Once Again on Person Marking in Tibeto-Burman: A Reply to DeLancey 2010

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Background:
Arguments for reconstructing a set of person marking suffixes on the verb to Proto-Tibeto-Burman:

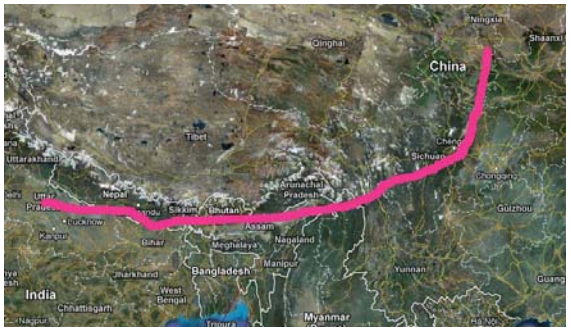
Scott DeLancey (1980*a*, 1980*b*, 1983, 1988, 1989*a*, 1989*b*)

Essential characteristics of the suffixal system, according to DeLancey 1989*b*: 317:

"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 person, regardless of which is subject or object."

LaPolla 1992 (first presented in draft form as BLS paper in 1989) presented factual, methodological, and analytical problems with both reconstructing the system to PTB and analyzing the extant systems as ergative:

- The number of branches manifesting the supposed cognate system is nowhere near a majority of the branches of the family (also pointing out the problematic nature of subgrouping in TB).
- The languages with the supposedly cognate verb agreement systems form a ring around the edge of the Tibetan plateau from north-west China down along the southern edge of the plateau, across to Nepal and the Western Himalayan area.



- The oldest written languages, Tibetan (7th century), Burmese (12th century), Newari (14th century) and Yi (Lolo; 16th century) do not show evidence of the pattern. Tangut (12th century), on the other hand, has an optional, morphologically simple, etymologically transparent verb agreement system that shows no signs of age.
- Discussed grammaticalization theory (Lehmann 1985) and argued the Tangut system must have been a recent grammaticalization at the time it was recorded, as the pronominal suffixes are exact copies of the free pronouns.
- Showed how the system found in Tangut and many of the modern languages is hierarchical, not split ergative. There is in fact no split ergative person marking system in the family.

- Argued that rather than reconstructing a system that tries to incorporate all of the modern features, 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. 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 historical linguistics, one does not reconstruct an obvious grammaticalization to the proto-language.

LaPolla 1992:
"We then have, aside from the Proto-Tibeto-Burman verb agreement system hypothesis, three other possibilities: (*a*) those languages with verb agreement systems are genetically related on a higher level; (*b*) a verb agreement system independently developed in one language and spread geographically; or (*c*) some combination of innovation within two or more subgroups and geographic spread or drift occurred. It is this last possibility that seems most likely given the fact that not all of the systems we find are of the same type (Thurgood, 1985: 337; Caughley, 1982: 206; DeLancey, 1989*b*: 315)."

In terms of the direction of development, I referred to Johanna Nichols' work on head-marking vs. dependant marking. Nichols did not make reference to any languages in Tibeto-Burman, but all of the Tibeto-Burman languages that do not have verb agreement systems are solidly dependent-marking (i.e., they have marking on the nouns for case or pragmatic function); those languages with verb agreement systems, a type of head marking, also have many dependent-marking features (of the same types as the non-pronominalized languages). The question, then, is which is older, the dependent-marking type or the head-marking (actually mixed) type?

Based on a separate survey of 86 languages in fifteen families, Nichols found that morphological marking type is 'a conservative, stable feature in languages' (p. 89), such that almost all of the changes she found in the groups she studied 'involved accommodation to areal patterns' (p. 98). The most common change she found was the development of head-marking (as in the clisis of pronouns in Romance). Nichols found that in several respects 'head-marking patterns appear to be favored and universally preferred' (p. 101). She suggests that based on her studies, '... 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). As this is the situation we have in Tibeto-Burman, we then have a typological argument for not reconstructing a verb agreement system for Proto-Tibeto-Burman.

