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Lightfoot, David. Review of S. J. Keyser & W. O'Neil *Rule Generalization and Optionality in Language Change*. *Language* 63.1: 151-55 (1987).

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prominently mentions the single occurrence in the entire Hittite corpus of an assimilation between a final nasal and a following initial stop (/halkin pianzi/ written once as ⟨hal-ki-im pi-an-zi⟩ 'they give grain') as evidence for the existence of sandhi. This surely gives a mistaken impression about Hittite and about sandhi in general: is such an assimilation really so unusual from a phonological standpoint? The example most likely shows a fast-speech phenomenon which happened (accidentally) to be recorded by a scribe; what is interesting about it is that such assimilations were usually suppressed graphically. Curiously, B completely overlooks the widespread occurrence of similar assimilations in ancient Greek inscriptions (and so says nothing about 'sandhi' in Greek!)

CHAP. 13: MINOR INDO-EUROPEAN LANGUAGES. Pp. 165 ff.: The treatment here is most uneven, with notable omissions both of languages (e.g. Ancient Macedonian) and references (e.g. Katičić 1976 on the ancient languages of the Balkans). P. 168: Curiously, in view of B's separation of Baltic and Slavic elsewhere, he here lists it as a single IE subgroup.

I have been quite negative—but of necessity, since Baldi has not been very successful at what he set out to do. He does end up providing a survey of the major branches of the IE family, but the material included in each chapter must be approached with extreme caution. However, the concept of the book itself is not flawed; a good survey of this sort could indeed be usable in the ways intended. Thus a competent revision could well yield a useful work. However, given the rather large number of problems with the book in its present form—the listing above is far from exhaustive—one cannot be too sanguine about the prospects of a successful revision.

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Rule generalization and optionality in language change. By S. J. KEYSER and WAYNE O'NEIL. (Studies in generative grammar, 23.) Dordrecht: Foris, 1985. Pp. vii, 153. f 52.00.

Reviewed by DAVID LIGHTFOOT, *University of Maryland**

Scratch a historical linguist and you are likely to find a belief that languages are moving inexorably toward or away from a state of paradise. Of course, it takes a scratch, because such a belief is rarely worn on the sleeve. Nonetheless,

* Thanks to Elan Dresher, Norbert Hornstein, and Jean-Roger Vergnaud for discussing an earlier version of this review. Discussion, of course, does not necessarily imply agreement.

it is frequently claimed that change shows a general directionality, that languages are gradually becoming less marked, that they are showing greater 'typological consistency', or that isomorphism between form and meaning decreases or increases. Such is paradise.

O'Neil has produced a lot of interesting work over the years on the phonological, morphological, and syntactic changes that English has undergone; and he has now collaborated with Keyser to produce a book committed to one central idea. After examining many different phenomena, they 'find that the same direction of change is taken by the language: namely surface allomorphy which exists at a prior stage is eliminated at the subsequent historical stage' (p. 3). They find a tendency whereby 'rules generalize and optional rules become obligatory', and they go on to explain this through U[niversal] G[rammar]: 'whenever possible the language acquisition device reduces the level of optionality, either by change of status or by rule loss ... [W]hen such change in rule status takes place, the result ... can lead to significant reanalysis in later stages of the language.' K&O also build into the theory of grammar the following S[tructural] H[ierarchy] P[rinciple]:

- (1) Whenever a paradigm contains more than one stem type on the structural hierarchy, reanalysis is based on the least marked type available.

They also state a hierarchy, based upon the structure of the syllables that characterize a stem, which arranges the stems from more to less marked as follows (with examples):

- (2) a. $\begin{array}{c} R \\ | \\ wi(n)- \end{array}$
- b. $\begin{array}{c} RR \\ || \\ wine- \end{array}$
- c. $\begin{array}{c} R \\ \swarrow \searrow \\ cyn(n)- \end{array}$
- d. $\begin{array}{cc} RR & R & R \\ \swarrow \searrow & \swarrow \searrow & \swarrow \searrow \\ wiite-, & heafud(d)- \end{array}$

So stem allomorphy typically comes to be replaced by uniformity. Together, 1 and 2 are intended to explain which allomorph is generalized; so lower positions in 2 are favored over higher positions.

K&O's strategy identifies a historical tendency whereby optional rules are lost. They explain this tendency by framing the evaluation metric so that grammars with optional rules are not as highly valued as those without such rules. I shall first sketch the cases they discuss, and then examine the claims about UG.

