

## A Case for Rule Simplification

Devising a theory concerning expectations of grammars in terms of their ability to predict linguistic changes raises the question of evaluation measures.\* The question here concerns the extent to which grammars ought to indicate „possible changes” from  $G_i$  to  $G_{i+1}$ . According to P. Kiparsky (1973): given two versions of linguistic theory,  $LT$  and  $LT'$ , leading to the respective grammars  $G_i$  and  $G_i'$ , and further observing that the following stage has the grammar  $G_{i+1}$  — if  $(G_i, G_{i+1})$  or  $(G_i', G_{i+1})$  are not possible pairs of successive stages, then one can state that  $G_i$  and  $G_i'$  are wrong grammars and  $LT$  and  $LT'$  are wrong theories. „The more sharply we can delimit possible changes, the more compelling we can make this form of argumentation” (258). In specifying possible changes, P. Kiparsky sets out three possible approaches:

- (1) in terms of language states, viz.  $L_p, L_{p+1}$
- (2) in terms of grammars, viz.  $G_p, G_{p+1}$
- (3) in terms of both language states and grammars, viz.  $(L_p, G_p), (L_{p+1}, G_{p+1})$

Now, as to these three possibilities, the first has deficiencies concerning which it is not relevant to go into any detail here. The third approach is, in general, the most efficient through its referring both to grammar and to surface structure. However, it must be pointed out that there are several occurrences of linguistic change — and these are precisely the ones which are clearly rule-governed changes — where the second approach seems to have particular applicability. It is obvious that the more likely the change that is rule-governed, the less relevant it is to consider surface structure forms for the purpose of detecting the characteristics of the change in question. Also, the more general the change — as is to be expected with clearly rule-governed changes — the less will be the likelihood of having to account for a quantitatively significant number of exceptions. It seems then that it would be possible to draw a demarcation line between the kind of linguistic changes examinable on the basis of their rule-governed nature as against the kinds of changes where the theory of lexical diffusion (Wang 1969) might seem a more appropriate approach to account for the nature of the change in question. One of the crucial differences between the presumably rule-governed changes and those that are of different status would be the degree of predictability. That leads us directly back to the assumptions of the second approach according to which,

\* This is an extended version of a paper read at the Central and East European Studies Association of Canada Meeting, Learned Societies of Canada (Saskatoon 1979).

concerning linguistic changes of a rule-governed nature, it ought to be expected and required that  $(G_i, G_{i+1})$  are possible pairs of two successive stages.

In what follows an example will be given of a grammar's  $(G_i)$  failing to account for a phonological change resulting in  $G_{i+1}$ . It will be shown that an alternative analysis of the data is available, on the basis of which  $(G_i, G_{i+1})$  will prove a possible pair. The theoretical implications of such an approach might be bidirectional: (i) the predictability of possible changes could qualify as an evaluation measure; (ii) a non-possible  $(G_i, G_{i+1})$  pair might indicate a wrong account of the data. What follows is a case revealing both implications.

I shall attempt to evaluate an influential approach (Vago 1975) to a frequently-encountered Hungarian phonological phenomenon, on the basis of my observations of a certain phonological change in the speech of English-Hungarian bilinguals.<sup>1</sup> It is my contention that Hungarian Rounding Harmony (RH) functions as a sub-part of a complex Vowel Harmony rule (VH), contra to the above mentioned approach. If we consider RH rule as an angled bracketed sub-part of a complex VH rule, we may look upon the process of the disappearance of RH rule in cases of imperfect language performance as rule simplification. To view RH as an independent rule, it would have to be regarded as rule loss in those cases. To account for the rule loss case would mean considering two possibilities, both of them implausible: (i) accepting Vago's solution would leave the change from  $G_i$  to  $G_{i+1}$  unmotivated, in fact, highly implausible (see in detail later); (ii) assuming the correctness of a similar solution operating with two separate rules, though with underlying representatives different from those of Vago, would not truly reflect the essence of the relationship between VH and RH rules (Aoki 1968, Rédei 1977, Szépe 1958).

