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Verb Agreement, Head-Marking vs. Dependent-Marking, and the 'Deconstruction' of Tibeto-Burman Morpho-Syntax[*]

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0. Introduction   Since the mid-1970's, the question of whether or not a verb agreement system[1] (VAS) should be reconstructed for Proto-Tibeto-Burman (PTB) has been a controversial topic, but because of the large amount of work published arguing in favor of reconstructing a VAS for PTB, especially by James J. Bauman (1974, 1975a, 1975b, 1979) and Scott DeLancey (1980, 1983, 1988, 1989, to appear), many people have begun to accept the existence of a VAS in PTB as received knowledge.   In a recent paper on verb agreement systems in Tibeto-Burman, Scott DeLancey states that 'There can no longer be any serious doubt that a system of verb agreement must be attributed to Proto-Tibeto-Burman (PTB)' (DeLancey 1988:1).   Though the number of papers supporting this position is quite large, I would like to raise several serious doubts about the theoretical and methodological basis for reconstructing a VAS for PTB, and at the same time argue for the use of functionally and typologically based theories of grammar, as exemplified by the head-marking/dependent-marking distinction developed in Nichols 1986, in diachronic syntax and syntactic reconstruction.

The essential characteristics of the VAS that the PTB-VAS proponents argue for are, according to DeLancey (to appear, p. 6), 'the personal suffixes 1st person *-ŋa*, 2nd person *-na*, and a split ergative agreement pattern in which agreement is always with a 1st or 2nd person argument in preference to 3rd, regardless of which is subject or object.'   In fact, we do not always find these same suffixes even in those languages that do have suffixal VASs, and in many languages the agreement system is prefixal.   I also do not agree that PTB was ergative, or even that agreement with speech act participants (SAP) over non-SAPs constitutes ergativity, split or otherwise.

1. Geographic/Genetic Distribution   An argument often made in favor of a PTB-VAS is that 'this pattern is manifested in at least one language in every recognized subbranch of the family except for Lolo-Burmese and Karen' (DeLancey 1988:1).   This is not as solid an argument as it may seem.   As Thurgood (1984b:3) points out, 'Tibeto-Burman subgrouping is in its infancy; not only does the composition of lower-level units still pose numerous questions, but the composition of higher-level units remains almost completely open.'   With the large number of languages in TB (Bauman 1979 puts it at over 200), the 20 or so languages that have VASs are nowhere near a majority, and almost all of them are in the Rung (Thurgood 1984a,b), Kiranti, or Kuki-Chin-Naga branches of TB. The possibility that these languages form a higher-level grouping cannot be dismissed.   For example, Ebert (1988) has argued for a Kiranti-Rung genetic

grouping. I will not argue further the question of higher-level genetic affiliation, but will look at other factors.

These languages are almost all geographically contiguous, forming a ring around the edge of the Tibetan plateau from northwest China down along the southern edge of the plateau, including the Himalayan region, forming what Sun (1983, 1985) refers to as an 'ethnic corridor', an area of large-scale language contact, multilingualism, and mutual influence, and a path along which many of the nationalities moved when they migrated south. Language contact, shared innovation within a subgroup, or a combination of the two then all are possibilities, yet Bauman (1974, 1975a) gives only the following possibilities for the development of the TB VASs: native (i.e. PTB) development, borrowing from Munda (an Austroasiatic group), borrowing from Indo-Aryan, and the Turanian hypothesis (the idea that all of central and eastern Asia's languages except the Indo-European ones are related). He states that '[n]o other possibilities seem forthcoming, with the doubtful exception of independent innovation wherever the feature appears.' (1974:118). The other logical possibility, and the one that seems most likely to me, that one or more languages in the family innovated a VAS and it spread geographically (possibly aided by similar features in local non-TB languages), has never been explored in any of the literature arguing for a PTB-VAS. Only Thurgood (1985) comes close to recognizing this possibility, in a discussion of subgrouping (p.378, fn. 4):

> ... many similarities between closely-related languages are what Sapir [1921/1945, Ch. 8] called "drift"; that is, the common starting point provided by a common origin often conspires with universal tendencies to provide parallel but historically quite independent paths of development among genetically related languages. The picture is further complicated by the areal convergence produced by wide-spread multilingualism. Finally, the detection of borrowing is more difficult between related languages.

