

REVIEW ARTICLE

RECENT DEVELOPMENTS IN PHONOLOGICAL THEORY

Review of: J. B. Hooper, *An introduction to natural generative phonology**

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1. Introduction¹

H.'s book (a revised and expanded version of her UCLA dissertation (IULC 1973)) presents and elaborates a phonological theory which has been developed mainly by Vennemann in a number of articles published since 1972. The book is divided into two parts. The first part (*Concreteness in Morphophonology*) deals with "the formal constraints on the theory", i.e. it concentrates on the abstractness and actual form of underlying representations (henceforth UR's), rule types, cyclic applications, rule order and phonological change. It includes several illustrations from Spanish. Part two (*Natural Phonological Structure*) deals with "substantive phonological issues" and contains a discussion of the distinction between morpheme structure rules vs. phonetic-phonological rules (the so-called duplication problem) and especially of the role and formal treatment of the syllable.

In section 1 of this article some general characteristics of standard (i.e. transformational) generative phonology are presented, focusing on the problems that led to the earliest versions of natural generative phonology. Section 2 is entirely devoted to NGP proposals concerning synchronic phonology. In section 3 the NGP theory of phonological change is discussed and in section 4 a number of conclusions are offered.

1.1. Transformational generative phonology

Natural Generative Phonology (NGP) is intended to be an alternative to *Transformational Generative Phonology* (TGP) as elaborated in the sixties and standardized in Chomsky and Halle's *The Sound Pattern of English*

* Published by Academic Press, New York/London, 1976, xviii + 234 pp. \$18.50.

(SPE). Characterizing properties of TGP are the rejection of the 'autonomous' phonemic level and the nonexistence of a principled distinction between phonological rules with and without nonphonetic information. As to rule *types* TGP distinguishes:

- (1a) *morpheme structure rules* (or conditions), specifying redundant feature specifications within morpheme boundaries. They only *fill in* feature values.
- (1b) *phonological rules* operating mainly² beyond the morpheme level. They *change* binary feature values.
- (1c) *low level rules* having the same function as (b) but being somewhat more subtle in that they incorporate changes which are gradual rather than binary.

There are two other very important properties, viz. extrinsic rule ordering and the fact that there are no constraints on the abstractness (or arbitrariness) of UR's except that they must be formulated in terms of the same distinctive features that are used for the surface forms (the so-called *Naturalness Condition*; Postal 1968: 55ff). These two properties make it possible to describe the phonological component of a language as a system of rules without exceptions, while the surface forms of that language strongly suggest that there are exceptions (as much as they suggest that there are rules). Zonneveld (1978: 120) calls these properties 'pseudo' exception devices. On the other hand, TGP offers also more straightforward ways to deal with exceptions, viz. the use of diacritic features and/or readjustment rules (Zonneveld 1978: 126-142).

The first method of dealing with exceptions widely used in the early days of TGP, has come to be known as the 'diacritic use of phonological features'. Some lexical items that meet the SD of rule A but should not undergo it are given a UR that does not meet this SD. After A has applied the correct surface forms are generated by a rule B that applies to all occurrences of these lexical items, a rule of *absolute neutralization* (Kiparsky 1968). It is clear that this strategy crucially depends both on abstract UR's and on extrinsic rule ordering (and on the assumption that rules like rule B are allowed): rule B must be prevented from operating before rule A. Extrinsic ordering provides the means to do this.³ Kiparsky (1968) pointed out that this way of analyzing data should be heavily constrained and proposed his *Alternation Condition* (AC). In its strong form, this condition requires that UR's should not contain *segments* that do not occur in the

surface paradigm; in a weaker form it permits this only (though under 'cost') when the segment is referred to by more than one rule (Kiparsky 1971).⁴ Although most phonologists accepted Kiparsky's condition in some form, the debate went on concerning the exact formulation which would allow enough but not too much freedom in postulating UR's that are remote from the surface forms. Some possible and impossible views are discussed in Kenstowicz and Kisseberth (1977: 1-62). However, Vennemann observed that the theory including an AC, although it ruled out *abstract segments*, still allowed several possible analyses of one set of data, both abstract and concrete. Assuming that there should be a *unique analysis*, he tried to remedy this situation by constraining the remoteness of UR's *and* the ordering of rules. In 1971 Vennemann proposed the following conditions:

- (2)
- 1. The NO-ORDERING CONDITION (NOC): Rules of grammar cannot be extrinsically ordered
- 2. The STRONG NATURALNESS CONDITION (SNC):
 - part 1. Lexical representations of nonalternating parts of morphemes are identical to their phonetic representations.
 - part 2. Lexical representations of roots are identical to one of the radical 'allomorphs' of the paradigm, plus an (often empty) set of suppletion rules.
 (cf. Vennemann 1974: 346-347).

Since as a result of the SNC lexical items contain all phonetic information all phonological rules (including the MSR's) function as redundancy rules, becoming only 'active' if in the course of a derivation a violation occurs. As Vennemann (1974) points out, adoption of the SNC sometimes leads to the conclusion that more than one allomorph has to be listed in the lexicon. If two allomorphs A and B differ in two respects and neither A nor B is basic to both, there can be no one basic allomorph. Since the SNC forbids UR's to be blends of surface forms, Vennemann decides that all allomorphs must be stored in the lexicon. However, to solve additional problems,⁵ this lexicon should not contain roots and affixes but rather words.

H.'s version of NGP differs in several respects. At some points she returns to older TGP views, although she basically maintains the criticism underlying Vennemann's ideas.

2. The theory of NGP (Hooper's version)

2.1. The formal constraints on the theory

2.1.1. Introduction

H.'s first chapter introduces the abstractness problem as one of the key problems in TGP. H. discusses Kiparsky's AC and concludes (as Vennemann did) that even the strong version of the AC is not sufficient as a constraint on the abstractness of UR's. However, according to H., Vennemann's SNC is too strong because it leads to the consequence mentioned above (i.e. lexicon of words). To avoid this consequence H. must assume that UR's can sometimes be composed of feature values of surface forms.⁶ The correct strategy to arrive at a unique analysis consists, according to H., in constraining the liberty in the actual *formulation* of the rules that have to relate the UR's to the surface forms.⁷

H. thus abandons the SNC and proposes instead a *True Generalization Condition* (TGC) (p. 13). "It [the TGC] would require that all rules express transparent surface generalizations, generalizations that are true for all surface forms and that, furthermore, express the relation between surface forms in the most direct manner possible". Under this condition "no rule could refer to a nonexisting segment"⁸ (p. 14) and when rules cannot refer to nonexisting segments "there is certainly no point in having abstract phonological segments or features" (p. 20). Ergo, rules of absolute neutralization are excluded. Furthermore, this condition reveals the true nature of rules. Especially the distinction between purely phonetically conditioned rules (P-rules) and rules which also (or solely) refer to nonphonetic information such as morpho-syntactic or lexical information (morphophonemic rules or MP-rules) becomes very clear. The *P-rules* correspond to "the physical details of articulation" (p. 16), "can be predicted on universal principles" (id.) and are therefore claimed to be *natural rules* (p. 17). P-rules describe what happens when the sounds are actually produced and "they usually do not have exceptions" (p. 14). Rules that refer to a conditioning factor that is not present on the surface, or rules that are contradicted by surface forms are ruled out by the TGC. For example, a rule of nasalization (proposed in Schane 1968):

$$(3) V \rightarrow [+nas] / \text{---} [+nas] \left\{ \begin{matrix} \# \\ C \end{matrix} \right\}$$

cannot be a P-rule of French, because in [bäfrer] 'bon frère' there is no conditioning nasal and in [bänsce] 'bonne soeur' the rule is contradicted. *MP-rules* "take part in the sound-meaning correspondence of a language and are therefore language-specific" (p. 17), "phonologically quite arbitrary" (id.) and "likely to have exceptions" (id.). "There are also differences between P-rules and MP-rules in the way they behave historically" (id.). An advantage of a constraint on rules (instead of a revised constraint on UR's) is that UR's are not directly accessible. Under the TGC however the correctness of rules can be directly deduced from the surface facts.

