

RULE INVERSION IN CHADIC: A CLOSER LOOK*

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Schuh [1972] has argued that the phenomenon of "rule inversion" has occurred in the historical development of Hausa and Kanakuru, while Leben [1974] maintains that this is not the case. This paper examines this controversy in the light of both the data discussed by Schuh and Leben and other data from Newman [1974]. It is concluded that, at least in the case of Kanakuru, Leben's analysis is incorrect and that some of the Kanakuru facts will pose extreme difficulties for any theory of phonology which does not allow for the possibility of rule inversion. Some discussion of the possibility of predicting when rule inversion will and will not occur is also given.

1. Introduction

Schuh [1972] has argued that in the historical development of the Chadic languages Kanakuru and Hausa, the phenomenon of "rule inversion" (cf. Vennemann [1972]) has occurred. In particular, Schuh claims that the synchronic grammars of these languages contain rules which convert underlying sonorants into stops, as opposed to the historical process(es) which converted stops into corresponding sonorants in certain phonological environments. (Newman [1974], in his synchronic account, refers to these as "hardening" rules, a term adopted by Schuh.) Leben [1974] challenges these claims. He maintains (pp. 265-6) that "the positing of a synchronic stage with 'conceptually anomalous' inverse rules constitutes a middleman which it would be advantageous to eliminate in principle from the realm of possible phonological systems" and presents alternative accounts of these phenom-

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ena which, he argues, have all of the virtues of Schuh's accounts without requiring the undesirable "middleman" of inverse rules.

This paper is a further contribution to the issue of deciding whether rule inversion has in fact taken place in these languages. It is argued that, while Schuh's and Leben's accounts both appear to give reasonable explanations for the behavior of the forms under discussion, there are other forms, at least in the case of Kanakuru, which afford a basis for preferring Schuh's account to that of Leben.¹

The Kanakuru facts thus seem to indicate that rule inversion must be allowed in phonological theory if a more or less standard generative approach is adopted. What is more, it is not possible to account for these data within either the framework of "upside-down" phonology (Leben and Robinson [1977]) or some versions of what is called "Natural Generative Phonology" without allowing inverse rules. Thus the only way of accounting for such data for any theory appears to be the use of inverse rules. I will also give some attention to the issue of how to predict, if this is possible, when rule inversion will occur.

2. The Kanakuru Controversy

Relying on Newman [1970, 1974], Schuh first presents data (pp. 380-382) which indicate that in Kanakuru earlier stops "weakened to corresponding sonorants in phonologically specifiable environments," so that *t, *d, *d̂, became r, *k and *g became γ, and *p, *b, *b̂ became w. These weakenings resulted in many cases in synchronic alternations in a synchronic grammar of Kanakuru. Schuh presents three arguments in support of his contention (p. 384) that "the rules producing the alternations ... are 'hardening' rules and the sonorant variants are underlying." I will now sketch briefly these arguments, together with Leben's counters to them and further

¹For the sake of brevity, I will not discuss here the Hausa case. Although the issues in this case do not appear to be as amenable to a clear resolution as in the case of Kanakuru, I would maintain that Leben's arguments against Schuh's analysis are not nearly as telling as he apparently believes they are (cf. Leben [1974, 1979]). For a brief consideration of this question, see Churma [1981a].

discussion relevant to choosing between the accounts of Schuh and Leben.

2.1. Simplification of alternation types.

2.1.1. Schuh's arguments. Although at one point in the history of Kanakuru there were alternations between each of the original stops and the corresponding sonorants, as well as non-alternating stops and sonorants, this is no longer the case for contemporary Kanakuru. Now γ alternates only with k , w with p , and r with t (or, in a few words, with d'); the other original stops are not found in these alternations. Moreover, etymological sonorants (which, of course, did not alternate at all for a time) now show precisely the same kinds of alternations that are found with the etymological stops. The reason for these changes, Schuh maintains, is that, since "for the majority of lexical items, sonorants had now replaced etymological stops in the most basic syntacto-semantic forms" (p. 384), they soon came to be taken as also phonologically basic. Now that the sonorants were taken as underlying, however, each lexical item had to be marked as to which stop (if any) it alternated with: some w 's alternated with p , some with b , some with β , and still others did not alternate at all. "Since such lexical marking is conceptually anomalous" (p. 385), the markings have been eliminated, and now only one of the stops shows up in the alternations, even when there had originally been no alternation. In sum, the appearance of the sonorants in the more "basic" categories caused them to become underlying phonologically and thus necessitated an inversion of the rules in question, as evidenced by the subsequent changes in the language. That is, unless the alternations were at some point due to an inverse rule (and the concomitant change in underlying representations), the changes observed would have been unlikely; but the changes did occur, and so it is likely that there has been a rule inversion.

