the foot structure in (4), involves the added complication of either Relabeling or Restructuring, neither of which is needed under the second proposal. I conclude, then, that the grammar incorporating the foot structure in (12) is formally simpler and therefore more highly valued than the alternative.[1]

It can be shown on the basis of nonprimary data that the second analysis is indeed the one selected by language learners. Such data come from the pronunciation of Classical or Literary Arabic words by native speakers of the Cairene dialect. As no universal standard for the accentuation of Classical Arabic exists, the typical situation is that each dialect area follows its own rule in stressing Classical Arabic words.

This observatio
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(15)

a. Penultimate
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šaǰaratuhúm
ʔadwiyatúhu

b. Antepenultin
šaǰarátuhu
ʔadwiyatúhu

It is apparen
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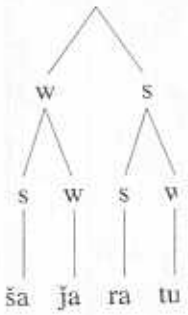
those in (16):

(16)

a.



This is, in fact,
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bic stress, alth

This observation is particularly useful because Classical Arabic has words that end in strings of light syllables longer than three. These show the curious pattern of accentuation in (15) when read by a speaker of Cairene Arabic.

(15)

a. Penultimate stress:

šaǰarátu	'tree (nom.)'
šaǰaratuhúmaa	'their (du.) tree (nom.)'
ʔadwiyatúhu	'his drugs (nom.)'

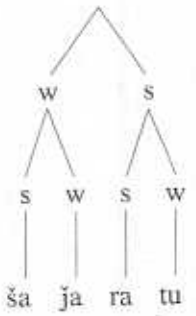
b. Antepenultimate stress:

šaǰarátuhu	'his tree (nom.)'
ʔadwiyatúhumaa	'their (du.) drugs (nom.)'

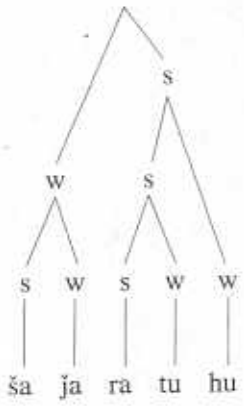
It is apparent that, under the first analysis, all of the forms in (15) should have antepenultimate stress since they have light penults and light antepenults, like the shorter forms in (3c). But the second analysis, which assigns the foot structure in (12), ultimately yields trees like those in (16):

(16)

a.



b.



This is, in fact, the desired output, and similar results can be derived for the other forms in (15).

Thus the accentuation of forms from Classical Arabic—essentially the naturalistic equivalent of a psycholinguistic experiment demanding production of nonsense words—demonstrates that only the second alternative provides a descriptively adequate account of Cairene Arabic stress, although both analyses are observationally adequate. The

choice between the two analyses made by the language learner conforms to that made by the evaluation metric, so the role of the metric in this type of learning is confirmed.

Maori Passives and Gerundives

Perhaps the most compelling example of an apparent failure of the evaluation metric to select the same grammar as that chosen by language learners is Hale's (1973) often cited analysis of the passive and gerundive formations in Maori. I will review the facts quickly, present Hale's interpretation of them, and then suggest a rather different understanding of the problem based on the evaluation of systems of readjustment rules.

The basic observation is that there is a consonant/zero alternation in the active versus passive and gerundive forms of many Maori verbs:

(17)

Active	Passive	Gerundive	
wero	werohia	werohaŋa	'stab'
hopu	hopukia	hopukaŋa	'catch'
aru	arumia	arumaŋa	'follow'
mau	mauria	mauraŋa	'carry'
awhi	awhitia	awhitaŋa	'embrace'

In addition, no Maori word ends in a consonant. In the light of these simple paradigms and this additional fact, a descriptively elegant and straightforward analysis presents itself. The grammar of Maori will be provided with base forms with final consonants like /weroh/ and /hopuk/, with suffixes /-ia/ and /-aŋa/, and with a rule of final consonant deletion formulated as in (18).

(18)

$[-\text{syll}] \rightarrow \emptyset \; / \; \_\_\_ \; \#$

Although this analysis is cast in generative phonological terms and notations, it is by no means confined to that tradition. For example, Bloomfield (1933, p. 219) sets up what he calls "basic forms in theoretical shape" to handle an almost identical set of alternations in Samoan. Nida (1949, p. 76) accepts this proposal as well, describing the loss of the final consonant as a phonologically conditioned process.

Yet Hale demonstrates that this phonological analysis of the Maori data, which is a model of formal simplicity and inherent plausibility, is actually inadequate and that it is inferior to a basically morphological

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(19)
Maori Readjustn
a. -tia → -hia
-taŋa → -haŋa
b. -tia → -kia
-taŋa → -kaŋa
c. -tia → -mia
-taŋa → -maŋ
d. -tia → -ria
-taŋa → -raŋa

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analysis along the lines followed by Hohepa (1967, p. 111) or Biggs (1961, pp. 33–34). These authors analyze the consonants preceding *-ia* and *-aŋa* as part of the passive and gerundive suffixes rather than part of the stem. Each verb then idiosyncratically selects one of the suffix alternants without reference to phonological considerations. In effect, Hohepa establishes arbitrary conjugation classes for Maori stems that determine which suffix allomorph is chosen, with *-tia* as the basic alternant from which others are derived by a set of lexically governed transformations.

Before considering Hale's evidence, let me attempt to make this morphological analysis of Maori explicit in terms of a theory of readjustment rules. Passive and gerundive appear in the morpheme list that I assume is part of the lexicon as two sets of morpheme alternants: {*-tia, -hia, -kia, -mia, -ria*} and {*-taŋa, -haŋa, -kaŋa, -maŋa, -raŋa*}. These sets can be considered partial orderings, with the first alternants *-tia* and *-taŋa* designated as basic or unmarked. The readjustment rules in (19) then apply to these basic alternants to select other members of the set:

(19)
Maori Readjustment Rules:
a. -tia → -hia
   -taŋa → -haŋa
b. -tia → -kia
   -taŋa → -kaŋa
c. -tia → -mia
   -taŋa → -maŋa
d. -tia → -ria
   -taŋa → -raŋa

These readjustment rules are to be understood as operations on morphemes taken from the two sets of suffix alternants, rather than as transformations of the segmental material in the morphemes. Furthermore, they must all be minor rules; that is, they must be unable to apply to any form not explicitly marked to undergo them. Therefore any verb that takes a suffix alternant other than *-tia* or *-taŋa* will necessarily bear a diacritic feature [+rule *x*], where *x* indicates one of the rules in (19). As in all conjugation analyses, I will redundantly rule out any stem bearing two such contradictory features.