There is a continuum across the pronominalized Tibeto-Burman 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 rGyalrong (e.g. Qu, 1984), where all nouns (and verbs) can be head-marked. This is in concord with Nichols's 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 Tibeto-Burman, there is 'cliticization of pronouns . . . and expansion of the head-marked treatment of inalienable possession' (Nichols, 1986: 99).

Thurgood (1985: p.378, n. 4) argued that 'many similarities between closely-related languages are what Sapir [1921/1945, ch. viii] 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.'

I then did a follow-up paper (1994; first presented at 1992 ICSTLL) investigating parallel innovations in the family, including person marking, agentive and anti-agentive marking, causative marking, directional marking, and semantically distinguished existential verbs. The following is some of the parallel innovations in person marking.

The earliest example we have of person marking in TB is in Tangut, a dead language in which there are texts dating back to the eleventh century. In Tangut the optional verbal suffixes have the same phonetic form, including the tone, as the free pronouns (adapted from Kepping 1975, 1979, 1981, 1982, 1989; there is also a 1st and 2nd person plural marker *ni*$^{\cdot 2}$, third person is not marked):

Table 2: Tangut person markers and free pronouns

	FREE PRONOUNS	VERB SUFFIXES
1sg	ŋa^{2}	-ŋa^{2}
2sg	na^{2}	-na^{2}

In the Kuki-Chin branch of TB we find a person-marking system very similar to that in Tangut. In this system we find the Proto-Kuki-Chin pronouns

**kai* '1sg', **naŋ* '2sg', and **a-ma* '3sg'

grammaticalized into the person marking prefixes

**ka-, *na-,* and **a-* respectively (Thurgood 1985).

Yet from the fact that the system is prefixal, and the fact that the pronouns that were the source of the prefixes are not the same as the Tangut forms (at least the 1sg and 3sg forms), and from the fact that the languages are not closely related, we can say that this system clearly developed independently of the Tangut system.

A third case of clear independent development is the person marking system of Angami Naga (Giridhar 1980), which involves prefixes clearly derived from the independent pronouns. The verbal prefixes are also isomorphic (except for the tone on the 1st person prefix) with the pronominal genitive noun prefixes (p. 22ff):

Table 3: Angami Naga person markers and free pronouns

	FREE PRONOUNS	VERB PREFIXES	NOUN PREFIXES
1sg	ā	ā-	â-
2sg	nō	n̂-	n̂-
3sg	puô	puô-	puô-

Again we see that not only is this a prefixing system, unlike the Tangut system, but it also derives from a set of free pronouns unique to Angami.

A fourth case is the person marking prefixes of Mikir (Hills Karbi; Jeyapaul 1987):

Table 4: Mikir (Hills Karbi) person markers and free pronouns

	FREE PRONOUNS	VERB PREFIXES
1sg	ne	ne-
1pl excl.	netum	ne-
1pl incl.	itum ~ etum	i- ~ e-
2sg	naŋ	naŋ-
3sg	alaŋ	a-

That this system is a recent development can be seen not only from the fact that the free pronouns and the prefixes are so similar in form, but also from the fact that the verb prefixes retain the inclusive/exclusive distinction of the free pronouns.

One last example is from the Delugong dialect of Sgaw Karen (Dai et al. 1991:400; third person is unmarked):

Table 5: Sgaw Karen person markers and free pronouns

	FREE PRONOUNS	VERB PREFIXES
1sg	ja^{33}	jă^{33}-
1pl	pu^{33} wɛ^{55} θe^{31}	pŭ^{33} kă^{31}-
2sg	na^{33}	nă^{33}-
2pl	θu^{55} wɛ^{55} θe^{51}	θŭ^{55} kă^{51}-

This system of verbal prefixes is very clearly of recent origin, being in the singular simply unstressed copies of the free pronouns, and unique to this dialect of Karen.

I also spent several years studying the history of migrations and their effect on the development of the family (published 2001).

Two major migration routes for the Tibeto-Burmans:

West and then south into Tibet and down into Nepal and Bhutan, forming the Bodish branch, and

South-west, along the river valleys down into Burma and across to India and Nepal.

The southern route was also supported by work in anthropology showing a shared cultural pattern along the route I had laid out as the migration route.

