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PENUTIAN AMONG THE RUINS:
A PERSONAL ASSESSMENT

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It has been 67 years now since the name 'Penutian' first appeared in print--in a 1913 article by Roland B. Dixon and A.L. Kroeber. Their genetic proposals for the California Indian languages were expanded in the now-classic monograph which appeared seven years later, in 1919: Linguistic Families of California. They coined the word 'Penutian' by combining the words for 'two' in Maidun--pen-- and Costanoan--uti. The Maidun part was undoubtedly taken from Nisenan [pe'n]. The uti must have been from Rumsen, with which Kroeber had done some work; the form in that language is [ʔutis]. Their Penutian included five language groups: Three Pen families, Wintun, Maidun and Yokutsan and two Uti families, Miwokan and Costanoan (or what is now often called Ohlonean).

How did they arrive at this classification? It is critical to take note that they did not do so by means of the application of the comparative method. The criteria were, in part, typological. A list of diagnostic features was compiled: noun cases, no prefixes, "Indo-Germanic" type verbs with mode, tense, number, person, etc. and "vowel gradation" by which they may have meant vowel harmony, though in later years a close inspection of more reliable data has shown many, though random, apparent alternations between e and o and between i and u.

The other criterion was lexical similarity. The judgments there were based on an inspection of resemblant forms. I was fortunate enough to be around the day that Kroeber brought his original worksheets for Penutian (and Hokan) in to the Linguistics Department to leave them with Mary Haas (I'm sure she still has them). The sheets are very large, with the languages across the top and the glosses down the side. The technique was to count resemblant forms--the sets of languages with unusually high numbers were grouped together.

Although we have amassed a vastly greater and more accurate amount of lexical data since it is very important to point out that the fundamental characteristics of the sets one finds are much as they were for Dixon and Kroeber. There are many resemblant forms--I believe Pitkin and I accumulated over three hundred for our 1958 article (Pitkin and Shipley) and there are lots more--but they are irritatingly unsatisfactory. Most of the consonant resemblances are identities, furthermore there is little parallelism from one set to another. The vowels also are either identities or seemingly random. The Penutian area looks as if it had been subjected to a massive and prolonged process of lexical diffusion, layered in like sedimentary rock. That postulation has its difficulties, however, since many of

the glosses are for body-parts and other simple, non-cultural things, the terms for which seem unlikely to be subject to replacement. It has been very puzzling, and has engendered a steady stream of cautionary statements from people familiar with the situation.

I should like to explicate here three traditional (though still unfounded) assumptions about Penutian which have gained wide acceptance.

1. The resemblances among the so-called Penutian sub-families are primarily genetic. This view has been continuously taken as axiomatic, in the face of the stubborn failure of the relevant data to provide any basis for establishing convincing sound correspondences or credible reconstructions. The only exception to this is the established genetic tie between Miwokan and Costanoan.

2. The California Penutian languages form a subset within a larger Penutian genetic framework. The rationale for this view would seem to be simply a result of Dixon and Kroeber's having proposed California Penutian first.

I certainly fell for this one myself, proposing a Penutian kernel, consisting of the California Penutian languages to which every new Penutian candidate was to be matched, like swatches in a drygoods shop. Dell Hymes was the one who saw through this first. He proposed, in 1964, that those of us engaged in work on California Penutian genetic problems should drop our reluctance to envisage a Penutian on the scale of Sapir's hypothesis and should recognize such a scale as both valid and the only way to resolve the apparent impasse of comparison within California alone.

Hymes' point of view did provide a sounder working principle. Unfortunately, bringing in the various Oregon languages which have been tagged as 'Penutian' has not, up to now, really shed any new light on the fundamental genetic problem: What is related to what?

Whistler's remarks on this point project what now seems to be a much more hopeful line of endeavour. He said, in 1977: "I now consider the hypothesis of a California Penutian kernel dead. It is inconsistent with the linguistic borrowing data, with expectations based on other historical linguistic principles, and more crucially, with the cultural sequences implied in the archaeological record. Penutian entry to California must have occurred in several stages and likely from different directions."

3. There is an adequately defined entity called 'Penutian' which includes an unknown number of languages. It is therefore valid and important to scout around and find these languages, by magic if necessary, and to identify them in print. The activities resulting from this hypothesis are certainly the most bizarre and mischievous of all the Penutian games which have been played.

