

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

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Twenty years ago my first book, the *Sino-Tibetan: a Conspectus (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 *-uʷ and *-iʷ by *-əʷ and *-əy, 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-T(h)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) "B[enedict] recognizes no loan-words, no cultural borrowings," whereas in actuality I went to great pains to point out that the Proto-At (PAT) $*\text{?}u$ - nominal/pronominal marker, a feature of Austronesian that is represented also in Kadai and Miao-Yao as well as in Japanese (cf. *vara-* < $*\text{?}u\text{-}a\text{la}[k]$ < PAT $*(\text{?}u\text{-})a\text{lak}$ '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) < $*(\text{?}u\text{-})a\eta ku$ (reg. $*\eta k$ > $*n$ shift via $*\eta$, lacking in Jp.), from PAT $*(\text{?}u\text{-})a(\eta)ku$ (> P-Tai $*ku^A$ ~ $*ka\text{v}^A$ 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 $*\text{?}u$ - prefix: Middle Chinese (7th century) 马 *ma* 'horse' > Jp. *uma*, 梅 *muai* 'plum' > Old Jp. *umē* (< $*u\text{-}ma\text{ }i$), 胡麻 *yuō-ma* 'sesame' > Old Jp. *ugoma* ~ 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 $*(\text{?}u\text{-})alak$ (> P-Tai $*luuk$ < $*lwak$ < $*u\text{-}alak$ vs. P-Kam-Sui $*laak$ < $*alak$, both with vocalic transfer) parallels AN in showing the $*\text{?}u$ - prefix as optional. The JAT account is incomplete; research since 1990 has shown that in kinship terminology the $*\text{?}u$ - marker of AT is typically used with terms for descending generations. In Japanese it also appears in *umago* 'grandchild'; PMP $*ma\text{-}kampu$ 'id.', from the core PAT $*(t\text{-})ama$ 'father/child' (signature AT self-reciprocity), with $*go$ (for ko) 'child' rather than PMP $*kampu$ '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 B[enedict]." 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: . . .").⁴ 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 Mantarō 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

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 $*taruŋ - *təruŋ$ (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. *Fi* < $*Fu$ < $*pui$ (> 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 *bêtise* 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 *makkuro* '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 $*(m)piʔi$ 'spirit/god' > P-Tai $*phri^A$ 'spirit/demon/god' (reg. initial asp. and $*l > /r/$); Old Jp. *Fi* < $*pi$ (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.

RAM also most emphatically does not like 'genital flipflop' (*LTBA* 5.1:21-24 (1974); 6.1:103 (1981); 14.1:143-146 (1991)). The JAT glossary has both a VULVA/PENIS and a PENIS/VULVA and it is clear that for RAM never the twain shall meet; vulvas are vulvas and penises are penises and (p. 166) "the less said about both [entries] the better." RAM should face up to the fact, however, pointed out in the first *LTBA* piece, that in mankind everywhere the two body parts are often found in close association, giving rise to a certain amount of switching. As a matter of fact, in the second cognate set mentioned above the Japanese switch is paralleled by one in the Philippines; for the benefit of *LTBA* readers, who by now should be well into 'genital flipflop,' this entry is repeated here, together with its NOTE (p. 228):

PENIS/VULVA P-Austro-Japanese *bo[t.C]oq

P-Austronesian *bu[t.C]uq: P-Hesperonesian *butu? 'penis'; also (Cebuano[Philippine]) 'vagina'.

Jp. Foto, OJ Fotō 'vulva'.

NOTE: For the semantic development, cf. VULVA/PENIS; also P-Austronesian *qutil: Puyuma (Paiwanic) qutil 'penis', Kanakanabu (Tsouic) utin 'id.'; P-Malayo-Polynesian *ʔu:ti = *ʔu:tin 'id.'; Northern Philippine: Inibaloi ʔu:tin 'vagina'. These involve the interplay of specifically paired antonyms, as in 'bow-and-arrow' switching, with one instance (Sino-Tibetan: Karen) of a complete 'genital flipflop'. In the case of P-Austro-Japanese *bo[t.C]oq (above), the basic shift appears to have been from 'penis' to 'vulva', with Japanese and Cebuano showing parallel developments, although in theory one might argue for an 'epicene' gloss at the Proto-Austro-Japanese level.⁵