Of course, other formulations of the morphological analysis of Maori are conceivable, but they all should have the salient characteristics of

this one: a list of morpheme alternants in the lexicon, designation of *-tia* and *-taŋa* as somehow basic, and lexically governed choice of other alternants. This choice will involve, as in (19), substituting one morpheme for another.

I turn now to the central problem presented by the Maori data. Hale's evidence shows that speakers fail to internalize the seemingly simple phonological analysis and instead select a morphological analysis along the lines in (19). Quite a mass of data can be brought to bear on this question:

(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 unassimilated 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. (Hale, 1973, p. 417)

The morphologically based grammar of Maori in (19) can account for these observations with few additional assumptions. Forms derived by extension of passive morphology to categories that are ordinarily nonverbal (1 and 3) will be marked by *-tia* because nonverbs will usually have no reason to acquire diacritics for the minor passive morphology. Use of *-tia* with causatives and compounds despite another passive suffix with the corresponding underived verb stem (2 and 5) can also be readily explained. Kiparsky (1973a, pp. 89–90) has observed that diacritic features (but not segmental material) are often lost under derivation. Therefore *-tia*, which appears on forms that do not bear minor rule diacritics, is correctly predicted for derived verbs. Similarly, loan words (4) will ordinarily fail to have such diacritics. When the diacritic for a particular form is unknown (6), *-tia* should also show up.

The morphological solution, then, will account for the ordinary paradigmatic data and for these additional facts cited by Hale. The phonological solution, although it handles the paradigms, makes no special predictions for any of these additional data. Of course, if the additional data were readily available to language learners, one would expect them to reject the phonological analysis simply as observationally in-

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adequate. Although this argument has not been made explicitly, it appears that the significance of the Maori data lies in the relative inaccessibility to learners of the information outlined by Hale.

The most striking evidence—the treatment of loans and of forgotten desinences—is plainly unavailable for language acquisition since it presupposes knowledge of either the etymology of the word or the mental state of the speaker. Instances of either sort can simply be treated as *t*-final stems in underlying representation in the phonological analysis. Furthermore, learners are likely to perceive the anomaly of surface consonant-final unassimilated loans and not be motivated to restructure their grammars solely on the basis of their selection of the *-tia* allomorph.

A similar analysis is available for the suffixation of *-tia* to spontaneous denominal verbs. The learner cannot judge with confidence that they are spontaneous; that would require a near-perfect knowledge of the Maori dictionary. Therefore such forms can also have underlying representations in final *t*. The passive forms of adverbials can likewise be handled by having all adverbials terminate in *t* in the lexicon. Conceivably this regularity in adverbials would even motivate postulating a suffix morpheme *-t* for this lexical category.

The only remaining fact is the use of *-tia* in derived verbs even when the nonderived stem has another final consonant. This will require a complication of the phonological analysis if that analysis is to be observationally adequate. One can expect that language learners would take note of this fact and express it by a rule taking any stem-final consonant to *t* in a derived verb form. In sum, this is the only fact cited by Hale that is directly available as input to the construction of putative grammars by a discovery procedure.

To complete the argument, we can observe that a serious problem for a theory of phonological acquisition based on the formal evaluation metric is presented by the fact that language learners select the morphological analysis over the phonological one. As an account of all the Maori data cited, the phonological analysis is not only observationally adequate but also more highly valued. The phonological rule system requires reference to a single feature and a boundary in the deletion rule (18) and uncertain but clearly small cost for the rule applied in derived verb forms. On the other hand, the morphological analysis requires the eight readjustment rules in (19), where each will be evaluated at two features, according to the principle I have proposed, since each makes reference to two different morphemes. I know of no likely

procedure by which one can evaluate the lexicons demanded under each solution. I will make the not unreasonable assumption that supplying each stem with a final consonant is formally equivalent to a set of rule diacritics and their corresponding list of morpheme alternants, so the two lexicons are equally valued. The conclusion is that the phonological solution has formally simpler rules and consequently is more highly valued than the morphological solution, apparently the wrong result.

Hale's (1973) solution to the problem of Maori passives and gerundives involves an axiomatic exclusion of the phonological analysis. Observing that underlying verb stems have final consonants but no surface forms do, Hale proposes that a universal constraint limits possible grammars to those in which there are no disparities of canonical pattern between underlying and surface representations. This would exclude the phonological solution as a permissible analysis of the Maori data.

Halle (1978), although he rejects this constraint on the basis of work by Kaye (1975), incorporates a similar observation into a proposal for the Maori problem based on a form of the evaluation metric. He points out that the phonological analysis, in return for an account of just two morphemes, requires the deletion rule (18) and the loss of the potential generalization that underlying representations must end in vowels as do surface ones. Against this requirement, the theory weighs the cost of a set of readjustment rules. Although Halle proposes no explicit procedure for evaluating readjustment rules, he suggests that the outcome will favor the morphological solution as more highly valued.

It will emerge that the evaluation metric is in fact the appropriate vehicle for the selection of the morphological analysis; nevertheless, the considerations cited by Halle as militating against the phonological analysis are not persuasive. A number of phonological analyses have appeared in recent work (Halle and Vergnaud, 1978; Kiparsky, 1979a; Lowenstamm, 1978) where surface constraints on canonical form, stated in terms of syllable structure, are apparently violated in underlying representation. Deletion rules like (18) or rules of epenthesis or vocalization bring these underlying representations into conformity with the required canonical pattern in an exact parallel to the phonological solution for Maori.[2] Furthermore, even though I proposed an explicit evaluation procedure for readjustment rules, the phonological solution is apparently still more highly valued. This evaluation procedure could, of course, be simply incorrect, but it already values

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(21)

$p \rightarrow \emptyset / \_\_\_$

Rule (21) mus

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(22)

a n + i a #
1 2 3 4 —

But V*n*+*ia*, w
passive *hokon*
(22).