2.1.2. Leben's reply. Leben argues (p. 267) that "if we do not assume that sonorants became basic, it is still possible to explain the historical developments." Recall that one phenomenon to be explained is the fact that sonorants now alternate only with voiceless stops. Leben proposes an alternative explanation for this: "In the examples given by Schuh, the voice-

less stops resulting from this regularization appeared in a typical devoicing environment, immediately preceding a voiceless stop ... If, in addition, etymological *d*, *b*, etc., ceased to surface phonetically as voiced stops, then future generations would be presented with no synchronic evidence for setting up underlying voiced stops in these words." Thus, the (voiceless) stops *could* become underlying for this reason in the case of etymological stops. As for the historical sonorants which now alternate with stops, Leben notes (p. 268) that "the only instances he cites of the extension of the alternation to historical sonorants occur in word final position, and Schuh himself notes (p. 386) that 'word final is a position of neutralization where stops and sonorants cannot contrast either phonetically or underlying [sic].'" The sonorants have been eliminated by this neutralization rule in favor of voiceless stops, which "will naturally be subject to the same alternations as any other instance" of voiceless stops. That is, there is another possible explanation for the changes at issue, one which does not entail the existence of inverse rules.

2.1.3. Discussion. There can be little question that Leben's account is on the face of it at least as plausible as that of Schuh. It is possible, of course, that there could be data from Kanakuru which would be counterexamples to Leben's analysis, e.g. there could be etymological sonorants which alternate with voiceless stops which are in other than word-final position. (Note in this regard that if there are in fact no data of this kind, Leben's account would appear to be supported, since the lack of stops occurring in other kinds of environments would appear a priori to be quite unlikely, unless perhaps some facts about the structure of Kanakuru preclude such data.) However, such data clearly do exist, even among the examples discussed by Leben, although he is correct that the examples cited by Schuh in his first argument contain no data of this type. Thus, we find *guwɪ* 'to forge' and *a gup-ro dɪyɪ* 'he forged a hoe for her' illustrating the w/p alternation (cf. Schuh, p. 385 and Leben, p. 268), where the *p* is clearly neither in a devoicing environment nor in word-final position. Therefore, since the alternating consonant comes from etymological **ɓ* (cf. Schuh, p. 385), Leben's analysis cannot be maintained. He thus has not

shown that there is an alternative explanation for Schuh's facts.

2.2. Schwa-epenthesis.

2.2.1. Schuh's arguments. Schuh first presents (p. 386) the data given in (1):

- (1) a. a wupə-ro 'he sold (it) to her' [cf. wupe 'to sell']
 b. a gup-ro diɣii 'he forged a hoe for her' [cf. guwi 'to forge']
 c. ʃi kukə-mai 'he is learning it' [cf. kuke 'to learn']
 d. ʃi duŋ-ŋai 'he is beating it' [cf. duɣi 'to beat']
 [cf. also a duk-ro 'he beat (it) for her']

He argues (pp. 386-7) that "if we were to take stops as underlying in all cases and derive the sonorants from them, there would be no way to distinguish the medial consonant in the verb root in [1a] from that in [1b] and the medial consonant in the verb root in [1c] from that in [1d] for the purposes of epenthetic ə insertion.... Likewise, by not distinguishing k and ɣ underlyingly, we would have no way to predict which words have velars which assimilate to the following nasal, as in [1d]."

