

Taking a False Step*

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1. Background

The question of abstractness considered by Postal (1968, 53-77) under the name of naturalness concerns the character of underlying representations: can elements of underlying representations be of a different nature from the elements of surface representation? A predominantly negative, or concrete, answer to the question says that a language can't be analyzed as having the underlying front rounded vowel y unless it has surface y--unless, in fact, it has some surface y's derived from underlying y. A predominantly positive, or abstract, answer to the question says that the language can be analyzed as having underlying y anyway, certainly if there is multiple justification for it (cf. Kisseberth 1969).

There is another question about sequences of elements in underlying representations: can sequences of elements (phonological segments within a morpheme, for example) be of a different nature from the sequences found in surface representation? Can a language, for instance, be analyzed as having morphemes with the underlying sequence l plus dental in them even if the language doesn't have that sequence superficially?¹

I am concerned here with some cases related to these. In each case, underlying representations are well-formed, in the sense that they contain only segments and sequences of segments that occur superficially, but some derivations go off the rails. This situation occurs in many published linguistic descriptions, both in phonology and in syntax. The Sound Pattern of English (Chomsky and Halle 1968) supplies many phonological examples, and any attempt at a comprehensive syntactic description is full of them.

Thus, we have in Sound Pattern (204) the treatment of words like push, pully, bullock, and full, which are said to have underlying lax u, to undergo an unrounding rule to \dot{u} , and then to be subject to a rule rounding \dot{u} back to u, which is the surface form. At the same time, other words assumed to have an underlying lax u, like pun, undergo an extension of Vowel Shift, which yields lax o, which is then adjusted to \wedge . The intermediate stages \dot{u} (for push) and o (for pun) are neither of them well-formed on the surface (in the dialect Chomsky and Halle are describing). Special rules are required to generate the actual forms.

In syntax, consider Langacker's (1965) treatment of French interrogatives, which posits a rule of Reduplication that takes an essentially² well-formed structure like the one associated with

- (1) Il pleut.
It's raining.

and converts it into the uninterpretable

- (2) *Il peut il.

which is then rescued by an Ellipsis rule, yielding the well-formed question

- (3) Pleut il?
Is it raining?

Let me try to formulate what is common to these, and similar, cases. First, at some stage A in derivations, all representations are essentially acceptable surface forms in the language; that is, there are neither unacceptable elements nor unacceptable combinations of elements.

Second, a rule R_1 applies to the representations at stage A and maps some of them into surface-unacceptable representations at stage B. Call R_1 the background rule and B a false step.

Finally, a later rule R_2 eliminates the unacceptable aspects of stage B, perhaps while performing other operations as well. Call R_2 the rescue rule. Speaking loosely, the background rule turns some good forms into bad ones, and the rescue rule fixes this up.

2. Criticism of the examples

The cases already given are very suspicious ones; it is instructive to see why this is so. First, the case of push. Here there is a special rule, Unrounding, taking underlying lax u to \dot{i} . This background rule has no motivation beyond the forms in question; it is designed to remove them from the domain of Vowel Shift so that there can be surface instances of u. The rescue rule, Rounding, has one motivation beyond the forms in question: it is used to rescue another false step, namely tense \bar{E} derived from underlying lax u in open syllables (194-5), which is diphthongized to $\dot{E}w$, extended to $j\dot{E}w$, and finally rescued by Rounding, giving $j\bar{u}w$. Summarizing, the background rule for push has no independent motivation, and the rescue rule is motivated entirely by two false steps.

The case of pun interlocks with this one, for in the Sound Pattern analysis, every lax u undergoes either Unrounding or Vowel Shift. The background rule for pun is Vowel Shift, some form of which undoubtedly figures in English phonology. What can be doubted is its applicability to one lax vowel in addition to the tense vowels. Vowel Shift must be 'generalized' in a peculiar way to accommodate the pun analysis.