Hooper develops the following view on the form of UR's. All phonetic information that is predictable by a purely phonetic rule that conforms to the TGC is left out of the UR. This means that the UR's are archisegmental.⁹ However, when the distribution of allomorphs is not purely phonetically motivated, they cannot be derived from one invariant UR (the 'item-and-process' model). To describe those kinds of alternations H. adopts a model introduced by Hudson (for example, 1974), which is basically of an 'item-and-arrangement' kind. I will discuss this model in section 2.1.2.3. In addition to the TGC H. retains Vennemann's NOC, because in some cases data can be analyzed in such a way that all rules are true generalizations, but that nonetheless the analysis is abstract in that nonexisting segments are postulated.¹⁰ Such a case will be discussed in section 2.1.2.2.

In chapter 2 H. compares TGP and NGP and she suggests that the choice between them can be made by observing linguistic change. The use of language change as (external) evidence started within TGP with Kiparsky (1968: 8): "the method, briefly, is this: examine changes that depend on linguistic structure and see what kind of structure they presuppose". In comparing the two theories, H. only incidentally follows this strategy (only explicitly in chapter 3), so the amount of external evidence she offers is rather limited. Recently Amastae (1978), Harris (1978) and Tiersma (1978) questioned the correctness of H.'s use of external evidence.

In the next three paragraphs I will discuss the core of H.'s proposals. First, I will examine the TGC in some detail and conclude that it is inaccurately defined. Second, I will argue that the NOC is not the right constraint to prohibit the kind of analyses mentioned above. Third, it will be demonstrated that the format that is used to describe morphophonemic alternations presupposes an additional naturalness condition.

2.1.2. Discussion

2.1.2.1. *When is a rule a true generalization?* To determine the context of H.'s TGC I will examine some of H.'s rules that are said to violate it or to be in agreement with it.

(i) On pp. 55–57 H. argues that sometimes a reformulation of rules which makes extrinsic rule ordering superfluous also leads to better formulations of those rules. The reason is that the reformulation adds “explanatory value to the rule”. The following example is a case in point: [kæŋt] ‘cannot’ can be derived from /kænt/ in two ways:

- (4)
- | | | |
|----|-------|--|
| I | (i) | $V \rightarrow [+nas] / _ [+nas] C_0 \$$ |
| | (ii) | $\left[\begin{smallmatrix} -syll \\ +nas \end{smallmatrix} \right] \rightarrow \emptyset / _ [-voice] \$$ |
| II | (i) | idem |
| | (ii') | $\left[\begin{smallmatrix} -syll \\ +nas \end{smallmatrix} \right] \rightarrow \emptyset / [+nas] _ [-voice] \$$ |

H. points to a connection between this reformulation of (ii) and the TGC.¹¹ However, she admits that rule (ii) does state a “valid” generalization. What then makes rule (ii') preferable to rule (ii)? Is it the fact that (ii') states what is actually going on at the surface while (ii) is an overgeneralization? As Crothers and Shibatani (1975: 526–529) point out, overgeneralization, which “exploits the nonexistence of certain segments or combinations to make a claim of seeming generality”, should be constrained in some way.¹²

As we will see in our discussion of the NOC, generalizations are also disallowed (cf. note 20) by H. This means that the TGC contains at least one extra subclause (or that there is still another condition on the formulation of rules).¹³

(ii) All segment structure rules are considered to be P-rules, because, according to H., they are true generalizations. Note, however, that segment structure rules (a) do not relate surface forms and (b) refer to nonexisting segments, viz. archisegments. It will appear below that this kind of rule can even refer to really fully specified abstract segments. The conclusion is, then, that one subclause of the TGC does not hold for all P-rules.

(iii) A rather strange effect of the TGC is this: synchronic deletion and

insertion rules are forbidden, “unless such insertions and deletions take place in environments created by derivation and not present in the lexicon” (p. 233). Therefore Spanish cannot have a rule like:

- (5) $\emptyset \rightarrow e / \# _ s [+cons]$

because in the position denoted by the bar of the rule we find other vowels as well, for example, [ospital], [istorya], etc., although, of course, these vowels are not inserted. This means that some rules, though truly phonetic, are barred from the set of P-rules.¹⁴ Now, it is true that rule (5) is in a way not true for all surface forms. But this is only so if our criteria for transparency are even more severe than those of Kiparsky (1976), who stated that a rule R of the form $A \rightarrow B/C _ D$ is opaque to the extent that there are surface representations of the form:

- (6)
- | | |
|------|--|
| (i) | A in environment C–D or |
| (ii) | a. B by rule R in environments other than C–D, or
b. B not by R in environment C–D. |

Kiparsky did not include a subclause of the form:

- (iii) CXD, where $X \neq B$.¹⁵

Rule (5) seems to be a violation of the TGC by case (iii). However, the TGC cannot simply be considered as a stronger version of the opacity condition¹⁶ because the TGC does not rule out cases like (iib) (the familiar rule of final devoicing is allowed).¹⁷

(iv) The TGC does not exclude some kinds of unnatural rules from the set of P-rules. An example of this will be discussed in section 3.2.3.

I think all this shows that the TGC is not accurately formulated by H. In my opinion, a reasonable constraint on P-rules should only include the opacity subclauses (i) and (iia). (iib) is certainly too strong: final devoicing in Dutch, a completely automatic rule, would be opaque because voiceless obstruents occur in UR's in final position. But even if a TGC were restricted to (i) and (iia) – negative as well as positive exceptions make a rule opaque – some remarks in Kiparsky (1975), where similar proposals are discussed, suggest that a constraint against P-rules that violate to a certain extend (i) and/or (iia) is too strong.

The set of all phonological generalizations that can be established for any given language ranges from rules for hardly perceptible low level adjustments, valid for the whole vocabulary, to rules for very crude replacement, valid only for a couple of lexical items. Within this set the TGC (whatever its proper formulation is) must isolate a particular subset that consists of rules that are statable in phonetic terms (phonological features and phonological boundaries; p. 14) and that (usually; id.) do not have exceptions. As compared to TGP the following rule types are included in this subset (cf. (1) above): low level rules, morpheme structure conditions (both for segments and for segment sequences) and a subset of the phonological rules, viz. those that conform to the TGC. In NGP these rule types are considered to be a homogeneous group. Though I agree that they indeed share common features, I will nevertheless question the correctness of this assumption in section 2.2.2.

Now the remainder of the whole set mentioned at the beginning is also considered to be a homogeneous group, though the only common feature of the rules that are included in this subset is that they do not belong to the first subset. However, the remaining subset still includes, on the one hand, fairly general rules that refer to the syntactic categorization of lexical items or happen to have a few exceptions and, on the other hand, rules that only apply to a few lexical items. Notice, for instance, that all linking rules in French must be included in the second subset, only because a few words 'act as if they began with a consonant' (cf. Klausenburger 1978). Kiparsky (1975: 194) writes: "What seems unmotivated is the assumption that rules which are opaque or have exceptions are *by that fact alone* to be treated as morphologically conditioned". Technically speaking a rule that has a few exceptions is not 100% transparent. But it seems unreasonable to treat such a rule in the same manner as a rule that only applies to a few forms. In other words, not all rules that are not completely productive are therefore completely unproductive. "[...] what many recent discussions [...] ignore is that productivity is traditionally and correctly viewed as a gradient phenomenon" (Kiparsky 1975: 195). Kiparsky then continues: "The strongest sense in which one might speak of a rule being productive is that any new word or formation which meets the structural analysis of the rule *must* undergo it. Another, weaker sense is that in which we speak of a rule as being productive if new words or formations *can* become subject to it, that is, if the scope of the rule is being extended in the language. I shall distinguish the two cases as *full* and *partial* productivity, respectively. (Note that since applicability to *new* items is made the

criterion, even fully productive rules need not be *automatic* in the usual sense of that term, for they can clearly have exceptions in the old vocabulary). The 'h aspiré' words in French seem to be a case in point. Several linking rules in French (e.g. *liaison* and *elision*) show full productivity, but have a number of exceptions in the old vocabulary (in addition to a number of other fixed exceptions and idiosyncrasies). It would be unreasonable to treat those rules as MP-rules. As an example of partial productivity Kiparsky mentions umlaut in German (*gast-gäste*), which has extended its domain considerably after the rule had become morphologized.