(23)

$i \rightarrow \emptyset / n + \_$

readjustment rules more highly than many other imaginable procedures do. In other words, readjustment rules are probably already being treated as liberally as possible if any phonological rules at all are to be permitted.

Much additional data of direct relevance to this problem can be found in Hale (1968). Some consideration of the further complications necessary in any observationally adequate grammar shows that the evaluation metric, with its means of treating readjustment rules, does provide the language learner with the correct choice between the two competing solutions.

First, there exist passive verb forms ending in V*a* with corresponding gerundives in V*ŋa*, instead of the usual C*ia* and C*aŋa* seen in (17): *patu* 'strike,' passive *patua*, gerundive *patuŋa*. These can be analyzed as underlying vowel-final stems like /patu/ with suffix vowel deletion in hiatus:

(20)

$$V \rightarrow \emptyset \,/\, V + \_\_ \begin{bmatrix} a \\ ŋa \end{bmatrix} \#$$

But other verbs do show V*ia* passives and V*aŋa* gerundives: *noho* 'sit,' *nohoia*, *nohoaŋa*. Hale (1968), observing that there are no passives in *pia* or gerundives in *paŋa*, proposes underlying *p*-final stems like /nohop/, with the *p* deleted before the suffixes:

(21)

$$p \rightarrow \emptyset \,/\, \_\_ + \begin{bmatrix} ia \\ aŋa \end{bmatrix} \#$$

Rule (21) must crucially follow rule (20), in counterfeeding order.

Second, putative *n*-final stems undergo several additional phonological processes. Underlying *an*+*ia* is realized as *aina*, as in *tua* 'fell,' passive *tuaina*. This alternation can be attributed to a metathesis rule:

(22)

a n + i a #
1 2 3 4 →132∅4

But V*n*+*ia*, where V is not *a*, simply loses the vowel *i*: *hoko* 'buy-sell,' passive *hokona*. This requires another vowel deletion rule ordered after (22).

(23)

$$i \rightarrow \emptyset \,/\, n + \_\_ \; a \; \#$$

Third, a slightly different complication emerges in the case of some final-*ŋ* verbs. One class simply suffixes *-ia* in the passive: *tohu* 'point out,' passive *tohuŋia*. But another, smaller class deletes the *i* of the passive ending: *kai* 'eat,' passive *kaiŋa*. This second class demands still another phonological rule:

(24)
$i \rightarrow \emptyset \; / \; ŋ + \_\_\_\_ \; a \; \#$

Conceivably (23) and (24) could be collapsed, though one would probably wish to exclude this possibility in view of the fact that the former is exceptionless and the latter is under heavy lexical government.

Fourth, a different complication arises in the case of gerundives of *n*- and *ŋ*-final stems. They show up with the termination *ŋa* rather than the expected *naŋa* and *ŋaŋa* (Biggs, 1961, p. 34). This haplology can be formulated as a rather complex deletion process:

(25)
$$\begin{bmatrix} +\text{nas} \\ \begin{bmatrix} +\text{cor} \\ -\text{ant} \end{bmatrix} \end{bmatrix} + \text{a ŋ a}$$
$$1 \qquad 2\,3\,4 \rightarrow \emptyset\emptyset 34$$

A fully articulated phonological solution for Maori passives and gerundives is rather more complex than originally suspected. On the other hand, these additional facts do not excessively complicate the morphological solution. They will require five new conjugation types for the passive form and two new types for the gerundive. The full morphological solution demands the morpheme sets {*-tia, -hia, -kia, -mia, -ria, -a, -ia, -ina, -na, -ŋa*} and {*-taŋa, -haŋa, -kaŋa, -maŋa, -raŋa, -aŋa, -ŋa*} in the lexicon, the readjustment rules in (19), and the new readjustment rules in (26):

(26)
a. -tia → -a
b. -tia → -ia
   -taŋa → -aŋa
c. -tia → -ina
d. -tia → -na
e. -tia → -ŋa
f. -taŋa → -ŋa

The basic distinction between the two solutions that emerges is that whereas the phonological treatment deals with the complex suffix allomorphy by the various phonological rules in (20) through (25), the morphological one simply adds further readjustment rules of exactly the same formal type.[3] The only difference between the readjustment rules in (19) and (26) is that the latter do not show the parallel allomorphy of passive and gerundive observed in the former. This is actually expected under the conception of readjustment rules followed here, an idea I will return to.

It is possible at this point, though not very revealing, to pursue the result the evaluation metric will reach when confronted with the choice between observationally adequate phonological and morphological solutions along the lines I have sketched. The translation of the informal rules (20) through (25) into conventional formalism by the familiar canons of generative phonology yields a total of approximately 49 feature specifications, to which must be added the cost of the deletion rule (18) and the unformulated rule to handle the occurrence of *-tia* passives in derived verb forms. Evaluating the morphological rule system by counting a stipulation of a morpheme as the equivalent of a single feature yields a total of 30 feature specifications. Consideration of the data cited in note 3 will serve only to widen this disparity between the two analyses. The more highly valued solution under the assumptions made here is, consequently, the morphological one, a result that concurs with the one drawn by Hale (1973) from the nonprimary data.[4]

We should not, however, take away with us from Maori the moral merely that such a choice can be made by a rather sterile numerology based on arguable claims about the form of the evaluation metric. This example is of far greater value for what it illustrates about the system of tradeoffs between a morphological and a phonological treatment of some observed set of alternations. Two readily apparent characteristics of the primary data will immediately tend to favor a morphological solution. First, if, as Halle (1978) observed for Maori, only a small number of morphemes participate in the same alternation, then readjustment rules will be the appropriate means of expressing the generalization. Readjustment rules are disfavored to the extent that they need to mention explicitly many different morphemes that could alternatively be collapsed into a single phonological environment. Second, readjustment rules will be preferred in systems where there is relatively greater allomorphy, greater divergence in form between different occurrences of the same morpheme. One need not refer this preference to

underlying representations; it suffices to observe that the same morphological category is marked in several widely disparate ways on the surface.