It is easy to see what RAM dislikes but rather more difficult at times to see what he likes. JAT (p. 2) indicates that the handful of Japanese/MP etymologies cited by RAM in a 1983 article leave a great deal to be desired;

⁵ Blust (1988) cites *til 'small protruding part', with representation including *i(n)til 'clitoris': for Bikol (Phl.) he cites the doublet itil/itin < PAN-level *itil/*itil 'clitoris', the latter exactly matching Paiwanic: P-Rukai (Maga, Tona) *itili 'penis' (-i is 'echo-vowel'); cf. *qutil 'penis'. It would appear that in this allofamily, at any rate, the clitoris served as a semantic bridge, so to speak. In another instance the underlying connection appears to have been 'split/slit': (Paiwanic) Thao tú:ra1 < PAN-level *tułay 'penis', PMP *təlay < *təlay: Kiput (Borneo) tə-təlay 'vagina'; Proto-Polynesian *tole 'pudenda muliebria', but Maori tore '(comp.) inflamed, cut, split'; cf. West MP *ipak 'split', Bintulu ipak 'female pudenda' (Blust: forthcoming *AN Comp. Dict.*); also PAN *bəlaq 'split', Paiwan vəlaq 'split, rip; vulva'.

one might have thought that RAM would let these sleeping dogs lie but on p. 154 he trots them all out:

In this article the reviewer cited . . . four Japanese words that appear to have likely Malayo-Polynesian etymologies, OJ *Fisi* 'sandbar', PMP **pat* 'iy 'seashore'; OJ *isa* 'whale', PMP **it* 'i 'flesh, meat'; J *ika* 'cuttlefish', PMP **ikan* 'fish' and *hana* 'flower', PMP **buŋa* 'id.'.

The first two of these, both from Old Jp., are "millerisms" (p. 37), combining dubious or impossible semantics with impossible phonologies. RAM wisely makes no attempt to defend them but it should be noted that Japanese does indeed have a cognate for the second PMP root: *shishi* 'flesh/meat', paralleling Formosan reduplicated forms: Bunun (Paiwanic) *titi*, P-Atayalic **hi* i? = **hi* ?[h]i? 'id.' (JAT: Glossary under FLESH). On p. 155 he states that his 1983 article "gave not two but four OJ examples" and criticizes JAT for citing only the two "millerisms." As can be seen from the above excerpt, however, that article cites only two Old Jp. words, both rejected as "millerisms," the other pair being from Mod. Jp. RAM charges that both were "copied out and taken over without credit to their source, i.e., plagiarized" (p. 155). The Glossary of JAT does indeed include an entry: FLOWER, with Jp. *hana*, but under the AT root: **baŋa* l, with regular /n/ for *ŋ, as in 'I' (above), and proper vocalic correspondences, represented not only in AN but apparently also in Kadai: Laha *baal* < **ba* [ŋ] a l. RAM appears to have introduced the fourth set, 'cuttlefish/fish', but with semantics in need of clarification, as provided by the JAT analysis: both 'sthg. eaten' = 'food' < PAT **ka* ? 'eat' (squid an important Japanese food source); as in the *Conspectus*, however, and, e.g., Coblin 1986, no attempt is made in JAT to track down or cite the original sources, many of venerable standing, for the host of cognate sets that are cited. The basic question about RAM's 1983 article remains the same: why not the core root for 'fire' (see forms cited above) and the like? Was RAM really trying?

The RAM review contains some more general remarks, both about JAT and about its author, and one would wish that at least some of them were accurate:

An infectious spirit of unalloyed confidence and self-congratulatory enthusiasm has long distinguished B[enedict]'s adventures in the historical-linguistic arena. More than anything else about his work, it is his striking élan that has largely accounted for his earlier Thai-Kadai and Sino-Tibetan lucubrations finding their way to prompt

and almost general acceptance in the secondary literature. . . . [I]n most quarters, scholarly as well as popular, Benedict's formulations have generally found ready acceptance, as also surely will the astonishing hypothesis of this new book. Indeed . . . even before publication, it was being hailed as a "masterpiece" by professional academics. (pp. 148-149)