I draw two conclusions from this. First, we must allow rules that have exceptions to be included in the set of P-rules. It is clear that certain kinds of exceptions, particularly words in the 'old vocabulary', or words that are still felt as loans, do not turn phonetic rules into MP-rules. Of course, further study must be made of just how transparent a P-rule has to be, to be still acquired as a *feature changing* rule, that is, as a habit of articulation. Speakers may systematically pronounce a certain sequence of segments in a limited group of words and, at the same time, apply a P-rule, that seems to be productive, to an identical sequence in another group. We must also take into account ongoing sound changes which, because changes seem to be lexically diffuse, are reflected as synchronic P-rules with exceptions, at least during a certain period of time. Second, we must use degree of productivity as a criterion to make certain distinctions within the set of rules that are truly nonphonetic, rules that do not involve feature changing but rather segment substitution (cf. section 2.1.2.3). Only then will it be possible to handle the problem why certain rules fade and disappear leaving only minor changes (analogical leveling), while others stubbornly remain or even extend their domain of application (analogical extension).¹⁸ In short, it seems wrong to treat all rules that do not conform to a constraint as severe as the TGC as unique cases of suppletion. However, in section 2.1.2.3 we will see that this is just what H. has chosen for.

2.1.2.2. On the need of a No-Ordering Condition. According to H., not all abstractness can be excluded by only adopting a TGC. She presents one example involving rules that are said *not* to violate the TGC (though many more examples of this kind could be cited from the literature). The analysis in question is abstract and not unique, because one can think of another 'concrete' analysis (presented in Vennemann 1972a). It is 'abstract' in postulating segments that never appear on the surface. This is not the place to give the details of this analysis (see Hyman 1975: 87-90). The

crucial point is this. In Nupe, consonants palatalize and labialize before front and back vowels respectively. Before an [a], however, sometimes the labialized consonant emerges and sometimes the palatalized consonant (and sometimes a 'normal' consonant). In order to 'explain' this it is tempting to postulate beside underlying /a/ also /ε/ and /ə/ for surface [a], which segments are mapped onto [a] by a rule of absolute neutralization after the assimilation processes have taken place. The rules then are:

$$(7) [+cons] \rightarrow \left[\begin{array}{c} -back \\ +high \end{array} \right] / - \left[\begin{array}{c} V \\ -back \end{array} \right]$$

$$\left(c \rightarrow c^y / - \left\{ \begin{array}{c} i \\ e \\ \varepsilon \end{array} \right\} \right)$$

$$(8) [+cons] \rightarrow \left[\begin{array}{c} +round \\ +high \end{array} \right] / - \left[\begin{array}{c} V \\ +round \end{array} \right]$$

$$\left(c \rightarrow c^w / - \left\{ \begin{array}{c} u \\ o \\ \partial \end{array} \right\} \right)$$

$$(9) \left[\begin{array}{c} V \\ +low \end{array} \right] \rightarrow \left[\begin{array}{c} -round \\ +back \end{array} \right]$$

According to H., rule (9) represents a true generalization, since indeed all low vowels that appear on the surface are [-round, +back]. Compare this to H.'s statement on p. 14 (cited in section 2.1.1 of this review) that "no rule could refer to a nonexistent segment" under the TGC. Apparently, this statement only expressed wishful thinking. H. then suggests, that we need a No-Ordering Condition to prevent this type of analysis.¹⁹ There is, however, another way to do this.²⁰

Assuming that the rules stated above are permitted a completely different way to exclude the abstract Nupe analysis is to forbid the use that is made of rule (9). I claim that the use that is made of this rule is suspicious. It is a context-free rule that *changes* feature-values; in other words, a rule that leads to unrecoverable loss of information. Because context-free rules are necessary to specify the predictable value of segments, we cannot do without them. We can however require that *context-free rules may not change/feature values*, but only fill them in.

Recently Ringen (1977) has proposed a revision of Kiparsky's latest version of the AC. Kiparsky (1976) restated his original AC condition into a condition on the application of rules: *neutralization rules may only apply to derived forms*.²¹ A form is 'derived' if it is created by combining morphemes or if other rules have been applied to it. This condition leads to the duplication of rules, when the same processes are involved in both intra- and inter-morphemic regularities (see section 2.2.1). Ringen, in an attempt to avoid this duplication, proposes a revision of Kiparsky's condition. In order to avoid that UR's are set up that contain an abstract segment, which has to be mapped into another segment in all its occurrences, we only have to demand that *feature changing applications are permitted to derived forms only* (Ringen 1977: 130). This condition seems to overlap completely with the narrower condition I proposed above. I will slightly reformulate Ringen's condition: it should not refer to derived forms, but to derived inputs. After all, forms to which rules (7) and (8) have been applied are, strictly speaking, 'derived':

(10) Revised Alternation Condition

A feature changing application is only permitted if the string meeting its SD came into existence through the application of another rule (morphological or phonological) in the same derivation.²²

The Nupe rule, if formulated like in (9), could already apply to the UR of the forms it has to apply to after the assimilation rules.²³ This condition, together with some version of H.'s TGC, prohibits all abstractness without a NOC.

The AC discussed here should be preferred to the NOC because it pinpoints the causes of abstractness more directly. Rule ordering is merely a consequence of setting up abstract morphemes. Nobody *wants* rules to be ordered, rules *have to be* ordered because of the abstractness of the analysis. However, I think it is appropriate to state the condition as a condition on rules, because the desire to formulate certain rules lies at the bottom of abstractness, not the desire for abstract morphemes itself. Furthermore, as H. also points out, rules can be better 'observed' than UR's.²⁴

2.1.2.3. Morphophonemic alternations. To describe morphophonemic alternations H. adopts a model that was proposed by Hudson (1974). Instead of using a format with rule features or other diacritic features, indicating which forms do and which forms do not undergo a certain

rule, the alternating segments are 'summed up' in the lexical item. The resulting complex segment triggers a rule that distributes the alternants, mostly according to grammatical information.²⁵ Hudson argued for this model by pointing out that no principled distinction can be made between unproductive alternation and totally suppletive alternation (of the type *go-went*). Rule features, the use of which is far from clear, can thus be dispensed with. Furthermore, linguistic change makes no distinction between the two kinds of alternation. Both are subject to the same leveling. The main distinction then lies between productive and nonproductive alternation. This distinction is too crude, as we have seen in section 2.1.2.1. The degree of productivity does play a role too. A particular 'non-productive' pattern may extend its domain, for example, unlaut in German. Thus language change *does* make a distinction between the two types of alternations. We will give an example of H.'s use of Hudson's model.

In Spanish there is a nonproductive alternation *o ~ we* and *e ~ ye*, found in a limited group of words (mostly verbs). Within TGP three approaches are possible:

- (11)
- (i) $k \left[\begin{smallmatrix} o \\ +\text{tense} \end{smallmatrix} \right] nt /$
 diacritic use of phonological features since only vowels that have an alternating diphthong are specified as [+tense].
- (ii) $k \left[\begin{smallmatrix} o \\ +D \end{smallmatrix} \right] nt /$
 phonological use of diacritic features since the feature [+D] is referred to in the diphthongization rule.
- (iii) /kont/
 [+rule dift.]
 rule feature approach. The diphthongization rule is a minor rule.