In his Hungarian Generative Phonology R. Vago provides a thorough analysis of Hungarian phonological facts and processes, and he also devotes much attention to a very detailed analysis of vowel harmony.<sup>2</sup> His undoubtedly very original approach to the phenomena of Hungarian vowel harmony has been the subject of significant commentary and discussion, most notably in LInq (1978).<sup>3</sup> However, the question I will discuss here has not been previously raised as far as I am aware. It seems that hitherto all discussions concerning Hungarian VH rules have tended to concentrate on the following issues: abstract versus concrete analysis; Vago's proposal for two separate VH rules (both designed to take care of palatal harmony); and the question of the VH rule's domain. The other aspect of vowel harmony, namely the status and nature of labial harmony, is presented as offering no problem in Vago's analysis. According to him, it seems most appropriate to treat it as a separate rule.<sup>4</sup> Thus he states: „I see no point in collapsing VH and RH” (1975, 57). Although he does indicate that there may be a possibility of col-

<sup>1</sup> Since 1971 I have been associated with the Hungarian community in Winnipeg (Manitoba), where I have been able to observe several cases of imperfect language usage which might be deemed noteworthy linguistically. The present paper is a preliminary study based upon one such case.

<sup>2</sup> See Chapter II and III (pp. 6–85).

<sup>3</sup> The discussion contains the challenging articles by J. Jensen (1978), E. Phelps (1978), C. Ringen (1978), and Vago's reply (1978).

<sup>4</sup> In Chapter III, 3.7. (pp. 56–8).

lapsing the two rules, that it is not a recommendable solution is nevertheless clear when he writes: „These two rules, taken by themselves, are natural assimilation rules. The angled bracket abbreviation obscures this naturalness and makes VH—RH a complicated rule. Surely, the angled bracket convention is illegitimate here (58). Now to go into somewhat greater depth on this point, I should emphasize that I consider that VH is indeed a complex rule which does have the angled bracketed sub-part. The complexity of the rule can be proved by: first, challenging Vago's own proof-material, showing that this does not represent an appropriate enough argument for rejecting a putative VH—RH rule; second, by applying as evidence the phonological change observed in the speech of Hungarian-English bilinguals.

According to Vago's analysis, the basic rules of Hungarian vowel Harmony are as follows. He posits two separate vowel harmony rules, labelling them VH-rules, though these actually refer to palatal harmony. One of these VH rules is marked (that is, it skips over the two neutral vowels, *i* and *e*), the other is unmarked. According to his analysis only suffix vowels are subject to the VH-rules, that is subject to either the marked or the unmarked VH-rule. Thus Vago's claim is that VH-rules do not operate within roots. Although quite a major controversy surrounds Vago's analysis in these two respects, we need not go into it here. That he splits the VH-rule into three parts (two separate rules, marked and unmarked, plus another rule requiring information concerning MSC)<sup>5</sup> may be considered a weakness in his analysis. Although these problems are not completely solved, there have been several suggestions concerning how to formulate Hungarian VH rules<sup>6</sup> ensuring that the problems of palatal harmony are sufficiently well-known. The rule governing palatal harmony in Hungarian is as follows:

$$(1) \quad [+syll] \longrightarrow [\alpha \text{back}] / \left[ \begin{array}{c} +syll \\ \alpha \text{ back} \end{array} \right] C_0 \text{ — } ^7$$

<sup>7</sup> Vago's 3.32b.

It will not be necessary to go into an analysis of those numerous cases when this generality does not hold. This rule takes care of morphemes like *asztalnak* ('table' SgDat.), *széknek* ('chair' SgDat.), *hajóból* ('boat' SgElat.), *kertből* ('garden' SgElat.) etc. Here the alternating suffixes are *-nak/-nek* *-ból/-ből*. There are many suffix pairs like these, having one front and one back alternate, the most common of which include *-ban/-ben* (Iness.), *-ba/-be* (Illat.), *-ból/-ből* (Elat.), *-ról/-ről* (Abl.), *-ra/-re* (Sublat.). These suffixes join onto the root according to the requirements of the rule (1), unless the roots are subject to controversial status; but as those particular „exceptions” are not relevant to the present discussion I will say no more about them here.

Now, there are several suffixes which have three instead of two alternants: suffixes which have short mid-vowels. Examples:

<sup>5</sup> For a critical evaluation, concerning the some fifty monosyllabic roots requiring an abstract underlying representative as a consequence of their MSC (Vago does not sufficiently specify what this special MSC consists of), see E. Phelps (1978) and C. Ringen (1978).

<sup>6</sup> See L. Rice (1970), and also see fn. 3.

