

Benedict's Sino-Tibetan:

A Rejection of Miller's Conspectus Inspection

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[Author's note]

This reply to Miller was originally supposed to appear in JAOS, the same journal in which Miller's piece itself was published. For a variety of reasons, this has turned out not to be possible. It is a shame to involve a new journal like LTBA in controversy so early in its existence. On the other hand, I feel it would be an even greater shame for the field of Sino-Tibetan linguistics to let Miller's attack on the Conspectus stand unchallenged.

JAM

In the Journal of the American Oriental Society 94.2 (1974) there appeared a piece of writing from the pen of Roy Andrew Miller that purported to be a review of Paul K. Benedict's book Sino-Tibetan: a Conspectus, which I had the joy and excitement of editing. Miller (henceforth usually RAM) peppered his "review" with German quotations, learned jokes, and other coquetties of style, heaping sarcasm upon Benedict's (and my) head. In the entire course of the long 15-page "review article" RAM did not say one single nice thing about the book, not even that it was printed on good quality paper.<sup>1</sup> The casual reader of RAM's prose comes away with the impression that Benedict and I must be some

kind of criminal halfwits for daring to foist so miserable a work upon the public.

Apparently Miller's goal was to demolish the Conspectus (henceforth STC) totally, so that it would be read only as a horrible example of the worst excesses in comparative-historical linguistics. Yet any real criticisms of STC that RAM might have are so buried under masses of malicious trivia that it is hard to ferret them out. Since RAM's "review" is more a work of polemical literature than a work of linguistic scholarship, it should really be discussed on at least three levels. First (psychological), what could have motivated a distinguished and highly respected scholar like RAM to write such a thing, anyway? Second (strategic), what rhetorical and dialectical devices does RAM use in his attempt to discredit the STC? Third (linguistic), what can one say about the validity of the theoretical and factual substantive issues which RAM manages to raise?

(Psychological) Miller is known in the profession for the savagery of his reviews. People still remember the hatchet-job he performed on Robbins Burling's monograph Proto-Lolo-Burmese, a work which did have flaws but which made several key contributions to the field which have led to productive lines of research by other scholars.<sup>2</sup> The bitterness of RAM's attack on the Conspectus has a rather different quality, however. What RAM is really responding to in the STC are the several politely phrased but unfavorable references to Miller's own articles on Tibeto-Burman which Benedict makes here and there in footnotes: p. 60, note 193 (re final stops in Maru); p. 69, note 217 (re RAM's Tibeto-Burman "ablaut"); p. 124, note 340 (re RAM's TB "infixes"); and p. 126, note 344 (ablaut again). Two of the articles in question, "The Tibeto-Burman ablaut system" (1956) and "The Tibeto-Burman infix system" (1958), are Miller's chief contributions to comparative TB studies. They are now museum-pieces, full of false etymologies and wrong inferences (see below). They have not led to "productive lines of research

for anybody. Benedict did not let those sleeping dogs lie; therefore he had to bear the full weight of RAM's vindictive wrath.

(Strategic) RAM's tricks of rhetoric merit careful study. One of his favorite techniques is this: (a) make some criticism of a particular point, no matter how trivial or obfuscatory that criticism may be; (b) claim that "tout se tient," and that the entire work stands or falls on the particular point in question; (c) beat the point elaborately to death; (d) claim that the whole book is thereby vitiated; (e) avoid having to go into too much detail by pleading lack of space -- "other points are too numerous to mention."

Let us take a closer look at these tricks:

Making the trivial look crucial.<sup>3</sup> One of the many points on which Benedict changed his mind between 1940 and 1970 (see below) involved the precise phonetic interpretation to give to his high tense proto-vowels, PTB \* $\bar{u}$  and \* $\bar{i}$ . As recent phonological theory generally accepts, the paradigmatic contrast of [tense] vs. [lax] may have any of a number of basically equivalent surface phonetic interpretations for high vowels. See Figure 1.

FIGURE 1. Alternative phonetic interpretations of the [tense/lax] opposition for high vowels.