Before proceeding further with this argument, it will be helpful to have a clearer understanding of the rules involved, since neither Schuh nor Leben gives an explicit account of the rules in question which is in accord with the data and rules given by Newman. First of all, the Schwa-epenthesis rule is actually somewhat different from either Schuh's or Leben's formulation: sequences of two consonants are subject to epenthesis if the first is voiceless or prenasalized; the second may be any consonant, not just a sonorant as stated by Schuh. The sequence dʀ is also subject to epenthesis as are all triconsonantal clusters (cf. Newman [1974:3]). The exact statement of the epenthesis rule is not crucial to the present concern. Secondly, the weakening (or strengthening) "rule" must apparently be stated synchronically as a set of at least four rule schemata (cf. Newman [1974:4-7], Frajzyngier [1976:207-8]). Since Newman's rules involve "archiphonemes", thus rendering them inappropriate for present purposes, I give below Frajzyngier's "translation" of them into a weakening version:

- (2) a. $C \rightarrow [+cont] / V_v^2$
 b. $\begin{bmatrix} C \\ <-lab> \\ acon \end{bmatrix} \rightarrow [+son] / \begin{bmatrix} C \\ <-acon> \end{bmatrix} \text{---}^3$
 c. $\begin{bmatrix} C \\ +con \end{bmatrix} \rightarrow [+son] / \text{---} \begin{bmatrix} C \\ \{ [-con] \\ [+con] \\ [+son] \} \end{bmatrix}$
 d. $\begin{bmatrix} C \\ -ant \\ -con \end{bmatrix} \rightarrow [+son] / \text{---} \begin{bmatrix} C \\ +son \end{bmatrix}$

That is, weakening occurs intervocalically for all consonants (but cf. note 2), labials weaken after any consonant, velars after dentals and before sonorants, and dentals before and after labials and velars and before *r*. The corresponding strengthening version would be:

- (3) a. $C \rightarrow [-son] / \text{---}\#$

²The "[+cont]" here is apparently a misprint, and should actually be "[+son]", since Newman's rules refer to the latter feature (as do the rest of those of Frajzyngier). The environment will undoubtedly also have to be complicated, since weakening does not occur when the second vowel is schwa or between a short vowel and *e*. It could be not implausibly maintained that all schwas are epenthetic and that epenthesis follows weakening (thus explaining why weakening does not occur before schwa), but Frajzyngier's attempt to account for the lack of weakening in the second of the above environments by positing underlying geminates in all and only such cases is ad hoc from a synchronic perspective (although geminates probably were present phonetically at an earlier period in at least some of these cases). An analysis which entails such massive "absolute neutralization" (Kiparsky [1973]) is almost certainly untenable. The actual form which (2a) would have to take, given these considerations, is:

$$(i) C \rightarrow [+son] / \begin{bmatrix} V \\ <-long> \end{bmatrix} \text{---} \left\{ \begin{array}{l} +high \\ +back \end{array} \right\}$$

i.e. weakening occurs intervocalically except between a short vowel and *e*. It is worth pointing out also that Frajzyngier's abstract analysis does not even attempt to account for the failure of stops in synchronic stop-sonorant alternations to correspond to their etymological counterparts, which is of course one of the main arguments for rule inversion.

³I have supplied the angled brackets here. Without them, labials would incorrectly never (rather than always—cf. Newman [1974]) be weakened after a consonant.

- b. $\begin{bmatrix} C \\ <-lab> \\ \text{acor} \end{bmatrix} \rightarrow [-son] / \begin{bmatrix} C \\ <-acor> \end{bmatrix} \text{---}$
- c. $\begin{bmatrix} C \\ +cor \end{bmatrix} \rightarrow [-son] / \text{---} \begin{bmatrix} C \\ +cor \\ -son \end{bmatrix}$
- d. $\begin{bmatrix} C \\ -cor \\ <-ant> \end{bmatrix} \rightarrow [-son] / \text{---} \begin{bmatrix} C \\ <-son> \end{bmatrix}$

That is, all consonants are strengthened word-finally, labials strengthen before any consonant, velars after labials and velars and before obstruents, and dentals after dentals and before dentals other than *r*.

Some simplification of these rules may be possible, but it is apparently an inescapable fact that the synchronic situation is not a simple one. It is worth pointing out in this respect that the environment for the synchronic rule in a weakening account is not the same as the historical environment and that for a strengthening account it is not "the complement of the former weakening environments" (Leben [1974:269]), nor for that matter, "that subset of [the complement of the historical environments] in which [the stops and sonorants] still alternate" (Vennemann [1972:211]). This may have important implications concerning the nature of rule inversion (but cf. the discussion in section 4). In any event, some additional changes must have taken place in the history of Kanakuru.

