TRANSITIVITY INDICATORS, HISTORICAL SCENARIOS, AND SUNDRY DENE-YENISEIAN NOTES

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These brief notes are a reaction to the extensive paper by Edward Vajda (this volume). From the outset I would like to mention those aspects of this paper that I do not feel myself an expert in. First, I am not a practicing historical-comparative linguist. Of course, I am familiar with the historical-comparative method and its application to several families, including Na-Dene. But my guess is that only someone who has done first-hand work in historical comparison and reconstruction can objectively assess the degree of rigor with which the comparison proposed by Vajda is implemented. Second, I have never studied Yeniseian and my acquaintance with this language family is rather superficial.

The fields of my expertise that are relevant to the paper in question are the following two. First, I have studied Athabaskan languages for many years and am familiar with certain languages belonging to all areal groupings of this family. Second, I am a typologist and prefer to assess any linguistic hypothesis not just in itself and by itself, but also from the point of view of its cross-linguistic feasibility. It is from these positions that I can offer some thoughts regarding Vajda's paper.

Vajda's paper includes two major parts. Section 2 is devoted to a comparison of Yeniseian and Na-Dene verb morphology, while section 3 deals with lexical comparison—an analysis of putative cognates, as well as sound correspondences. I will begin with some brief comments on the lexical part and then discuss the morphological part in more detail.

The lexical/phonological comparison (section 3) looks promising. Sound correspondences appear quite robust. My problem, however, is that this positive evaluation of mine is purely subjective. What are the objective criteria one uses when evaluating a distance relationship hypothesis? The assessments Vajda is using himself are of the following kind: cognates in basic vocabulary are "sufficient in number to establish systematic sound correspondences"; "a modest but sufficient number of lexical cognates (about 100 roots or simple words, so far)" (beginning of section 3); "a modest number of cognate compounds and derived words displaying structural and semantic parallels unlikely to have arisen through chance" (section 4). I have the following questions. How many lexical items are necessary to establish a relationship (even when stable correspondences are in place)? What can and what cannot be due to chance coincidence? At this time I do not have an answer to these questions, and I am not sure historical linguistics has them.

In historical linguistics, far too often attention is paid to sound alone, and a theory of what "semantic likeness" is supposed to be is left to pure intuition. Also, historical linguists are notoriously bold in their hypotheses on what semantic relations between cognates can be like. Against the background of such practices, I would like to emphasize that Vajda's comparisons most of the time involve roots of an identical or really close meaning, so his hypotheses are on the safe side from the semantic point of view. I noted just a few items that looked suspicious to me. For example, in subsection 3.5.2.7 PA *-la ‘point, end, hand' is connected to Ket words for ‘barb on the end of a fishing hook' and ‘string'. The shape of this morpheme is so cross-linguistically common, and the morpheme is so short, that assuming cognacy under such a shaky semantic relationship seems just too bold. By the way, in the recent years there are substantial efforts from several groups of typologists (Zaliznjak 2006, Croft et al. 2009) who are trying to establish an empirical semantic foundation for the search for cognates. Perhaps these efforts are worth attention of historical linguists.

One more comment is due regarding the lexical part of the study. In section 4 Vajda remarks that the attested cognates "include words for biota, natural history, and skill sets that specifically reflect hunter/gatherer life in the northern subarctic taiga forests". It is most likely indeed that both the prehistoric Na-Dene and the proto-Yeniseians resided in the Subarctic. However, the nature of the geographical zone linking these two ancestral areas is different. It is Beringia, that is the Arctic. No matter on which side of the Bering Strait could be the DY Urheimat, proto-Yeniseians or proto-ND could not have gotten from one taiga area to the other in a flash. There must have been some centuries spent in the Arctic. And if so, it would not be expected to see fully parallel flora, fauna, and economy vocabulary. (Cf. Johanna Nichols' (2008) counts of how long it might take in the prehistoric times to get from point A to point B.) Vajda actually suggests (section 4) that "these items are for the most part also congruent with subsistence in Arctic environments", but apparently this claim needs to be fully elaborated and confirmed with biological and geographical evidence, in order to become fully convincing.

Despite these reservations, I would like to repeat that the lexical/phonological part of the paper gives an impression of credibility. As Vajda himself points out, massive further work is in order that hopefully confirms the DY hypothesis.

Now I proceed with the morphological part of the paper (section 2). This is where knowledge of Athabaskan is much more essential for evaluating Vajda's suggestions, compared to the lexical part.

The most ancient Athabaskan prefixes stacking in front of the verb root form the following template: "mode" – 1 and 2 person subject – perfective – transitivity indicator (= "classifier"). Eyak and Tlingit largely share this structure, so it is safe to assume that it was established morphologically at the proto-ND stage. Vajda proposes that the Yeniseian has a congruent morphological structure and posits a number of specific comparisons between the Na-Dene and Yeniseian prefixes (see below). What bothers me most of all is that the ND transitivity indicators do not find a clear counterpart in Yeniseian: "there are no classifiers in Yeniseian, even though Yeniseian does possess morphemes cognate to some of the classifier components" (section 2.2.4).