LAX	TENSE	CRITERIAL FEATURE ISOLATED FROM THE FEATURE-SYNDROME
[V]	[V:]	short vs. long [i]/[i:]; [u]/[u:]
[V <sup>˘</sup> ]	[V <sup>ˆ</sup> ]	lower vs. higher [ɪ]/[i]; [ʊ]/[u]
[V <sup>ʔ</sup> ]	[V]	centralized vs. peripheral [i>]/[i]; [u<]/[u]
[V <sub>κ</sub> ]	[V <sub>α</sub> G <sub>α</sub> ]	monophthongal vs. homorganically diphthongal [i]/[iy]; [u]/[uw]

(G = w, y. "κ" indicates that the glide agrees with the nuclear vowel in frontness/backness or labiality/palatality.)

[V]	[V <sub>(ə)</sub> G]	monophthongal vs. heterorganically diphthongal (centralized falling diphthongs) [i]/[əy]; [u]/[əw]
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In other words, \*iy and \*əy, \*uw and \*əw are little more than notational variants of each other. As every adept in the comparative method (see below) should know, reconstructed phonemes are merely formulae for correspondences, and to pretend that you can always specify with unique certitude the fine phonetic content of a proto-opposition is unrealistic. It makes very little difference to the proto-system of vocalic oppositions that Benedict reconstructs whether the tense high vowels are interpreted as homorganic or heterorganic diphthongs. Yet typically, RAM fixates on this minuscule point (p. 202), making the extraordinary claim that Benedict's notational change of mind is a "particularly massive internal contradiction" that "threatens to wipe out the entire system of reconstruction for Tibeto-Burman and Sino-Tibetan



Significantly enough, RAM constantly uses the imagery of "collapsing," sounding the knell of doom again and again. If he can show Benedict to be mistaken on one little point, then, he wistfully hopes, the whole "house of cards" will come crashing down (pp. 196, 197, 198, 205, 208). We might call this the "I'll-huff-and-I'll-puff" attitude.

Rhetorical withholding of "damaging information." RAM would have us believe that if he only had had more space, he could have launched even more devastating attacks. "One hardly knows where to begin giving examples [of internal contradictions]" (p. 202); "a few random examples must suffice to illustrate this charge" [that Benedict's stylistic inadequacies "raise problems in linguistic methodology"] (p. 206); [Benedict's analysis of final consonants so obviously violates all principles of the comparative method that it] "neither requires nor deserves elaborate rehearsal here" (p. 205); "this evidence, which could be elaborated extensively, is surely sufficient to demonstrate..." (p. 199); "the interested reader will find it instructive to trace the circularity of these arguments [about final \*-t] through a variety of passages... 61, 103-131, and 196 (note 494) are particularly informative" (p. 205). (RAM neglects to tell the reader that pp. 103-131 is the whole section devoted to "TB prefixes," closely reasoned and packed with hundreds of forms!) He is in effect asking the reader to please write the review for him, thereby saving him the trouble of substantiating his criticism. One can only regret that Miller used up so much of his space on literary embellishments that there was so little left for serious argumentation.

\* \* \*

(Linguistic)

Miller's more substantive remarks fall into several categories:

(a) Global criticisms of format, general quality, and self-consistency: [STC is hard to use; Benedict changed his mind on many points between 1940 and 1970; there are too many footnotes and they're too long; the older materials on which STC is based are largely inaccurate; the book has not been brought up to date to take account of recent scholarship.]

(b) Criticisms as to the scope of the investigation: [There is not enough about Chinese, and nothing at all about Tai.]

(c) Methodological criticisms: [Benedict does not subject his data to "the rigorous methodology of the comparative method" (n. 209), and in fact does not seem to understand the method at all.]

(d) Criticisms on specific details of reconstruction -- Benedict proto-system and its reflections in the daughter languages: [Voicing and aspiration of initial obstruents; proto-affixation; syllable-final consonants; "vowel gradation" and other morphological processes; metanalysis; semantic shifting through time.]

