



The Indo-European Drift and the Position of Hittite

Henri Wittmann

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consonants as given by Chomsky and Halle are essentially correct, any problem concerning lexical representations which involves a choice among two or more of the segments /p t k s n/ could not be resolved on the basis of the markedness of the segments in question, since all five of these consonants are marked for just one feature (cf. the table on p. 412 of Chomsky and Halle 1968). It may of course be the case that one of the competing forms can be chosen on other grounds, but that is not the point at issue.

An instance of indeterminacy involving vowels can be cited from Turkish. Of the eight short vowels of Turkish, /i e ü ö ï a u o/, six participate in vowel alternations in suffixes: /a/ alternates with /e/, and /i/, /ü/, /ï/, and /u/ alternate with one another, the choice of a particular vowel being—in general—determined by certain features of the immediately preceding vowel. In the case of the /a/ ~ /e/ alternation, considerations of markedness would lead to the selection of the /a/-variants of suffixes for their lexical representations; thus the plural suffix /+lar/ ~ /+ler/ would be represented as /+lar/, with this underlying form being changed to /+ler/ by a phonological rule. For the other alternation, however, no unique choice emerges; /i/ and /u/ are less complex than /ü/ and /ï/, but are of an equal degree of complexity with respect to one another (both are assigned the complexity measure 1; cf. Chomsky and Halle 1968:409). Thus considerations of markedness do not tell us whether a suffix that is subject to the high vowel alternation should be represented with /i/ or /u/. One might of course argue that the marking system for vowels as given by Chomsky and Halle is, at least in certain respects, incorrect, and there does in fact seem to be some evidence that front vowels should be considered as less marked than non-low back vowels: historically, fronting of back vowels appears to be more common than backing of front vowels; furthermore, granted that u

round is +round for non-low back vowels and —round for front vowels, one could perhaps say that vowels whose unmarked state involves an additional departure (i.e. rounding) from the position of articulatory rest are more complex than vowels whose unmarked state does not involve such a departure; and finally, as William Wang has reminded me, the tongue positions for front vowels such as [i] and [e] are on the whole closer to the rest position than those for back vowels like [u] and [o]. But even if the particular marking conventions with which we were concerned in the above example are changed, and the /i/ vs. /u/ indeterminacy thereby resolved, the possibility of indeterminacy for choices involving other segments of course remains. The appeal to considerations of markedness thus does not make possible a unique resolution of all cases of indeterminacy of lexical representations, although it obviously reduces their number.⁴

UNIVERSITY OF CALIFORNIA, BERKELEY

THE INDO-EUROPEAN DRIFT AND THE POSITION OF HITTITE

HENRI WITTMANN

In 1960, Kroeber predicted that "(Proto-)Indo-European may prove to be an essentially middle-of-the road or average type of

⁴ It has been suggested to me by Wallace Chafe that one should perhaps impose on any theory of markedness the requirement that it never assign an equal degree of complexity to any two segments. Under such a view indeterminacies of the kind we have been considering would simply be an indication that the theory of markedness has not been sufficiently refined (cf. our discussion of the relative markedness of /i/ and /u/). It would still be theoretically possible, however, that an alternation between a sequence of m segments and a sequence of n segments (where at least one of the two numbers m and n is greater than 1) could confront us with alternatives of equal complexity. Whether the requirement proposed by Chafe is in fact appropriate cannot, I believe, be determined at the present still fairly primitive stage of our understanding of markedness.

human language, instead of being the most advanced or perfect as used to be thought."¹ Up to then, reconstructions had pictured the language not only as a predominantly but more so as a very highly synthetic one. Cowgill's application of the Greenberg index-scoring method to the Indo-European languages validated Kroeber's predilection as a quite accurate one.² He found that the Indo-European drift through time reflects an early rise in synthesis followed by a decline, AND A GENERAL RISE IN AGGLUTINATION. The diachronic morphology of Greek in particular shows a marked rise in synthesis, i.e.

| | |
|---------------|------|
| Homer | 2.07 |
| Hesiod | 2.19 |
| Plato | 2.30 |
| New Testament | 2.45 |

The rise in agglutination for the same period is from .10 to .12. These results suggested to Cowgill that there was indeed a steady increase in synthesis within Greek of the first millennium B.C. Miss Tallboy came to similar conclusions in her count for pre-classical and classical Latin respectively.³ She measured a synthesis rise of 1.93 to 2.17 but noted an agglutination fall of .19 to .18. Greenberg himself showed in the diachronic development of Old English to Modern English a decline in synthesis from 2.12 to 1.68 and a continued rise in agglutination from .11 to .30. For the same time span, the synthesis index for Modern Greek fell to 1.82, whereas the agglutination index rose to .40. All of the counts completed up to now suggest an evolutionary pattern going beyond Cowgill's conclusions: a drift from low synthesis and high agglutination indices for early Indo-European to a synthesis peak

and an agglutination low around the beginning of the Christian era, and a subsequent reversal to low synthesis and high agglutination indices for the modern Indo-European languages. It is of course likely that synthesis peak and agglutination low happened a few centuries apart. This is obvious from the Indic data:

| | | |
|--------------------|------|-----|
| Vedic | 2.56 | .08 |
| Classical Sanskrit | 2.59 | .09 |
| Asokan | 2.52 | .26 |

There is also no evidence of a uniform rate of change for the two indices: the fall of 2.59 to 2.52 does not correlate to the .09 to .26 rise.