Langacker's analysis of French interrogatives involves a background rule that must play some rôle in a grammar of French: Reduplication is responsible for the occurrence of the pronoun elle in

- (4) Cette femme est-elle folle?
Is this woman mad?

What Langacker does is make this rule absolutely general in questions, so that it has to be rescued by the combined effect of two rules, Pronominalization and Ellipsis:

- (5) Q cette femme est folle
→ by Reduplication
Q cette femme est cette femme folle
→ by Pronominalization
Q cette femme est elle folle [= (4)]
- (6) Q il pleut
→ by Reduplication
Q il pleut il
[not altered by Pronominalization]
→ by Ellipsis
Q pleut il [= (3)]

The weakest point in this analysis is the special pronominalization rule required for (5). It generates simple, rather than reflexive, pronouns, even though it applies within a simple S, and in order to generate alternative forms for wh-questions, as in

- (7) Quel tableau Henri préfère-t-il?
(8) Quel tableau préfère Henri?
[both] What picture does Henry prefer?

it has to be made optional in exactly these environments (which looks like an ad hoc complication of the rule and which goes against our expectations that pronominalization within a simple S will be obligatory rather than optional). Moreover, the Ellipsis rule in (6) lacks independent motivation.

3. Contra false steps

On the basis of examples like these, it would be natural to try to restrict linguistic theory by ruling out false steps entirely--by saying that any description involving a false step is ill-formed. Scattered throughout the literature there are criticisms of false step analyses, so that there is some implicit support for outlawing them.

Thus, Zimmer (1967) expresses some unhappiness with Lightner's (1965) analysis of Classical Mongolian vowel harmony, in which the back-harmonic vowel generated from *i* is *ɨ*, which does not occur in the language and has to be merged with *i* by a rescue rule. Similarly, many people experience twinges when they consider the English rule of Whiz-Deletion, which yields a man sick with envy from a man who is sick with envy. For one-word adjectival phrases the result of

Whiz-Deletion is unacceptable--*a man sick from a man who is sick--and has to be rescued by an Adjective Preposing rule. Some critics have felt that the 'unreal' intermediate stage should be avoided by restricting the application of Whiz-Deletion and by deriving pronominal adjectives directly from relative clauses with predicate adjectives, or by deriving pronominal adjectives from some other source altogether, as suggested by Winter (1965). Indeed, there is surprisingly little hard evidence for Adjective Preposing.

Even if false steps are not explicitly ruled out, we find in syntactic discussions a preference for derivations all of whose steps are 'grammatical', as it is often put.³ The UCLA grammarians, for instance, criticize Rosenbaum's (1967) treatment of sentences like

(9) Bill is said to work hard.

for its (in their words) 'excessively ingenious' derivation (Stockwell, Schachter, and Partee 1972, 531):

- (10) *One says it [for Bill to work hard].
 → *It [for Bill to work hard] is said.
 → *It is said [for Bill to work hard].
 → *Bill is said for to work hard.
 → Bill is said to work hard.

a derivation that is bad right up to the last minute. The UCLA grammarians support a substitute analysis by saying (533) that 'with all but one small set of verbs of this class, all steps in the derivation are grammatical' (and for the three exceptional verbs, among them say, only one step is ungrammatical).

Quite often it is argued that an analysis is good because apparently unmotivated intermediate steps actually have surface realizations. This line of argument is an indirect indication of a prejudice against false steps. A lovely example is Langacker's (1968) treatment of French possessives like ma maison 'my house', which he assumes to be derived through the stages

- (11) la maison [la maison est à moi]
 → la maison qui est à moi
 → *la maison à moi
 → *la maison moi
 +MOD
 → *la moi maison
 +MOD
 → moi maison [= ma maison]
 +MOD

Of this approach Langacker says

Not only is it very economical to derive possessive adjectives in this way; there are compelling reasons why they must be so derived. A number of other possessive constructions

result quite naturally as reflexes of the postulated intermediate stages of the derivation; these appear to be wholly idiosyncratic if considered in isolation from the analysis of possessive adjectives we propose. (56)

The other possessive constructions referred to include

- (12) C'est une maison à moi
It's a house of mine.

for the third line of the derivation,

- (13) Cette maison est la mienne.
That house is mine.

for the fourth, and Old French and Italian constructions for the fifth.