Benedict's postulations, no matter how far-fetched or divorced from linguistic reality they may be, appear to have an inherent ability to gain a firm and loyal following, as witness the earlier successes of his Thai-Kadai and Sino-Tibetan. In the way of the world, Japanese/Austro-Tai cannot be far behind. The more one documents that Benedict's forms are wrong, his methods wrong, and his conclusions highly unlikely, the more they win acceptance in most quarters. This book, already hailed as a "masterpiece," will surely be well received. (pp. 166-167)

I do devoutly wish that at least some of this were true. I have long tried to cultivate a suitable élan; it has never worked very well with women and until this review by RAM I had naively believed that hundreds and hundreds of cognate sets and scores and scores of phonological rules and morphosyntactic observations had played a role in whatever success my works actually have achieved. I'd gladly settle for élan, to be honest about it, but unfortunately all the evidence indicates that once again RAM has blundered. Chinese scholars, apparently unaffected by my élan, continue to present a kind of classificatory "Monroe doctrine" for SEA, attaching both Kadai and Miao-Yao to Sino-Tibetan, while even Western scholars at times give a certain credence to this linguistic skimble-skamble, presenting a "broader" as well as a "narrower" view of the phylum. Austro-Tai? Here RAM has apparently been in touch with a different set of scholars than I have—say, from Mars. The scholars whom I know about, on Earth, have left the whole AT field to me, to wander about at will over the years, picking up goodies here and there. They do, to be sure, write about Austro-Tai at times, generally in a tone of vast ambivalence ("interesting," "thought-provoking," etc.), as if they were running for office. Matisoff did call JAT a "masterpiece," as reported by RAM, but he added the abjurational "in its way," which RAM very wisely omitted—and Jim is an old, old friend of mine! The ANists refuse to travel to the mainland, feeling apprehensive even about Hainan, and if you gave a Malay dictionary to Bill Gedney, dean of Tai scholars, he'd probably hide it somewhere. I've spent years scolding my colleagues about their failure to "see" Austro-Tai, at times even implying a degree of idiocy on their part. Now with this review of JAT by RAM I can at least look forward to a good sale of the book, perhaps even some followers.

But I can't help wishing that RAM somehow turns out to be right about my élan!

RAM concludes his review by accusing me not only of destroying Japanese linguistics but also of threatening to subvert the very foundations of Western civilization:

In all these pages he destroys much of what is already known and understood of the history of Japanese, and what he does not destroy he conceals and obfuscates. (p. 167)

Finally, all this matters because of a reason that far exceeds the narrow concerns and confines of Japanology. It matters because of the great and ever-present danger that more than one unwary student of language and linguistics will surely be seduced into confusing Blenedict's phonological necromancy and his semantic mumbo-jumbo with the comparative method of the neo-grammarians, and as a further consequence of that seduction, conclude understandably but unfortunately that since the former are patently absurd, so also must be the latter. . . [T]he comparative method of the neo-grammarians is . . . one of the most creditable intellectual edifices of Western man. To play it as if it were a game . . . is to consign the entire achievement of the neo-grammarians to the junk-heap. This is where the real and greatest danger of this book lies. It cannot be denounced in terms too strong or in tones too unfriendly; too much of value to us all is at stake. (pp. 166-167)

I haven't been denounced that strongly since my days in psychiatry, when I was frequently accused of trying to subvert Western civilization by advocating sexual freedom (here civilization has finally caught up with me) as well as the decriminalization of prostitution and drug use (here civilization still lags). I was thrown off drug councils and even achieved a certain notoriety in the New York metropolitan press, attaining what my children still view as the pinnacle of my career even though I did fail to get on the Nixon blacklist. RAM's vilipending and pettifoggery make me feel almost at home. He is right about one thing, however: linguistics is indeed a game of some sort for me, in part because I love it, in part also because I've never been able to get anyone to pay me for doing it. Sapir was my first, and greatest, teacher and perhaps I should blame him for showing me what a lovely mistress linguistics can be. Really, I don't want to consign anyone's anything to a junk-heap even though I suspect that a good bit of it might well belong there. And I do wish that RAM and all my colleagues would devote themselves to playing the game—and may I add, better?

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