Let us now return to Schuh's argument. He apparently intends that the underlying form of the verb stem for the above forms be the same as the surface form of the infinitive; after a rule which deletes final *-i* in verbs everywhere except pre-pausally has applied, the epenthesis rule applies, and then a rule which assimilates velars to a following nasal (see (1d) above) and the inverted version of weakening applies to produce the stops in the left column of (1). Sample derivations for (1a,b) are given in (4):

(4)	/wupe-ro/	/guwi-ro/
i- deletion	---	guw-ro
epenthesis	wuɸe-ro ⁴	---

⁴Leben [1974:269] suggests that "Epenthesis" also converts stem-final *-e* in verbs to *ə* between a stop and a sonorant consonant, thus treating the change here as part of the epenthesis rule. It is not clear that the phenome-

(assimilation)	---	---
strengthening	---	gup-ro

That is, unless there has been rule inversion, the only way of telling whether epenthesis takes place for a given form is assigning an ad hoc marking to each lexical item.

2.2.2. Leben's reply. Leben argues (p. 270) that it is possible to account for the varying susceptibility of stop-sonorant clusters to epenthesis without an inverse rule. He makes use of the same rules as those mentioned in connection with Schuh's argument, except that a rule of weakening replaces Schuh's strengthening. Recall (cf. fn.2) that this process does not affect stops followed by ə or preceded by a short vowel and followed by e . These rules result in derivations such as those in (5):

(5)	/wupe-ro/	/gupi-ro/
l- deletion and epenthesis	wupə-ro	gup-ro
(assimilation)	---	---
weakening	---	---

The isolation form of /gupi/ weakens to guwi, but that of wupe (as in all other forms with stops between a short vowel and e) does not. That is, Schuh's account of the reasons for the behavior of schwa-epenthesis is not the only plausible one.

2.2.3. Discussion. Again the issue of which of these accounts is closer to the truth can be decided by examining the empirical data provided by the phonetic forms of Kanakuru. Though Schuh's and Leben's analyses generate the forms discussed by Schuh, they make different predictions about the behavior of other possible forms. Leben's analysis essentially claims that all verbs with stem-final $-\text{e}$ or with a final consonant exhibit "epenthesis", while Schuh's predicts that verbs with etymological sonorants (which now alternate

na in question can actually be treated as a single rule, however, since there are cases where $-\text{e}$ alternates with \emptyset rather than with ə (cf. fn. 6). A rule of "e-deletion", which may or may not be formally distinct from that of l-deletion, will probably be necessary at this stage, in addition to that of epenthesis.

with stops because of the analogical changes mentioned by Schuh) will not. That is, it is possible that, in addition to verbs with stops inhibited from alternating by a preceding short vowel and following *-e*, there are verbs in final *-e* with etymological sonorants which alternate with stops. Leben's analysis predicts "epenthesis", since the *e* would be between a stop and a sonorant at the point in the derivation at which this rule is applicable, but Schuh's analysis predicts none since at the corresponding point in the derivation *e* would be between two sonorants (see below for an example). The two analyses also make different predictions concerning the behavior of verb stems in final *-e* but with a long vowel preceding the pre *-e* consonant: Leben's implies epenthesis, but Schuh's implies none since the sonorant would be underlying for Schuh, but not for Leben.

Again there are synchronic data from Kanakuru which indicate that Schuh's account is to be preferred over that of Leben. For example, Newman [1974:9] gives the pair *a dowe* 'he tied it' (where the *w* is presumably an etymological *w*, since it is in a phonological environment which prevented weakening) vs. *a dop-taru* 'he (went and) tied it'. Leben's account predicts that the *p-t* cluster should be broken up by epenthesis, but it is not. Schuh's account, on the other hand, predicts no epenthesis, in accord with the Kanakuru facts. Moreover, Newman specifically states (p. 4) that "the invariant voiceless stops [i.e. those never in a weakening environment] ... are ... subject to [epenthetic schwa insertion], while the still unspecified archiphonemes are not." (Archiphonemes are used by Newman to represent the alternating consonants.) That is, etymological sonorants (archiphonemes for Newman) which alternate with voiceless stops will not exhibit epenthesis even in words with final *-e*, contrary to Leben's analysis, but in accord with that of Schuh.

