Miller: all about Japanese
(A review of a review)

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Twenty years ago my first book, the Sino-Tibetan: a Conспектus (STC), appeared, with Jim Matisoff as contributing editor. The Sino-Tibetan field was even more esoteric in those days than at present and I feared the worst: few would read the book and even fewer appreciate it. I was hardly prepared for what followed, however. Roy Andrew Miller (hereafter RAM) reviewed STC at great length; more accurately, he wrote a 'review' article containing a great number of words. German as well as English.¹ He found absolutely nothing good about the book, not even (as Matisoff has observed) the paper on which it was printed. He made it all somehow seem to be a vast conspiracy against linguistic scholarship, the product of a collegiality of oligophrenics. Who in the world would read the book, let alone buy it?

My fears, as it turned out, were ill-founded. Matisoff helped by contributing a devastating refutation of the few substantive complaints made by RAM, notably about the replacement of my earlier reconstructed *-uɣ and *-iɣ by *-əɣ and *-əɣ, which yield a far better fit with Chinese.² The book did sell and people read it and even cited it. I was puzzled by this at first, but in time, making good use of my psychiatric background, arrived at the solution. Readers of RAM's review simply became curious: was it possible that two scholars like Benedict and Matisoff—or any two scholars for that matter—could have produced such a scandalous work? A good many of those readers clearly felt that only by actually reading the book would they be able to come up with an answer: fortunately, many read and some even bought the book as well.

I realized, as a research psychiatrist accustomed to handling problems of this sort, that this tentative conclusion needed a bit of substantiation and it was not long in coming. Three years later, in 1975, my second book made its appearance: Austro-Th(l)ai Language and Culture (ATLC), this time without the benefit of Matisoff's editing. Haudricourt contributed a brief Notice of it and later told me that no one would ever read it because "it makes the reader do too much work." He could have been right about that but, far more significantly, RAM chose not to review the work. Perhaps he

¹ See Miller 1974.
² See Matisoff 1975.
was still feeling exhausted from his labors on the Conspectus review—I've never asked him. In any event, ATLC has continued on its lonely way to the present day, unreviewed and unloved, probably also unread by all except the hardest souls. The publishers won't even answer my letters and I can't say that I blame them. I do blame RAM, however, for his failure to review it, which I'm sure has deprived me of a large readership. But I suppose that it's too late now.

We come, now, to my third book, Japanese/Austro-Tai (JAT), which RAM as a Japanologist was sure to tackle. Again his review takes up a lot of words and he finds absolutely nothing good about the book. He clearly has read parts of the book, as he must also have read sections of the Conspectus, but he apparently skipped some of the author's favorite passages, e.g., he writes (p. 167) "[enedict] recognizes no loan-words, no cultural borrowings," whereas in actuality I went to great pains to point out that the Proto-At (PAT) *?u- nominal/pronominial marker, a feature of Austronesian that is represented also in Kadai and Miao-Yao as well as in Japanese [cf. vara- < *?u-ala[k]< PAT *(?u-)alak 'child']3, remained productive up to a fairly later period, as indicated by the alternation in Japanese-Ryukyuan *anu - "vanu "I" (Old Jp. enclitic a- = va-) < *(?u-)aniku (reg. *?γk > *n shift via *γ, lacking in Jp.). from PAT *(?u-)a(γ)ku ( > P-Tai *kuA - *kavA via vocalic transfer), with prime support coming from early loans from Chinese (character coding replaced by characters).

Even better testimony to this effect is provided by three early Japanese loans from Chinese, all faunal or floral items and all provided with the *?u- prefix: Middle Chinese (7th century) 马 ma 'horse' > Jp. uma, 梅 muai 'plum' > Old Jp. umē ( < *u-mai). 胡麻 yuo-ma 'sesame' > Old Jp. uroma - Jp. goma. The 'sesame' doublet probably reflects, rather than loss of SYL-1, parallel lines of development from prefixed and unprefixed forms of the early loan" (JAT, 127).

RAM complains about everything in the book, even the presence of a "highly selective" list of References instead of a "Bibliography." I can only

3 See the discussion in Benedict 1988. P-Kadai *(u)alak ( > P-Tai *luuk < *luwk < *u alak vs. P-Kam-Sui *laak < *alak; both with vocalic transfer) parallels AN in showing the *?u- prefix as optional. The JAT account is incomplete: research since 1990 has shown that in kinship terminology the *?u- marker of AT is typically used with terms for descending generations. In Japanese it also appears in umago 'grandchild'; PMP *ma-kampus 'id.'; from the core PAT *'i-jama 'father/child' (signature AT self-reprocity), with -go (for ko) 'child' rather than PMP *kampus 'grandparent/grandchild'.
plead guilty here. I suppose; I did indeed "highly select," for all the key comparative linguistic sources on all four groups of Austro-Tai and for all sources of the multitude of forms cited in the work, including 22 sources in Chinese and 15 references for Robert Blust, a leading ANist. to only 14 for myself (this could get me banned from most linguistic societies). RAM also complains that much use is made of "unverifiable" sources, especially a 1979 mimeographed version of Samuel Martin's great "Early Japanese" work, which was of paramount importance in the preparation of JAT. By way of example here, RAM (p. 166) is reluctant to accept 'sugarcane' as an added gloss for ogi 'reed/cane,' even though JAT specifically credits it to Martin, concluding "no one has ever suggested it means 'sugarcane,' at least not until Blenedit]." Not until Martin, maybe, inasmuch as that scholar does indeed cite it, precisely as given in the Glossary of JAT. I don't like mimeographed source material any better than RAM does, but unlike him, perhaps because of my long dependence upon unpublished SEA sources, I've learned to live with it and I can only ask that RAM do likewise.

