

The human factor or when the evident is not seen: Language archaeology and fieldwork in Ladakh

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The unique and highly intransparent Old Tibetan verbal morphology is preserved only in the so-called ‘archaic’ dialects of Amdo (north-eastern Tibet). The similarly ‘archaic’ West Tibetan dialects of Baltistan (Pakistan) and Ladakh (India) have preserved much of the Old Tibetan phonology but apparently no trace of the complex pattern of prefixes, root consonant alternations, and *ablaut*. This has been taken as sufficient evidence to claim that West Tibetan descends from a stage of the language where the complex verbal morphology had not yet developed (Shafer 1950/51).

Among the set of prefixes, which may be either grammatical markers or lexicalised components in Old Tibetan, the *b-* prefix is of special interest. It is preserved in many Tibetan dialects in bound lexemes following an open syllable, particularly at the morpheme boundary of compounds. This feature was discussed for Ladakhi by Shawe as early as 1894. Furthermore, a number of Balti verbs show an originally grammatical *b-* prefix (Bielmeier 1998, in preparation). Despite this evidence in his own data, Bielmeier (2004) explicitly supports the Shafer hypothesis.

The reasons for the misperception may be manifold. On the theoretical side, the tree model of language divergence does not account for creolisation or multiple inheritance of languages and further leads to a ranking of varieties in terms of their position in the tree: the closer to the root the more important for the reconstruction of the proto-language. Other factors can be seen in the institutional settings (the increasing pressure for publication output with limited resources and the fact that superlatives, such as ‘most archaic variety’ are important for fund raising) as well as in the competition between different academic branches or schools. E.g. the harsh and over-political critics of Shafer’s pioneering work from the philologist and neo-grammarians side apparently triggered an attitude of solidarity among the comparative linguists working on the Tibeto-Burman languages, and thus the methodological problems of Shafer’s working hypotheses have never been addressed.

While this example could serve as a precaution against theoretical preconceptions that may serve as an unconscious and certainly unwanted filter against all evidence not in accordance with one’s assumptions, I would likewise want to emphasise that one cannot successfully conduct empirical fieldwork without a substantial set of theoretical expectations. Informants will usually talk about their language only in answer to explicit questions, and these can only be formulated on the base of one’s

present preconceptions. Holding on to one's prejudices can at times also help to get the relevant information against the first evidence by reformulating the question.

E.g. when investigating sentence patterns in Ladakhi, the informants unanimously rejected the use of the instrumental or the comitative case marker for the medium argument of a verb of filling as in Classical Tibetan *bumpa chu-s gaŋ* 'the pot is filled **with** water' (instrumental). One could have stopped here, but after some consideration I realised that I had checked only sentences where the container was filled with an every-day item such as water or grain. When I suggested a not so common content, the construction with the comitative case marker suddenly became acceptable: /zaŋsbu ser-**naŋ** gaŋseduk/ 'the pot had filled with/was full of gold'. What the informants from Lower Ladakh did not tell me at that stage, was that the classical construction with the instrumental case marker has, in fact, survived, although only in the disguise of the genitive marker (a construction borrowed from Upper Ladakh where the difference between genitive and instrumental case marker is neutralised): /dziŋ chu-**i** gaŋseduk/ 'the pond is filled up with/full of water'. By chance, this construction showed up during the transcription of one of the narrations, otherwise it might have remained unnoticed.

This leads to the problem how to get the evidence for which one has not looked. Besides a comparative (synchronic as well as diachronic) perspective, the combination of various methods (elicitation, transcription of narrations, participating observation), and a continuous self-reflection, it sometimes also matters to be at the right place at the right time (e.g. to find an originally grammatical *b-* prefix fossilized in a verb form that should not have had prefixes; preserved in only one verb, attested in only one village, and already in the process of being replaced by the 'regular' prefixless form, cf. Zeisler 2005), in other words: one needs a certain amount of luck.

Furthermore, the study of lesser-documented languages and particularly the situation in the field confronts the researcher with very basic practical problems and personal conflicts that at first sight do not seem to be relevant for the average linguist dealing with a well documented and well researched language. The way informants respond to questions may vary considerably from day to day due to their own personal situation (having spent the whole night with friends does not enhance the understanding of abstract questions the next day). The researcher thus has the additional obligation to monitor the condition of the informants, but of course only indirectly, in a highly sensitive way. The informants also might have different standards of serenity or politeness and might not be able to give a negative answer. Or they might not be able to formulate a sentence from the speaker's perspective, which alone could prove a certain semantic property, if such statement would be against the social norms (e.g. positively affirming that one had a certain negative attitude).

Informants of minority communities often lack an adequate education and thus might have difficulties with abstract questions. For the same reason they might not be very competent in a second language. But the use of a second language or an abstract level is imperative to establish the correct meaning of an expression. E.g. Ladakhi has sentence pattern variations similar to the *spray/load* type in English. In the case of the verb /pok/ ‘unload’, one gets three patterns: /khos rhta poks./ ‘S/he (ergative) unloaded the horse(s) (absolutive)’, /khos khur poks./ ‘S/he (ergative) unloaded the load(s) (absolutive)’, or /khos rhtekana khur poks./ ‘S/he (ergative) unloaded the load(s) (absolutive) from the horse(s) (ablative)’. Having discussed this example, I asked the informant whether one could find the same pattern with the verb /khal/ ‘load, send off’, thus especially if one could also say /boṅbu khals/ ‘loaded a donkey’ (in the sense of ‘loaded something on a donkey’). The informant answered in the positive: “Yes, of course!”, and I did not insist on a retranslation into English. When I had the opportunity to check the example with another informant, it turned out, that the first informant, despite my long explanations, had simply been thinking of the meaning ‘send off’ or of the possibility of loading a donkey on a truck.

The situation in the field can be seen as an extreme, or perhaps even as the worst case scenario, but it throws some light on the more general problems when dealing with depersonalised data, whether primary texts, secondary literature, corpora, or the statements of anonymous online-informants. In social anthropology, the reflection of the settings and methodologies, the position of the researcher in his or her own society and in the field, the chances and limits of the fieldwork takes a prominent place. The initial example shows the importance of including such reflections in the current metatheoretic discussion on linguistic evidence, not only for field studies.

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