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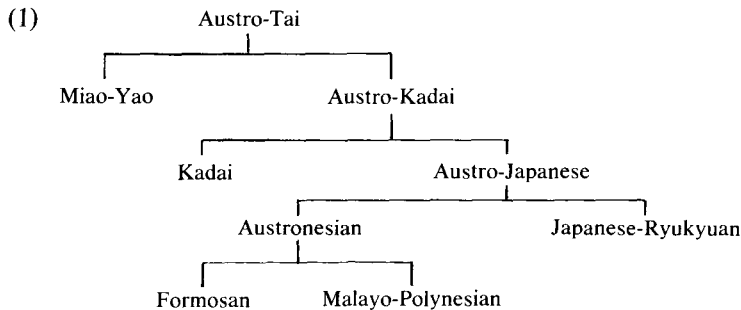
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1. INTRODUCTION.¹ Benedict's thesis is that Japanese is a member of the Austro-Tai stock, fitting into the family tree in 1. Japanese is thus the latest addition to a stock that began as a three-way hookup of 'Thai, Kadai and Indonesian', to quote the title of B's original 1942 article. In the intervening time the proposed genetic group has received one major addition, Miao-Yao, and several minor adjustments of terminology and subgrouping (the original article's 'Indonesian' and 'Thai' are now called Austronesian and Tai, and Tai is just one branch of a larger stock now named Kadai).² As for the status of Austro-Tai, both skepticism and acceptance can be found among specialists.

¹ Abbreviations: AN = Austronesian, AT = Austro-Tai, Jp = Japanese, KD = Kadai, MY = Miao-Yao, OJ = Old Japanese, and AJ = Austronesian-Japanese.

² 'Kadai' began as B's coinage to cover a collection of little-known languages that he conceived of as a 'bridge' between Tai and Austronesian. There is now consensus (although perhaps not unanimity) that the languages formerly labeled 'Kadai' (a) are more closely related to Tai and its sisters than they are related (if at all) to Austronesian, and (b) do not form a well-defined subgroup as opposed to Tai et al.—leaving the name 'Kadai' free for application to the grouping containing Tai and all its recognized congeners. The terminological adjustment is originally due to Haudricourt 1967; see also Edmondson & Solnit (1988:3–5) for discussion.

But the acceptance is largely limited to using Austro-Tai as a parking place for Kadai and Miao-Yao when a wide-ranging classification is called for, as in Maddieson 1984 (although Maddieson puts Miao-Yao in Sino-Tibetan). What is missing so far (except from Benedict himself) is diachronic work assuming the Austro-Tai framework and actively involving Kadai, Miao-Yao, and Austronesian, or some subset.



B's book consists of a glossary of reconstructed etyma (161–264)—about 200, at a very rough guess—preceded by discussions of the correspondences between Japanese and Austro-Tai (rather, of the Japanese reflexes of proto-Austro-Tai phonemes). The main discussion sections deal with morpheme shapes (i.e. the parts of the complex Austro-Tai etyma that are retained in the simpler Japanese reflexes; 19–32), Japanese vocalic reflexes (33–49), Japanese consonant reflexes (50–108), suprasegmentals (109–20), morphological features (121–36), and lexical features (i.e. patterns in small closed semantic fields such as numerals and kinship terms; 137–50).

2. JAPANESE/AUSTRO-TAI OR JAPANESE/AUSTRONESIAN? To put the good news first, B does, in my opinion, demonstrate a significant degree of regular phonological correspondence between Japanese and Austronesian. He is by no means the first to make the attempt (for a summary, see Shibatani 1990:103–9), as he recognizes; but he is dismissive of the earlier work (which may explain why he cites so little previous scholarship on the topic). The main distinction of *Japanese/Austro-Tai* at first glance is its attempt to fit Japanese into the Austro-Tai stock rather than simply pairing it with Austronesian. One might even hope that, besides solving the riddle of the affiliation of Japanese, adding this new member would have the reciprocal benefit of providing new evidence for the Austro-Tai grouping.

Such a hope is for the most part unfulfilled: the bulk of the book is devoted to Japanese-Austronesian correspondences. The parent Austro-Japanese is reconstructed, but the main theme of the book is still a binary comparison. Even when non-Austronesian evidence (usually Kadai, less often Miao-Yao) is adduced, it has little bearing on the Japanese-Austronesian relation (more on this below). Of course the dominant role of Austronesian may, as B claims, simply reflect the facts, namely that Japanese and Austronesian are closer in the Aus-

tro-Tai tree than either is to Kadai or Miao-Yao. But the result is that *Japanese/Austro-Tai* effectively comprises two independent books, a more substantial one comparing Japanese and Austronesian and a lesser one reworking the higher levels of Austro-Tai, sometimes in the light of the Japanese-Austronesian relation, but most often simply revising the Proto-Kadai reconstructions.