Because of the indeterminacy of genetic groupings in Tibeto-Burman, I tried to develop a more scientific methodology based on Nichols' 1996 article arguing for using statistical significance in assessing genetic groupings, essentially arguing that only paradigms of morphology (or fixed sets of other features) can provide the statistically significant evidence for a common source. I used a particular set of features which included certain features of the person marking paradigm, but other features as well, such as the reflexive/middle marking and particular lexical items such as *rung 'sit/existential verb', APPLIED AS A SET. In this way I identified a set of languages that had an overwhelming statistical probability of having come from a common source based on them all having THE ENTIRE SET of these shared innovations.

These results matched the results of the migration study, and also the anthropological studies of Huber and Blackburn (2011). So we have three independent studies, from linguistics, history and anthropology, all pointing to the migration pattern having been along the southern edge of the Himalayas.

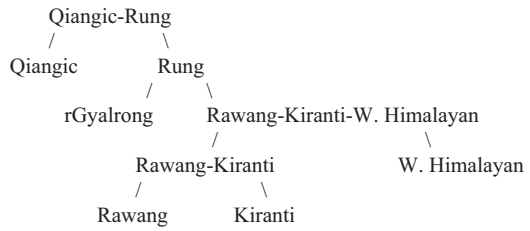
The results of this methodology were first presented at the 2000 ICSTLL, and summarized in my overview of ST morphology in the 2003 book *The Sino-Tibetan Languages*. At first I called this grouping the rGyalrong-Rawang-Kiranti-Western Himalayan (GRKW) group, but later felt this to be a bit unwieldy, and so adopted Thurgood's (1984) term "Rung", even though the grouping I defined was not the same as his and was not posited on the same evidence. I now see this change in terminology as a mistake because it has led scholars such as DeLancey to ignore the differences between my Rung and Thurgood's and criticize it based on Thurgood's conception of Rung.

Aside from the 2000 paper, which is to supposed to appear in an edited volume, I recently (2012) published an article in *Language and Linguistics* arguing for a more scientific methodology in reconstruction and subgrouping.

In an encyclopedia article (2005) I mentioned Rung as well, and expanded on the relationship of Rung to Qiangic:

"LaPolla 2003a, with reference to the morphological paradigms, argues that rGyalrong, the Kiranti languages (Bantawa, Athpare, Dumi, Khaling, Camling), Dulong-Rawang-Anong, Kham, and the Western Himalayan languages (Kinauri, Rongpo, Chaudangsi, Darmiya; also often grouped with Bodish) should be seen as forming a single higher-level grouping. This grouping was given the name "Rung", due to the similarity (but not identity) of this proposal to an earlier one by Thurgood (1985). The Rung languages most likely split off from an even higher level grouping with the Qiangic languages, then rGyalrong split off from the group as migrations moved south, then Western Himalayan split off from Kiranti and Rawang, and then these two groups split (see Figure 1; see LaPolla 2003a for the evidence)."

Figure 1: The subgrouping of Qiangic-Rung



DeLancey's most recent paper on this question (2010) misrepresents my views, claiming that I have changed my position and moved closer to his view, but I have not changed my view at all, and in fact have found much evidence to support my original suggestions.

The paper makes it seem as if DeLancey came up with the idea of the systems being hierarchical, and that he has now convinced me of that fact, when actually it was the other way around.

The paper misrepresents my approach and reasons for positing the Rung branch, and assumes that any language that has a velar nasal 1st person marker qualifies as belonging to Rung, and states that I posited Rung just as a way to put all of the languages with person marking into one branch, calling it a "grab-bag", but that is a major misrepresentation. This shows DeLancey has not understood or is ignoring my methodology.

The paper also misrepresents my view of the relationship between Qiangic and Rung, even though it sites my 2005 article.

But the main problem with DeLancey 2010 is that it still does not address the main objections I first presented back in 1989.

