



More on Nasalized Vowels and Morphophonemics in Mezquital Otomi: A Rejoinder to Bartholomew

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Although there are a few words in Boas' list that I did not collect, all the present-day Ulkatcho forms are in very good agreement with his Nētcā'ut'in. The one exception is Boas' [ali's] *knife*. The modern Ulkatcho term is [ʔətes]; neither of these forms occur among the synonyms given in Morice's grammar.⁵ Since I did not specifically try to elicit [ali's], it may be that it is the word for an older or special type of knife.

Anahim Lake until about 1868 was a population centre of the Chilcotin tribe. For various reasons the Chilcotin moved further east about that time and the area appears to have been almost completely abandoned by Indians for many years. A few Chilcotin families remained nearby (some are there to this day) and apparently a very sporadic migration of Ulkatcho Carriers into Anahim Lake took place over the years. The village of Ulkatcho is about 40 air miles north of Anahim Lake. In 1916 some land was set aside near Anahim Lake for a few Ulkatcho families,⁶ and by about 1945 the move from Ulkatcho was complete. The reserve at Anahim Lake was officially established in 1949;⁷ Ulkatcho is now totally abandoned and in ruins. According to informants, the boundary between Chilcotin and Carrier was traditionally about half way between the two villages. With the Chilcotin exodus to the east, Carrier territory, beneath the White overlay, has expanded south.

Until recently I was unaware that Boas had ever been in the Anahim Lake country where he would most likely have encountered Ulkatcho Carriers, but Rohner⁸ states that

⁵ The Carrier Language, Anthropos, Collection Internationale de Monographies Linguistiques, Vol. 10 (Wien, 1932).

⁶ Report of the Royal Commission on Indian Affairs for the Province of British Columbia, Vol. 1, Victoria, 268-71 (1916).

⁷ Personal communication, Mr. Roscoe Wilmeth, National Museum of Canada. Information originally from Department of Indian Affairs, Ottawa.

⁸ Franz Boas: Ethnographer on the Northwest Coast, in *Pioneers of American Anthropology*, ed.

Boas travelled overland from Williams Lake to Bella Coola in 1897. Boas' own account of the trip⁹ indicates that he passed near Lake Nacoontloon (modern Anahim Lake). Although he makes no mention of Nētcā'ut'in or Carrier, it would have been at this point or somewhat further along the trail to Bella Coola that he would most likely have encountered representatives of the Ulkatcho band.

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MORE ON NASALIZED VOWELS AND
MORPHOPHONEMICS IN MEZQUITAL
OTOMI: A REJOINDER TO
BARTHOLOMEW¹

H. RUSSELL BERNARD

Recently, in a note to IJAL (34.215-17), Doris Bartholomew discussed my previously published suggestion that nasalization is being eliminated in Mezquital Otomi. Several of the points made by Bartholomew bear further discussion, clarification and amplification.

1. My suggestion, based on historical and in-the-field evidence, was that nasalized /a/ is actually pronounced with a low back articulation. This would, I suggested, imply the existence of a phoneme /ɔ/ which had the peculiarity of ALWAYS being nasalized. Bartholomew characterizes my description of this phoneme as a 'low back vowel with redundant nasalization'. I never used the word 'redundant' in my article, because I don't know if nasalization is redundant here at all. I performed an experiment with several Otomis where I asked them if my pronunciation of the verb *to sleep* was cor-

by June Helm, Monograph 43, The American Ethnological Society, p. 191 (1966).

⁹ American Museum of Natural History Memoirs, Vol. 2, pp. 9-10 (1900).

¹ I am grateful to Kenneth Hale of MIT and to Arnold Satterthwaite and James Goss of Washington State University for helpful comments and suggestions during the writing of this note.

rect. This verb may be phonetically rendered /hǒ/, with rising tone. I purposely mispronounced it /hǒ/, without nasalization, and they rejected it completely. Then I mispronounced it /hǎ/ and they responded with something like "that's better; not quite, but better!" This suggests that while nasalization might be PHONOLOGICALLY redundant it is probably 'backness' which is PSYCHOLOGICALLY redundant and the nasalization which is distinctive for native Otomi speakers.