4. Pro false steps

Despite the widespread prejudice against them, I claim it would be wrong to outlaw false steps. Some are bad, some not. In general, it depends on the extent to which the background rule and/or the rescue rule are justified.

4.1. Clear cases

Consider first a large class of cases in which no one has ever criticized false steps. Each of these involves a phonological rule of great generality and regularity which feeds a rescue rule that acts to create sequences pronounceable in the language. The English rules of Auxiliary Reduction and Progressive Voicing Assimilation interact in this way. There is no question about the existence of the background rule, Auxiliary Reduction (which gives contracted forms of is, has, would, had, am, are, and will). In certain cases it creates word-final sequences of voiceless obstruent plus voiced obstruent, which are unpronounceable in English (perhaps universally)--*[kætʒ] from [kæt Iz] cat is, for example. Progressive Voicing Assimilation (which applies also to noun plurals like cats and verb presents like hits) then automatically shifts the false step tz to the correct ts. When the rescue rule shifts not only derived sequences, but also the same sequences across morpheme boundaries in underlying forms, the case is especially strong. If the underlying shape of the English noun plural and verb present morphemes is z (instead of vowel plus z), the Auxiliary Reduction example is of this type. But clear cases abound. In Karok, 'basic y and y are lost when, through morphological processes [i.e., through affixation-AMZ], they come to stand between two short vowels; vowel contraction...then occurs' (Bright 1957, 33). Vowel contraction takes place for original sequences of vowels across morpheme boundaries as well as those derived by the deletion of v and y; and there are no vowel sequences within words on the surface.⁴

Analogous examples in syntax would be reorderings to fit some required surface order. Perhaps the English Whiz-Deletion case is of this sort.

4.2. Syntactic support

One area of syntax in which the possibility of a false step analysis has been much discussed is that of chopping rules (Ross 1967). A chopping rule, which moves a constituent without leaving a trace in its former position, is to be contrasted with a copying rule, which leaves something behind. Although the end products of a chopping rule and of a copying rule followed by deletion would be identical, Ross claimed that only chopping rules were subject to his constraints. It would then be possible to tell, by checking the behavior of a rule with respect to the constraints, which class it belonged to. Note that the application of a chopping rule will not (*ceteris paribus*) lead to a false step, while a copying rule might, if there was a succeeding deletion rule that was obligatory in some of the structures generated by the copying rule. There are no such cases in Ross (1967).

Since 1967 there has been a lively debate⁵ surrounding the possibility that chopping rules might all be eliminated in favor of copying plus deletion, beginning with Sanders and Tai (1972), who argue from data in Mandarin Chinese, English, and Lebanese Arabic. A response by Neubauer (1970) pointed out that some deletion rules were not subject to Ross constraints. Since then Drachman (1970) has attempted to reanalyze Modern Greek reordering transformations as copying followed by deletion, and Perlmutter (1972) has argued in great detail for a copying analysis of French relatives. Perlmutter supports the claim that a relative clause like the one in

- (14) les hommes à qui Marie parle
the men to whom Mary is speaking

has a remote representation like

- (15) *les hommes à qui Marie parle à eux
*the men who whom Mary is speaking to them

which is a false step, since (14) could be derived directly from its underlying structure (as relative clauses were in all early transformational descriptions).