If one plots all the synthesis and agglutination figures given on a time depth chart, one would expect to obtain for the oldest Indo-European language, Hittite, a score more or less equal to a score for a modern Indo-European language. Indeed, we find for Hittite indices of 1.95 and .42 comparable to the 1.90 and .46 indices for Modern Bengali. We witness for the time segment from Hittite to the modern Indo-European languages predictable directional tendencies, notably an oscillating relationship between synthesis and agglutination, which answers to the idea of 'drift' put forward by some theoreticians such as Saussure, Sapir, and Martinet.

Consequently, the position taken here differs from that of Cowgill's on the fate of agglutination in Indo-European only. He assumes in the IE diachrony a general rise in agglutination, for which neither a bottom nor a ceiling is suggested, thus denying it any of the cyclic qualities postulated for synthesis. To do so, he has to refer repeatedly to the 'aberrant' position of Hittite (126, 133, 138). Cowgill suggests that the Hittite agglutination index ought to be the result of 'innovation', leaving only its synthesis index to 'archaism'. Finally, the speculation is hazarded that Hittite may be a 'Creole', the descendant of a pidgin originally used for communication between speakers of Indo-

¹ A. L. Kroeber, *On Typological Indices I: Ranking of Languages*, IJAL 26.171-177 (1960).

² W. Cowgill, *A Search for Universals in Indo-European Diachronic Morphology*, in J. H. Greenberg, *Universals of Language* (Cambridge, Mass., 1963).

³ F. Tallboy, *The Diachronic Development of Latin Morphology*, ms. (Montréal, 1967).

European and non-Indo-European languages in Anatolia.

Indeed, the notion of creolization has been applied to practically all the IE daughter languages in order to throw light on substratum problems. The suggestion by Feist may be said to be the most convincing.⁴ Yet, I cannot help but feel that the idea of a 'Creole' language is far too often simply a substitution for 'hybrid' language. If we accept Hall's definition of a creole, then we ought to mean for example Haitian or Sranan as opposed to English or Yiddish.⁵ Consequently, creolization is a change the effect of which may be said to be 'grammatical', whereas the effect of hybridization is largely 'lexical'.⁶ A cursory count of a Mauritian Créole sample gave figures of 1.26 and .25 for the first two indices as compared to 1.99 and .24 for Standard French.⁷ If Hittite, as compared to Indo-European, were to have been a créole, one would have expected this to show in the synthesis rather than in the agglutination index. Indeed, if Hittite were at all to be considered a contact language, then the profile of Hittite would suggest this contact to have been more of a hybridizing than a creolizing nature. It is doubtful to what extent hybridization would show in any of Greenberg's indices.

However, the vulnerability of Cowgill's hypothesis lies mainly in his refusal to account for the developmental differential of older to late Hittite. In choosing a sample of late Hittite only, he reasons (118): "Although Hittite texts older by several cen-

tures are available, my experience with the language leads me to doubt that they would show up very different typologically from the sample here studied." Cursory counts on the older Anittas-text prove Cowgill to be at least half-wrong, by yielding 1.97 for the first but .57 for the second index.⁸ Indeed, a fall of .57 to .46 in agglutination is quantitatively too obvious to be statistically irrelevant.

McGILL UNIVERSITY

ON MAXAKALÍ, KARAJA, AND MACRO-JÊ

ERIC P. HAMP

Irvine Davis properly remarks (IJAL 34.35, 1968) that presently available evidence is insufficient to yield a definite classification of the Macro-Jê languages. Short of this ambitious goal, it is also impossible at present to specify the relation of Maxakalí to Karajá within Macro-Jê. However, Davis's careful and lucid treatment of the correspondences suggests some further thoughts.

Matters become clearer if we reduce Davis's two tables of Proto-Jê/Maxakalí and Proto-Jê/Karajá correspondences to a single tabulation. For compactness and clarity I incorporate in the single tabulation a brief indication of relevant environments, most of which Davis states elsewhere in his article but a few of which I add as provisional guesses. Since we are now dealing with those cognate sets that embrace all three language subgroups, the total number of cognate sets that may be used (shown by Davis's numbers) will naturally be reduced. My consolidated tabulation follows:

| PJ. | M | K | |
|-----|---|---|---------------|
| | p | w | 48 |
| | p | ∅ | 51 (before r) |
| | m | | 50 (v̄) |

⁸ Of course, I distrusted at first my own figures, and I decided to recount Cowgill's sample so as to reveal any idiosyncratic tendencies I might have. However, my results turned out to be practically the same (1.95 and .42), except that I counted 101 words where he saw only 100 for the passage in question.

⁴ S. Feist, *The Origin of the Germanic Languages and the Indo-Europeanising of North Europe*, Lg. 8.245-254 (1932).

⁵ R. A. Hall, *Pidgin and Creole Languages* (Ithaca, 1966).

⁶ Cf. here the polarity principle of 'lexical-grammatical' in F. de Saussure, *Cours de linguistique générale* (Geneva, 1916), and in A. Martinet, *Eléments de linguistique générale* (Paris, 1960).

⁷ The French text on which the count was taken consisted in the translation I made of the Mauritian sample. The latter stems from original material I am presently working with.