2. Apparently, the extent (or lack of it) of my claim for the disappearance of phonemic nasalization in Mezquital Otomi was misunderstood. So there can be no doubt about my position, let me state once more that /a/ is being replaced by /ɔ/, that /i/ ~ /i/ and /u/ ~ /u/ are recognized as practically free variants by every worker in the area, and that /e/ ~ /ɛ/ seems to be the only remaining minimally distinct oral-nasal pair. I did say, however, that /ɛ/ appeared to be distinctive only in a very few cases, such as those cited by Bartholomew. In some cases informants seemed to fluctuate between /e/ and /ɛ/ in different renderings of the same lexeme. This led me to suggest that /ɛ/ was in the process of merging with /e/, thus completing the circumstances which would eventually lead to the general phonemic denasalization of the language. All I claimed for the /e/ ~ /ɛ/ pair was (a) that young Otomis manifest more freedom of variation than do old people; (b) that the variation between /e/ and /ɛ/ is a reduction of a previously heavy load on nasalization; and (c) that the 'problem' of nasalization (i.e. is it really very important in this language or not?) will solve itself by ceasing to exist. I did not intimate that nasalized /e/ was not now phonemic. It is my prediction, though, that nasalization will finally disappear from the phonology of Otomi.

Bartholomew acknowledges that Otomi is in the process of change and she points out that it is the learning of Spanish which is

the key variable in this change. I thought this was obvious. It is indeed the direct influence of Spanish which is creating an innovating, nonnasalizing dialect of Otomi. Two factors not presented in my earlier article are relevant here: (a) women tend to preserve nasality in all contexts (/ɔ/, /i/, /u/, /e/) far more than do men; and (b) of the monolinguals in the Mezquital Valley probably over 90% are women.

3. Bartholomew says: "...once young people acquire reasonable facility in Spanish it affects their Otomi, weakening some of the distinctive characteristics of the language." It is clear from the above I am in complete agreement with this statement. No anthropological linguist could be otherwise. However, Bartholomew goes on to say that the "...modified Otomi is not passed on. Such bilinguals do not teach Otomi to their children, but make a changeover to the more prestigious Spanish." And this is simply not true!

An Otomi is an Otomi is an Otomi—as far as his countrymen are concerned—unless he (a) stops speaking Otomi entirely; (b) leaves his natal village and region; (c) adopts Mestizo clothing and other non-Indian cultural characteristics. Very, very few Otomis—even the most Hispanicized—manage to accomplish this herculean set of tasks. Their children learn Otomi in the villages whether they (the parents) like it or not. Moreover, in recent years a whole new social class has arisen in the Mezquital. For lack of a better term I call them Bourgeois Otomis. They speak excellent Spanish and have adopted Mestizo dress and mannerisms. They have become financial successes and have spent their money on brick homes, T.V. sets, and high school educations for their children. Yet, they are still Otomis. They live in 100% Otomi villages and remain Otomis among their own people. They become village *políticos*, patrons, keepers of the faith and fiestas (*mayordomos*). They are keenly interested in seeing to it that things Otomi do not perish before the rush

of 'castellanización,' the government's program to level cultural differences in Mexico into a miasma of traits called Mestizo. This group is small, but active; after centuries of being second class citizens, a few Otomis are actually trying a 'revival' in the best sense of A. F. C. Wallace's use of that term.

4. By far the most important point of Bartholomew's article is her reservation about the "total economy gained from elimination of nasalization as a distinctive feature" (216). Her criticisms are relevant and deserve serious comment. Bartholomew's main point is summed up in her statement that if I recognize "... a merger of /e/ and /ɛ/ in the innovating dialect" and if I eliminate "nasalization as a distinctive feature," then I am "left without a simple way of explaining two kinds of morphophonemic alternation." Furthermore, she categorically assumes it a desideratum of generative grammar to explain (or explain away?) all morphophonemic alternations in a language. The alternations in Otomi to which she refers are, specifically: (a) a difference in stem formative vowels /i/ or /e/, depending on whether the root vowel is /ɛ/ or /e/; and (b) a relationship between the variation in stem-initial consonants and the stem vowel. In corroboration of these two points Bartholomew cites the following two sets of data (I have added the translation and tones):

- (1) /thédí/ a lesson or warning, also a tamal of frijoles;
 /hètʔí/ ~ /hñètʔí/ ~ /hñétʔí/ to spin into thread;
 /hèkí/ ~ /hñèkí/ to pierce, as with a needle;

as opposed to:

- /thédé/ to laugh;
 /hètsʔé/ ~ /hñètsʔé/ to sneeze;
 /héké/ ~ /hyéké/ to divide, as to partition.

and:

- (2) /pé/ to steal, alternates with /be/ and

- /phe/, while
 /pé/ to weave, alternates with /me/ and /me/ and /be/.

The first set of data is contradicted, however, by such occurrences as:

- /ɛhé/ to come;
 /bèté/ traitor;
 /dòbèhé/ Lent, draught;
 /gèhé/ fever;
 /ntʔété/ witchcraft;
 /ñʔéhé/ to possess.