Analyses like (i) and (ii) are rejected by Kiparsky (1968), who favored (iii). Now Hudson and Hooper reject (iii) because of the vagueness of rule features combined with the minor-major rule format and they propose (iv):

- (iv) $k \left\{ \begin{smallmatrix} o \\ we \end{smallmatrix} \right\} nt /$
 the 'disjunctive format'.

plus a rule:

$$(12) \left\{ \begin{smallmatrix} o \\ we \end{smallmatrix} \right\} \rightarrow \left\{ \begin{smallmatrix} o \\ we \end{smallmatrix} \right\} / [-\text{stress}]$$

The correct vertical ordering of the alternating segments, to be established on the basis of diachronic evidence, can be a matter of dispute (see Amstae 1978; Harris 1978). Since the disjunctive representations are not well formed as an input to P-rules, the MP-distribution-rules are intrinsically ordered before P-rules. It is obvious that this model reintroduces a familiar way of describing MP-alternations, the item-and-arrangement model. Recently Harris (1978) attacked the 'disjunctive theory', comparing it to the rule feature theory (by which he means format (ii)). I refer to his article for this discussion. Three difficulties will be mentioned here.

A disadvantage of H.'s model is that the rule that collapses the distribution of *o* and *we* and of *e* and *ye* does not show that in both cases the same feature change is involved, whereas in Harris' notation there is one rule expressing one change.

$$(13) \left[\begin{smallmatrix} +\text{stress} \\ D \end{smallmatrix} \right] \rightarrow \left[\begin{smallmatrix} -\text{syllabic} \\ -\text{high} \end{smallmatrix} \right]$$

(Harris 1978: 43). But perhaps a feature notation should not be used at all in cases like these. Many processes that require nonphonetic information seem to deal with segment *substitution*, rather than with what is often called *mutation* (cf. Skousen 1975).

The second point (cf. Harris 1977: 58-59) concerns the abstractness of the lexical items that trigger the distribution rules. It must be noted that there are no constraints in H.'s version of NGP preventing the symbols used to trigger the MP-rules to be completely arbitrary. Probably aware of this problem H. casually mentions an additional constraint on p. 21: "one constraint is necessary in this framework: The underlying forms must contain only feature values that actually occur in the surface allomorphs". This means that we cannot do without a constraint which directly refers to UR: a naturalness condition of some kind.

A final point concerns the question (also touched upon by Harris), of how we can choose between all the alternatives we find within the field of generative phonology. As pointed out before, we can find external evidence in language change. But this 'evidence' never 'proves' anything

apart from the fact that it does not point in one direction. Some might find it suggestive, others might not. In proposing alternatives for many undesirable properties of TGP, H. has come up with a more attractive theory. She gives reasonable argumentation for this theory, but it is not so that she has *proven* that TGP is 'wrong' and her theory is 'correct'. The fact that rules which are surface generalizations have a higher degree of falsifiability does not *a priori* mean that we have a better theory, because it might be the case that the right rules are not falsifiable, given the present state of our knowledge about what is going on in the human brain. For example, I think H. has a point in arguing that a distinction should be made between phonetically motivated rules, expressing constraints on what can be pronounced, and 'other rules' (apart from the question whether or not there should be more distinctions along the lines suggested in Crothers and Shibatani 1975 and Anderson 1975), but we never can be sure that speakers also make these distinctions. The strongest 'suggestions' for these and other claims surely come from what we know about language change. However, *language change seems to indicate that several analyses exist at the same time*. With regard to the Spanish situation leveling occurs in both directions (cf. Amastae 1978; Harris 1978; cf. also Tiersma 1978).

In the next section part 2 of H.'s book will briefly be discussed. My main goal here is to mention some weak or undesirable aspects of H.'s proposals and not to offer any full-fledged alternatives.

2.2. Substantive phonological issues

2.2.1. Introduction

In part 2 H. discusses what she calls 'static conditions', i.e. constraints on possible segments and constraints on possible sequences of segments. In TGP these constraints are expressed by redundancy rules called *morpheme structure conditions* (MSC; or earlier rules, i.e. MSR).

In NGP the question is asked whether there really is a need to separate MSC's from P-rules (see also Clayton 1976). A strong argument in this connection comes from the observation that in some cases MSC's are (partially) duplicated by P-rules. In those cases, where the generalization expressed by a P-rule holds both within a morpheme and across morpheme boundaries TGP assumes that a P-rule 'happen[s]' to apply internally to a lexical item" (SPE: 382).

Strictly speaking, since matrices must always be fully specified in this theory (according to the conclusions reached in Stanley 1967), the general-

ization must be expressed twice. Kiparsky (1968), discussing vowel harmony, explicitly chooses for this duplication. One of the arguments is that identical P-rules and MSC's can have different sets of exceptions. Nevertheless, in NGP the separation is considered to be harmful, because generalizations are missed. So at least in some cases the distinction between MSC's and P-rules should be abandoned. This line of argumentation is, by the way, completely parallel to the classical Chomsky and Halle argument that led to the rejection of an autonomous (i.e. phonemic) level. Now what about the MSC's that are not duplicated by P-rules, are they not true MSC's? According to H. (using Ockam's razor), there is no need to bar the *segment* structure rules (a subset of the MSC's) from the set of P-rules: they are completely transparent and require the same formalism as P-rules. As far as *sequence* structure rules are concerned that are not duplicated by P-rules, H. claims that they are in practically all cases not *morpheme* structure conditions at all. The proper domain for formulating these rules is the *syllable*.²⁸ From *syllable structure conditions* we can also deduce predictable feature values, but, as H. claims, this predictability is not used to increase the number of blanks in lexical matrices. It is unclear, however, whether this claim applies only to the major class feature [consonantal] and [vocalic] or to all features (cf. p. 194). The (re)introduction of the syllable in phonological theory is one of the merits of NGP.

2.2.2. Discussion

As will be clear now, H. does not make any distinctions in her theory among the rules that constitute the class of truly phonetic P-rules. Yet it is desirable to assign theoretical status to a few distinctions, because not all P-rules are functionally the same. First, only *dynamic* P-rules cause alternations. Second, *static* P-rules do not involve assimilation phenomena, but only distribution (of features or segments); and third, static P-rules are always restricted to fill in feature values, while dynamic P-rules also change feature values. It seems appropriate then to distinguish the following types of phonological generalizations, also argued for by Crothers and Shibatani (1975: 529):

(14)

- I. Static constraints
 - a. segment structure
 - b. syllable structure
- II. a. phonological rules that conspire to avoid violations of I.
 b. phonological rules that account for all sorts of co-articulation.

It is necessary to distinguish both constraints and rules (instead of speaking of two functions of one rule type), because the two do not have to be identical *in form*. Sometimes there are several rules that are all subordinated to one constraint (cf. Kisseberth 1970). One might decide to state static constraints for co-articulation phenomena, for example:

$$\sim \begin{bmatrix} \text{obstr} & \text{obstr} \\ \text{voice} & -\text{voice} \end{bmatrix}$$

but probably *all* co-articulation rules aim at the abstract scheme: $[\alpha F][\alpha F]$. The active counterpart of Ia will be of use only in the adaption of loan words.

2.3. Syllable structure and syllabification

Because in NGP great value is attached to the syllable as a phonological unit, H. devotes three chapters to it.

As regards the syllable three basic questions have to be answered (cf. Hyman 1975: 188): how does one define it, how does one mark its boundaries and does one need it? A convincing answer to the last question is given in NGP by showing the relevance of the syllable for the statement of the phonotactics of a language. The second question has not received a satisfactory answer in H.'s book. An attempt to give an explicit definition – the third question – is not made, although a lot of interesting observations concerning characteristic properties of the syllable are discussed.