Before starting to wade through all this, it might be well to point out that several other scholars have already reviewed the STC, notably André-Georges Haudricourt, Søren Egerod, Kamil Sedláček, F. K. Lehman, Chou Fa-kao, and Kun Chang.<sup>4</sup>

Says Haudricourt: "...cet ouvrage est indispensable à tous les linguistes comparatistes de ces langues asiatiques, puisque depuis l'ouvrage de Wolfenden paru en 1929 aucune synthèse sérieuse et documentée n'avait été faite." (p. 495)

Says Sedláček: "Benedict's work is a deep source of knowledge on Sino-Tibetan and a necessary guide for everyone engaged in the study of the Sino-Tibetan family of languages." (p. 206)

Says Egerod: "It is, I think, a general feeling among specialists in the field that a real breakthrough is on its way, certainly partly aided by the clarity of thought exhibited by the Conspectus..." (p. 4)

Says Lehman: "...this is for Sino-Tibetanists a compelling work that attempts with considerable success to cover the entire field insofar as it is known, and to synthesize a critical new reconstruction of the language family and its history. It is the only such work available to date... As such, the book is indispensable for students and professionals... Nothing else in the field comes near it in combined scope and reliability... We are grateful to Benedict for all he has done to make comparative ST linguistics come of age."

(a) In connection with the matters raised in (a), I can do no better than to quote Professor Egerod further:

"The text of the published volume is substantially that of the 1942 manuscript. This is a wise decision, since it represents a coherent and ingenious summing-up of what could be known or surmised in the field of Sino-Tibetan at that time. Emendations and annotations are contained in an extensive apparatus of footnotes. To these have been added further footnotes by Matisoff... elucidating special points in the light of languages which have only recently come into focus, and adding further bibliographical information. The work is thus extraordinarily up-to-date in spite of its unique and drawn-out history." (ibid.)

Benedict has had some new ideas since 1940. He is not afraid of changing his mind when he feels he is in error or when new information forces him to rethink a problem. He does not feel that he is in possession of the absolute truth for all time. Instead of sitting back and enjoying the edifying spectacle of a great mind rethinking problems he has been consumed with for thirty years, RAM monotonously hammers away at the fact that sometimes the new footnotes contradict the text.<sup>5</sup> Of course -- that's one of the reasons that they are there!

No one denies that the quality of the sources used by STC was very uneven. Yet it is an insult to the memory of such brilliant and industrious scholars as B. H. Hodgson, Ola Hanson, W. Pettigrew, J. O. Fraser, J. H. Telford, A. Grünwedel, G. B. Mainwaring, J. R. Lorrain, F. W. Savidge, A. Judson, H. A. Jäschke, R. E. Neighbor, G. D. Walker, R. G. Brown, B. Houghton, and many

others, to imply that the data they collected was inadequate for serious comparative-historical work. Benedict used whatever he could get his hands on, but with a critical discrimination which his brilliant but eccentric colleague on the "Sino-Tibetan Linguistics Project," Robert Shafer,<sup>6</sup> unfortunately did not possess to the same degree.

It is true that even with all the new footnotes, the STC is still not totally "up-to-date" in the sense that it incorporates every bit of research in all the Sino-Tibetan languages that has been conducted up to the present day. The bulk of the editorial work on STC was done in 1968-69, and there has been an explosion of new material since then. In Nepal, the Summer Institute of Linguistics is producing highly accurate monographs on a dozen TB languages.<sup>7</sup> I just received in the mail a packet of 18 new dictionaries of Kuki-Naga and Barish languages put out by the Nagaland Bhasha Parishad.<sup>8</sup> The Loloish Tonal Split Revisited (TSR) came out only in 1972, and The Grammar of Lahu (GL) did not appear until 1973. The highly important papers produced for the annual Sino-Tibetan Conferences since 1968 are still largely unpublished, undergoing revisions by their authors.<sup>9</sup> (Incidentally, despite the fact that RAM prides himself on his expertise in the "labyrinths of the literature," he has remained on the sidelines throughout all this recent activity, and hasn't produced a paper for a Sino-Tibetan Conference since the first one, in 1968.)