Later in the same work, in a bracketed note, Thurgood's tone is a bit stronger: '[Note: it is already clear that at least some of the innovation patterns here are due at least in part to parallel but independent development.]' (p.399).

Throughout South and Southeast Asia we see the spread of areal features (either through outright borrowing or by (morphological) calque) of all types, such as tone systems, phonetic inventories, noun classifier systems, double causativization, and word order patterns, yet nowhere other than in Thurgood's footnote is this possibility mentioned.[2]

2. Time Depth Those languages that do not have verb agreement systems, possibly 90% of all TB languages, have *no trace whatsoever* of ever having had one.[3] These languages include four of the five languages for which we have writing systems more than two hundred years old, the best examples being Tibetan (7th cen.) and Burmese (13th cen.). It is highly unlikely that Tibetan and Burmese

would both have lost every trace of their VASs while Tangut (12th cen.) had a totally regular, etymologically transparent VAS that shows no signs of age. In Tangut (Kepping 1975, 1979, 1981, 1982) the agreement morpheme marks that SAP (i.e. 1st or 2nd person) most affected by/involved in the action of the predication:

SUBJ	OBJ	PRONOM. CLITIC		INTRANSITIVE		FREE PRONOUNS	
1	2	-na		1sg.	-ŋa	1sg.	ŋa
1	3	-ŋa		2sg.	-na	2sg.	na
2	1	-ŋa		3sg.	Ø	3sg.	Ø
2	3	-na					
3	1	-ŋa					
3	2	-na					
3	3	Ø					

Agreement is with the SAP even when the SAP is the indirect object or the possessor of the object if it is the only SAP in the sentence (Kepping 1982). These facts make it clear that agreement is related to SAP affectedness, and not grammatical function. This system also was optional, and does not seem to have been used in anything like the majority of clauses even in the Tangut texts that Kepping studied. Kwanten (1982) in fact could not find any trace of it in two Tangut texts he studied.[4]

The etymological transparency of the VASs (the independent pronouns become attached to the verb) and their clear discourse function marking the most salient speech act participant[5] (Ebert 1987; DeLancey 1981) show that they are relatively recent grammaticalizations of discourse prominence.

A possible example of evidence within the history of one language[6] for the development of a VAS is the Singpho dialect of Jingpo, mentioned by DeLancey (to appear, p. 22) as a case of how rapidly a language can completely <u>lose</u> an agreement system. This dialect is 'spoken well to the west of the other dialects', and 'the time of separation of Singpho from its eastern siblings can hardly be even as much as a millennium' (p. 22). It seems to me more likely that that dialect, out of range of the areal features to the east, never developed a VAS at all. If this were the case, it would give us a time depth of less than 1000 years for the development of the VAS, just what we would expect judging from the Tangut data.

3. Theoretical/Methodological Considerations

3.1 Reconstruction methodology The discussion of Tangut points up a difference in methodology between myself and most of those supporting a PTB-VAS: DeLancey, Bauman, LaRaw Maran (e.g. 1978) and others reconstruct the most complex system possible based on the data from a few languages, and consider those languages that have the most complex systems, such as Gyarung, as the most conservative (DeLancey, to appear, p. 7). For example, Bauman

(1974:134) suggests that a complex system such as that for Nocte, with a tense-aspect split, is closer to the original PTB VAS than a simpler system such as that of Tangut or Kham, languages which would supposedly have 'leveled out' the tense-aspect system.

I feel that we should reconstruct only those features for which we can show no clear line of development, i.e. opaque = archaic; we should reconstruct only those shared patterns for which we can find no motivation.[7] This is what I mean by the term 'deconstruction' in the title of this paper: morphology is built of grammaticalizations (cf. Hopper 1987; Thompson 1988), so we should strip back the layers of grammaticalization from the grammar until we can go no further. What is left is what we should 'reconstruct'. In this way it could be said that we do not *re*construct a language's morphological system, we *de*construct it.