The point, then, of the elaboration of the evaluation metric and its mode of application to readjustment rules proposed here is to provide a formal means of considering these two properties, where the properties and their inverses may be seen as tendencies toward the selection of a morphological or phonological analysis. This proposal correctly reflects the facts of Maori and, I believe, a widely held descriptive practice in generative phonology.

Spanish Epenthesis

It is well known that Spanish does not tolerate word-initial *s*C clusters, but the proper mode of incorporating this generalization into the grammar is not entirely clear. One view is that there exist underlying representations with initial *s*C clusters which are subject to an exceptionless rule of vowel epenthesis, yielding surface *es*C (Harris, 1969, 1979). Thus, *escala* might have the underlying representation /skala/, and the grammar of Spanish would contain something like the rule in (27).

(27)

$\emptyset \rightarrow e \; / \; \# \_\_\_\_ s \; [+cons]$

Very likely, rule (27) should be recast in terms of a theory of the interaction of syllable structure and segmental processes like that mentioned in connection with the constraints on canonical form in Maori. This move, however, would not affect the argument advanced here, since it would still be necessary to stipulate that the particular vowel *e* is inserted, perhaps by mapping it onto a slot in a syllabic template (Harris, 1980).

Another possible analysis holds that *s*C clusters are ruled out entirely by a constraint on well-formed underlying representations, stated crudely as (28):

(28)

*#sC

Under this analysis *escala* has the underlying representation /eskala/, and a hypothetical underlying representation like /skala/ is blocked by (28).

Although these two analyses are based on rather different conceptions of the problem, both are possible within the generative phonological theory followed here. I take it as given that rules of vowel epenthesis, responsive to certain types of consonant clusters or syllable configurations, must be recognized by linguistic theory. Constraints on well-formed morphemes or syllables appear to be needed as well. Thus these are two of the possible analyses given by phonological theory, and both so far appear to be adequate accounts of the familiar data.

Ordinarily, primary linguistic data in the form of morphophonemic regularities are sufficient to decide between a rule-based and a constraint-based phonological analysis solely on grounds of observational adequacy. Some morphophonemic evidence from Spanish supports the epenthesis rule in (27) over the constraint in (28).

First, there are *e*/∅ alternations like *escribir*/*suscribir*, although for morphological reasons such alternations are apparently confined to compounding and derivation and do not appear in more transparent inflectional processes. Second, Harris (1970) notes that the irregular final stress of *estóy, estás,* and *está* can be accounted for if these forms are monosyllables at the point in the derivation when the stress rule applies. In fact, this stress pattern reflects a larger distributional regularity pointed out to me by Harris: with the sole exception of the demonstrative *éste*, no word has stressed *é* in the context # ___ *s*C, which follows immediately from ordering epenthesis after stress assignment. Finally, Harris (1979) observes that selection of the *-ecit* allomorph of the diminutive suffix, which is restricted to a phonologically defined subset of words with disyllabic nondiminutives, presupposes an underlying disyllabic base word like /studyo/ for forms like *estudio,* diminutive *estudiecito*. So the process of diminutive formation must have access to a level of representation before epenthesis.

It is a matter for careful judgment to determine whether these facts would be available to motivate the language learner's rejection of the constraint analysis as observationally inadequate. On this determination hinges the question of whether we have here an authentic projection puzzle of the sort I described in the introduction. I would claim that the puzzle is authentic, although it is difficult to construct a rigorous argument for this position. The putative learning of epenthesis solely from the data given here must contend with the following difficulties. The evidence of *e*/∅ alternations and of diminutive formation is relatively obscured to the language learner by the lack of inflectional alternations in the first case and by additional phonological complica-

tions of diminutive allomorphy in the second. The apparently irregular final stress, confined as it is to three forms, is not compelling, nor is the distributional gap of *#ésC*, since it is not without exception. In the latter case, inference from lexical distribution also raises the problem of the learner's imperfect knowledge of the dictionary, discussed earlier in connection with spontaneous denominal verbs in Maori.

All of these considerations suggest that the choice between a rule and a constraint in Spanish cannot be made solely from the primary data on grounds of observational adequacy. This is not to say that this evidence is without significance. It is of obvious value to the investigator as reflecting the state attained in adult grammars, and it is clear that an account of these regularities confirms for the learner the choice of the epenthesis analysis made, as I will show, by the evaluation metric.

Before pursuing the Spanish case, let us consider the following example from English where the question of a rule or a constraint is completely uncontroversial. English obviously does not permit word-initial *pt* clusters in surface representations. By analogy to Spanish, two different accounts of this observation are possible. The constraint would rule out the sequence *#pt*, probably as a part of a more general set of constraints on possible syllables in English. The phonological rule, on the other hand, would delete word-initial *p* before a nonsonorant consonant. The first grammar would block underlying representations with word-initial *pt;* the second would transform them to surface initial *t*. Clearly the conclusion of any investigator would be that the constraint analysis is the correct one. This conclusion would undoubtedly persist in the face of apparent support for the rule analysis from alternations like *pterodactyl, pterogoid / helicopter, hymenoptera, archeopteryx*. It is unlikely that one could profitably pursue this hypothetical phonological deletion process.

By comparing the parallel cases of Spanish and English, one can see why there might be some doubt as to the correct analysis in the first case but none in the second. Marginal alternations aside, a *p*-deletion rule in English is a needless complication of the grammar, since no underlying representations that have themselves not been needlessly complicated with initial *p*'s before *t* would ever be subject to it. In other words, the choice between these two essentially observationally adequate analyses of English can be made on the basis of a form of the evaluation metric. This choice is possible, however, only if the evaluation metric considers the set of rules in conjunction with its lexicon, as I have proposed and as in Chomsky and Halle (1968).

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The evaluation metric will reach the opposite choice for the correct analysis of the Spanish phenomenon, however. Eliminating putative epenthetic *e*'s from underlying representations actually involves a simplification of the whole grammar—a general reduction in the number of phonological feature specifications in the lexicon. The slight complication that comes of having an *e*-epenthesis rule will be more than offset by the removal of all initial *e*'s before *s*C in the list of morphemes. Therefore, since any lexical entry of the form /esC . . ./ constitutes a needless complication of the grammar under this analysis, all lexical entries that meet the structural description of (27) must take the free ride with epenthesis. The unique exception to this generalization is *éste*, where there is direct evidence to the contrary from the observed initial stress.