In diachronic typology it is commonplace that morphology emerges from syntax (Givón 1971:413). This means that those affixes that are closer to the root froze from erstwhile function words into bound morphemes earlier than more remote affixes. Therefore, transitivity indicators (TIs), located in the immediately pre-root position, must constitute the earliest acquisition of the proto-ND inflected verb. However, there is no obviously comparable set of morphemes in Yeniseian, and, as a matter of fact, no comparable morphological position. Vajda (subsection 2.2.4) does offer several observations on possible Yeniseian cognates of the Na-Dene TIs, but these certainly do not qualify as a fully fledged counterpart of the TI position. My point is the following. If one proposes a homology of two not exactly identical morphological structures, one must also come up with a possible scenario of how differences between these structures historically emerged. I am not sure what scenario could fully explain the absence of the TI position in Yeniseian.

There are two alternatives on the timing of the putative DY relationship with respect to the TI position. First, what we know as the Na-Dene template could have emerged still at the proto-DY stage. In other words, the Na-Dene template could also be the proto-DY template. (Note that the valency-related functioning of TIs is strikingly similar across Athabaskan (see e.g. Kibrik 2008), both structurally and semantically, and is also similar in the rest of Na-Dene, and it therefore appears that the coherent system of valency marking through TIs evolved very early.) Under this scenario proto-Yeniseian supposedly must have had the same morphological system as well. If so, how could the TI position be missing in Yeniseian? There is a theoretical possibility that the TIs could have eroded through morphophonemic processes in Yeniseian, and then a specific diachronic machinery of such erosion needs to be postulated.

According to the second scenario, the Na-Dene template could have evolved after the split between Na-Dene and Yeniseian. Then, given that the TIs are missing from Yeniseian, it would be reasonable to assume that the TIs still were not a part of the inflected verb at the proto-DY stage. All the more so, the more remote morphological positions (including perfective, subject pronouns, and mode) could not have emerged at the proto-DY stage. There is no chance that the TIs could have wedged inside the inflected verb in Na-Dene and/or develop a new set of functions after Na-Dene split from proto-Yeniseian.

I am not sure which of these two incompatible scenarios is consistent with Vajda's theory. On the one hand, he seems to entertain the first scenario, saying (in section 2.2.4) that "it is likely that such consonantal prefixes simply elided before the consonant onset of Yeniseian verb roots" and that "in Yeniseian, the pre-root verb prefixes shown in Table 21 merged with the root to create the modern verb base". On the other hand, he goes on to say that the TIs "never developed the productive grammatical valence-change functions found in Na-Dene." Given this equivocality, I am not sure what the value is of similarities between the Na-Dene and the Yeniseian verb templates. I am afraid that, as long as the status of the immediately pre-root TIs is not clarified, morphological argument for the relationship largely fails.

To recapitulate this central criticism of mine, in order to convincingly put forward the morphological homology between the verb templates of the two families, one needs to offer a specific historical scenario, involving a timing of changes. Given the really fundamental difference between the two templates—the salience of TIs in Na-Dene and the conspicuous absence thereof in Yeniseian—the morphological parallelism between the two templates remains unconvincing.

I also have several more local comments on the morphological part of the paper that are worth mentioning, including the following.

- In the discussion of different verb templates in section 2.1 it is not exactly clear how their similarities or differences are assessed. It seems that the judgment is largely intuitive.
- The Yeniseian morpheme *n-*, the putative correlate of the Athabaskan classificatory element for roundish objects, appears too rarely ("in a tiny handful of verbs", section 2.2.3) to propose a connection for such a phonetically common affix. Moreover, some of the examples are questionable—in particular, by Athabaskan standards birchbark or rawhide (first example in (4)) qualify as flat rather than roundish objects. Examples for *d-* also seem to be too few for being conclusive.
- Highly unlikely seems the hypothesis that the Na-Dene *ł*-TI might be related to the homophonous instrumental/comitative postposition, connected to the preceding subject pronoun (section 2.2.4), for the reason that the pronouns appearing in the subject position and those attached to postpositions belong to two very different pronominal sets.

Generally, I feel that most of the hypotheses put forward in the morphological part of the paper are not yet sufficiently convincing. Probably the most plausible connection is that between the Yeniseian and Na-Dene telic and atelic aspect markers (section 2.2.1), assuming that they are corroborated by identical sound correspondences in the lexical materials (section 3.5.2). However, given Vajda's suggestion that these morphemes are originally auxiliary stems, their cognacy in the two families does not tell anything about the relatedness of verb templates per se.

One final comment. The paper abounds with specific proto-Athabaskan and proto-Na-Dene reconstructed forms. Sometimes the argument is not easy to follow because the sources of such reconstructions are less than obvious. On the other hand, the effort associated with the Dene-Yeniseian hypothesis apparently makes the Na-Dene data itself more accessible and brings them into focus. In this sense, this effort is beneficial for the Na-Dene studies even at the stage when the external connection is not yet sufficiently supported.

I hope that my criticisms can count as constructive and that some of them can help further develop this promising hypothesis about long-distance relationship.

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