3.2 Grammaticalization The methodological difference just mentioned also highlights a difference in the understanding of the way grammaticalization works. I follow Lehmann (1985) in assuming that grammaticalization involves the `attrition' (loss of integrity) of a sign, so that as grammaticalization progresses, there is a lessening in the phonological and semantic weight (including demotivation) of a sign so that the stages will be as follows (Lehmann 1985:309):

lexically		free		clitic		agglutinative		fusional
empty	>	personal	>	personal	>	personal	>	personal
noun		pronoun		pronoun		affix		affix

Along with attrition there is the concomitant `paradigmatization', `obligatorification' (loss of paradigmatic variability), `condensation' (reduced scope), `coalescence' (increased bondedness), and `fixation' (loss of syntagmatic variability) (Lehmann 1985:305-309). We see advanced stages of all of these processes in the complex VAS languages, the prime example being Jingpo (Maran 1978). This is part of the reason why I feel that among the VASs that do exist in TB languages, Tangut should be considered the most archaic and least grammaticalized. Maran (1978), on the other hand, argues exactly the opposite position, that morphology is 'stripped loose' and then becomes lexicalized, and that the highly complex and rigid morphology of Jingpo is closer to the archaic pattern. Bauman (1974:137) has all of the transitive suffixes 'initiating in a syncretic system', and tries to show how non-syncretic affixes develop from syncretic ones.

3.3 The Question of Ergativity [8] It does not seem proper to me to speak of, for example, the Tangut VAS as an ergative or split ergative system, as it is clearly not marking semantic role or grammatical function, but simply discourse prominence. Even Kepping, who supports the idea of PTB ergativity, says that 'verbal agreement too [as well as noun marking[9]] gives us no grounds for assigning Tangut to either the nominative or the ergative type.' (1979:267). If we

accept Du Bois' (1985, 1987) association of absolutive marking with the information status 'new' and accusative marking with discourse pressures to mark the topic, then this should be seen as an accusative system rather than an ergative one, since these clitic pronouns are typical of the most unmarked topics (Lambrecht 1986). A similar line of reasoning is followed by Givón (1980), who considers ergative morphology to be semantically based on the contrast of agent vs. non-agent, which is in conflict with discourse-pragmatic pressures towards the pragmatically based nominative morphology.

The type of agreement system we are talking about here is very clearly one based on person rather than clause syntax or semantics. This gives us no grounds for assuming ergativity in the proto-language. An interesting extension of the classic paradigm in Dulong (a phonologically very conservative language — LaPolla 1987) gives us further evidence that semantic role and grammatical function are not the main factors in the oldest TB agreement paradigms. In general, the pattern of agreement in Dulong (Sun 1982) is similar to that in Tangut in that agreement is with the most affected SAP, regardless of semantic role or case, but the marking is a bit different. There is a prefix, *nɯ-*, that is generally considered to be the reflex of the PTB 2nd person pronoun **na*. The problem for those attempting to reconstruct a VAS for PTB is that this prefix is also used in clauses where no 2nd person argument is involved. This same pattern occurs in several other languages as well, such as Dumi Rai (though with an *a-* prefix) and Rawang (a language closely related to Dulong, but which has an *e-* prefix). This leads DeLancey (to appear, p. 33) to remark 'it is impossible (as far as I, or others who have looked at the individual languages, can see) to assign any coherent function to the prefix in any of the languages' (i.e. Dumi Rai, Rawang, and Dulong). If we look carefully at the distribution of this prefix, we can see that it occurs only and in every case where a SAP is involved (as an argument, oblique, or possessor), but the speaker is *not* the agent of the clause. This innovation could be seen to be the beginnings of a role-marking system (though marking what role something *isn't*, rather than what role something *is*), but the basis for agreement is still affectedness, as can be seen from the fact that in some cases where both SAPs are involved in a clause the agreement can vary depending upon whose involvement the speaker wants to emphasize (Sun 1982:93-94).[10]

Another language where the basis of agreement is still pragmatic is Hayu. Boyd Michailovsky (1988:111-113) has shown that the verb agreement system in Hayu is clearly not ergative (though the language has ergative marking on the nouns), as agreement is with whichever argument is highest on the person hierarchy 1 > 2 > 3, *regardless of case role*.