Schuh also suggests (pp. 387-9) that what he terms (following Newman) "plural hardening" provides evidence for rule inversion in Kanakuru. I will not discuss this matter here, since Leben is apparently willing to concede (p. 270) the possibility that morphologized rules such as this one can be inverted.

3. Other Possible Analyses

I have shown that Leben's proposed analysis of Kanakuru is incorrect. This does not imply, of course, that there is no analysis of Kanakuru which does not involve inverse rules and is not contraindicated by the Kanakuru facts. In this section, I will examine the possibility of accounting for the Kanakuru facts within several other recently proposed theories of phonology, most extensively concerning the "'upside-down' phonology" of Leben and Robinson [1977] and the version of "Natural Generative Phonology" advocated in Hudson [1974, 1975, 1980] and Hooper [1976]. I will argue that neither of these theories can provide an adequate account of these facts. I will focus here only on the facts concerning epenthesis in the case of upside-down phonology, since they appear to present the best case against this theory.

3.1. Upside-down phonology. Leben and Robinson [1977] have proposed that phonological rules, contrary to the traditional view, serve only an "interpretive" function, relating phonologically words which are (putatively) morphologically related. Within this theory of "upside-down phonology", words are entered in the lexicon in essentially their surface form,⁵ and rules of the traditional type are "undone" until the words in question are relatable by a morphological rule or rules, subject to the following conditions (cf. Leben and Robinson [1977:2]):

- a. If, in a conventional generative treatment, a form is derived by three rules A, B, C, applying in that order, they apply in our account in the reverse order, C, B, A, except as provided below.
- b. A rule of the form $X \rightarrow [-F]/Y_Z$ is undone by replacing $[-F]$ with $[+F]$ on segment X in the environment Y_Z . Analogously, a rule of the form $\emptyset \rightarrow X/Y_Z$ is undone by deleting X from the context Y_Z .
- c. A rule is blocked if undoing it would not increase the compatibility of forms A and B with respect to Word-Formation Rule R.

Let us now consider how the Kanakuru data might be treated within such a

⁵Leben [1979] has proposed that the level of lexical representation in upside-down phonology should actually be somewhat more abstract, so that it corresponds to that of "natural phonology" (cf. Stampe [1973], Donegan and Stampe [1979]). The difference between this proposal and the earlier one does not appear to be relevant to the present discussion.

theory; given the rules proposed by Leben [1974], let us attempt to relate the problematic forms of section 2.2.3 above in an upside-down fashion. Such an attempt is illustrated in (6):

(6)	Lexical Representations	d ^h owe	d ^h op-t ^h eru	Morphology
	(a) weakening	---	---	-t ^h eru affixation
	(b) (assimilation)	---	---	"
	(c) i- del. and epenthesis	---	---	"

Note that weakening cannot be undone at stage (6a) since it is not applicable in the environment in question, i.e. between a short vowel and e. Thus, these two forms cannot be related using these rules within the theory of upside-down phonology.

There remains the question whether it would be possible to account for these data in an upside-down framework using rules other than those just considered. Such an account is indeed possible using Schuh's rules,⁶ as illustrated in the derivations in (7):

(7)	Lexical Representation	d ^h owe	d ^h op-t ^h eru	Morphology
	(a) strengthening	---	d ^h ow-t ^h eru	-t ^h eru affixation
	(b) (assimilation)	---	---	"
	(c) i- del. and epenthesis	---	d ^h owe-t ^h eru	"

This account, however, makes use of an inverse rule, namely that of "strengthening" (cf. (7a)), and of course Leben's main point is that it is possible to do without such rules.