In a very important sense it is invalid to cite Sapir's Penutian superstock formulation in this light. Sapir's own claims were modest and equivocal; only his imposing reputation was responsible for turning his interesting hunches into doctrine. But it was a house built on sand.

In 1940, in separate papers, J. Alden Mason and Frederick Johnson proposed a Macro-Penutian phylum, adding Aztec-Tanoan, Mayan, Totonacan, Xincan and Lencan to Sapir's Penutian. This vast collocation was primarily due to Benjamin Lee Whorf--at least, according to Mason and to George Trager, who made the following incredible pronouncement in 1945: "Whorf established, to the satisfaction of those who saw his material, that the grouping of Penutian, Sahaptian, Azteco-Tanoan, Zuni, Kiowa, probably Mayan and Totonac, and possibly Tunican, as stocks constituting a phylum which he called Macro-Penutian, was at least as good as the Algonkian Mosan or Na-Dené groupings of Sapir." This pronouncement of Trager's is sheer moonbeams and attar of roses. The Algonkian-Mosan grouping has long since, I believe, dropped through the trapdoor of time. According to Michael Krauss, (1973, pp. 953-963) the Na-dene hypothesis is, to say the least, provisional. In a way, then, Trager was saying that nothing is at least as good as nothing.

In 1956, Morris Swadesh went, as it were, the whole way and argued for an expansion of Whorf's Macro-Penutian group into a huge phylum which he called 'Penutioid'. He excluded Aztec-Tanoan, Kiowa and Tunican; thus Penutioid was not simply an expansion of Macro-Penutian but actually a different theory--the boldest and most far-flung proposal with which the name 'Penutian' has ever been associated. There were twenty sub-groups, stretching from Tsimshian on the Northwest Coast to Quechua-Aymará in Peru and Bolivia.

Now, don't get me wrong. I knew Swadesh somewhat and I liked and admired him. He had imagination, courage and talent. Many of his ideas on language classification were, as the kids would say, 'far out' and most of them will undoubtedly never be substantiated. But imaginative and speculative flights do a lot to break up the logjams in people's heads. Maybe he wasn't serious, for all I know. The unfortunate part has to do with what comes after--with the way other people solemnly and humorlessly embalm such fantasies. Let me quote the total entry for Penutian from the 1976 edition of the Encyclopaedia Britannica (Volume 10, p. 671):

"Penutian. The Penutian phylum is the only group of languages in North America for which relationships with languages in South America have been traced convincingly. The Penutian languages are thus distributed from the Northwest Coast and Plateau areas through California (with a possible extension into

the Southwest) and Meso-America into Bolivia, Chile, and Argentina. Many of the more than 24 Penutian languages north of Mexico are either no longer spoken or are spoken by fewer than ten people. In Meso-America, however, many native languages have a considerable number of speakers; e.g. Mixe, in the Zoque family, has over 30,000 speakers, and the Mayan family includes some languages with several hundred thousand speakers, as Maya, Quiché, Kekchí, Cakchiquel, and Mam."

If the rest of the articles in the Encyclopaedia Britannica are as pin-point accurate as this one, then all that leather and gold leaf isn't wasted!

As a counterpoise to these assumptions, I should like to propose the following two working principles:

1. The term 'Penutian' has no genetic definition at all. The very use of the term prejudices the case and sets us off to working from a kind of axiomatic entity which we have not defined. For instance, suppose our researches led us to the valid conclusion that Wintun and Utian are related and that Maidun and Yokutsan are related but that the evidence will not support a genetic connection between the two pairs. Then, which pair is Penutian? Or, to put it another way: suppose we take some known genetic entity--Klamath or Maidun or Mayan or whatever--and designate it as Penutian (we could do this right now, as a matter of fact). We are really prejudicing our case by multiplying entities, or at least naming the Devil twice. If we ever find real genetic connections somewhere among these languages, then the term Penutian might be all right to use again, although it is pretty shopworn. I think we should stop misleading everybody and drop the term out of our working vocabulary even though it might produce an identity crisis in some of us. It is not that I feel there are no genetic connections to be found--I just don't want to name something until I have something to name.

2. We must develop new techniques which will take into account all the cultural, historical and archaeological factors we can muster. This point, of course, simply echoes Whistler. I think he's absolutely right. Using the insights which he has provided has brought the whole enterprise back to life--for me, at least. I feel I can put my years-long accumulation of knowledge about the minutiae of these languages to some new, exciting and rewarding uses.

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