In terms of methodology there is also the problem that in most of the papers on TB VASs, (Sherard 1986 is a welcome exception), comparisons are done on highly simplified and selected parts of total agreement systems, and little is said of how the affixes are really used. For example, Bauman (1979:423) gives neat paradigms for Vayu and Chepang, comparing the intransitive subject and transitive

object suffixes of each language to show how ergative they are, with only a parenthetical aside mentioning that these correspondences only hold when the subject of the transitive clause is 3rd person. As we have seen in Tangut, the basic pattern of agreement is with any SAP in the sentence, regardless of role, if the other participants in the clause are non-SAPs, clearly a pragmatic rather than a structural principle. This type of paradigm comparison then is of no use in trying to prove ergativity.

Nichols (1986:114) has suggested that '[h]ead-marked patterns contribute to a flat syntax which minimizes intra-clause and inter-clause structure, freeing a language to concentrate on the grammaticalization of discourse prominence and cohesion. In fact it turns out that it is precisely for head-marking languages that a number of traditional grammatical questions prove to be somewhat moot, because pragmatic and discourse relations (rather than strictly syntactic relations) are being grammaticalized.' As the older agreement systems are clearly pragmatically-based grammaticalizations of the discourse prominence of SAPs, there is no justification for reconstructing an ergative system of agreement for PTB.

4.0 Head-marking vs. Dependent-marking Nichols 1986 outlines the facts and implications of head-marking vs. dependent-marking morphology based on a careful survey of sixty languages. Nichols did not make reference to any languages in TB, but the bulk of TB languages (those without VASs) are solidly dependent-marking; those languages with VASs, a type of head marking, also have many dependent-marking features. The question, then, is which is older, the dependent-marking type or the head-marking (actually mixed) type? Nichols found that in several respects 'head-marking patterns appear to be favored and universally preferred' (p. 101). She suggests that based on her study, '... in the event that we have two clearly related languages with clearly cognate morphology, one of them strongly head-marking and one strongly dependent-marking, we should reconstruct the dependent-marking type' (p. 89). This then is one typological argument for not reconstructing a VAS for PTB. Two further arguments, also based on typological data, support this view.

4.1. The Continuum of Marking Types There is a continuum across the TB VAS languages in terms of the strength of head-marking. We can see for example the beginnings of head-marking in Angami Naga (Giridhar 1980), where only kinship and body-part terms are head-marked for possession (and only certain stative verbs have person agreement), and its full development in Gyarong (Qu 1984), where all nouns (and verbs) can be head-marked. This is in concord with Nichols' observation that the development of head-marking of nouns for possession will begin with cases of inalienable possession. We see the same process of dependent- to head- or double-marking (and not the opposite) through cliticization of pronouns occurring in other language families, such as the Oregon Penutian groups (Silverstein 1979), and the Pama-Nyungan languages of Australia.

In the latter, just as in TB, there is 'cliticization of pronouns, ... loss of core cases, and expansion of the head-marked treatment of inalienable possession' (Nichols 1986:99).

4.2. The Development Process There are many ways for head-marking to develop: 'they may arise as isolating languages become agglutinating, and pronouns are cliticized to verbs ... or they may develop from dependent-marking languages, through migration and clisis' (Nichols 1986:88). It is just such cliticization of pronouns to verbs that we see in the TB VAS languages. We can see the development of very similar VASs in other parts of Asia (e.g. in Turkic and Mongolian languages — Comrie 1980a, and in eastern Siberian languages — Comrie 1980b), and in North America and Australia, as mentioned above. Dependent-marking, on the other hand, evolves only 'through extensive use of boundary shifting ... so that the adposition becomes an affix on its former dependent', as occurred in the western languages of the Uralic family (Nichols 1986:88). We see no evidence of this process in TB morphology. In fact in a language such as Written Tibetan, the occurrence of modifiers between the head and the postpositions would effectively block this type of reanalysis. The dependent-marking system, or at least a non-head-marking system, must then be the original pattern. Bauman (1979:430) suggests that there is a drift away from what he has defined as ergativity, but not towards accusativity, rather towards 'non-ergativity', as there are no unequivocally accusative TB languages. He sees this 'non-ergativity' as the endpoint of historical change in TB. I would propose the opposite: that TB began as a morphologically simple 'role-dominated' (Foley & Van Valin 1977) language (similar to Chinese (LaPolla 1988a,b), with which we must eventually link it), whose daughter languages later developed various means of coding either pragmatics (Tangut), grammatical function (Kuki-Chin), or semantic role (Tibetan — Andersen 1987), or some combination of these three.[11] For me, then, the typical Lolo-Burmese role-dominated system is closest to the original PTB system of grammatical relations, rather than being the most degenerate, as assumed by those supporting a PTB VAS.