One example of an optional rule which became obligatory is Old Kentish vowel syncope, which accounts for forms like *deem(e)st*, *frem(e)st*, and *help(e)st*. The rule eventually became obligatory, and *deemst* etc. were the only surviving forms:

- (3) $V \rightarrow \emptyset / \text{ — } C_1]$

where this rule is assumed to be morphologically restricted to strong verbs, and to Class I weak verbs in the singular forms of the present tense indicative.

This analysis has some striking features. First, the rule is complex enough to lead a syntactician, used to thinking in terms of rules like 'Move α ', to wonder how the restrictions could be attained by a child without access to negative data—and thus whether the rule could ever exist. Second,

the change whereby 3 became obligatory is said to be an innovation in the ADULT grammar (30). Third, the factual basis is flawed; the reader is told that the paradigms resulting from the generalization of a rule of high vowel deletion, the new obligatoriness of 3, and the resulting reanalysis 'are not attested in the Kentish texts. However, we suspect that a careful re-examination of earlier Kentish records might very well uncover evidence consistent with the data [in those paradigms]' (26).

A second piece of evidence which K&O offer for their central claim concerns the evolution of the OE strong nominal paradigms; this is where the SHP plays a role (Ch. 3). They point out that it is unlikely that all forms of a particular paradigm will be found in the Anglo-Saxon corpus; consequently, the rich set of paradigms traditionally assumed is often based on comparative evidence from other Germanic languages and is hardly beyond question, even for attested stages of the language. However, the earlier stage for the changes discussed here represents 'the prehistoric picture provided by the handbooks', not attested forms. K&O take the traditional paradigm set to represent a prehistoric stage of Old English, and they discuss the putative changes which led to a simpler system. Saying that the traditional paradigm set is prehistoric, of course, does not preempt the question of whether it could ever have existed in that form. K&O offer some insightful comments on the traditional organization of OE nominals; but their account of the changes is hard to evaluate because it depends on the validity of the reconstructions, which are not discussed.

Further evidence (Ch. 4) deals with the evolution of the plural rule, by which the new system minimized optionality and maximized simplicity. Here K&O argue that the developments were set off by contact between Middle English and Anglo-Norman. The epenthesis rule of modern English came into the language from Old French, through Anglo-Norman; and by the time of Chaucer, the epenthesis rule applied with much variability. This was eventually eliminated and the phonology was consequently simplified.

Ch. 5 postulates a rule of Weak Foot Drop which accounts for alternations like *haali* vs. *haalie* 'holy' and *fondin* 'to try' vs. *luvien* 'to love'; the rule deletes the *e* from the relevant forms. It is taken to apply optionally in the Vespasian Psalter but subsequently became obligatory in certain 13th century West Midland texts, and took on a more general form in other dialects.

The remaining two chapters assume that UG assigns a low value to optional rules—examining some consequences, and essentially arguing against postulating *prima-facie* optional rules in contexts where the optionality hypothesis would predict that they could not survive. K&O cut too many corners, and their discussion will not impress philologists; thus they simply assume, without discussion, that 'the traditionally accepted distinction in Old English between short and long diphthongs does not hold at either the underlying or surface levels of Old English phonology' (127). This is not necessarily an unreasonable position; but it would have been useful to discuss the contrary position, which is widely held.

Another omission, however, vitiates the central claim of the book: nowhere do K&O justify postulating optional rules to account for stem allomorphy. The fact that certain people say *tomeyto* and others *tomahto*, or that some said *haali* and others *haalie*, does not motivate optional rules if one acknowledges that there is no such thing as a grammar of English codifying such alternations, but only various grammars of different English speakers, differing to some extent. It is likely that such allomorphy is not manifested in any individual's grammar. Even in the case of an Englishman living in the US who may use both forms of *tomato*, depending on circumstance, it does not follow that his grammar has a rule relating the two forms. It is even harder, of course, to claim that the grammars of some English speakers 600 years ago had such rules. Mere occurrence of multiple forms in a single text does not force the

conclusion unless one shows that those multiple forms do not represent the usual effects of textual transmission, different spelling conventions etc. This is particularly true for the kind of phenomena discussed here. These questions are worth raising, because if K&O are right in assuming that rules may be optional or obligatory, then one must ask how language learners acquire such distinctions without access to negative data. Such considerations have led syntacticians to view all rules as optional, and to postulate well-formedness constraints for phonological and logical outputs. Alternatively, phonological theory might allow only obligatory rules; but acquisitional problems—not necessarily insuperable, but certainly worth discussing—arise if the theory is loose enough to make both types of rules freely available.¹