*-tok/-tek/-tök* (Ind. Prs. Pl. 2.)    *-hoz/-hez/-höz* (All.)

*mostok* 'wash'

*vertek* 'beat'

*törtök* 'break'

*várhoz* 'castle'

*képhez* 'picture'

*könyvhöz* 'book'

Vago believes that the *o/ö/e* alternation has to be derived from underlying */o/* or underlying */ö/*. To motivate the exclusion of the possibility of an underlying */e/*, he uses evidence from other alternation-pairs, namely suffixes which have alternants with *a* and *e* such as *-nak/-nek* (Dat.), e.g. *labdának* 'ball', *könyvnek* 'book'. His argument is that because the front vowel *e* underlies these alternating forms, it follows that it cannot underlie the *o/ö/e* alternations. He uses the empirical evidence of the independent forms *velem*, *veled* 'with me, with you' etc. for positing underlying */e/* for the alternation forms with *a/e*. But here two problems arise. First, should */e/* not underlie the *o/ö/e* variations simply because it already plays a similar role in another alternation type? In my view it would be more convincing to suggest a similar pattern of derivation from an underlying */e/* — once it appears on the surface as one of the front alternants — than to posit underlying */ö/*, which, except in one rather doubtful instance (to which I shall refer later), never actually appears as an alternant in any of the suffix-pairs. Second, although it does sound convincing to posit underlying */e/* for alternations of *a/e*, nevertheless the independent morphemes do not in every case show that */e/* is the only possible candidate for the role of underlying phoneme in all *a/e* alternations. Consider for example the suffix *-ra/-re* (Sublat.). The independent form is *rajtam*, *rajtad* 'on me, on you' etc. Thus, if we regard the quality of the independent forms as a deciding factor in choosing the underlying form, then *a* also has an opportunity to be considered the underlying phoneme. It must be said though that this is more often the case when *a/e* alternating suffixes have *e* as the characterizing vowel in independent morphemes. Nevertheless in Vago's analysis concerning choosing the underlying representative it does seem that he gives preference to a vowel that never appears in an independent morpheme in any alternation pair in the system over another vowel (*e*) that does have a significant role elsewhere. This is despite the fact that its appearance in the independent forms is the criterion for determining the selection of the underlying phoneme. If *a* is rejected as the vowel likely underlying the *a/e* alternation, then on what grounds would *ö* be preferred as suppressing *e* in the *o/ö/e* alternation? Briefly then, through this reference and my comments upon the possible underlying representative of the *a/e* alternations, I seek to emphasize that it is by no means clear that Vago has succeeded in establishing the kind of hierarchy which would put the pair *o/ö* above the unrounded third alternant. The consequences of this approach are serious for the realization of palatal harmony.

Vago has demonstrated the two rules — the Rounding Harmony rule and the *e*-adjustment rule<sup>8</sup> (both of which would apply after the VH rule has applied) — in the following way. Supposing that */o/* underlies the *o/ö/e* alternations — and this, on the basis of the independent form of the allative

<sup>8</sup> Because the output of RH is realized as an unrounded mid-vowel, another rule: namely the *e*-Adjustment rule, is needed to lower it to [e]. The *e*-Adjustment rule is Vago's 3.23 rule.

suffix appears correct (*hozzám, hozzád* 'to me, to you' etc.) — first /o/ gets fronted to *ö* after a front vowel (remaining back after a back vowel); then if a preceding vowel is unrounded it becomes *e*.

Now, before we confront this approach to RH with the kind of empirical evidence mentioned earlier, let us look at Vago's other argument against collapsing VH with RH. He considers *-nok/-nök* as derivational suffixes, although he admits that they are not listed as such in Tompa's grammar.<sup>9</sup> The examples Vago lists are of doubtful status, because they were either created artificially by applying the particular method of adaptation during the late 18<sup>th</sup> and early 19<sup>th</sup> century language reform, or were coming into restricted use on the analogy of these.<sup>10</sup> These are the only such structured morphemes, and thus constitute the only evidence for *o/ö* showing on the surface with the exclusion of *e* (which occurs whenever rounding harmony has to be applied with the three alternants *o/ö/e*). Therefore in view of the above the *-nok/-nök* alternation pair is of doubtful status and as such does not support the argument which claims that *ö* has a higher rank than *e*.