(b) Few of RAM's insinuations are more unjust than the implication that the Chinese branch of ST is under-represented in STC: "There is precious little 'Sino-' here..." (p. 206). On the next page he modifies this, saying that it's just hard to see how much Chinese there really is in the book, since there's no Index of reconstructed Chinese forms. In point of fact, pp. 152-197 of STC (45 pages) are exclusively devoted to Chinese, mostly in the form of massive footnotes set in small type. This

is the area where Benedict's ideas have developed most dramatically over the years, and this section of the book is full of provocative new material. It is true that this part of *STC* is hard to use without an index. Fortunately the eminent Chinese scholar Chou Pa-kao has taken care of this once and for all with his handsome 85-page review article "Archaic Chinese and Sino-Tibetan." Chou has carefully gone through the whole Chinese part of *STC* and compiled a glossary of all Chinese morphemes discussed, arranged by their number in Karlgren's Grammata Serica Recensa and again alphabetically according to their English gloss, with numerous cross-references. Next to each Chinese character, Chou assembles everything Benedict says about its reconstruction, its likely TB cognates, etc. Can it be that RAM, with his bibliographical expertise, did not know of the existence of this article (distributed at the Sixth ST Conference, San Diego, October 1973)?

Besides the 323 Chinese morphemes for which *STC* provides etymologies, there are well over 500 TB and Karen roots reconstructed. From reading RAM's "review" one would never guess that such an enormous body of material is presented. Rather Miller complains that the Tai languages are not dealt with as well!<sup>10</sup> For RAM it is "almost perverse" (p. 207) not to include Tai, since this "(removes) from consideration a group of languages about which we have today a great deal of very reliable information... This seems a great pity." To this Benedict might well answer that it is an equal pity that he didn't also consider the Romance languages, since we know so much about them!

As is well known, Benedict has for 30 years been saying that Tai is related to Austronesian rather than ST. The long-awaited publication of his book Austro-Thai will indeed "turn more than the linguistic history of Asia upside down," as RAM fears (p. 207).



(c) Throughout his "review" Miller presumes to give Benedict lessons on the "methodology of comparative-historical linguistics." Benedict, says RAM, uses unscientific language, doesn't know about the regularity of sound-correspondences, twists the data to fit his preconceived theories, etc. To hear him talk, one would think that Miller was the world's most rigorous practitioner of strict neogrammarian orthodoxy, the "received tradition of the comparative method" (p. 199). In fact Miller's own theoretical views on methodology seem to have congealed rather later than that, perhaps somewhere in the mid-1930's. "Leonard Bloomfield would have been difficult to convince about this" (p. 199). (The point at issue is whether there can ever be "sporadic survival of rare, unshifted sounds continuing on into the later, changed languages, sometimes only in a single word." Apparently Miller is unacquainted with the copious recent literature on "lexical diffusion," though it is hard to imagine that he has never heard the phrase "Chaque mot a son histoire.")

Benedict is castigated for not sharing some of RAM's most cherished theoretical preconceptions. One of these is the "principle of maximum differentiation," which RAM interprets to mean that the proto-language must always be "more complex" than any of the daughter languages (pp. 200, 204-5). Since many modern ST languages have more than two manner-series of obstruents, how can Benedict only set up two series for the proto-language (see below)? This principle, as RAM appears to understand it, is so much hogwash. A daughter language may easily become more differentiated than its ancestor language if a conditioned split is followed by a loss of the conditioning factor for the split, so that the formerly redundant phonetic difference is phonologized. Lahu has nine vowels in stopped syllables, while Proto Lolo-Burmese had only six (see TSR). Shorto 1973 sets up only 7 simple vowels (long and short) and three diphthongs for Proto Mon-Khmer, but modern Cambodian has

no fewer than 31 vocalic nuclei.

RAM likes everything to be super-tidy and super-rigorous. He conceives of a proto-language as something entirely regular and homogeneous, "the original linguistic unity," with no internal variations whatsoever. Therefore every separate sound-correspondence among the daughter languages must automatically reflect a separate proto-entity.<sup>11</sup> He ridicules Benedict's discussion of proto-variation ("interchange") between PTB \*r- and \*y- (STC p. 34). "Does this mean," he asks, "it didn't matter which phoneme began a word in the proto-language?" (p. 206). In fact, of course, no language in the world, at any stage in its history, has ever been a completely regular and invariant system. Would RAM admit that there is an "interchange" between modern English w and r? Has he never played with a wittle wabbit?<sup>12</sup>

Miller even seems to find it difficult to believe that a morphological process could have been sporadic and irregular in a proto-language (p. 199). Does English have no sporadic morphological processes? Has Miller never seen teeth, geese, mice, children?