Even if it were demonstrated convincingly that some optional rules applied obligatorily in later grammars, this would not motivate adding a statement to the evaluation metric that optional rules are low-valued and so are gradually eliminated. What would be needed would be a demonstration that some generation of speakers had an obligatory rule, despite exposure to THE SAME CHILDHOOD EXPERIENCE as earlier speakers for whom the rule applied optionally. That is hard to imagine. Failing such a demonstration, one is free to attribute the new grammar to a different childhood trigger experience. Then the question becomes: Why did the childhood experience change for the two generations?

If one asks why the inventory of expressions that a child might hear could differ from one generation to another, the answers are diverse. It might result from new contact with another language, as K&O argue interestingly for English pluralization processes, or from the introduction of novel forms for internal, expressive reasons (see Lightfoot 1981). We know that new forms can be introduced into a language, that they may under certain circumstances help to trigger some new grammatical process; and this might include novel allomorphs. It is then likely, but by no means necessary, that one or another form will eventually spread and supplant the other. Surely there is no reason to suppose that this process reflects a genetic principle which predisposes us to eschew optional rules where possible. If alternating forms are robust in a child's environment, then multiple forms will be attained, as K&O show.

For a person interested in discovering something about genetic information relevant for language acquisition, the nature of language change is interesting in that the clustering of diachronic phenomena may help to define the parameters of UG more accurately. (This is to view language change as a particular kind of comparative grammar, where the trigger experiences of two generations differ only minimally, but yield different grammars.) In addition, the existence of 'drag chains' and the effects of heterogeneous trigger experiences (where,

¹ The acquisitional problems would be changed if there were a principled distinction between obligatory and optional rules, such that rules of a certain form are necessarily of one type. K&O in fact suggest this with respect to two rules collapsed by parentheses: there the shorter of the two rules is necessarily optional, and the longer is obligatory (15). For useful discussion, see Dell 1981.

Although there is no such thing as 'the grammar of Old English', the notion is sometimes invoked as a convenient abstraction, in much the same way that one speaks of the French liver, which also does not exist in any real sense. But one must beware when the abstraction becomes misleading.

for example, some expressions become obsolete, i.e. are heard but not perpetuated) can also be instructive. However, this does not mean that, if some general historical tendency is observed, it should automatically be attributed to the effects of UG—or least of all that UG should incorporate essentially a summary statement of the historical facts. There is no reason to suppose that this is a reliable mode of discovery for genetic principles of grammar.

Lamarckism has exercised a powerful hold over biologists, who are often trapped into explaining evolutionary developments by taking acquired features to be inherited. Similarly, historical linguists often succumb to the temptation to see a general directionality to change, and to explain this either by invoking laws of history (see Lightfoot 1981) or by attributing historical effects to genetic predispositions. The particular case that Keyser & O'Neil offer is especially strange. They believe 'that the elimination of optionality constitutes a contribution to a phonology's simplification' (86), and they build a statement into UG predisposing us against optional rules. But for optional rules to be lost, they must first be introduced; if we are predisposed not to attain optional rules, one wonders how they would be triggered in the first place.

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English word stress: A theory of word-stress patterns in English. By IVAN POLDAUF. Edited by W. R. LEE. Oxford: Pergamon, 1984. Pp. ix, 169. £11.40.

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This monograph will be useful to scholars who are trying to formulate an account of stress placement in English words, since Poldauf investigates a large amount of data, and many of his generalizations are of value. However, in spite of the subtitle, P does not present a satisfactory theory.

There have been roughly two approaches to an account of how English words are stressed. One, best exemplified by Kingdon 1958, is based on orthographic forms, and relies heavily on recognition of affixes. The other—the generative approach of Chomsky & Halle 1968, Halle & Keyser 1971, Halle 1975 and their followers—assumes the existence of abstract forms, 'underlying representations', on which stress rules operate. P's work falls into the former group. In an unnumbered chapter, 'The state of the art: Approaches to the problem', he takes pains to distance himself from the Chomsky–Halle–Keyser approach, though he does not actually explore its theoretical basis. Chap. I, 'Introduction', discusses the physical nature of stress and intonation; Chap. II, 'Derivation', the 'stress-determining initials' and 'stress-determining finals' of English. Chap. III, 'Composition', and Chap. IV, 'Compounds', treat the stress