In the past many definitions of the syllable have been given. For a survey see Awedyk (1975). Most definitions use either articulatory criteria or acoustic criteria or both. All those definitions have something in common. It is assumed that there is a property of the syllable, call it X. This property is present during the whole realization of a syllable but not all the time in equal intensity: when you draw a line that depicts the intensity changes you get something like this: $\cap$ or: $\cup$ depending on what 'X' stands for. In articulatory definitions X can denote aperture of the vocal tract, friction in the vocal tract, air pressure, tension of the muscles, etc. In acoustic definitions something called sonority, or less often loudness, is dominant. For those who combine definitions the most favorite candidates are aperture of vocal tract plus sonority (cf. p. 201). As regards the second question, most authors decide that demarcation remains a problem, especially because phonetically speaking there do not seem to be any boundaries in most cases. This caused the distinction between a *phonetic*

syllable and a *phonological syllable*, the relation between the two being that the boundaries that mark the phonological syllable are obscured by phonetic resyllabification principles. It is not my intention to go into these controversial points. I summed them up in order to stress the complicated nature of the issues involved. Let us now see if H. sheds any light on this problem concerning the syllable.

Property X is this time called *strength*, one of the vaguest and most poorly defined notions of current phonological theory (cf. Foley 1977). The marking of syllable boundaries is quickly dealt with. H. has devoted an article to this issue earlier (Hooper 1972). There she argued that syllable boundaries are inserted at certain points, without giving much justification. The definition problem was 'solved' by stating that a string of sounds that stands between two syllable boundaries is a syllable. Thus, syllable boundaries were considered to be predictable on the basis of segment arrangements. Another possibility would have been to predict segments on the basis of syllable boundaries (p. 192). In her book, H. does not want to choose between those two points of view, because "a comprehensive treatment of the syllable structure should be able to move in both directions" (p. 192). This 'comprehensive treatment' seems to be the following. On several grounds, especially by their behavior in sound changes, we can see that sounds show a hierarchy of 'strength'. It appears now that the sounds that constitute a syllable show the following strength pattern. The most marginal consonants have the greatest strength and those that stand nearer to the syllable peak show relatively less strength. The peak is of course the point of the relatively weakest strength. Furthermore, the first segment of a syllable has more strength than the last one. Thus there seems to be a

(15) Universal condition on preferred syllable structure

$$P(C): \$ C_m C_n C_p C_q V C_r C_s C_t \$$$

where $m > n > p > q$

$$r < s < t$$

$$m > t$$

$$m \neq 0$$

(on p. 229 the third line of the condition reads $r > s > t$ but this is a misprint). Using the same formalism, language-specific syllable structure conditions (one for each style of speech) must be set up, which can contain

violations of the universal scheme but are basically in agreement with it. These language-specific schemes are now used to *insert syllable boundaries*: "the SSC apply at the level of word formation to organize the string into syllables" (p. 193). SSC's also have a generative capacity in that they may alter consonants to meet their strength requirements. Thus segments are altered on the basis of syllable boundaries. For example, if a syllable begins and ends with identical consonants, say /p/, then the SSC requires that the syllable final consonant be weaker than the syllable initial consonant. If we have now some redundancy rules that specify that unreleased [p'] is a weaker version of the released cognate, then the generative capacity of the SSC is that it alters syllable final /p/ to [p'], thus accounting for low level allophonics. This weakening may even go further than necessary, for example, it may result in a glottal stop. The syllable initial consonant may also be altered, but for that we need two additional principles. First an appendix to (15) (p. 220):

- (16) if $XVC_r \$ C_m V$, and there is no pause between C_r and C_m , then $m > r$.

A syllable initial consonant may thus undergo strengthening because the final consonant of the preceding syllable is stronger. But even if this is not the case an initial consonant may become stronger. This is a result of what H. calls the *Optimal Syllable Principle* (p. 225) saying that strong positions favor strong consonants and vice versa. This explains why syllable initial /p/ in English becomes aspirated. It probably also explains why weakening in final position may go further than necessary.

All this looks consistent and plausible and surely expresses really existing tendencies as is illustrated by H. in discussing the syllable structure of Spanish in chapter 11. I wonder, however, whether this proposal can be made precise enough to be incorporated in the machinery of the language-specific grammar. H.'s explanations of the generative capacity of SSC's really concern the transition area from *synchronic* low level phenomena to *diachronic* changes. We must clearly distinguish between the universal SSC and additional principles on the one hand and the language-specific SSC's on the other hand. The first parallels the meta-rules proposed by Chen (1973). They should be considered 'predictions' (based on observed tendencies) about what rules and conditions languages most likely will

have and how these rules and conditions will change.²⁷ The second belong to the language-specific static phonology. Low level allophonics then should be handled by low level *rules* that 'serve' these conditions (cf. section 2.2.2 of this article). Chen's meta-rules are of course not the same as the meta-conditions discussed here. The difference between them parallels the difference between language-particular dynamic and static phonological generalizations.

Another critical note with regard to H.'s treatment of the syllable is this. It may look as if H. has solved the demarcation problem by postulating a syllable structure and deriving boundaries from that. But the question remains of course how she knows what is the actual syllable structure of a given language. Everything rests on the premiss that a syllable boundary is placed before the strongest consonant in an interlude, which is exactly what Vennemann's original proposal was. Now this may be very plausible but it does not follow from anything. It does not follow from the fact that the notion strength can be independently motivated by the behavior of sounds in language change or by synchronic assimilation processes (the two independent (?) sources for this notion that H. deals with), although nobody will deny the striking correspondences between the strength hierarchy thus acquired and the 'rough hierarchy of suitability for initial and final positions' of sounds in the syllable. The link cannot be that processes (diachronic or synchronic) which we call weakening (!) or strengthening (!) take sounds as their input that stand in weak or strong positions in the syllable, because this again presupposes knowledge about the structure of syllables. However, it seems to be impossible to avoid this kind of circular reasoning, since in all definitions of the syllable I have seen, no matter how ingenious, it is present.

I conclude that both problems dealt with in part 2 of H.'s book, viz. rule typology and syllable structure, though extensively and clearly discussed by H., have not received a satisfactory solution. Other interesting approaches to 'syllable-based generalizations' can be found in Basbøll (1974), Kahn (1976), Rudes (1977), and Bailey (1978). I do not know of a satisfactory typology of phonological rules.

In the next section I will discuss the treatment of phonological change within NGP. It is clear that a theory on change crucially depends on the organization of the model that accounts for the synchronic facts. For example, if rules are not considered to be ordered there can be no such thing as rule reordering. It is therefore necessarily so that H.'s theory of phonological change differs from the standard theory.

and rule addition bear to 'grammar'. Simplification can be said to be grammatically *motivated*, while rule addition can only be grammatically *conditioned* (that is, prevented from application in certain grammatical environments).

The question now is whether rules are grammatically conditioned from the very start of their existence or whether these grammatical conditions are in some way secondary, i.e. reactions to a situation in which the rule was not blocked from application. The hypothesis set forward by H. is that all added rules start as rules that are only phonetically conditioned. These 'added' rules can be traced back to universal phonetic tendencies. Instead of rule addition H. regards rule *modification* to be a more appropriate term, i.e. modification of a universal tendency so that it receives language-specific traits. H. has no suggestions concerning the question as to why such 'exaggerations' of phonetic tendencies occur in a particular language. But probably nobody has yet found a conclusive answer to that question (though Labov's theory of sound change is very promising in this respect).²⁹

