

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.

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ON MAXAKALÍ, KARAJA, AND MACRO-JÊ

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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.

t—t	∅	57, 58, 60 (initial; before w?)
	r	41, 49 (medial)
	d [∅]	33, 56 (with nasals; NB also 41) (56 3rd pers. marker from medial or clitic use?)
	n	40, 59 (also 41? (\check{V}))
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c—c	č	2
	ñ	8 (\check{v}) (perhaps 41, with dissimilation of nasality in M, and K. d [∅] < *ñ < c/- \check{V} ?)
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k—k	∅	19, 25 (before r, w) (also K 17 rad [∅] i < *krāñ-, and 18 -rəəš- < *kr \check{V} rV-? Probably 22 also before r)
	c	k 7, 22 (K women's) } (M before non-grave V)
	c	∅ 13; also 9-11, 18 }
	k	∅ 15; also 39? (loss of k elsewhere in men's; but NB 3 kai)
	ŋ	23
	ñ	8, 59 (with non-grave V) } (\check{V})
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m—p	b	32; also 13, 27, 60 (V)
	m	28, 30, 31 (\check{V})
n—t		34, 37 (V)
	r	35, 36 (V, \check{V})
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ñ—ñ	d	38, 41, 42 (\check{V})
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ŋ—k	∅	45, 46
	ñ	5 (\check{V} ; with non-grave \check{V} ?)
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r—t	r	19, 22, 25, 32, 46, 51, 53, 55; also 18 if K is *kr \check{V} ₁ r \check{V} ₂ -
	t	e 18, 45 (non-grave V)
	n	r 46 (\check{V} , probably grave)
	n	20 (\check{V} , probably non-grave)
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z—c	d [∅]	6, 24, 62
	ñ	63 (\check{V})

It is immediately seen from the above tabulation that if any shared developments are to be inferred from this aspect of the phonology they will be found among the splits and fresh mergers shown by Maxakalí and Karajá, and not between PJê and one of the others. Certain developments in M and K are obviously independent, and possibly relatively recent: M has undergone palatalization of the velars, merger of *t and *r, and

merger of *c and *z; these are not shared by K. On the other hand, K shows a weakening of the dental oral stop in medial position if the development of *t > r is correctly analyzed. Moreover, except for k in women's speech, all simple oral stops (hence not *c and r) have suffered weakening in K, leading to total loss of the stop; with *p only the lip-rounding feature is left. On this reasoning, medial weakening of K *t to r must have preceded loss of stops. The development of the old nasal stops to voiced stops (also as an intermediate state for *n > r?) is of course a phonetic detail not affecting their distinctiveness, and is present to a greater or lesser extent throughout the family (see Davis's fn. 5, p. 43); this feature is therefore not diagnostic for the chronologies we are occupied with.

Further, K shows a split in *r, perhaps dependent on the gravity of original vowels. One could wonder, for a start, whether the split in K *r was related to the split in M velars. Apart from the phonetic non-obviousness of such a hypothesis, it seems that this is excluded by the fact that M merges *r with *t, without trace of palatalization in either. Therefore the palatalizations in M and in K appear to be independent phenomena.

This leaves one principal feature to be discussed, the assimilation of stops to nasality in M. All original stops have aligned themselves in M on a new principle: instead of being distinguished by an inherent feature of [\pm nasal], they have assimilated to the [\pm nasal] feature of the adjoining segment. Thus, the feature of nasality in M now has a domain of more than a single segment's length; whether it is to be attributed to more than one segment by a phonetic context rule, or as a feature to an entire morpheme, requires more data to judge. At any rate, it is to be noticed that this interesting change also affects the descendants of *r and *z; this fact could be used as evidence to show that the change is independent of K, but again more information would be desirable. On the other hand, we are badly lacking

cognates in K for the correspondences in question. K shows us no matches for M m < *p, M n < *t, M ñ < *c, M ŋ/ñ < *k; nor for M m < *m, M t < *n, M ñ, < *ŋ. Thus we cannot see clearly what the supposed result would be. M n = K r < *r might seem to argue against a development in K like that of M (though developments in *r might have also been separate and more recent in M); but since K r < *n seems to appear with both nasal and non-nasal vowels, it is difficult to construct a consistent argument here.

In fact, there are a few stray suggestions that K may have undergone an assimilation at least in part like that of M. Thus we see that K replies with complete loss to M k < *ŋ; however, this may simply mean that K lost all stops marked by a velar articulation. Note, however, that set 41 may show signs of a nasal (i.e. voiced) treatment of *c. Furthermore, it seems that we find K d ^ʔ < *t when another nasal is present in the word.

Obviously, we very much need more data from K including forms with etymologically opposed nasality involving both vowels and consonants. Of course, we want to see much more of these interesting languages in any case, and we are greatly in Davis's debt for the knowledge gained so far.

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CONTRIBUTIONS TO ANTHROPOLOGY: LINGUISTICS I (ALGONQUIAN). Bulletin No. 214, Anthropological Series No. 78. Ottawa: National Museum of Canada, 1967. Pp. vii, 162.

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This volume contains the papers read at a conference on Algonquian¹ linguistics held at the National Museum of Canada in August, 1964, under the chairmanship of Don DeBlois, plus some others that were written as a result of the conference. Although it has

¹ The word is spelled both this way (by most) and also 'Algonkian' (by Mary Haas among others). I am informed by Teeter that 'Algonquian' is the 'good' spelling, and accept his advice.

some lacks² and some articles are uneven in quality, it is an outstanding contribution to Algonquian linguistics, literally brimful of information, and contains several extremely penetrating and insightful articles about Algonquian. It is weighted on the side of historical linguistics: of the 13 articles, 7 deal exclusively with some aspect or other of historical development, 2 are half descriptive and half historical, and 4 are descriptive. The descriptive articles are quite short, between 3 and 6 pages in length. We will consider them first.

In two articles, *Some aspects of Arapaho morphology* (128-34), and *On the inflection of transitive inanimate verbs in Arapaho* (135-39), Zdeněk Salzman gives some preliminary notions of noun and verb inflection in Arapaho. The first is a condensation of two articles on the Arapaho verb published after the conference but before the appearance of the volume under review.³ The article deals primarily with morphophonemic alternations in noun stems and affixes, and is straightforward, excellent, and unremarkable. The second article tackles the problem of how to handle the immense complexity of subject and object in Algonquian verbs. Salzman's approach is to establish morphemic identity, not in terms of subject or object, but in terms of constant and variable, whether subject or object, within smaller subsets of the whole set of forms possible with one verb stem. The constant always appears immediately after the verb stem, the variable after the constant. For example, in the set of forms meaning *you-us*, *you all-us*, and *they-us*, the constant is a morpheme -éiʔéé- meaning *us*, i.e. an object; in the set of forms meaning *we-you*, *we-him*, *we-you all*, and *we-them*, the constant is a morpheme -ee- meaning *we*, i.e. a subject.

² For example, the tremendous activity and new ideas resulting from the emergence of generative grammar seem to have left the field of Algonquian studies completely unscathed.

³ Arapaho V: Noun, IJAL 31.39-49 (1965), and Arapaho VI: Noun, IJAL 31.136-51 (1965).