Let us now look more closely at the derivation Vago proposes and try to comment upon it on the basis of the tests I conducted among my bilingual subjects. Vago asserts that after the VH rule has been applied, the result will be *o* after a back vowel and *ö* after a front vowel. If the preceding vowel is unrounded, then this *ö* becomes unrounded to *e*. To demonstrate how his derivation works we shall consider the following example:

	/mos + tok/	/ver + tok/	/tor + tok/
VH	mostok	vertök	törtök
RH	—	vertek	—
e-Adj.	—	vertek	—
	[mostok]	[vertek]	[törtök]

The formalization of the RH rule is as follows (Vago's 3.20.):

(2)

$$\left[ \begin{array}{l} +\text{syll} \\ -\text{back} \\ -\text{high} \\ -\text{low} \\ -\text{long} \end{array} \right] \longrightarrow [-\text{rnd}] / \left[ \begin{array}{l} +\text{syll} \\ -\text{rnd} \end{array} \right] C_0 \text{ —}$$

(*ö* becomes *e* if the preceding vowel is unrounded.)

If this type of derivation is correct it follows that in the course of imperfect language performance there is more of a chance that the *mostok*,

<sup>9</sup> J. Tompa (1970).

<sup>10</sup> G. Bárzsi—L. Benkő—J. Berrár (1965. 73—74): „Adaptációval keletkezettnek tekinthetjük a *-nok, -nök* képzőt; ez ugyan a szláv nyelvekben is hasonlóan foglalkozásnévképző *-nik* hangalakban; de a magyarba nem közvetlenül a képző került át, hanem több ilyen szópár, mind *udvar — udvarnok, pohár — pohárnok*. Ezekből vonták el aztán a közös végződést, sajátos képzőszeretpet értve rá: *mérnök, szónok, elnök* stb. A jövevény képzők átvételét tulajdonképpen mindig az adaptáció egy tökéletlen esetének tekintetjük” . . .

*törtök* kind of strings will be achieved more naturally. However, once an assimilation rule does not seem to be functioning in exact accordance with the requirements of the rule, one may suspect that the rule is marked. In the above derivation the unrounding process has to be regarded as marked. Thus one would expect the *vertök*, *törtök* forms to surface in the case of imperfect language learning. However, I have not observed this to be the case among my sample of bilinguals. They have never produced forms like \**vertök*. On the contrary the overwhelming majority of my tests and recordings of spontaneous talk showed them using strings such as *törtök* (Ind. Prs. Pl. 2.) and the like. The following are some typical examples taken from my material:

*főzek* 'cook' (1Sg.), *töltek* 'fill' (1Sg.), *jövek* 'come' (1Sg.) etc.,  
*föztek* (2Pl.), *fésültek* 'comb' (2Pl.), *jöttek* (2Pl.),  
*lőttek* 'shoot' (2Pl.) etc.,  
*könyvhez* 'book' (All.), *kőhez* 'stone' (All.), *tűzhez* 'fire' (All.) etc.,  
*tüzen* (Adess.), *földen* (Adess.) etc.

It should be noted that the very few cases when some of the informants did seem to apply the rule correctly involved only some of the most frequently used words. The fact that only the forms (*ti*) *törtök* 'you (Pl.) break', (*ti*) *lőttek* 'you (Pl.) shoot', *kőhez* 'stone' (Sg.All.), *söprűhez* 'broom' (Sg.All.) etc. and never the \**vertök* 'beat' (P12), \**keltök* 'get up' (P12), \**néztök* 'look' (P12) etc. forms appear, lends great weight to the argument challenging the validity of the kind of derivation demonstrated above. As Vago goes on to mention — and in this he is on firmer ground — both VH (that is, palatal harmony) in this he is on firmer ground — both VH (that is, palatal harmony) and RH rules are natural assimilation rules. Then how can we account for the fact that VH appears the more stable, indeed the kind of assimilation rule which is applied by my bilingual subjects without any exceptions? (I do not refer now to the kind of controversial instances of palatal harmony referred to earlier.) Why would the other natural assimilation rule (RH) be less stable? The very fact that VH always occurs whereas RH does not occur at all — or at the most may occur optionally in only some of the most frequently used strings — suggests that the relationship between the two rules is more than just structural. It seems very likely that RH is a sub-rule of the main vowel harmony rule, and that we are witnessing a case of rule simplification. More specifically, it seems very likely that the sub-rule part of the VH rule — which is formalized with the angled bracket convention — is the one which is not being completed during the application of the complex rule. It is this which is the more likely to be dropped when the natively-acquired language fails to obtain sufficient native-like reinforcement (as is in fact the case in a languages-in-contact situation). Application of the angled bracketed part of the rule results in RH; once RH no longer appears on the surface it will follow that this particular sub-rule is dropped from the complex rule. Then only the remaining part of the VH rule (the palatal harmony rule) applies.