This leads me to a disclaimer: such expertise as I can claim concerns Kadai and Miao-Yao, and does not extend to Austronesian or Japanese; to the latter especially I am an outsider. I thus approach the main, Austronesian-Japanese, portion of the book as an interested general diachronist. I will by and large take the Japanese and Austronesian data at face value, leaving its evaluation to others. I will of course offer some 'insider's' commentary on B's use of Kadai and Miao-Yao.

This is perhaps the place to mention B's earlier publication describing the proposed Japanese/Austro-Tai connection. Benedict 1985 is published only in Japanese translation, and is known to me only through the review by Miller in this journal (1987). Miller criticizes what in his view is B's misinterpretation of many Japanese and Old Japanese forms, and B has evidently accepted many of these criticisms, judging by his deletion of some of the etymologies based on such forms (for instance, *hani* 'ocher', *beni* 'saffron', and *hanikan-* 'bashful' are no longer related to AN **baŋik*).

3. THE JAPANESE-AUSTRONESIAN COMPARISON. A few words are in order on the roles played by the three Austro-Tai branches (Austronesian, Kadai, and Miao-Yao) in the comparison with Japanese. The preponderance of Austronesian is evident from the small number of etyma (I count 14) in which Kadai and/or Miao-Yao provide the only cognates to Japanese in the absence of Austronesian candidates. When both Austronesian and non-Austronesian cognates are given, the non-Austronesian items usually make no difference to the Japanese-Austronesian correspondences. As an example,³ consider HORN: AN **tsuŋu* = Japanese *tuno*, all derived from Austro-Kadai **tsuŋəw*. The segmental correspondences can all be supported by several other Austronesian-Japanese sets. Bringing in the Kadai and Miao-Yao words for 'cattle, ox' (e.g. Siamese *wua*, Shan *ŋó*, pMY **ŋo(ŋ)*⁴) neither adds to nor detracts from the Austronesian-Japanese comparison; and, by the same token, the Austronesian-Kadai-Miao-Yao comparison can be evaluated independently of the Austronesian-Japanese comparison.

There are a few exceptions to the rule, in which the non-Austronesian evidence does make a difference; one example is GOD/SUN-GOD/SUN. Simplifying somewhat, Japanese *hi* <OJ *Fi* 'sun', also 'spirit' in compounds (e.g. *Fiko* 'prince; male god', from *Fi* 'god' + *ko* 'child'), is compared with Saaroa

³ Notation as in works cited, except that I use /t l/ (retroflex stop and lateral) where Austronesianists conventionally have *t l* (with subscribed dot). Note also for Austronesian the use of apostrophe to show palatality, most often as [t']; B's distinction of 'alveolo-palatal' [tʃ dʒ ʃ] versus 'palatal' [tʃ dʒ ɛ ʒ]; for Japanese the use of F for [ɸ], assumed to be the intermediate stage between older **p* and modern /h/. Words in CAPITALS are the names of Benedict's Austro-Japanese etyma.

(Formosan) *pili* 'shadow'. This Japanese-Austronesian comparison is less than supremely convincing, both semantically (B offers English *shade* in support) and because of the single-language attestation in Austronesian. Matters are improved by bringing in Tai (Southwestern and Central) **phri*^A 'devil, ghost, demon', which has a plausible phonetic resemblance to both Japanese and Austronesian, and is a good semantic fit with the Japanese.

The Austronesian-heavy approach is also evident in the Morphology section (Ch. 9), in which B discusses Japanese reflexes of reconstructable Austronesian affixes. These elements are in most instances either already fossilized in Old Japanese (though recognized as such by Japanologists) or not even analyzed as affixes (e.g. the *a-* in *abara* 'ribs').

I turn now to the Japanese-Austronesian comparison itself. As mentioned above, there is a significant degree of regular correspondence here. But it is very hard to get a feel for what degree, because B's presentation leaves things fuzzy around the edges.