It follows, then, that the application of the evaluation metric to sets of rules with their concomitant lexicons serves two purposes in this case: it selects a rule-based over a constraint-based analysis of Spanish, and it determines that all forms with surface *#es*C have underlying representations without the *e*.

Substantial empirical support can be found for the first claim, both from the evidence of alternations, stress, and diminutive formation already cited and from the treatment of loan words. Loans into Spanish invariably receive *e* if they have initial *s*C clusters in the source language: *esnob, esmoking, esprey*. The constraint analysis predicts in this case only that, say, *snob* is not a possible word of Spanish; it does not indicate how this form can be modified to make it pronounceable and thus fails to anticipate the systematic appearance of *e* before *s*C. A constraint like the one in (28) would allow ad hoc means of dealing with these clusters, either by insertion of some other vowel, by insertion of a vowel after *s*, or by deletion of one of the consonants in the offending cluster. The epenthesis analysis predicts regular insertion of *e* and nothing else; it is therefore confirmed by these observations.[5]

The second result—that all forms with surface *#es*C (except *éste*) will be analyzed as underlying /sC . . ./ and so will take the free ride offered by epenthesis—is much more difficult to support, though the question remains in principle an empirical one. It could be partly tested by inspection of the diminutives of all forms that have surface *#es*C followed by two syllables in the base word and that also meet the other phonological conditions for selection of the *-ecit* allomorph. The predicted outcome is that these forms will invariably have *-ecit* diminu-

tives, whereas without the strict free ride demanded by the evaluation metric they would potentially differ arbitrarily in their diminutive allomorphy.

Minimally, what has emerged here is that the learning of phonological and morphological rules and representations is a good deal more complex than envisaged in the trivial analog-to-digital acquisition procedure I described earlier. An adequate phonological theory must clearly contend with an extreme lack of primary data or with apparently crucial evidence that is unavailable to the language learner. More specifically, a particular form of the evaluation metric has been supported as a part of linguistic theory with direct application to the problem of language learning. This metric, like its counterpart in Chomsky and Halle (1968), must evaluate entire grammars on the basis of specific criteria of formal simplicity in the domains of phonological processes, readjustment rules, and the lexicon.

Notes

I am grateful to Lee Baker, James Harris, and Jonathan Kaye for their insights into the problems discussed here. As usual, all errors are my responsibility alone.

1. Another stress shift solution, similar in formal value to the one based on the foot structure in (4), is possible. Briefly, it involves assigning penultimate stress in all forms and then shifting stress, by relabeling or restructuring rules, to a light antepenult off of a light penult. It is apparent that this analysis would encounter the same problems with the data in (15) as the analysis in (4) does.

2. A certain amount of evidence internal to the phonological solution for Maori supports such a syllabic analysis of the final consonant deletion rule. First, Maori lacks syllable-final consonants as well as word-final consonants in surface forms. Second, syllable-final C/∅ alternations are attested in reduplicated forms. Consider the verb *koorero* 'speak,' passive *koorerotia*. Frequentative reduplication, which apparently copies the last two syllables of the stem, yields *koorerorero* 'chatter incessantly,' passive *koorerorerotia*. Application of this reduplication rule to the underlying form /koorerot/ should yield **koorerotrero* after word-final consonant deletion versus the correct *koorerorero*

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if all syllable-final consonants are deleted. Of course, complicating the reduplication rule so as to copy up to four segments leftward, starting at the second last one, would avoid the problem. But this sort of internal reduplication seems at best unusual, a property that follows from the theory of morphology presented in McCarthy (1979a). Notice too that reduplicated forms engender no difficulties under the morphological analysis.

3. There are a few additional facts that would require further complications of the phonological solution with relatively little corresponding elaboration of the morphological one. First, two verbs apparently form a passive/gerundive conjugation distinct from all others: *heu* 'separate,' passive *heuea*, gerundive *heueŋa*; and *keu* 'move,' passive *keuea*, gerundive *keueŋa*. Second, Hale has pointed out in class lectures that under some conditions the passive suffix attracts stress anomalously. Under either solution both facts can be dealt with in ways similar to those treated in the text.

4. There is a hidden assumption partly underlying this argument. Here the evaluation metric compares two solutions, each of which treats the passive and gerundive homogeneously by phonological rules or readjustment rules. One could as well imagine a whole family of mixed solutions, where, say, the irregularities in (20) through (25) are spelled out by readjustment rules but the more regular alternations in (17) are dealt with phonologically. It is by no means clear whether such a solution could actually be made to work or, if so, whether it would be more or less highly valued than the alternatives. In any case, I suggest that such a possibility—the differential treatment of morphology of the same general type—should be ruled out in principle. A more precise formulation of this constraint will have to await further study of systems of the Maori type.

5. Hooper (1976) presents an analysis of this Spanish phenomenon based on syllable well-formedness constraints that is claimed to handle the loan word data without recourse to a rule of epenthesis. The position of the inserted vowel, before the *s* rather than after it, is recoverable in this system by a preference for allowing the original order of the consonants to remain the same as in the source language. In fact, the order of the consonants is unchanged if a vowel is inserted in either position; it is rather the arrangement of consonant clusters in the form that is maintained in borrowing. It is clear that such a principle of loan phonology cannot be universal; for example, it is regularly violated in

Japanese and in English. Yet a language learner could discover a language-particular rule specific to loan phonology only by having access to pairs of source language/target language forms displaying the relevant alternation.

The quality of the inserted vowel is deduced in this system from the universal principle that it must be the lowest vowel on a language-particular strength hierarchy. The strength hierarchy can, in general, be discovered by examining synchronic and diachronic vowel reduction rules. The evidence given by Hooper for placing *e* lower on the Spanish strength scale than the other nonround vowels is limited solely to two diachronic reduction rules and no synchronic data.

In sum, Hooper's analysis eschews an epenthesis rule in Spanish only at the cost of two language-particular devices that cannot be discovered except from data that are clearly not available to language learners.

Chapter 8
Comments

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Chapter 8

Comments

Jonathan Derek Kaye

Good articles are all alike; every bad article is bad in its own way. Paraphrasing Tolstoy somewhat, I wish to draw attention to my unhappy task: to comment on a good article, or at least an article with which I agree in all important respects. In such cases one is reduced to quibbling or discussing one's own work. I shall do a bit of both.