A second stage in the 'life cycle' of an added rule is that its application is blocked under certain conditions. It might be the case, for example, that the rule destroys grammatical information if it is not blocked. Findings of Labov (e.g. 1972) seem to be in agreement with this suggested development. Labov has observed that in an early stage a rule applies to all forms that meet its SD, but not always with the same frequency. The frequencies are systematically relatable to certain morphological and grammatical categories. Kiparsky (1971) suggests that at a certain point in time this application according to frequency constitutes a situation which is rather complicated for language learners, who then take the radical decision to block the rule in cases where the frequency is low and to make the rule obligatory in the other cases. A similar synthesis of Labov's ideas and the standard view is now proposed by H. It is difficult, of course, to test this hypothesis with regard to sound changes that have taken place in the past. The first 'free application stage' does not leave traces, unless perhaps sporadically in written sources (when there are any). Van Coetsem et al. (unpubl.) refer to this difficulty when they say that later developments can *mask* earlier added rules. The blocking conditions which our diachronic rules (that are merely based on observed correspondences between different stages of a language) refer to, can have come into existence simultaneously with the 'birth' of the rule or can simply be a later development. However, Labov's findings can be regarded as a plausible hypothesis about developments in the past strongly supported by developments in our own time that

3. Phonological change

3.1. Introduction

A theory of phonological change within TGP has been developed by a number of authors, mainly Kiparsky (1971 [1965]), and it is standardized by King (1969). Basic assumptions of the model, some of which were added after 1969, are that given a phonological component in its standard form rules can be *added*, *reordered*, *simplified*, *lost* or *absorbed* in the lexicon' (causing restructuring of UR's). These changes take place because they result in a formally simpler grammar. Children strive for the simplest possible grammar given a set of data (this causes rule lexicalization), but sometimes this search for simplicity is stronger than the need to produce a correct output (this causes rule simplification and rule loss). If in addition to formal simplicity children also prefer certain unmarked ordering types, and, for instance, paradigm uniformity (so-called *substantive conditions*), then these substantive conditions cause rule reordering. All the types of change take place in the competence resulting in changes in its output (except for rule lexicalization). Ergo, *change is change in competence*. That all this cannot be applicable to rule addition, which is always in conflict with the simplicity condition (after all there is one rule more) and usually also with the substantive conditions (especially paradigm uniformity), was apparently not a sufficient reason to make a principled distinction between rule addition and the other types of change.²⁸ Of course, most historical phonologists were – from the outset – aware of the correspondence between rule addition and sound changes on the one hand and the other mechanisms of change and analogy on the other hand. Perhaps the differences between simplification (in the broad sense of rule change, reordering, loss and lexicalization) and analogy are not radical (except in one case that we will discuss below) with regard to the phenomena they are meant to describe or with regard to the understanding they express of what causes certain phenomena. The disagreement is rather centered on the proper view of sound change as rule addition.

3.2. Discussion

3.2.1. Rule addition

The fact that certain synchronic rules refer to nonphonetic information does not necessarily imply that such rules can be added to a grammar. First, let us clearly differentiate between the relations which simplification

can be observed. Actually H. manages to trace the history of a diachronic rule of shwa-deletion in Spanish back to the 'variable' stage (pp. 104-110). But this will not always be possible, because of lack of sufficient data. In NGP we thus have a strong constraint on the notion 'possible rule addition', since all rules containing nonphonetic information are excluded. A further refinement of the notion rule addition will probably have to take into account more seriously Labov's theory and also the theory of *lexical diffusion* proposed by Wang (1969). For an excellent study in this direction see Ralph (1975).

3.2.2. Rule simplification and rule loss

Another way in which a phonetic rule may start interfering with 'grammar' is by functional interpretation of the effects of this rule. Usually rules cause alternations in paradigms. H. suggests that there may be a desire on the part of the speaker to interpret alternations as meaningful, an instance of the famous *one form one meaning principle* (also referred to as *Humboldt's principle*) which was reintroduced into linguistics by linguists like Anttila (1972) and Vennemann (1972b). What results is rule *morphologization*. This view contrasts with the more traditional one that morphologization takes place because the phonetic conditioning of a rule is lost. The introduction of the functional background of the type of rule change called morphologization is a major difference between TGP and NGP. Since no principled distinction is made between purely phonetic rules and morphologized rules in TGP, this type of change has no theoretical status. Yet the distinction between the character of a rule before and after this change is of great importance with regard to a better understanding of the notion rule simplification as used in TGP. Rule simplification in TGP seems to refer to two independent phenomena. On the one hand the 'simplification' of phonetic rules and on the other hand the 'simplification' of morphologized rules. H. only deals with the second type of simplification.

Once a rule has lost its phonetic character it becomes subject to two tendencies traditionally referred to as *analogical leveling* and *analogical extension*. When leveling occurs, the domain of application of a rule becomes generally more and more restricted under the pressure of a tendency to make paradigms regular. This may finally result in the loss of a rule. Some traces of the rule may remain outside paradigms. To account for these in NGP *via-rules* where introduced, a rather vague type of rule, which is nowhere clearly defined by H. Via-rules express relations between

forms that cannot be considered to be derived from one basic UR, but are still felt to be related. Probably *via*-rules must be used to express relations outside inflectional paradigms. The lack of a clear principle to distinguish between MP-rules and *via*-rules is due to the fact that, within NGP (as within TGP), no sufficient attention is given to *morphemic analysis* and hence to the question what sort of words constitute an alternation (cf. Ohlander 1975). Whether or not a *via*-rule is postulated depends on what a given speaker regards as related forms. Writers of etymological dictionaries probably have incorporated a lot of *via*-rules. An interesting side of H.'s view is, however, that rule loss is not seen as a sudden phenomenon, but rather as the end point on a scale of 'rule fading'. This can be seen as lexical diffusion 'in the reverse', something which Dressler (1972) calls 'lexical fading'. See Robinson (1977), who also discusses this possibility (and Ralph 1975: 157-162). The phenomenon of lexical fading could be taken as evidence for the claim that the locus of phonological change is the *lexicon* rather than the phonological component (Van der Hulst 1979a).

One problem with rule loss is that sometimes not the basic allomorph appears in all paradigms but a derived allomorph, or that in one paradigm the basic UR appears and in another paradigm a derived allomorph.³⁰ H. cannot handle these cases any better than the standard theory, though one could argue that the 'disjunctive' UR's are slightly better because at least both allomorphs are present in the lexical item (cf. Tiersma 1978).³¹

What seems to be the situation when a rule becomes subject to fading, is that the attempt to functionally interpret the effect of the rule has not been too successful. As a consequence the formal effect of the rule is felt to be redundant and is eliminated, not, as suggested in TGP, because of formal simplification in terms of the number of features, but because of simplification in terms of *surface regularity*. Extension of a rule then seems to occur when the functional interpretation is successful. A typical example is the extension of Umlaut in German (see section 2.1). H. does not give us much information as to why a certain rule will successfully gain functionality. She says that arbitrary rules "are bound to be lost". However, the examples she has in mind are so-called inverted rules, a type of rule which has no place in H.'s version of NGP. We will return to this below. But this absence probably reflects the present state of knowledge concerning analogical processes.

H. does not pay attention to simplification in phonetic rules, the second phenomenon covered by the TGP notion *rule simplification*. In TGP this is precisely the type that provides the evidence for viewing formal simpli-

fication as the motivation behind a situation in which a phonetic rule extends its domain of application to a larger natural class of segments (i.e. works on more input segments or in more environments). A strange consequence of the TGP theory of rule change is that, since naturalness of rules is a function of the number of features, the *least natural* rule is first and most often added to a language (cf. Labov 1972: 126). Naturalness, then, cannot be related to frequency (as is usually done). It seems that this so-called simplification of phonetic rules has nothing to do with formal simplification, but everything with the gradual extension of a phonetic tendency to sounds that are more resistant to a particular change, although the reasons behind this gradual extension are largely unknown.³² This is where the correspondence between the TGP notion of simplification and the notion of analogy breaks down. Even if we see this gradual extension as a kind of analogical process, it is still of a completely different nature, because 'meaning' is not involved (Sturtevant 1917: 80). Of course simplification can be used to *describe* such phenomena, but it can hardly *explain* them. Traditional theories of sound change do not regard this type of simplification as analogy (as in the former case) but as one aspect of sound change (see Bynon 1977: 125). The explanation of simplification of phonetic rules seems to lie 'outside the grammar', tied up with our explanation of rule addition.