It might be suggested that a modification of the weakening rule could allow for the desired relating of the forms in question. In fact, within the theory of upside-down phonology, it appears that it would indeed be possible to account for these data without recourse to inverse rules by making the "weakening" rule context-free, i.e. stops are weakened to the corresponding

⁶A change in or addition to the rules used in stage (7c) will be necessary, so that there would be "deletion" of non-final e. Such an alteration would be required in a Schuh-type account as well, however, so this problem is not unique to an upside-down theory. Given this, of course, it would also be possible to restore the e in (6c), but the forms still could not be related.

sonorants in any phonological context. Such a version of this rule seems extremely counter-intuitive (though this is apparently formally irrelevant for the theory—cf. Churma [1981b]). In fact, Leben [1979] has argued, if I understand him correctly, that a context-free (upside-down) rule would be objectionable in a case of this type.⁷

Even if an analysis of this nature were permitted by the theory, moreover, there are other issues which must be considered. In particular, why should a change from the original rule to the context-free version occur? A possible answer to this question is that this is just an instance of rule simplification by elimination of contextual restrictions. If the rule in fact has been simplified, however, then it would be unexpected that the etymological stops which were not caused to alternate with sonorants due to the weakening rule should still show no alternation. What *would* be expected, assuming that the reason for the change which resulted in the bringing in of the historical sonorants to the alternation was the simplification of the weakening rule,⁸ is the complete disappearance of stops in favor of sonorants. This, of course, is not what actually occurred.

A perhaps somewhat more reasonable alteration of the weakening rule would restrict application to *intervocalic* environments. This would allow weakening to be undone at stage (6a) above, and the two forms in question could eventually be related (cf. fn. 6). But this approach would encounter precisely the same type of problem which the previous one did. In this case, it would be only those etymologically intervocalic stops which should change, but again the expected changes do not occur: formerly non-alternating stops are not brought into the alternation, but sonorants are.

Any other alternations in the weakening rule, it seems to me, would result in corresponding difficulties—the alternation is extended only in the

⁷For further discussion of why such rules might be undesirable in upside-down phonology, see Churma [1981b].

⁸This assumption is in accord with the upside-down approach to "abductive change" (cf. Andersen [1973]) proposed in Robinson [ms.], and I can conceive of no reasonable alternative explanation for the extension of the alternation to etymological sonorants within the framework of upside-down phonology.

case of etymological sonorants, and never in that of historical stops which did not originally alternate. Thus, it appears, upside-down phonology cannot provide a reasonable account of the Kanakuru changes unless inverse rules are permitted, since the directionality of the analogical changes cannot be explained.

3.2. Natural Generative Phonology. The term "Natural Generative Phonology" appears to have been used to refer to three quite different theories of phonology. One of these is that advocated by Vennemann in his papers of the early 1970's, e.g. [1972, 1974a], one which is not radically different from "standard" generative phonology, except for rather severe restrictions on abstractness and such matters, and a corresponding preference for considering "restructuring" (cf. Vennemann [1974a]) to have occurred relatively early in historical change. Of particular importance here is the relatively frequent (and controversial) treatment of synchronic phonological systems as involving inverse rules; in this respect, the present paper can be considered as an argument in favor of at least this aspect of this theory.

The second version of "Natural Generative Phonology" to be considered here was also advocated by Vennemann (cf. Vennemann [1974b]). Here, what is proposed (p. 353) is "adopting the hypothesis that the lexicon contains words ..., but no items below the complexity of words, in particular, no roots, stems, or affixes." Rules in this theory (p. 349) "function entirely as redundancy rules for forms already registered in the lexicon, and as generative rules only when unknown words are adapted to the lexicon, or new words are created by the speaker" It is difficult to evaluate this theory with respect to the data at issue, since Vennemann gives no examples of rules within this theory, but assuming that the "basic forms of paradigms" can in fact be identified by the "strategies" referred to by Vennemann (p. 369), and assuming also that the basic forms in Kanakuru contain sonorants under this approach, then it is not clear, as Vennemann himself suggests (p. 349), that this position is empirically distinct from the previous one. Insofar as no such "strategies" can be found, however, it appears that this theory could not explain the directionality of the analogical changes, as was shown above to be the case with respect to upside-down phonology.

The final version of "Natural Generative Phonology" to be considered here has been proposed by Hudson [1974, 1975, 1980] and advocated also in Hooper [1976]. Here each lexical item which is involved in a non-allophonic alternation is marked in the lexicon in some way to indicate that this is the case.