McCarthy discusses the role of the evaluation metric in the acquisition of phonology. He outlines the position of this metric in the organization of an acquisition model. This model with its accompanying metric has roughly the form of that proposed in Chomsky and Halle's *Sound Pattern of English* (1968). McCarthy addresses two questions: (1) Is an evaluation metric essential to linguistic theory? (2) If it is, what form should it take? These days, his positive response to the first question is neither unexpected nor controversial. The more interesting question concerns the form of this metric. McCarthy presents the *Sound Pattern of English* model, with some interesting discussion involving readjustment rules and the role of morphological features in evaluating grammars. The remainder of his paper is devoted to the discussion of four sample analyses. In these four cases he shows convincingly that the proposed evaluation metric obliges one to choose a descriptively adequate grammar (or portion thereof) over one that is merely observationally adequate. He shows further that the evidence that leads ultimately to this conclusion—that is, that the grammar selected is indeed the descriptively adequate one—is based on facts that are presumably unavailable to the language learner. The moral of all this is that, yes, there is an evaluation metric and in the form presented by McCarthy it yields the right results in a variety of interesting examples.

With all of this I am in wholehearted agreement, and so I can begin with my quibbles. The model presented by McCarthy distinguishes, on the one hand, a discovery procedure which furnishes a set of observationally adequate grammars and, on the other, an evaluation metric which selects the observationally adequate grammar that most closely models the knowledge acquired by the speaker. I am not convinced that there are two distinct entities here.

Local and Global Uses of the Evaluation Metric

The evaluation metric resolves two sorts of problems. First, it constrains us to seek the most general solution compatible with the available data. I call this the *local* use of the evaluation metric.

Consider the case of a language lacking a labial series of consonants and having a rule of syllable- or word-final devoicing. I assume that the correct representation for such a process in such a language is (1) and not (2).

(1)

$$\begin{bmatrix}-\text{syll}\\-\text{son}\end{bmatrix} \rightarrow [-\text{voiced}] \,/\, \_\_\#$$

(2)

$$\begin{bmatrix}-\text{syll}\\-\text{son}\\\begin{Bmatrix}+\text{cor}\\-\text{ant}\end{Bmatrix}\end{bmatrix} \rightarrow [-\text{voiced}] \,/\, \_\_\#$$

In (1) it is proposed that any nonsonorant will undergo devoicing. Labials (which do not exist in this hypothetical language) are exempted from undergoing rule (2). Leaving aside the doubtful status of curly brackets, I assume that rule (1) is selected on the basis of having two fewer features, all else being equal in this case. The empirical consequences of such a choice are not immediately obvious. The implications become clearer when it is looked at from a slightly different angle.

A language learner acquiring Polish, a language with a final devoicing rule, is confronted with a form *xlep* 'bread' and a paradigmatically related form *xleba*. Many possible "grammars" are compatible with these data as shown in (3).

(3)

a.

$$\begin{bmatrix}-\text{syll}\\-\text{son}\\-\text{cont}\\+\text{ant}\\-\text{cor}\end{bmatrix} \rightarrow [-\text{voiced}] \,/\, \_\_\#$$

b.

$$\begin{bmatrix}-\text{syll}\\-\text{son}\\-\text{cont}\\+\text{ant}\end{bmatrix} \rightarrow [-\text{voiced}] \,/\, \_\_\#$$

c.

$$\begin{bmatrix}-\text{syll}\\-\text{son}\\-\text{cont}\end{bmatrix} \rightarrow [-\text{voiced}] \,/\, \_\_\#$$

d.

$$\begin{bmatrix}-\text{syll}\\-\text{son}\end{bmatrix} \rightarrow [$$

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In (3a) the devoicing rule is limited to labial stops, in (3b) to labial and dental stops, in (3c) to any stop, in (3d) to any obstruent. And of course many other possibilities exist. Given the model presented by McCarthy, the discovery procedure will make available "grammars" (3a–d), along with many others, to the learner. The evaluation metric will then select the most highly valued one, namely (3d), and all is well. In my view the evaluation metric forms part of the discovery procedure in the following sense: potential grammars are entertained in an ascending order of complexity as determined by the evaluation metric. The learner accepts the first grammar compatible with the available data. It follows that this will always be the most highly valued grammar.

McCarthy's discussion of Expletive Infixation in English provides a structurally identical use of the evaluation metric. McCarthy notes that expletives may be infixed at foot boundaries. He proposes two observationally adequate analyses: one contains a partial environment requiring a stressed syllable to immediately precede the infixation site; the second would be identical except for the lack of this stipulation regarding the stressed syllable. With the local application of the evaluation metric, the learner would always adopt the latter solution: the solution without the additional stipulation.

Integrating the evaluation metric into the discovery procedure yields a number of conceptually satisfying results. We see how descriptively adequate grammars may be attained based on ridiculously small and hopelessly skewed bodies of data. It becomes clear how rules that are not "surface true" in the sense of Natural Generative Grammarians may be acquired in the face of apparently contradictory data. The role of positive versus negative data is seen here. A learner having acquired a highly valued grammar, or portion thereof, may be expected to cling tenaciously to it, seeking alternative treatments of recalcitrant facts not involving the abandonment of the original hypothesis. Determining what conditions may force the learner to abandon this first hypothesis constitutes an extremely interesting research problem for acquisition models.

A second role of the evaluation measure as seen in McCarthy's examples involves ranking entire analyses—in other words, comparing two solutions to a given problem, solutions that may involve different rules, different underlying representations, and different lexical constraints. I will call such cases *global* uses of the evaluation metric. I shall argue that all such global uses are illicit and that the selection is made by other means.

McCarthy's discussion of the prothetic *e* in Spanish provides the first example of a global use of the evaluation metric. He notes that there are at least two ways of dealing with the prothetic *e* found in words lke *escala* and *escribir*.