RAM even complains (p. 156) that in JAT I don't cite my own previous work on Japanese! He supplies it himself in fn. 17, a CAAAL publication of 1985, in a Japanese translation by Y. Nishi, with English title/subtitle: Breakthrough: Southeast Asia to Japan: Anthropologists and Linguists at Work (RAM crudely mistranslates as "A Rupture: . . . "). RAM deprecates this work but in a review in Language (1987) writes what I took to be complimentary things about me and my work while at the same time, as anticipated, not accepting the linkage of Japanese. I even wrote him a note of appreciation, giving him also the strange history of Breakthrough (BT). For some years the late Mantaro Hashimoto, a good friend of mine, had been after me to write an account of my work in SEA for the "popular" Japanese market. I had some misgivings about this but finally, in the early 1980's, did complete a draft of the work. By that time, however, Bob Ramsey had convinced me (see the BT account) that I should look over Japanese as a possible additional member of Austro-Tai and I did so, discovering before very long that it does indeed belong in the phylum. I naturally added a chapter on this to my BT draft, which somehow led to a decision by Hashimoto and his associates to publish the work not as a popular book for the mass Japanese market but as a scholarly work, in Computational Analysis of Asian and African Languages (CAAAL)! And without any note to that effect. BT naturally lacks even the hint of a scholarly apparatus and RAM, unaware of the history of the work, made much of this in his review. It appears that he had forgotten all about this historical note of mine when he wrote the JAT review, in which no mention is made of the obvious point that a

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4 See Benedict 1990b for a chapter of the original English version.
basically popular work such as BT has no place in the JAT list of references. Incidentally, this review by RAM does not appear to have helped BT very much; the rule obviously holds only for scholarly works.

The RAM review also contains some complaints about both the phonology and morphology of the JAT analysis, e.g., (p. 166), "anything and everything may all be dropped at will." Actually, JAT is if anything over-careful at times, e.g., (pp. 87-88) it cites regular loss of final *-ŋ (lacking in Jp.), as anticipated, in eight cognate sets, with the exception that after initial *t- the final -ŋ was assimilated to *-n, yielding the regular *-i, as shown in no fewer than four other cognate sets. The single possible counter-example here was excluded from the Glossary: PMP *tarunŋ - *tərunŋ (reduced doublet) 'cylindrical' (Dempwolff gloss); Jp. taru 'barrel', yet it can be argued that this form reflects dialectal variation, the old "Eastern" dialect regularly having had final -u for earlier *-ui (JAT, p. 45), as in the core cognate set for 'fire': Fu < *pu for Old Jp. Fū < *Fūi < *pu ( > Mod. hi); PMP *ʔapuy.

As regards morphology, RAM in the past has displayed an unfortunate tendency to be overwhelmed or "smothered" by affixes of one kind or another—an "affixation." If you like—e.g., he once claimed to have uncovered a whole system of infixes in Tibeto-Burman; see the discussion of this bétsé in STC (p. 124, fn. 340). This remains a problem for him. It appears, inasmuch as on p. 155 he confuses PAT *ma-, the "stative" prefix contrasting with the "transitive/causative" *pa- (JAT, p. 201), with the entirely distinct "intensive" *ma- + Cons. prefix of Japanese makuro 'jet black,' masshiro 'pure white' (both RAM citations, as well as massao 'deep blue' (JAT, p. 201), of undetermined origin).

Then there are RAM's dislikes. He doesn't like "split cognates," for one thing, attacking (pp. 149-50, 163-64) even a classical example of the genre: PAT *(m)pilī 'spirit/god' > P-Tai *phrīA 'spirit/demon/god' (reg. initial asp. and *l > /r/); Old Jp. Fī < *pi (reg. reduction-on-right) 'god' (in Kojiki deity names) > 'sun-god' > 'sun' (Mod. hi) and in compounds, e.g., hiko 'god (hi-) child (-ko)' = 'prince; male god'; also the morphan (Matisoff) -ri (reg. *l > /r/) preserved in Inari 'rice (ina-) god (-ri)' = 'god of harvests'. In reading over RAM's account I kept waiting for 'the real Inari to stand up,' only to find the disclaimer (fn. 5), "Actually, no satisfactory explanation or etymology of inari from within Japanese itself has yet been devised." Exactly so! One must step outside the language and look around in Austro-Tai, accepting also a Matisoffian morphan.