(d) Voicing, aspiration, prefixes, and tonogenesis. RAM's weak references to these matters betray unfamiliarity with the recent literature.<sup>13</sup> He does little more than lament that Benedict's treatment of the TB prefixes is "both unnecessarily and misleadingly complex" (p. 200). Let us quote Egerod again:

"Matisoff has shown elsewhere and summarizes in an important footnote in the Conspectus how the very complicated reflexes of TB stops in proto Burmese-Lolo can be reconciled with the reconstruction of only two sets of stop phonemes (voiceless and voiced) in PTB. Also in this case the actual modern result then depends on the presence and absence of certain prefixes in the reconstructed language." (op. cit., p. 501)

Besides affecting the voicing or aspiration of the root-initial,



the prefixes could metathesize with it, palatalize it, drive it out entirely ("prefix-preemption"), fuse with it into a single segment, drop altogether, be substituted for by another prefix -- and any or all of these activities could be accompanied by an effect on the tone of the syllable. It's hard to see how we can convince RAM that the behavior of the proto-prefixes really was complex, beyond suggesting that he start by reading some of my papers on the subject (Matisoff 1970, 1972a, 1972b, 1973b, 1973c, 1974a).

Vowel gradation. Miller's remarks on this matter carry no weight, in view of his own wild publication on the subject, alluded to above (Miller 1956). There, in his section "Burmese evidence for original PTB ablauts," he cites 26 sets of forms in Burmese which he says "are sufficient to demonstrate beyond question the existence of regular Ablaut relationships."<sup>14</sup> For 25 of these sets, the best that can be said is that the forms all begin with the same letter. Otherwise they bear no etymological relationship to one another whatsoever. The remaining one, cited as an example of \*i/\*a ablaut, does indeed involve the same morpheme:

"li:x 'the penis (vulg.)', la in laprwat 'private parts of a man'. Unfortunately, however, that which RAM transcribes as a is really nothing more than an unstressed shwa resulting from the reduction of the first syllable when it comes to stand in close juncture with a following syllable in the compound. (This stress-reduction is a frequent though "sporadic" morphological process in Burmese!)

Semantic shifts. Although RAM recognizes that we have to "reckon with semantic change in linguistic comparison" (p. 207) he implies that most if not all of Benedict's Chinese/TB comparisons involve unacceptably wide divergences of meaning.<sup>15</sup> He stops just short of accusing Benedict of dishonesty in tampering with the meanings

of the forms he compares:

"It would not be difficult to add further examples of the same problems, most of them originating in the slight and surely unwitting deflection of dictionary glosses in the direction of the meanings of the words with which a comparison is being sought. Chinese hsin 薪 is 'fuel'; but the gloss in these pages is altered ever so slightly, so that it becomes 'firewood', and from that point on an entire range of comparison with other words meaning 'tree, wood' -- but never 'fuel', 'firewood'!... This innocent deflection of meanings... soon becomes a very dangerous and misleading game indeed." (p. 208)

RAM could scarcely have picked a worse example to huff at!

The word hsin 薪 has meant FIREWOOD (and not 'fuel'!) from the earliest Chinese classical texts until the present day. Karlgren (GSR, p. 109, #382n) reconstructs the morpheme as \*siēn both for Archaic and Ancient Chinese, glossing it as 'firewood' and citing the Shī King as authority. Reflexes of the TB root \*sin - \*sik do indeed mean both 'tree' and 'wood/firewood' in innumerable TB languages. One example RAM could have found in the STC (p. 79, note 242) is Trung,<sup>16</sup> which has the forms: sin 'tree', sin-lap 'leaf', sin-wat 'flower', but also sin - sik 'firewood', sik-si 'fruit.' Also, if RAM had been reading the recent material from Nepal, he would have found a Tamang-Gurung-Thakali root \*syin meaning 'firewood'...