(4)
Rule: $\emptyset \rightarrow$ e / # ___s [+cons]
Underlying forms: /skala/, /skrib+ir/

(5)
Underlying form constraint: *#sC
Underlying forms: /eskala/, /eskrib+ir/

McCarthy argues that solution (4) is favored over solution (5) by the evaluation metric. The small cost of the rule of prothesis is more than made up for by the elimination of all initial *e*'s preceding *s*C clusters in the lexicon. In terms of my suggested model, it is a little difficult to imagine solutions (4) and (5) being part of an ordered list of available solutions. It would seem that if McCarthy's assessment of the situation is correct—that is, if solutions (4) and (5) are indeed available to the learner—the evaluation metric must be considered independent of the discovery procedure. In fact, I suggest that solution (5) is not available to the learner and that solution (4) is incorrect as it stands.

McCarthy's discussion of the Maori case involves a similar use of the evaluation metric. In that case also I will argue that the solution rejected by the evaluation metric is not a possible phonological analysis and hence not available to the learner. Not the evaluation metric but, rather, the theory of generative phonology is at work here. It would be interesting if all such global uses of the evaluation metric could be eliminated by further refinements of the theory.

Given the current direction of research in phonology, in particular the work of Kean (1975; 1979), Kiparsky (1979), Halle and Vergnaud (1978), and Kaye and Lowenstamm (1980; to appear), a richer, more highly constrained theory of phonology is developing. Much of the work once done by the evaluation metric is now accomplished by the reduction in the number of analyses compatible with any reasonably sized data base. Such a consequence is particularly clear in the recent work on markedness, both segmental and syllabic, and in the emergence of the theory of metrical phonology. Such questions are not essential to McCarthy's point. They do, however, have an impact on the role of the evaluation metric within a grammar. To illustrate this point I turn now to a discussion of McCarthy's remaining examples.

Phonological Theory and the Role of the Evaluation Metric

I have suggested that solution (5) for the prothetic *e* in Spanish is not a possible solution within the current theory of generative phonology. If I am correct, what is at work in this example is not the evaluation metric but the refinement of phonological theory. Let us look at the example more closely. What nature of beast is the constraint *#*sC* in Spanish? It is clearly not a morpheme structure constraint. Morphemes with initial *s*C clusters abound in Spanish: *su+scribir, con+stitucion*. It is for this reason that the # boundary appears in the expression of the constraint. In the light of theoretical developments in phonology, all (or at least most) cases of word structure constraints are seen as constraints on syllable structure. This is true in the case of Spanish. Following this line, the type of constraint in (5) is not available to the learner. Further, all the facts of Spanish prothesis follow as a consequence of the fact that *s*C is not a possible onset in Spanish. The alternative to solution (5) is, as I shall show, without cost.

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In a theory of syllable structure along the lines of that proposed by Kaye and Lowenstamm (1980; to appear), syllable constraints and syllable structure are expressed at the level of lexical representation. A morpheme like *scrib* has the following representation:

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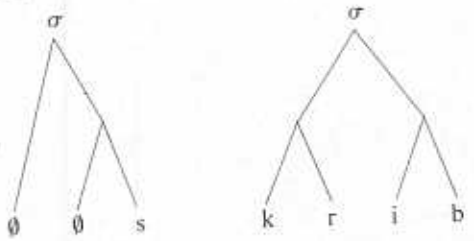

This morpheme consists of two syllables. An initial syllable is of the form CVC, but the onset and nucleus are null elements; only the coda has a segmental representation. The second syllable, *krib*, is a garden variety CCVC. Resyllabification takes place when morphemes are juxtaposed. The guiding principle of this enterprise is to resyllabify, eliminating nulls where this does not violate the syllable constraints of the language.

Consider now the derivation of the forms *escribir, suscribir* (the underlying form of the prefix is of no concern here).

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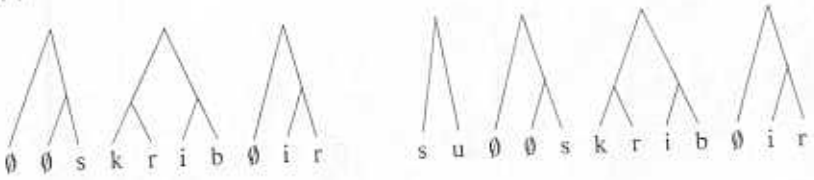
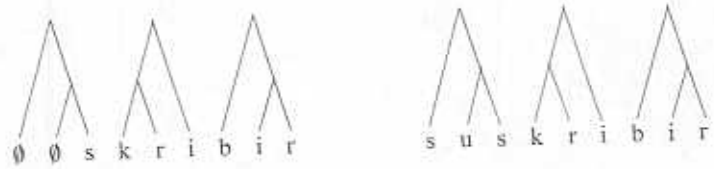


Resyllabification:



Spelling rule:

(?)eskribir

In (7) I give the underlying representations of the two forms in question including the syllable structure of their constituent morphemes. After resyllabification, all of the null elements have been eliminated from the representation of *suscribir* and nothing further need be said. As for *escribir*, two null elements remain. All that is needed is to posit a spelling rule to specify how the null elements are to appear in the Spanish surface forms: in the case of the null

onset, one may hear a glottal stop; the null nucleus is spelled out as *e*. Spelling rules may be viewed as parameter fixing. The unmarked case appears to be the elimination of nulls from phonological representation. The sole aspect peculiar to Spanish is how the nulls are to be spelled out. There is no rule of prothesis in Spanish. The work is done by a universal process of resyllabification, which constitutes part of phonological theory.

It is quite possible that these facts can be handled by another theory of syllable structure than the one followed here (Kaye and Lowenstamm, 1980; to appear). The key point is that Spanish prothesis should, in any adequate account, follow from the fact that *s*C onsets are illicit in Spanish—a fact that exists independently of the choice of analyses and accordingly has no cost. The Spanish example demonstrates clearly how the enrichment of phonological theory restricts the available options and hence reduces the work of the evaluation metric.