B says (18) that his approach in this book is 'ultra-conservative, reductionist'. This is certainly true in comparison with his previous works, notably Benedict 1975, to which the present volume is in a way a sequel. In particular, in the present work the proposed regular correspondences are carefully exemplified (by lists referring to the Glossary entries), and that is all to the good. But on a more absolute scale the presentation is not all that conservative, and its impact is much diluted in consequence. Conservatism is indeed appropriate, since the position B is arguing for, the Japanese-Austronesian link, has in essence already been put forward by others—but unsuccessfully, insofar as the question of the genetic affiliation of Japanese remains controversial. A more convincingly ultra-conservative approach would include:

(i) High standards for attributing a given form to a protolanguage; in particular, attestation in Old Japanese and presence of reflexes in some minimum number of branches/subgroups of Austronesian. In contrast, B does not hesitate to cite Japanese forms not attested in Old Japanese, and to posit proto-Austronesian etyma on the strength of forms from a small number of languages, often only one.

(ii) Limitations on posited semantic shifts. While this is a notoriously difficult area, I think most would agree that it is less than ultra-conservative—contra B's assertion that 'most of the cognate sets uncovered in this study do not exhibit any significant range in meaning' (147)—to posit cognacy of words meaning 'sun' and 'shadow' (above; even with the intervening 'spirit'); 'penis' and 'vulva' (PENIS/VULVA, VULVA/PENIS); 'swamp', 'irrigated rice-field' on one hand and 'millet' on the other (SWAMP/FIELD(WET)/RICE/MILLET); 'star' and 'moon' (STAR/MOON). It is true that in nearly every such case B cites a parallel example, usually from a cognate language, but that simply shows that the proposed shift is possible, not that it falls within any particularly narrow range of semantic shift.

The foregoing is not meant to imply that all or even much of B's material falls outside such conservative limits; the problem is that B has left it up to

the reader to do the sorting. By the same token, material that fails such tests is by no means to be discarded: it has its place, which is AFTER regular correspondences have been established according to the conservative criteria.

Finally, there is the fact that any given Japanese form could, in B's framework, regularly correspond to a great many possible Austronesian forms. This is in part compelled by the data itself, since Austronesian CVC syllables with a rich consonant system (especially in place contrasts) must match Japanese CV syllables with a far simpler consonantism. Matters are compounded by B's assumption of a regular correspondence of Austronesian obstruents to Japanese nasals, via Proto-AJ prenasalized obstruents. Thus Jp /n/ could correspond to at least /n ñ ŋ t d k/ (Japanese has no velar nasal phoneme). It does not help to note that Austronesian, which does have word-medial prenasalized stops (called 'stops with nasal increment' in the literature), often does not have them in the etyma in question. Moreover, Japanese often 'retains' only a single syllable of a bisyllabic etymon, as in ROOT: AN **ʔaka*[r] = Japanese *ne* (via **ŋkai* from Proto-Austro-Japanese **ʔa(ŋ)kaz*; or in extreme cases a single segment, as in HAND/FIVE: Jp *i-* (in *itutu*) = AN **lima* or **lima*. It's not that such things don't happen; the difficulty is to demonstrate convincingly that it did happen in this particular case. My only suggestion in the face of such a possibility of many-to-one correspondences is statistical testing. Usually, when diachronists speak of resemblances as unlikely to be due to chance, we do not bother to quantify just how much the resemblance in question exceeds what could be expected as the result of chance. If any given Japanese morpheme could correspond to a fairly high number of possible Austronesian morphemes, the possibility of chance resemblance ought also to be fairly high. It remains possible that the degree of Japanese-Austronesian resemblance is still significantly higher (in the technical sense), but it would be good to see it demonstrated.

Here it should be noted that the correspondences are segmental only: in discussing suprasegmentals, while expressing hope for the future, B acknowledges an inability to find correspondences between the Japanese pitch-accent system and either the Kadai/Miao-Yao tones or other suprasegmental features found in several Austronesian subgroups (but not so far reconstructable at any level higher than Proto-Philippine).

4. KADAI AND MIAO-YAO. As for B's revamping of the upper levels of Austro-Tai, its most noteworthy component is what amounts to a partial schema for reconstructing a bisyllabic Proto-Kadai (the Miao-Yao material is much scantier and, with a few exceptions, involves relatively minor changes in the source reconstructions). This Proto-Kadai schema is not explained, but is implicit in a chart explaining transcriptions (17) and in Proto-Kadai reconstructions spread through the glossary, and may be reconstituted as follows.