This does not mean, of course, that nasalization of /e/ is a totally defunct issue in Otomi. The following three lexemes simultaneously indicate a phonologic distinctiveness of /ɛ/ while refuting Bartholomew's notion of /ɛ/'s explanatory power for the difference in stem formative vowels:

- /hñètsʔí/ high, to float;
 /hñètsʔé/ to find shelter, to cover oneself;
 /hñètsʔí/ pin cushion.

The second set of data (the /pé/ ~ /pé/ pair) is more interesting. Here, I admit, I am at a loss to contradict Bartholomew's point concerning the apparent relationship between the /ɛ/ ~ /e/ alteration and consonantal alternations at other levels in the grammar.

However, my original claim that Otomi is in the PROCESS of denasalization, still stands. The free variation of /ɛ/ and /e/ in some NASAL environments was indicated by the SIL in their 1956 Diccionario Otomi Castellano:

- /hñèní/ ~ /hñèní/ to become sick;
 /ñʔété/ ~ /ñʔété/ witch;
 /ñʔéthí/ ~ /ñʔéthí/ remedy;
 /ñʔétsʔí/ ~ /ñʔétsʔí/ to presume;
 /éní/ ~ /ɛní/ to play.

My research in 1962 indicated that this phenomenon of variation in nasal environments was spreading among younger, innovating speakers to non-nasal environments as well.

Another matter of concern are Bartholomew's assertions about the goals and methods of generative grammar; they would hardly be accepted universally by T-G linguists. She says, for example: "but granted that the contrast has been eliminated (between oral and nasal /e/ in the innovating dialect), the morphophonemic alternation of stem formative /e/ and /i/ must be explained somewhere."

It is clear that I am not convinced of the role of nasalization in explaining the alternation of stem formative /e/ and /i/. What must also be kept in mind, however, is that there is no reason why THIS alternation 'must be explained somewhere' in a descriptive grammar of Otomi.

Apparently, it is this last desideratum to which Bartholomew gives primacy. She says: "Here we have a conflict between the desire to recognize linguistic change in action and to construct a vowel system with the most economical set of distinctive features, and the explanatory goal of generative grammar, which in many cases favors the earlier stages of the language. If the predictability of morphophonemic alternation is vital to the description of a language, the economy of eliminating nasalization is specious." Generative grammar does not 'favor earlier stages' of a language any more than it insists that ALL morphophonemic alternations be explained somewhere. Consider, for example, the English nouns *wife*, *leaf*, *knife*, and their plurals. There must be a historical explanation which accounts for this occurrence as compared with plurals of nouns like *tough*, *stuff*, *puff*, and *gaff*, but it is unlikely that a generative grammar of English would rely on such an explanation. Rather, the morphophonemic alternations in the first set would probably be contained in a special subset of words which undergo particular changes based on special rules.

In sum, I never claimed that nasalization was not phonemic in /e/. I did predict its disappearance. And I submit that it is not

'specious,' as Bartholomew claims, to eliminate nasalization from the phonology. I suspect Bartholomew's insight into the relationship between the occurrence of nasal and oral /e/ and certain morphophonemic changes is at least partially correct. But if she is correct now (synchronically), then she will be diachronically correct in 3 generations from now. Generative grammarians then won't 'favor the earlier stage' of the language; but one can be sure they won't reject diachronic evidence as part of their explanatory arsenal.

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NOTES ON THE CLASSIFIERS IN THE
NA-DENE LANGUAGES

HEINZ-JÜRGEN PINNOW

In his recent comprehensive essay, *On the Classifiers in the Athapaskan, Eyak and Tlingit Verb* (IUPAL, *Memoir 24*)¹, Michael E. Krauss demonstrated in a most exact manner the unquestionably very close relationship of the so-called classifiers in Tlingit to those of Proto-Athapaskan-Eyak. Krauss's conclusions are remarkable, evidently correct in their details and coincide with my own studies in this field. Since Krauss is able to offer considerably more extensive material, it is superfluous to present my work which matured in complete independence from Krauss's studies; only the results should be noted briefly, as they nonetheless differ in certain particulars from Krauss's results and should therefore be elucidated.

It follows beyond question from Krauss's discerning arguments that Tlingit and Athapaskan-Eyak are genetically related; the parallels which Krauss brings forth rule out any other conclusion. Even if it should be objected that Tlingit as well as Proto-Athapaskan-Eyak derived considerably from non-related precursor languages, as represented in the following diagram,

¹ The manuscript was kindly provided to me by the author.