Maori provides an amusing example of theoretical overkill. What started out as a problem for generative phonology—why are Maori children "smarter" than phonologists?—is now an *embarras de richesses* of sound reasons for rejecting an apparently highly valued phonological solution. McCarthy shows that once all the facts are considered, an evaluation metric of the *Sound Pattern of English* type imposes what Hale has shown to be the correct solution. I will not review the facts. They are quite ably presented by McCarthy. I wish only to point out that the fact that the bad solution would be ruled out by the evaluation metric does not mean that the evaluation metric was the hatchet man in this case. In Kaye (1975) I presented another explanation for the Maori case. What is attractive about that proposal is that it unifies the explanation of the facts of Maori with a similar case in French (Kaye and Morin, 1978; Morin and Kaye, to appear). I am not claiming my approach is the correct one, only that the issue is far from settled. If my claim concerning the role of the evaluation metric as an arranger of possible phonologies is correct, it would be another reason not to close the books on the Maori case.

Cairene Arabic stress involves the theory of metrical phonology of which McCarthy is one of the pioneers. The novelty of this theory precludes detailed discussion of the question of evaluating competing analyses. I will assume that everything McCarthy has said on the matter is correct. There are then two analyses, one involving a rule of foot formation plus either a relabeling or a restructuring rule, the other involving a rule of foot formation only. It seems that this is not a case of global evaluation but, rather, is part of the role of the metric in providing the learner with an ordered set of grammars compatible with an array of data. This is what I have called the local use of the evaluation metric. I would assume that phonologies containing only rules of foot formation (such grammars presumably being ranked among themselves by a theory of markedness) would appear before all grammars containing complicating factors such as relabeling and restructuring rules. The Cairene Arabic case then reduces to a purely local use of the evaluation metric. It resembles the expletive infixation and final devoicing examples.

I have shown that the evaluation metric operates in a number of cases at a purely local level. It provides a strategy of selection for the learner. If grammar

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kitaye:kkos
kitine:ntam

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A is properly included in grammar B, grammar A will be entertained before grammar B. I use "properly included" in the obvious sense: one grammar wholly includes another, with something to spare. This notion is to be refined to cover such cases as Cairene Arabic where cross-analytic rules of foot formation are assumed to have the same status. It does not preclude an eventual ranking of the foot-formation rules themselves.

Global uses of the evaluation metric, feature-counting of noncommensurate analyses, is thus excluded. What I have in mind is that all decisions involving noncommensurate analyses will be decided by the phonological theory. Put another way, for any array of data, noncommensurate analyses will not be entertained by the learner. Only concentric grammars of increasing complexity will be involved in the decision procedure.

Let me give a possible counterexample: two noncommensurate analyses both of which are possible portions of a phonology. Consider the following forms from Ojibwa:

(8)

a. nima:ca:	'I leave'	ma:ca:	'she leaves'
nitakoššin	'I arrive'	takoššin	'she arrives'
kipakis	'you swim'	pakiso	'she swims'
kiwi:sin	'you eat'	wi:sini	'she eats'
b. nitanokki:	'I work'	anokki:	'she works'
nita:kkos	'I am sick'	a:kkosi	'she is sick'
kitaye:kkos	'you are tired'	aye:kkosi	'she is tired'
kitine:ntam	'you think'	ine:ntam	'she thinks'

Analysis of these data quickly reveals two prefixes: *ni~nit* '1st pers,' *ki~kit* '2nd pers' (in nouns one also finds *o~ot* '3rd pers' in the same circumstances); *ni* and *ki* appear before consonant-initial stems, *nit* and *kit* before vowel-initial stems. Two possible analyses can treat these data.

(9)
Underlying prefix forms: /ni/, /ki/, /o/
Rule:

$$\emptyset \rightarrow \begin{bmatrix} -\text{syll} \\ +\text{cons} \\ -\text{cont} \\ -\text{son} \\ +\text{cor} \\ +\text{ant} \end{bmatrix} / \underset{\text{prefix}}{\underline{\quad}}] + [+\text{syll}]$$

(10)
Underlying prefix forms: /nit/, /kit/, /ot/
Rule: $[+\text{seg}] \rightarrow \emptyset / \underset{\text{prefix}}{\underline{\quad}}] + [-\text{syll}]$

In (9) it is assumed that the *t* is epenthetic. Prefixes contain no final consonant in their underlying representation. A rule of epenthesis inserts *t* between a prefix and a following vowel-initial element. Solution (10) posits a series of prefixes all ending in *-t*. A deletion rule will erase the *t* before a consonant.

How is one to choose between the two solutions? Note first that neither rule is otherwise motivated in Ojibwa. Vowel sequences occurring elsewhere are not broken up by *t*. They undergo a truncation rule. The first member of an illicit consonant cluster is not generally deleted. Such a cluster is broken up by an epenthetic *i* or else the first consonant undergoes a dissimilation rule changing it to *h*. Whichever rule is adopted, (9) or (10), it will have to apply in a context limited to prefixes.

Solution (9) is a relatively costly rule of epenthesis. However, the underlying form of the prefixes contains no final consonant. Solution (10) is a simpler rule of consonant deletion, but with the expense of adding a final consonant to the lexical representation of the three prefixes. I can think of no additional evidence, internal or external, that would decide the issue. Counting features might favor solution (9)—the five additional features of the rule are more than offset by the three additional *t*'s in the underlying representations of solution (10). A redundancy rule stipulating that all prefixes end in *t* might narrow the difference between the two. My point is that I believe the Ojibwa case is just the sort of problem that an evaluation metric would not settle. Given the total lack of empirical consequences of such a decision, I can imagine that speakers of Ojibwa might adopt one or another solution in a more or less random manner. Perhaps some improved theory of markedness favoring, say, insertion rules over deletion rules may eventually decide the question. I wish to claim only that an appeal to the evaluation metric seems inappropriate in this case.

I have tried to show that the evaluation metric as discussed by McCarthy does indeed have a place in the theory of phonology. Given the recent advances in phonology that have sharply constrained the number of possible analyses compatible with an array of data, the role of this metric seems to be limited to presenting the learner with an ordered sequence of commensurate analyses. The evaluation metric, operating in concert with a theory of markedness and, of course, the now enriched notion of a possible phonology, constitutes a significant part of the discovery procedure.

Chapter 9
Strict Bounding

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in a model of grammar that cl
sense if one simply views a
describing the facts, as does

A second possibility is that
a part of universal grammar a
that children bring to bear ir
their language. Linguistic an
for example, posit output c
Chomsky and Lasnik, 1977) a
ous rules (Ross, 1967).

A nonlearnable linguistic ar
generalizations must, then, b
realist conception of gramma