The basic assumption is that the monosyllabic forms, attested in the daughter languages and reconstructed for the intermediate-level proto-languages (Tai, Kam-Sui, Hlai=Li), in many cases descend from disyllabic Proto-Kadai

forms,⁴ the reduction nearly always deleting the initial syllable but leaving behind some effects on the remaining syllable. The lost 'extra' syllable is reconstructed with varying degrees of specification, ranging from syllabicity only (e.g. *[SYL]puak) to partial specification of the initial consonant (spirant or stop, palatalizing or labializing) and the vowel (palatalizing or labializing).⁵ All such reconstructions represent effects on the surviving syllable.

I am in agreement with the concept of a bisyllabic Proto-Kadai with partially-specified initial syllables. The notion has been advocated elsewhere; cf. Haudricourt 1956, 1975, Edmondson & Solnit 1988, and Edmondson & Yang 1988. And the same basic approach is utilized for Proto-Viet-Muong by Thompson 1976. What is needed now is a rigorous and detailed exposition of the whole problem, with explicit consideration of what can be known from Kadai-internal evidence and what advantages may be derived from assuming cognacy with Austronesian.

5. COMPETING PROPOSALS. Along with the link to Austronesian, two long-standing proposals on the genetic affiliation of Japanese are one linking Japanese with Altaic (via Korean) and one involving some sort of Altaic-Austronesian combination, usually with Austronesian as a substratum. B has virtually nothing to say about these competing theories, apart from two passing references to the Korean (-Altaic) proposal (1, 158), the second suggesting that four Japanese-Korean pairs, which are 'without likely Altaic cognates' and for which he proposes Austro-Tai cognates, may be loans from Japanese to Korean. It would be asking too much to expect B to have included a full-scale evaluation of the Korean proposal, but I find it unrealistic to pretend that the Austronesian-Japanese link can be considered in a vacuum.

As one example of the type of evaluation that might be made, I offer the following comparison of core lexical items. The basis is an 89-item list from a recent lexicostatistical survey of Altaic, including Korean and Japanese (Starostin 1986). I first compare Starostin's list with all of B's etyma whose gloss includes the meaning in question, even if the Japanese word does not exemplify the meaning—as with OJ *mōtō* 'base, foundation, root', included under 'belly' as cognate with AN (MP) **bətəŋ* 'belly'.⁶ The resulting comparative list, given in Table 1, includes more than 89 items, since (1) both authors offer more than one etymon for some meanings, and (2) the authors sometimes differ on which Japanese word they are claiming cognacy for; under 'tree', for example, Starostin gives an Altaic comparison for OJ *mōri* 'woods' (Proto-Altaic **mōrV*), while B has an Austronesian comparison for OJ *kē, kī* (Proto-AN **kaS₂iw*). I

⁴ In this summary I ignore the occasional Proto-Kadai form that does not conform to the schema described, e.g. those with more than two syllables, full-specified vowel in the lost initial syllable, or other unexplained notation.

⁵ It is unclear why C and V should both be specifiable for [palatalizing] and [labializing], suggesting a four-way contrast for each feature, e.g. C_{pal}V, CV_{pal}, C_{pal}V_{pal}, CV).

⁶ B cites as parallel the Fijian reflex *boto-* 'bottom'; it is unclear whether Austronesianists agree with B that this Fijian form is a reflex of the cited P-Austronesian etymon.

have made no attempt to evaluate the quality of the comparisons; the idea is to compare both proposals at face value, with the lists regarded as showing the maximum that the two advocates feel able to claim. In the tables, ‘competing’ means that a single Japanese item is compared to Altaic by Starostin and to Austronesian by Benedict. The Austronesian figure goes up if we add some core-vocabulary items not on Starostin’s list, on the assumption that Starostin would have included these items if he had been able to formulate Altaic etymologies for them. Adding ‘bird’, ‘die’, ‘horn’, and ‘short’ yields the figures in Table 2.

AN cognates only	31	28%
Alt cognates only	45	40%
competing	25	23%
missing altogether	10	9%
total	111	

TABLE 1.

AN cognates only	35	30%
Alt cognates only	45	39%
competing	25	22%
missing altogether	10	9%
total	115	

TABLE 2.

Some surprisingly basic items are completely without etymologies in the present comparison: *ash*, *give*, *fat* (n), *head*, *liver*, *neck*, *sit*, *tongue*, *what*, and others. Although, I repeat, these numbers represent no more than a preliminary run-through, it is suggestive that they seem to mirror the notion of co-existing Austronesian and Altaic strata in the Japanese lexicon.