4.3. Fell swoops versus chains

Situations in which unacceptable phonological segments are in question are on the whole less clear than those in which sequences are at issue. The problem here is whether the background rule should be restricted or complicated, or whether it should apply generally and call a rescue rule into play. Consider, for instance, a language

with vowel harmony or umlaut, and with the very common asymmetrical superficial vowel system

$$(16) \begin{array}{ccc} i & & u \\ e & & o \\ & & a \end{array}$$

In such a language, front harmony or palatal umlaut necessarily involves neutralization; if o and a are fronted, they will both be realized as e. Disregarding the rounding feature, should the shift be described as a complication of

$$(17) V \rightarrow [-back]$$

that is, as

$$(18) V \rightarrow \begin{bmatrix} -back \\ -low \end{bmatrix}$$

or should the shift rule retain its generality and feed a neutralization rule

$$(19) \begin{bmatrix} V \\ -back \end{bmatrix} \rightarrow [-low] \quad ?$$

For a nonhypothetical example, consider the four French nasalized vowels, $\tilde{\epsilon}$ $\tilde{ɔ}$ \tilde{a} $\tilde{ɔ}$; there are no surface vowels \tilde{i} \tilde{u} \tilde{y} $\tilde{\delta}$ etc. In the analysis given by Schane (1968, sec. 2.2), the nasalized vowels are derived from oral vowels, so that (as in the previous hypothetical example) some neutralization must occur. Schane assumes a general nasalization rule,

$$(20) V \rightarrow [+nas] / _ \begin{bmatrix} +cons \\ +nas \end{bmatrix} \left\{ \begin{array}{c} \# \\ [+cons] \end{array} \right\}$$

and then reduces the resulting ten-vowel system to the actually occurring four-vowel system by two neutralization rules--

$$(21) \begin{bmatrix} V \\ +nas \end{bmatrix} \rightarrow [+low]$$

which realizes both \tilde{y} and $\tilde{\delta}$ as \tilde{a} , and both \tilde{i} and $\tilde{\epsilon}$ as $\tilde{\epsilon}$; and

$$(22) \begin{bmatrix} V \\ +nas \\ +low \\ +tns \end{bmatrix} \rightarrow [+back]$$

which realizes \tilde{a} and some $\tilde{\epsilon}$ as \tilde{a} . But it would also be possible to complicate the nasalization rule itself and derive the correct outputs (including the correct associations of alternants) in one fell swoop:

$$(23) \begin{bmatrix} v \\ \langle +low \rangle \\ \langle +tns \rangle \end{bmatrix} \rightarrow \begin{bmatrix} +nas \\ +low \\ \langle +back \rangle \end{bmatrix} / - \begin{bmatrix} +cons \\ +nas \end{bmatrix} \left\{ \begin{array}{l} \# \\ [+cons] \end{array} \right\}$$

To my knowledge, no one has suggested this fell swoop treatment, probably because the result is so obviously several rules crammed into one, the sort of rule one would not expect to find in real languages; whereas all of Schane's rules are plausible. But I know of no evidence from within French that would argue for the three-rule solution over the single-rule analysis--no evidence that Schane's rules cannot be ordered together, that they are subject to grossly different conditions on application, that they have disparate sets of exceptions, or the like.

There is at least one phonological problem, Finnish vowel harmony, for which both the fell-swoop and the rule-chain solutions have been suggested in the literature, although the writers do not in fact attempt to justify either treatment. Finnish has three front-harmonic vowels, ä ö ü, three back-harmonic vowels, a o u, and two neutral vowels, i e, which occur with both of the other sets. Suffixes agree in backness with roots. Kiparsky (ms. 1968), attacking an earlier suggestion by Lightner (1965) that backness be a property of roots as a whole (with individual vowels unmarked for backness), points out that roots with only neutral vowels take front harmony, a fact that cannot be explained in Lightner's system. He proposes that vowels in roots all be marked for backness and that suffix vowels not be marked in underlying forms; they harmonize to the last nonneutral root vowel by the rule