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I would like to finish on a more positive note. The long drought in Tibeto-Burman and Sino-Tibetan historical linguistics is over. The field is opening up now as never before. Where once it was the esoteric preserve of a few, it is now being enthusiastically taken up by a new generation of lively, inquiring, talented students. For much of this exciting new activity, we have Paul K. Benedict to thank.

## FOOTNOTES

- 1 Actually he does say (p. 197) that STC is "supplied with two excellently arranged Appendices."
- 2 Miller 1970. Compare my own review of Burling's work, Matisoff 1968.
- 3 "Mit gier'ger Hand nach Schätzen gräbt,  
Und froh ist, wenn er Regenwürmer findet." Goethe, Faust,  
lines 604-5. ["He grubs for treasures with a greedy hand/  
Rejoicing when he finds an angleworm." Trans. by JAM.  
Pace Fafner.]
- 4 See References. My colleague Chang's review is rather unfavorable, and is quoted with relish by RAM (note 8, p. 200), who calls it "definitive." See below, note 15, for Chang's views on semantic shifting. A further reply to Professor Chang is to be found in Matisoff, forthcoming.
- 5 "...a new footnote, that unblushingly contradicts the text ... " (p. 207); "...all this wavering back and forth..." (ibid.); "this type of confrontation simply shows contempt for the reader..." (p. 202). In one case, RAM actually plays fast and loose with the dating of the footnotes: "And how can one possibly claim, in 1972, that "the only adequate description of Tibetan tones" is to be found in Chao and Yü 1930 and Roerich 1931 (p. 86, note 254)?" (*italics mine*). But, as RAM realizes from the non-italic footnote number 254, this is an old footnote, not one added in the 1970's.
- 6 Shafer 1966-67 has also received the onslaught of Miller at his most sarcastic (1968a). A special Shafer retrospective presentation at the Eighth International Sino-Tibetan Conference, Berkeley, October 24-26, 1975, will pay tribute to Shafer's enduring contributions to Tibeto-Burman linguistics.
- 7 See, e.g., Hale, ed. 1973.
- 8 "The Linguistic Circle of Nagaland," Kohima, Nagaland, India. The languages for which dictionaries have already appeared are Angami, Ao, Chakhesang, Chang, Garo, Kheza, "Konyak," "Kuki," Liangmai, Lotha, Mao, Phom, Pochury, Rengma, Sangtam, Sema, Yimchungrü (Yimchurr), and Zeliang. We are now carrying on a

Berkeley an intensive fieldwork study of Angami Naga, with a native informant enrolled as a graduate student in archeology.

- 9 See Matisoff 1973e for a complete list of papers presented through 1972.
- 10 As Professor Chang says, "the author's decision to exclude Miao-Yao and Tai from his treatment was a wise one...the historical and comparative study of the TB languages (is) a topic large enough to consume anyone's talent and energy" (op. cit., p. 335).
- 11 Perhaps this is really what RAM means by his "principle of maximum differentiation," but his thinking is so confused on this point that it is hard to be sure.
- 12 For a discussion of variation between Lahu v and q̃ (< PLB \*w and \*r), see GL, p. 9. The subject of variation in all areas of linguistic structure has been widely discussed in the general linguistic literature during the past ten years, sparked largely by the work of William Labov.
- 13 The intensive research that has been going on in this area has just been brilliantly analyzed and put into the general perspective of diachronic phonological theory by Mazaudon 1975.
- 14 Pp. 47-49. Italics mine. RAM bitterly reproaches Benedict (passim) for stating his views with so much certitude and self-assurance.
- 15 In this RAM follows Chang (op. cit., p. 336), who objects to such "far-fetched" semantic comparisons as Chinese 'dry up (as a river)'/TB 'dry weather, heat, drought.' For a full-scale presentation of the evidence for massive semantic shifting that has gone on in TB, see my paper on TB body-part nomenclature (Matisoff 1974b, presented at the Seventh ST Conference, Atlanta). See also Matisoff 1973d.
- 16 A Nungish language studied by Lo Ch'ang-p'ei 1945. The Trung form is also mentioned in STC p. 180, right next to the Chinese form, so that RAM could scarcely have missed it.

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