I will first give a simple illustration of the use of this framework in accounting for the alternation in the form of the English indefinite article, and then proceed to an examination of the Kanakuru case. The lexical representation for this morpheme, under this approach, would be $\left\{ \begin{smallmatrix} \text{an} \\ \text{a} \end{smallmatrix} \right\}$. To account for the alternation, rule (8) would be employed:

$$(8) \quad \left\{ \begin{smallmatrix} \text{an} \\ \text{a} \end{smallmatrix} \right\} \rightarrow \left\{ \begin{smallmatrix} \text{an} / _V \\ \text{a} \text{ elsewhere} \end{smallmatrix} \right\}$$

In Kanakuru, we would presumably find such lexical representations as $gu \left\{ \begin{smallmatrix} p \\ w \end{smallmatrix} \right\} l$, $d\alpha \left\{ \begin{smallmatrix} p \\ w \end{smallmatrix} \right\} e$, etc. (these would actually be in feature notation but I will oversimplify here in this respect and others for purposes of exposition). "Rules of phonetic structure" (Hudson [1980:95ff]) would specify the distribution of the segments enclosed in braces in the above lexical representations. Presumably, these rules would be analogous to those in one of the sets given in section 2.2.1, but it is not clear to me which set would be employed within this approach, since both sets of rules are "surface-true" and hence violate no principles of the theory (at least, if the 'weakening' rules are altered to include the schwa complication—cf. fn. 2). I will assume here that analogues to the "strengthening" rules would be adopted, but nothing of importance appears to hinge on this assumption; in the cases at hand, p would occur word-finally and before a consonant, and w elsewhere.

Let us now consider the picture of the historical development of these forms suggested by this account. Recall that at one stage $d\alpha w e$ did not show a stop-sonorant alternation, which would imply that its lexical representation did not have any segments in braces. But with the extension of the alternation to etymological sonorants, the lexical representation changes to $d\alpha \left\{ \begin{smallmatrix} p \\ w \end{smallmatrix} \right\} e$. Thus, in this system, there is a formal increase in complexity in this and other similar lexical representations, despite the fact that the change pretty clearly represented a simplification in the grammar of Kanakuru. Since Hudson [1974] claims that the treatment of diachronic simplifications

as formal simplifications in the grammar is a virtue of his system, it can only be concluded that the failure to give such a treatment in cases like this one constitutes a serious problem for the theory.⁹ It should be clear, then, that the historical developments which have taken place in Kanakuru cannot be given a reasonable account within a Hudsonian version of Natural Generative Phonology—at least, not without incorporating (the equivalent of) inverse rules.

4. Conclusion

I have argued in this paper that certain historical changes in the phonological system of Kanakuru can be explained only if "rule inversion" is a possible mechanism of change in grammars of natural languages. In particular, I have argued that it is not possible to give a reasonable account of these changes within the theory of "standard" generative phonology, that of "upside-down phonology", and those versions of Natural Generative Phonology which do not allow inverse rules. It seems quite likely to me, moreover, that no theory which does not allow for (the notational equivalent of) inverse rules can provide such an account.

What I have not done so far is suggest an explanation for why rule inversion has occurred in Kanakuru, while it apparently has not in other superficially similar cases. This is an important question, since without such an explanation, the account of these historical developments is in some sense circular: if we have no independent reasons for believing that rule inversion did in fact occur, then attributing these developments to rule inversion would appear to be ad hoc. From a synchronic point of view (which is not really that different from the diachronic point of view just mentioned), this is essentially a question of explanatory adequacy, in the sense of Chomsky [1965]; one would like to be able to predict, given a reasonable cor-

⁹Note that *any* extension of an alternation (not necessarily ones involving rule inversion) would present such a problem, since the relevant lexical representations would have to increase in complexity over time. This is not at all uncommon occurrence, as Hudson himself is apparently aware, since he notes (Hudson [1980:121]) that alternations can be "introduced in new lexical items" (although he also states (p.115) that "the fact is that alternations are eventually leveled").

pus, the system which a child will unconsciously posit to account for this corpus. In this case, what kind of corpus will cause a child to come up with a system which entails a rule inversion, as opposed to the kind which will not? It is conceivable that it is impossible to answer this question, i.e. that Chomskyan explanatory adequacy is not in principle achievable (perhaps because different children encounter different corpora, etc.). But given the lack of any evidence that this is in fact the case, this question clearly deserves some consideration.