$$(24) V \rightarrow [\alpha \text{ back}] / [\alpha \text{ back}] X _$$

(leaving out details not essential to this discussion). (24) generates the back unrounded vowels ī and ē as well as the front unrounded vowels i and e. The false step must be rescued by a neutralization⁶ rule

$$(25) \begin{bmatrix} v \\ -low \\ -round \end{bmatrix} \rightarrow [-back]$$

Rardin (1969) has since pointed out a class of suffixes that have back vowels after neutral roots. He proposes that the suffix vowels as well as the root vowels be lexically marked for backness and formulates the harmony rule as

$$(26) \begin{bmatrix} v \\ \gamma \text{round} \\ -\gamma \text{low} \end{bmatrix} \rightarrow [\alpha \text{ back}] / \begin{bmatrix} \alpha \text{back} \\ \beta \text{round} \\ -\beta \text{low} \end{bmatrix} X _$$

(again ignoring inessential complications), so that no neutralization rule is needed. As it happens, both the Rardin, or fell-swoop, solution and the Kiparsky, or chain, solution is consistent with the facts

discussed. Rardin's preference for the single-rule solution may arise from the fact that his analysis compels him to specify that only nonneutral vowels condition harmony; once the class is mentioned in one part of the rule, it is natural to refer to it in another; in addition, Rardin remarks (230) on the way that *i* and *e* seem to function as a natural class in Finnish.

Fell-swoop solutions will always be at least marginally simpler, in terms of the number of features mentioned, than rule-chain solutions. But the difference between the two formulations will almost surely be less than the 'slack' in present descriptive systems (the indeterminacy in feature-counting due to inadequacies in feature systems and notational conventions and to uncertainty about how to weight different conventions). However, the real issue is the adequacy of the rules, not simplicity simpliciter.

4.4. The Welsh soft mutation.

A ban against false-step analyses would decide the questions in the previous section; but I believe the decision would be wrong in a great many cases. Here I will consider one such case, one similar to those just described. This is the soft mutation in Welsh, a rule that shifts stops in certain environments as follows:²

$$(27) \quad \begin{array}{ll} p \rightarrow b & b \rightarrow v \\ t \rightarrow d & d \rightarrow \delta \\ k \rightarrow g & g \rightarrow \emptyset \end{array}$$

That is, except for *g*,

$$(28) \quad \left[\begin{array}{l} \text{obst} \\ -\text{cont} \\ \langle +\text{vcd} \rangle \end{array} \right] \rightarrow \left[\begin{array}{l} \text{vcd} \\ \langle +\text{cont} \rangle \end{array} \right] \quad \text{in some contexts}$$

If we are not permitted to let (28) take a false step, we must either use nested angle brackets and the feature [segment], as in

$$(29) \quad \left[\begin{array}{l} +\text{obst} \\ -\text{cont} \\ \left\langle \begin{array}{l} +\text{vcd} \\ \langle -\text{ant} \rangle \end{array} \right\rangle \end{array} \right]_1 \rightarrow \left[\begin{array}{l} +\text{vcd} \\ \left\langle \begin{array}{l} +\text{cont} \\ \langle -\text{seg} \rangle \end{array} \right\rangle \end{array} \right]_1 \quad \text{in some contexts}$$

or else state the shift as two rules,

$$(30) \quad g \rightarrow \emptyset \quad \text{in some contexts}$$

plus (28). The fell-swoop treatment in (29) seems to me to be utterly hopeless; probably both of the notational tricks used to achieve this solution ought to be disallowed. I take seriously only the second alternative, (30) followed by (28).

A false-step analysis uses the shift in (28) as a background rule, allowing an intermediate stage γ to be derived from g , and rescues with the context-free rule

$$(31) \gamma \rightarrow \emptyset$$

Now what would make us choose this somewhat abstract analysis over the relatively more concrete solution?