But how can we properly describe and explain both rule addition (i.e. modification) and rule simplification (i.e. phonetic rule extension)? The mechanism that seems most promising is the theory of meta-rules of Chen (1973), already mentioned in section 2.3. Essential to such a theory is the observation that sounds can be hierarchically classified by virtue of their resistance to undergo or stimulate a phonetic change. Whether or not there can be one hierarchy for all processes is still a matter of dispute, but given a phonetic tendency and a hierarchy of resistance for that tendency it can be predicted that, if a rule is added at all it will be a rule that has sounds with the lowest resistance as its input. Extension of the rule will proceed gradually to sounds with more resistance. Now it must still be explained, of course, why certain sounds are more resistant than others, but the crucial point is that formal simplicity has nothing to do with it. It also remains unclear whether the parameter for the hierarchical ordering is the mysterious cover feature *strength* or direct phonetic parameters as sonorancy, height, etc.³³ Meta-rules then predict what rules will most likely be added and what direction phonetic rule extension most likely takes. Perhaps Dinnsen's theory of complement rules can be used to express the precise

incorporation of phonetic tendencies in the language-specific grammar (Dinnsen 1976).³⁴

3.2.3. Rule reordering, rule inversion and restructuring

In NGP there can be no such thing as rule reordering, since rules are not considered to be extrinsically ordered. H. thus has to show that those facts which have been described as changes in rule order can also be described without it. I will not recapitulate in detail how she proposes to do this. I will discuss one typical case. In doing this I will question the proper use of the mechanism of *rule inversion* (cf. Van der Hulst 1979b). In addition I will draw attention to the fact that the TGC allows nonphonetic rules as P-rules, referred to in section 2.1.2.1.

In the history of German (and Dutch) the following change took place. The alternating pairs like [dɪŋk] (from underlying /dɪŋk/ by final devoicing) and [dɪŋgə] changed to [dɪŋk]–[dɪŋə], through addition of a rule which deleted /g/. If this deletion rule is formulated as:

$$(17) \quad g \rightarrow \emptyset / \eta _$$

it must be extrinsically ordered *after* final devoicing for otherwise a form [dɪŋ] might arise. But in a subsequent development this form did actually arise, which can then be described by ordering the deletion rule before final devoicing. The principle that predicts this reordering was considered to be paradigm uniformity in the standard theory. Within NGP the situation before leveling took place can be described without extrinsic ordering by formulating the deletion rule as:

$$(18) \quad g \rightarrow \emptyset / \eta _ \emptyset$$

But how is the analogical leveling described and explained? In Venne-mann's original analysis of this case it was argued that, when word internal deletion has taken place, the UR can no longer be /dɪŋk/ since this form appears nowhere in the surface paradigm (i.e. the strong naturalness condition; see (2) above). The UR must either be /dɪŋ/ or /dɪŋk/. In the first case we need a rule that inserts a /k/ in the singular (an *inverted* rule), in the second case a rule that deletes a /k/ in the plural.

$$(19) \quad \emptyset \rightarrow k / \eta _ \#$$

$$(20) \quad k \rightarrow \emptyset / n _ \emptyset$$

The second rule should be lexically marked because of nonalternating forms like [bank]-[bankən]. So rule (19), the inverted rule, is chosen. This rule is phonetically implausible and thus subject to loss. As usual, rule loss is now responsible for leveling. It might be considered advantageous that rule loss and leveling are always connected. In TGP leveling is described by at least two mechanisms, viz. rule loss and reordering. It seems that H. considers this analysis valid also within her own version of NGP.

A first critical note is that we must ask precisely what constraint in H.'s version of NGP prohibits an abstract UR /ding/. Unlike Vennemann, H. cannot simply say that the form does not correspond to one of the surface allomorphs, because H. has rejected the strong naturalness condition. Clearly both final devoicing and the deletion rule (18) are not contradicted at the surface. The answer is probably that the *g*-deletion rule does not relate surface forms directly. A rule that directly accounts for the surface forms would be (19) or (20). Of these two rules (19) conforms to the TGC and must therefore be a P-rule, although it is an unnatural nonphonetic rule. Obviously the alternation involved is morphophonemic and should thus not be described by a P-rule. The proper alternative within NGP is Hudson's model, i.e. a disjunctive lexical representation /din{g}/ and a MP-distribution rule. The leveling can now be described by a loss of the 'elsewhere case' in the distribution rule. It appears then that we do not need inverted rules at all within the framework H. herself proposes. I conclude from this that the mechanism of rule inversion cannot be supported by cases such as this one. I do not doubt that some kinds of inverted rules are needed, for example, to account for hypercorrection phenomena in the sense of Decamp (1972) or to account for liaison found in English *the idea-r-is*. H.'s book is not consistent in incorporating V.'s original analysis in the NGP version presented here.

4. Concluding remarks

Within the field of generative phonology the book under discussion is a step forwards, because it takes several steps backwards. While clearly maintaining a number of characteristics of standard generative phonology NGP re-establishes a few important traditional insights, especially in the nature of phonological change, which the standard theory flatly denied or at least obscured, for example, the distinction between P-rules and MP-rules, between sound change and analogical change. The 'loss' of the

mechanism of rule reordering is very attractive since now each 'event' is described by a unique mechanism. Just like in the standard theory the phonemic level is not incorporated in NGP, though one would have expected that H. had given the phoneme (the most important of all possible insights in the field of phonology) a place 'somewhere' in a theory that claims psychological reality. After all, it is possible to reject the phonemic level, while maintaining the notion in the theory (Fischer-Jørgensen 1975: 287-289).

As I have tried to show, NGP must still be further elaborated on a number of points. Reintroducing traditional notions, although an improvement in this case, is obviously not enough if those notions are themselves badly defined. I regard it as a serious weakness of H.'s theory that the major condition on abstractness, the TGC, lacks a clear definition. In as far as H.'s examples illustrate the 'strength' of this condition, it seems that the TGC is as extreme as Postal's naturalness condition, but in the opposite direction. The position Kiparsky (1975, quoted in section 2.1.2.1) takes on this issue seems to be more 'natural'. Furthermore it seems absolutely necessary that criteria are established which make it possible to decide when one is dealing with an alternation (and thus a MP-rule) and when not (accepting that in the latter case a *via-rule* may or may not be assumed by speakers).³⁵

From all this it should not be concluded that I regard H.'s book as uninteresting, and that we should hold on to the standard theory. On the contrary: I find H.'s book very stimulating and thought-provoking. H. has made some progress, following Kiparsky en Vennemann, in the formulation of a phonological theory that is far more interesting than the standard theory. The book is particularly strong in giving sensible interpretations of formal mechanisms of grammar and arguing against 'pseudo exception devices' and 'tricks' that produce the correct surface forms, but do not allow for such interpretations. In this review I did not refrain from detailed criticism because I think that Natural Generative Phonology is essentially on the right track.

Notes

¹ I would like to thank Cor van Bree, Didier Goyvaerts, Teun Hoekstra, Jan Kooij, Michael Moortgat and especially Wim Zonneveld for their critical comments on an earlier version of this article. All references to pages, without source, apply to Hooper (1976).

² Some phonological rules "happen to apply internally to a lexical item" to avoid duplication of rules (SPE, 382). Cf. section 2.2.1.

³ Rule A and B may also work simultaneously, in which case the 'order' cannot be called extrinsic. This means that a prohibition of extrinsic ordering would not rule out absolute neutralization (cf. Koutsoudas et al. 1974; H.: 64-70). However, it is not necessary to forbid simultaneous application (as H. does; p. 55), because the constraint on abstractness I will discuss in section 2.1.2.2 rules out absolute neutralization on other grounds. Cf. note 23.

⁴ Jensen (1974) offers an elaborate discussion of this condition showing that it represents at least four distinct constraints. The constraint mentioned here, the *segment paradigm condition*, is only one of them, though it is the most important one.