Unfortunately, there is very little evidence which bears on this issue. Very few putative rule inversions have been described in the literature (and at least one of these is questionable—cf. Klausenburger [1974, 1977, 1978], Churma [1977]), but some of the factors in question seem reasonably clear, and Vennemann [1972, 1974a] has given some attention to them. As Vennemann [1974a:139] points out, his principles cannot always predict rule inversion (since, for example, they may be in conflict), but it seems likely that such factors do play a role in effecting rule inversion. One factor which may have played a big role in the Kanakuru case is mentioned by Vennemann [1974a:139]: this is "the relative predictability of the alternants from each other." Let us briefly look at the Kanakuru case in terms of this factor.

Let us assume, first of all, that the historical change responsible for the synchronic alternations did not involve all of the complications discussed in section 2.2.1 (cf. Newman [1970:43ff] for reason to believe that this is in fact the case). That is, the change weakened non-nasal stops intervocalically, except possibly when the first vowel was short and the second was *e* (cf. fn. 2). Formally, this change would be analogous to rule (2a). However, constraints on abstractness such as those proposed in Kiparsky [1973] would disallow (2a) and the associated abstract geminates as a synchronic analysis for Kanakuru learners as soon as surface geminates had disappeared; the only possible "weakening" analysis would involve the version given in fn. 2. Why was this rule not adopted, i.e. why was there inversion? Note that the environment is fairly complex, and requires the use of both angled brackets and braces.¹⁰ Consider now the form which would be taken by the required

¹⁰It would be possible to do without the braces by employing "class com-

rule if the sonorants are underlying, given in (9):

$$(9) \quad C \rightarrow [-\text{cont}] / \left\{ \begin{array}{c} \overline{C} \\ \overline{C} \\ \overline{\#} \end{array} \right\}$$

The environment in this case seems intuitively to be considerably simpler than in the former case; this is reflected in the formalism, as well, and further formal simplifications are possible if a "mirror image" convention (cf. Bach [1968]) is permitted. That is, it is easier to describe where stops are found than it is to describe the environment for sonorants. Of course, it is not so easy to say *which* stop (if any—some sonorants do not alternate) will be found: the only way to do so is by lexical markings. This suggests a refinement of the principle in question to the effect that the *environment* for changes in synchronic rules has considerable importance as far as rule inversion is concerned. The semantic basicness of the forms in which the sonorants are found was also undoubtedly a factor in this case (cf. Vennemann [1972:237], Hooper [1979]; cf. also Schuh's remarks quoted in section 2.1.1), and it is not unlikely that relative frequency (cf. Vennemann [1972:236]) played a role in determining which particular stop ended up being represented in the alternations.¹¹

Clearly, such discussion does not fully answer the question at issue; we are undoubtedly a long way from an explanatorily adequate theory of (morpho) phonology. This is perhaps not too surprising, given the relatively recent recognition of the possibility of rule inversion and the small amount of attention given to this possibility. What is more, Hudson's [1975, 1980] dis-

plement" notation (cf. Zwicky [1970]), which in this case would yield something like $\vee \left[\begin{array}{c} -\text{hi} \\ -\text{back} \end{array} \right]$ instead of the brace expression. It is not clear to me this is in any obvious sense an improvement, and in any case two notational conventions would still be required, at least one of which is questionable.

¹¹It is worth pointing out that many of the considerations which appear to be relevant as far as predicting rule inversion is concerned do not appear to lend themselves to being characterized in terms of an evaluation measure of the type generally envisioned by generative phonologists. This should not seem particularly surprising, since the necessity of including markedness considerations in such a measure indicates that, if it in fact exists, it will not involve solely simple-minded feature counting.

cussion of the problems encountered by more or less standard generative theories which allow for the possibility of rule inversion seems to me to merit serious attention; if he is correct in his assessment of these problems, then we must be able to predict not only when rule inversion will occur, but also when a Hudsonian analysis should be adopted. But the Kanakuru facts can be seen to indicate that rule inversion is indeed possible, even in purely phonologically conditioned cases which require a stage with "conceptually anomalous" inverse rules. An explanatory adequate theory must come to grips with this possibility, and, eventually, predict when rule inversion will or will not occur.

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