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Medieval Tibeto-Burman Languages II  
Edited by Christopher I. Beckwith  
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Reviewed by Scott DeLancey

As is evident from the title, this volume is the second of its kind, following Beckwith 2002a. The range of this second volume is less broad than the first; most of the nine papers are concerned with Tibetan or Burmese, with one paper each on Qiangic and Lolo, and two addressing more general issues in Tibeto-Burman. These latter are, in my opinion, less useful than the others, which all make very focused and specific contributions.

Two papers deal with earlier stages of Burmese. Christian Bauer, "Reflections on early Mon-Burmese Grammar", presents inscriptional evidence for Mon "epigraphic activity" continuing much later than has previously thought. The substance of Bauer's contribution is a list of six grammatical morphemes which occur in both early Mon and early Burmese inscriptions, thus demonstrating grammatical influence of Old Mon on Burmese (and, possibly, in the other direction as well).

Rudolf Yanson, "Notes on the evolution of the Burmese phonological system" reconstructs the sequence of changes in initial clusters and rhymes from Old Burmese to Modern Burmese. Much of the discussion of initials is devoted to the development of original clusters of stop + medial liquid or glide; one notable suggestion is that the ascription to Old Burmese of the initial cluster  $\eta r$  is erroneous, as it occurs in OB only in loanwords. This cluster has been reconstructed for PTB (Benedict 1972:44; Matisoff 2003:81) purely on the basis of its occurrence in the modern spelling of three Burmese words, whose cognates in other languages consistently have a -y- medial. Thus Yanson's argument that these spellings are secondary to the merger of \*r and \*y in Burmese, and not traceable to the earliest attested stage of the language, is a striking example of the value of the contribution which historical studies of this kind can make to comparative TB.

Three papers deal primarily with Tibetan. Beckwith's "Old Tibetan and the dialects and periodization of Old Chinese", is an addition to Beckwith's work on interrelated issues of Chinese reconstruction, the "Sino-Tibetan problem", and prehistoric language contact in Inner and East Asia (see Beckwith 2002b). In this detailed and very interesting essay in Sino-Tibetan etymology, Beckwith examines Tibetan and Chinese forms for 'black', 'ink', 'dark', 'iron', 'night', and 'moon/month', rejecting some relations among them, and proposing other new connections. The conclusion of the broadest interest is the observation that some of the etyma discussed here have evident Indo-

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European connections, and thus cannot be adduced in support of the genetic unity of Sino-Tibetan (and thus, as the author points out, are, *a fortiori*, not consistent with van Driem's Sino-Bodic hypothesis). While many of the papers in this volume, quite appropriately, deal with a level of language-specific detail which will fall outside the interest of most readers, this paper and Zeisler's (below) are packed with detailed data, analysis, complex conclusions, and well-informed side comments among which virtually any Tibeto-Burmanist or Sino-Tibetanist should find much worth pondering.

A second paper by Beckwith, "The Sonority Sequencing Principle and Old Tibetan Syllable Margins" is more of a contribution to phonological typology and theory than to Tibeto-Burman studies as such. Beckwith points out that the orthographic Tibetan syllable canon, like that of modern Polish, contradicts the widespread belief that the internal structure of consonant clusters in syllable onsets and codas follows the Sonority Hierarchy, so that less sonorous consonants will be closer to the syllable margin, and more sonorous consonants closer to the nucleus. To take a famous example, the sequence of consonants in *brgyad* 'eight' violates the SSP in that the liquid /r/ is more sonorous than the stop /g/, but nevertheless precedes it.

The longest and most substantial paper in the volume is Bettina Zeisler's "The Tibetan understanding of *Karman*: Some problems of Tibetan case marking". Case marking in Tibeto-Burman generally is quite problematic, primarily because in many and probably most languages of the family, it is determined by discourse-pragmatic factors as well as the more usual syntactic and/or semantic categories which we are accustomed to seeing reflected in case systems (see LaPolla 1992; 1995; DeLancey and Lowes 2007, *inter alia*). And Tibetan poses problems of its own connected with the case marking of non-subject core arguments, which follows a pattern reminiscent of but clearly different from the better-known phenomena of "differential object-marking" (Bossong 1985) and "primary/secondary object" systems. Zeisler focusses first on the interpretation in the Tibetan grammatical tradition of the Sanskrit term *karman*, which has generally been interpreted in the Western literature as equivalent to accusative. The problem is that in Classical Tibetan the so-called "dative-locative" form in question is used consistently for locations, goals, and recipients, but not generally for arguments that we would confidently recognize as true "patients". Zeisler suggests an original function of this form as marking "Targets" or goals (cp. DeLancey 2001). The final substantive section of the paper provides a wider Tibeto-Burman context for some of the Tibetan phenomena discussed. As seems to be typical of this author, the paper is rich in peripheral observations which are every bit as interesting as the main thread of the paper; examples here include the two-page footnote 12 on transitivity, and the very perceptive discussion of the inadequacies of the prevalent concept of "Patient" as a case role on pp. 81-3.

Languages other than Tibetan and Burmese are less represented in this volume than in its predecessor. Guillaume Jacques, one of a set of younger scholars who are at last providing us with extensive descriptive materials for the rGyalrong languages (e.g. Jacques 2004), gives us a very useful "Essai de comparaison des rimes du Tangoute et du Rgyalrong". This article presents a brief but sufficient outline of Tangut studies, as introduction to a list of several hundred comparisons of evident cognates between Japhug

rGyalrong and reconstructed Tangut, arranged according to their rGyalrong rime, with corresponding Tangut character and phonological reconstruction according to Gong Hwangcherng. This kind of work (of which we need a great deal more) is obviously of incalculable value to the reconstruction of both Tangut and of Proto-Qiangic.