⁵ E.g. syllabification rules cannot apply to grammatical units such as morphemes.

⁶ Only when purely phonetic rules are involved, as will appear below. If the rule involved is neutralizing, the UR contains the neutralized value; if the rule is nonneutralizing, the UR is archisegmental (cf. H.: 116-119). See also note 9.

⁷ To constrain the theory H. mentions a third possibility (besides constraints on UR or rules): constraints on the notion *possible alternation*. The fact that this possibility is not explored in NGP will be criticized in section 3.2.2. Cf. note 35.

⁸ As will appear in section 2.1.2.2 rules can in fact refer to such segments under a TGC.

⁹ See note 6. H. (pp. 119-120) assumes that the choice between UR's that are fully specified and UR's that are archisegmental cannot be made on empirical grounds. She chooses for the second position because she sees "no reason whatsoever to suppose that lexical forms are pronounceable" (p. 126). In Hooper (1977) she claims to have found empirical evidence that points in the direction of the first position.

¹⁰ Cf. note 8.

¹¹ "A reformulation that improves the rules is possible when the rules in question are true generalizations [...]. [...] if rules are not true generalizations, reformulations to avoid rule order will lead to more and more ad hoc rules" (H.: 57).

¹² In rule (ii) the nonexistence of $[\pm \text{stress}] [\pm \text{voice}]$ is exploited. The rules given by H. (valid for her own speech) are inaccurate. Rule (ii) must contain the special $[\pm \text{syll}]$ to the left of the bar. At least I assume that H. does not pronounce 'doesn't' as $[\text{dast}]$.

¹³ In fact, rule ordering can be dispensed with in this case if we allow simultaneous application of rules. As H. points out herself, the best formulation for this kind of processes would be a 'transformational' P-rule. Cf. SPE, 360.

¹⁴ Wim Zonneveld drew my attention to two unpublished papers: Hooper (unpubl.) and Harris (unpubl.). He also showed me a copy of the second paper. Harris points out that storing e.g. *estudio* as a *three-syllable* base leaves unexplained the fact that this word behaves like a *two-syllable* base with regard to a process of diminutive suffixation in some dialects of Spanish. Unfortunately I have not been able to obtain a copy of Hooper's paper.

¹⁵ X may also not be A, but this follows from (6i).

¹⁶ Apart from the fact that it should be stressed (as Wim Zonneveld suggested to me) that Kiparsky's condition is not a constraint against certain rules, as H.'s TGC is.

¹⁷ Recently Jensen (1978) has expressed more or less the same points with regard to the TGC. Cf. note 20.

¹⁸ Note that these diachronic mechanisms are traditionally considered to be nonphonetic changes, which indeed involve *segments* and not features.

¹⁹ The NOC does not only forbid extrinsic rule order but (apparently) also simultaneous application of rules. Cf. note 3.

²⁰ Hyman has shown that rule (9) could be reformulated so that all rules involved are intrinsically ordered, by building the SC of (7) and (8) into the SD of (9). The resulting "rule [...] directly incorporates the motivation for the rule [...]" (Hyman 1975: 89). According to H., the TGC does not allow this reformulation because "Low vowels are back and unround everywhere, not just after C' and C''" (H.: 68). As regards the reformulation of nasal deletion (see (4) above), which uses the same technique, it appears that the same reason-

ing (though in the opposite direction) is implicit: rule (ii') is better than (ii) because nasals are not deleted everywhere but only after nasalized vowels. Both the rejection of Hyman's rule and rule (4ii) imply that rules may neither be undergeneralizations nor overgeneralizations: rules have to state exactly what is going on. Jensen (1978: 669) is wrong in assuming that rules (7) and (8) are in conflict with the TGC in generating C''s and C''s in positions where the conditioning factor is not present on the surface. The point is that in the NGP analysis C' and C'' before /a/ are lexical (cf. H.: 68). The rules are opaque by case (11b) which is not a subclause of the TGC.

²¹ This is the strong version. The weak version refers only to *nonautomatic* rules. As Ringen (in her second footnote) points out, this weaker version is not a constraint on abstractness precisely because most rules of absolute neutralization are automatic. I would not like to call this a 'striking property' as Kiparsky (1976) does, since abstract segments are usually not taken from the segment inventory of the language at issue and can therefore be changed or deleted *everywhere*.

²² This condition is, of course, not applicable to optional (stylistic) rules nor to rules that only specify gradual changes, i.e. low level rules. This last proviso does not have to be made if it is assumed that UR's contain all phonetic information. In that case the condition could simply refer to "the application of a rule..."

²³ This means that we do not have to disallow simultaneous application of rules to rule out the abstract Nupe analysis: rule (9) may not apply at all.

²⁴ The failure to see that it is not in the first place extrinsic ordering that has to be attacked has led, I think, to the unconvincing theory of Koutsoudas et al. (1974) that constantly must be 'enriched' (and thus weakened) by new 'universal' principles, and still does not work properly (see Trommelen and Zonneveld 1978). However, I do not deny the need of some universal principles that have to be invoked when a form meets the SD of two rules or when two rules are mutually feeding. E.g. in Dutch, sequences like $[\pm \text{stress}] [\pm \text{voice}]$ first meet the SD of final devoicing and then of regressive voice assimilation but then again of final devoicing, etc. clearly to avoid a 'synchronic derivation'. A principle must be invoked that the rule with the more specific SD takes precedence, which implies that the principle of *proper inclusion precedence* also has relevance for rules that are mutually feeding.

²⁵ Ergo, in NGP rules do not have exceptions, because when there is an exception there is no (process) rule, but a (distribution) rule. The latter are only triggered by UR's that contain the appropriate complex segments. One could say, of course, that the complex segments are a kind of diacritics and that all forms are handled as exceptions except the 'exception' itself. Cf. Harris' critique mentioned below.

²⁶ There may, in addition, be constraints on word structure; H.: 191.

²⁷ Perhaps also about what kinds of differences there will be between rules and conditions of different styles of speech.

²⁸ One could maintain that the claim that *only adults* add rules makes the desired distinction. However, the point is that within the standard theory there is no motivation for rule addition. One must assume therefore that the only motivation for phonological change that is provided for by the theory is also applicable to rule addition.

²⁹ The effect of a phonetic tendency, perhaps only present in the words that are most frequently used, may become, at a low level of awareness, an indicator of a certain social group. Identification with this group may then lead to exaggeration of the effect, which as a result reaches a higher level of awareness. If the group of words provides a sufficient basis for a rule this rule is also applied to other, less frequent words. This initiates a next stage. Cf. below.

³⁰ In NGP the basis allomorph is the one mentioned in the 'elsewhere' case of the MP (distribution) rule. Strictly speaking the other allomorphs cannot be referred to as 'derived'.

³¹ The fact itself that bidirectional leveling occurs cannot be taken as evidence for Hudson's model, as Tiersma assumes. It only means that in 'comparable' cases a different allomorph is felt as basic (this may probably even vary from speaker to speaker). This poses as many problems in an IP model as it does in an IA model. Cf. 2.1.2.3.

³² Cf. note 29.

³³ It is clear that the notion of *strength* in Foley's theory, since it receives no phonetic interpretation at all, does not explain anything either (Foley 1977).

³⁴ However, as far as I understand it, this theory is only an explicitation of the claim that a phonetic rule will lose its features in a particular order, resting entirely on the assumption that the features that are now used are in fact correct. Further elaboration must make clear precisely what this theory contributes to our understanding of phonetic rule extension.

³⁵ From Harris (unpubl.) I understand that Hooper (unpubl.) addresses herself to the problem of morphemic analysis. She proposes the *Semantic Transparency Hypothesis* which claims that a speaker will choose as basic the alternant that is primitive from a semantic point of view. I will not go into the merits of this proposal. The point is that H.'s hypothesis is not meant to help deciding whether or not a speaker assumes that two words contain the same morpheme. Cf. note 7.

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