The idea that Japanese has some sort of combination of Altaic and Austronesian connections is also not without its partisans (Miller 1987 cites many works by Shichirō Murayama). B does not offer extensive arguments against the possibility of borrowing, although perhaps the discussion of ‘“core” quality’ cognate sets, mostly body parts⁷ (137–8), counts as germane to the question. Especially interesting in this connection is the sizable number of his Austro-Japanese etyma that have only Formosan cognates in Austronesian; two examples (note also the single-language attestation in Formosan): BONE: Ci’uli (Atayal) *bani*?; Japanese *Fone*, *-bane* in *kabane* (*kara* ‘husk, corpse’ + *bane* ‘bone’); RICE/COOKED RICE¹¹: Japanese *momi* ‘hulled rice’, Atayal *mami*? ‘cooked rice’.

Such resemblances could be attributed to the retention in conservative Formosan of etyma lost in the rest of Austronesian, but B goes further and suggests

⁷ But the ‘virtually complete Austro-Tai/Japanese *manikin*’ lacks not only internal organs, as B recognizes, but also head, nose, tongue, nails/claws, and neck. Starostin gives Altaic etymologies for nose, claw, and heart; tongue, liver, head, and neck have nothing.

that 'Japanese and Formosan ... may have been in prolonged contact for a period of time following the primary Formosan/Malayo-Polynesian split' (151).

But 'prolonged contact' is precisely the situation required for extensive borrowing. B allows for only one or perhaps two Austronesian-to-Japanese loans (BARK CLOTH plus maybe HAMMER).

6. MECHANICS. The book is not very well bound: my copy is shedding pages after only several months of use. There are many printing errors, more even than can be expected in a text using a fairly large repertoire of phonetic symbols. Some are simply skewed formatting and various garden-variety typos, including at least one complete omission of the Japanese cognate in a glossary entry: HOUSE (213) should include Jp *ya*. There are also a great number of what appear to be survivals of pre-printout versions of the text, in the form of the name of a special character enclosed in brackets; for instance, $*W_1a[\text{theta}]u$ (181) must mean $*W_1a\theta u$. One example, particularly awful because it spans two lines, appears under HORN (213): '[Chinese character #4737] Archaic $\eta[i \text{ sub-inverted-breve}]w[\text{schwa breve}]g^A$ '. I found myself compelled to write $\eta j w \ddot{a} g$ in the margin.

The foregoing example leads me to another type of complaint, namely, the incomplete explanation of notational and other conventions used in citations. I am unsure whether to blame B or the publisher, but we are not told what numbering system is used to identify the Chinese characters, or what the source of the Archaic reconstructions is. Although it is usually possible to determine the source of intermediate-level reconstructed forms, the frequent cases in which B cites another author's reconstruction along with his own modification of it follow the form Proto-X $*y = *z$, where B never tells us which is which; I infer that $*y$ is the original and $*z$ is B's modification. Other warnings to readers: B labels the Kadai tones as in his previous publications, with B and C standing for what Li Fang-kuei and most others would call C and B respectively (Siamese script *máj thoo*, *máj èek*). The Southwestern branch of Tai is called 'Southern': no harm done, but why not be explicit about the change?

7. CONCLUSION. So, is Japanese Austro-Tai, Altaic, both, or neither? I would say that Benedict has established a *prima facie* case for a Japanese-Austronesian correspondence, largely independent of additional affiliations for Austronesian. The case needs to be examined by experts in Japanese and Austronesian, and to be compared with the case made for correspondence with Korean(-Altaic). Finally, the correspondences with Austronesian and with Altaic, to the extent that both are valid, need to be evaluated and placed in relation to each other, whether that entails choosing one as inherited and the other as borrowed, or whether Japanese is one of those rare cases having in its past a break in normal genetic transmission (Thomason & Kaufman 1988).

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The London-Lund Corpus of Spoken English: Description and research. Edited by JAN SVARTVIK. (Lund studies in English 82.) Lund: Lund University Press, 1990. Pp. 350.

Reviewed by W. NELSON FRANCIS, *Brown University*

The thirteen papers which comprise this volume are all concerned in one way or another with the London-Lund Corpus of Spoken English (LLC). The dominant theme is the development of a realistic text-to-speech computer program—one in which written or printed English is converted into a naturalistic counterpart of spoken English. Such a program must produce not only the