Kazue Iwasa's "Mamuteyi: Lolo Manuscript No. 6 (2) in the Library of the École Française d'Extrême-Orient, Paris", is a philological analysis of the manuscript named in the title, concentrating on two grammatical particles, *li*<sup>33</sup> and *na*<sup>33</sup>, which correspond neatly to, and can thus be identified with, two topic marking particles in contemporary Liangshan Lolo.

Two articles dealing not with facts of particular languages, but with more general issues in Tibeto-Burman, are the least successful in the volume. Vadim B. Kasevich, "The category of causative in Tibeto-Burman languages and the Iconicity Principle" devotes 5 pages to a general discussion of causativity, mostly rehashing (without attribution or serious discussion) a set of issues which were largely exhausted during the field's paroxysm of interest in causatives during the 1970's—and then one page to a desultory discussion of one construction in Eastern Sgaw Karen (illustrated by two example sentences), and one half-page-long sentence to two examples of lexical causatives derived by tone change, one from Burmese and one from Lhasa Tibetan. Since both examples are cited in their modern forms, this article actually makes no reference whatever to any "medieval Tibeto-Burman language". The conclusion seems to be that the Iconicity Principle which maintains that a causative form will always be more marked than a corresponding simplex form (no source is cited), is not uniformly and without exception true of every datum from every Tibeto-Burman language. This conclusion is both intrinsically likely and empirically self-evident enough that it can hardly be doubted, but this article presents little evidence or argument for it.

The introductory chapter to the volume, Beckwith's "Introduction: Toward a Tibeto-Burman theory" is longer than Kasevich's paper, but in the end not that much more substantial. From the title and the first few pages, this seems to be conceived as a complaint about the sorry state of the contemporary field of comparative Tibeto-Burman:

[W]hat has been missing in the Tibeto-Burman field is a solid, precise theory that would connect the languages together in their presumed 'genetic' Tibeto-Burman family relationship: an outline of comparative Tibeto-Burman phonology, morphology, and syntax, with a historical model of chronological and regional divergence. (p. 1)

There is not really any news here. Leave out a few contentless emotive flags like "solid" and "precise", and this basically amounts to saying that there's a lot of work left to do in comparative Tibeto-Burman. After all, for how many large families of significant time depth do we actually have a "solid, precise" theory which would satisfy this demand? Beckwith specifically states as an ideal that our understanding of Tibeto-Burman should one day "rise to the level of, for example, work on Bantu or Salishan languages" (p. 4), but Salish, at least, is a ridiculously inappropriate comparator. The Salish family

consists of about two dozen languages, for most of which we have lexical and grammatical documentation adequate for comparative work. Given that there are at least ten, perhaps more like twenty, times that many TB languages, a substantial majority of them still virtually or completely undocumented, the task of bringing order to the Tibeto-Burman field is greater than for Salish by an order of magnitude.

The whole first section is simply a string of dismissive statements with no actual facts or claims included:

Because ‘Sino-Tibetan’ has long been known to be characterized by irreconcilable differences between the Tibeto-Burman and Chinese (Sinitic) branches—encompassing phonology, morphology, syntax, and the lexicon—it can only be characterized as a ‘distant relationship theory’, i.e. speculation. (p. 2)

And, as this quote implies, the paper is primarily concerned not with Tibeto-Burman, but with Sino-Tibetan, and the question of whether the relationship between T-B and Chinese is genetic or simply a reflection of contact. Beckwith’s earlier paper on this topic (2002b) included a set of serious etymologies for putatively Sino-Tibetan etyma which occur also in neighboring languages, and thus might be areally rather than genetically shared by TB and Chinese. The present paper, alas, has nothing of the sort, and indeed presents no Tibeto-Burman data at all.

I can deal here with only one of Beckwith’s points. In perhaps the most regrettable part of the paper, the two-page section on “Syntax” (19-21), he attempts to resuscitate the long-dead notion that the differences in basic word order between Tibeto-Burman and Chinese constitute some sort of compelling argument against the idea that they could be genetically related. Beckwith expresses considerable scepticism about the possibility of word order change, adducing millenia of consistent SOV order for Tibeto-Burman (although we may note that this argument only holds water if we accept as an assumption precisely the genetic unity of TB which is the basic issue here) and of SVO for Chinese as evidence against the “deception” (p. 20) which counsels us ignore basic constituent order in comparative work.

But it is quite well-known that basic constituent order is changeable over time, typically under areal influence, and this conclusion hardly rests on claims about Tibeto-Burman. The substantial body of work produced in the 1970’s and thereafter on word order change was not all illusory; the literature provides ample documentation of the phenomenon. But there is really little need even to go to the literature: it is sufficient simply to note the basic order of Irish, English, and Hindi—if these descend from a common ancestor, then at least two out of the three have drastically changed their basic word order in the meantime. And the same is true of Indo-Iranian Panjabi (SOV) and Kashmiri (SVO), Semitic Arabic (VSO) and Amharic (SOV), Austroasiatic Munda (SOV) and Mon (SVO), and so on.

In any case, it is clear, and he makes no bones about it, that Beckwith knows little or nothing about syntax, so he closes this section with the plea that “a syntax specialist

focus his or her research on the question”. Well, there are many different sorts of “syntax specialist” in the modern world, and I am afraid not all of them will have anything useful to offer to comparative studies. As someone who has dabbled a bit in this area, let me here express my opinion that pure syntax—i.e. patterns of combination of grammatical categories—is of little use in diagnosing or demonstrating genetic relationship, and that basic constituent order patterns are of no use at all.

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