It instead argues there are only two issues of disagreement between us, (1) whether the Nocte and Jinghpaw systems are related to each other, and (2) how most of the TB family, including four of the five oldest written languages, lost the supposed system without a trace. The first one is irrelevant to my arguments, and seems to be due to a misreading of my work. But it is also problematic, as he says the two system are cognate due to shared innovations only those two systems share. How is that relevant to reconstructing PTB?

The paper attempts to explain the loss of the supposed systems in most of the family by arguing that since it is possible for the languages to lose such morphology, then they must have. But the argument that Tibetan had the supposed PTB person marking paradigm and lost it ignores the fact that if we want to say that we would have to say that Old Tibetan lost the person marking system (a supposed TB trait) without losing the earlier prefixes and suffixes, which are assumed to predate the split of TB and Sinitic. This is quite unlikely.

In the paper it is claimed that the supposed cognate pattern is found "through the length and breadth of the family" mentioning Western Himalayan to rGyalrong, but we have seen that that is a misrepresentation of the distribution. He says there is no way to explain that distribution, completely ignoring the evidence from migration history.

DeLancey tries to discount the evidence of parallel innovation by citing an irrelevant example (Sangkong) and then criticizing it as irrelevant. He does not address my own examples showing clearly parallel developments.

The paper misrepresents the work of other scholars as well, for example claiming that Sun Hongkai showed that Geman Deng has a person marking system derived from supposed the PTB system, but what Prof. Sun said was simply that the second person marker *-i* probably derives from the initial of the second person pronoun *ŋ-*. If this were true, it would mean it was a Geman Deng-internal innovation, and not part of the assumed PTB paradigm.

The paper also opportunistically cites older, less reliable sources rather than more reliable sources when it suits the argument.

In mentioning the Meitei reflexive marker as supposedly evidence that a language outside Rung has a form cognate with the reflexive/middle marker found in the Rawang-Kirant-Western Himalayan branches of Rung, DeLancey ignores the clear statement by Chelliah (1997:205) that the form is a transparent language-internal grammaticalization from the word for 'body'.

One of the arguments presented in the paper is simply that my view of genetic groupings in TB differs from everyone else's, so it must be wrong, but my results are different because my methodology is radically different from those who use word lists and/or geographic contiguity in establishing groups.

Those who want to say there are no isoglosses between TB and Sinitic, yet argue for a PTB agreement system, are positing a major isogloss.

"It is important to note that in arguing that the 'original' Proto-Tibeto-Burman verb agreement paradigm was quite complex (such as in fig. 1, below), and that those languages that have simpler systems (or no systems at all) have lost the 'missing' forms due to phonological attrition or levelling, those scholars are saying that Tangut inherited a complex system, yet through the process of phonological attrition and levelling distilled out a perfectly regular (i.e. morphologically simple), transparent system where the markings on the verb correspond exactly to the free pronouns in phonological shape. This type of teleological development seems to me a very unlikely possibility."

kɛ-				-a ~ -ŋ ~ -ŋa					-ni
2					1s		-u		2p
							3P		
a-	me-	VERB	-nši	-tɛ	-na	si		-si	-i
1	pA	STEM	REF	PT	2	dA		dP	12p
							-a		
ta-/na-					-nya		3		-k
marked					1s>2				1p
scenario									

Fig. 1: *Proto-Tibeto-Burman agreement system as reconstructed by van Driem (1993: 320).* (A = agent, d = dual, p = plural, P = patient, PT = preterit, REF = reflexive, s = singular, > = direction of transitive relationship; 1, 2, 3 = 1st, 2nd, 3rd person)

It also seems necessary for us to consider the relationship between Tangut and (at least some of) the modern Qiang languages, all of which have complex agreement systems involving tense/aspect and portmanteau morphemes. One or more of the Qiang peoples, particularly the Muya, have been said to be descendents of the Tangut (Ran, Li and Zhou, 1984: 184-5; Li, 1989: 222; see also Sun, 1991 on the relationship between the Qiang languages and Tangut). If the Muya language (Huang, 1985) is descended from Tangut, then to accept DeLancey and van Driem's view we would have to say that there was originally a complex system, Tangut then distilled out a simple system, and then that language again developed a complex system (presumably identical to, or at least cognate to, the old one). Again we have a very unlikely scenario.

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