5. Conclusion I have here argued, using the question of Proto-Tibeto-Burman agreement system as an example, that in doing morphological reconstruction, what we should do is not build up morphological systems, and often end up engaging in 'paradigm stuffing', but in a sense we should *deconstruct* the systems that we have evidence for, to strip back the layers of transparent grammaticalization to arrive at an opaque core. Typologically and functionally based theories which point out the direction of grammaticalization allow us to do exactly that.

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[1]By 'verb agreement system' I am only referring to the marking of participants in the clause with clitic pronouns, not to evidential systems like that in, for example, Lhasa Tibetan.

[2]Bauman (1974:144) does mention areal (Lolo-Burmese and Barish) influence as a possible reason why some VASs *don't* have the complex number distinctions that other languages have. Those without such distinctions would supposedly have 'leveled out' the distinctions because of contact with the morphologically simpler languages.

[3]By 'trace' here, I mean either some remnant of an originally full system which no longer has any agreement functions, possibly some phonological alternation in the verb stems, or unexplained verbal suffixes (cf. Wolfenden 1929 on the possibility that some TB verbal prefixes were originally pronominal).

[4]Two other Tangut scholars, Nishida (1964-66) and Sofronov (1968), have also analyzed Tangut as a non-pronominalizing language (both cited in Kepping 1975 and Kwanten 1982).

[5]The coding of speaker-hearer involvement is marked in various ways aside from this particular agreement pattern in many TB languages; see for example Caughley 1980, Toba 1980, and Watters 1980 for three different systems within Nepalese TB languages.

[6]There is one other case, that of Tiddim Chin (Henderson 1957), but I am not sure what to make of it. In Literary Tiddim Chin there is no trace of the proposed PTB suffixal agreement system, though there are pronominal prefixes for both nouns and verbs. Colloquial Chin, on the other hand, prefixes nouns, but suffixes verbs, as in Kiranti. The puzzle is that rather than assuming the formal literary style to be more conservative, Henderson seems to be suggesting that the colloquial style is more conservative, as she suggests that perhaps 'pronominalization is after all a genuine Tibeto-Burman family trait' (p. 327).

[7]Cf. the following quote from Meillet (Watkins 1969:17), pointed out to me by Gary Holland:
La grammaire comparée doit se faire en utilisant les anomalies — c'est à dire les survivances — bien plus que les formes régulières ... Les traités de grammaire comparée ont souffert de ce que, pour la restitution de l'état initiale, l'importance attribuée aux formes normales des états de langue historiques est trop grande.

[8]I am taking as the minimum definition of ergativity a system in which the S & O roles are consistently marked the same way (possibly zero) while the A role is marked differently, *because of the fact that they are those roles*. I am dealing here only with marking on the verb, a type of head-marking, not the marking of ergativity on the NP, a type of dependent-marking.

[9]There is an optional 'special marker' after the NP that is the agent in certain constructions, but it is still semantically transparent as literally meaning 'do action' (Kepping 1975:226-7), so I do not think it represents any trace of the supposed PTB ergativity, especially as there is also a topic marker for intransitive subjects and an unrelated accusative marker for transitive objects which can be used in the same clause as the agent marker. All of these markers are used only when the context would be ambiguous without them (Kepping 1979:272).

[10]As Martine Mazaudon has pointed out to me (pers. com.), the possibility of choice such as that in Dulong might be more common in TB languages than has been noted, as linguists are often loath to report variation that would confuse a nice neat system!

[11]The development of the *nɯ-* prefix in Dulong (see §3.3) is also consonant with this hypothesis.

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