First, there is evidence internal to Welsh, which concerns the intrinsic connection between g -deletion and lenition of the remaining stops. To begin with, an argumentum ex silentio: there is no reason to suppose that the two rules are not ordered together. More important, they apply in exactly the same environments. This fact would in itself carry little weight if it were not that the environments for the rules are a marvel of morphological conditioning. (30) and (28) would have to apply to the initial segment of: a feminine singular noun after the definite article (but not a masculine noun, or any plural), a noun after any one of a list of prepositions, a noun in an expression of time or space, the object of an inflected (but not periphrastic) verb, an adjective after the predicative particle yn, an adjective in the comparative (that is, after the particle cyn or mor), a verb after the negative, interrogative, future, and relative particles (or initially in a clause from which one of these particles has been deleted), and so on.⁸ Consequently, it would be preposterous to treat the two processes as independent.

Second, there is a modicum of cross-linguistic evidence in favor of the false-step analysis. The argument is based on the following hypothesis:

- (32) If a language has a rule of lenition by which underlying g is deleted, then any instances of underlying γ are also deleted.

Solid support for (32) is hard to come by, since verification hangs on finding languages with (a) underlying g , (b) a lenition rule affecting g , and (c) underlying γ . In what follows I rely on the plausibility of (32) and hope that appropriate language data will be forthcoming.

There are two ways in which the postulated linguistic universal (32) could come about:

Most favored segment. There is a type of lenition by deletion. γ is the most favored segment, so that if a language has γ and deletes any segments at all, γ will be affected. The process may be generalized to delete other consonants (e.g., γ , or in the case at hand, g).

Compounding of processes. Universally, there are two distinct types of processes--a lenition of g to γ , and a lenition by deletion of γ . The first process shifts

some or all voiced stops to continuants (perhaps affecting some voiceless stops as well), the second deletes some or all voiced continuants.

The proposals have somewhat different implications, in that compounding of processes predicts the occurrence of languages with only the shifting lenition, besides languages with only the deletion lenition. The most-favored-segment proposal predicts only lenition by deletion, for *g* at least. But the lenition of *g* to γ seems to be even more common than deletion; this is a well-known historical change in English, Greek, and Spanish, among other languages, and there are synchronic gradations of this type in Gilyak and Loma, cited by Ultan (1970). I conclude, tentatively, that $g \rightarrow \emptyset$ always proceeds in two stages, $g \rightarrow \gamma$ and $\gamma \rightarrow \emptyset$. If so, the false-step analysis of the Welsh soft mutation must be the correct one.

Footnotes

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1. On rather weak evidence, I once suggested just such an abstract analysis for certain occurrences of the Sanskrit retroflex consonants (Zwicky 1965, sec. 3.3).

2. The hedge 'essentially' here and in what follows is intended to separate the operation of some rule in question from the effects of other rules not relevant to the issue at hand. Thus, the structure associated with (1) at the point in derivations where Reduplication applies might very well not be 'readable' as (1) because some obligatory rules not mentioned in the text (say, affix placement and number agreement) haven't yet applied. We then ask whether the structure associated with (1) is otherwise well-formed. Compare note 3.

3. Grammaticality of intermediate stages is, of course, not quite the issue here. Rather it is whether an intermediate stage in a derivation would lead to a grammatical output if operated upon only by (independently motivated) obligatory rules other than those in question.

4. My thanks to Lawrence Schourup for pointing out this reference.

5. I am indebted to Ronald Neeld for reminding me of the significance of this literature for the false step question.

6. Kiparsky's article argues against absolute neutralizations of underlying distinctions. Therefore, since *i* and *ĩ*, *e* and *ẽ* are not claimed to be distinct in underlying forms, this neutralization rule is no violation of Kiparsky's principle.

7. The rule also shifts *m* to *v*, but since this alternation doesn't affect the argument, I prefer to ignore it here rather than complicate the exposition.

8. Bowen and Rhys Jones (1960, 166-7) provide a convenient list.

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