Let us now propose a different approach concerning how to handle VH and RH on the basis of the above examples of bilinguals' usage. A possible way of collapsing the two rules is formulated by Vago as follows (his rule (3.24)) — which however, as previously mentioned, he rejects on the grounds that it is an unnecessary complication of two simple assimilation rules:

(3)

$$[+syll] \longrightarrow \left[ \begin{array}{l} -back \\ \langle -rnd \rangle \end{array} \right] / \left[ \begin{array}{l} +syll \\ -back \\ \langle -rnd \rangle \end{array} \right] C_0 \text{---} \left\langle \begin{array}{l} -high \\ -low \\ -long \end{array} \right\rangle$$

What this rule means in terms of derivation is that, first of all, palatal harmony applies, fronting the suffix vowels when required; then afterwards, if the preceding vowel is unrounded, the suffix vowel becomes unrounded too. Vago's rule presupposes of course that VH would derive rounded suffix vowels, and RH would then make the front vowels unrounded after a preceding rounded vowel.

Now whether we treat VH and RH as separate rules or collapse them in the way Vago suggests, the course of the derivation will be similar. The key issue here is two-fold: (1) the above empirical evidence does not support the idea that palatal harmony will produced an o/ö pair; (2) the lack of application of RH in the case of language deficiency proves that there is no motivation for separating the two vowel harmony rules in question (Vago's preference). I do not find that Vago provides a convincing enough argument to make the derivation presented above seem plausible. Certainly such a derivation is not supported by my records of relevant data supplied by these bilingual subjects. But on the other hand, if we were to suggest that the realization of palatal harmony is o/e pair and in the case of a preceding rounded vowel e becomes rounded, then the rule would have to be rewritten as follows:

(4) VH (PH—RH)

$$[+syll] \longrightarrow \left[ \begin{array}{l} -back \\ \langle +rnd \rangle \end{array} \right] / \left[ \begin{array}{l} +syll \\ -back \\ \langle +rnd \rangle \end{array} \right] C_0 \text{---} \left\langle \begin{array}{l} -high \\ -low \\ -long \end{array} \right\rangle$$

A sample derivation is the following:

	(mos + tok)	(ver + tok)	(tör + tok)
PH	mostok	verték	törtök
RH	—	—	törtök
e-Adj.	—	verték	—
	[mostok]	[verték]	[törtök]

The first part of the complex VH rule accounts for the palatal harmony. The second part using the angled bracket convention accounts for the sub-part of the complex rule, i.e. the rounding harmony. Thus to summarize the structure of the rule: the complex VH (Vowel Harmony) rule consists of two parts: PH (Palatal Harmony) and RH (Rounding Harmony) rules.

Hence I would argue that the foregoing data, demonstrating the phonological change in question, certainly indicate (i) the complexity of VH rule, and (ii) the psychological reality of an underlying o/e pair for RH. Accordingly an analysis embodying these two assumptions should be able to provide

a possible (Gi, Gi+1) pair. Further, in consider that confirmation of the high probability of (i) and (ii) is provided by diachronic evidence which definitely supports the probable relationship between palatal harmony and rounding harmony. In Szépe (1958. 106) it is stated: „A labiális illeszkedés már sokkal ritkább és a történeti adatok tanúsága szerint későbbi keletkezésű, mint az előző típus [i.e. the palatal harmony]. A labialis illeszkedés csak olyan nyelvekben fejlődött ki, amelyekben a palato-veláris 'alapforma' már megvolt.” The same concept is expressed in Rédei (1977. 170) where he traces the history of palatal and labial harmony in Uralic and Turkic families. There he states very explicitly that the latter appeared at a much later period: „Die Labialharmonie ist nicht bis in uralisch/finnisch—ugrische Zeit zurückführbar. Im Ungarischen wie Tscheremissischen ist sie aller Wahrscheinlichkeit unter türk. Einfluß entstanden.”

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